The Relationship Between Targeted Social Assistance and Political Participation

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Abstract

Does targeted social assistance in developing countries affect participation at and beyond the ballot box? In the last 25 years, conditional cash transfer (CCT) programs have become popular welfare policies across Latin America, Africa, and Asia for combating poverty and building human capital. Despite some success in reaching these goals, CCTs sometimes receive criticism for encouraging clientelism and patronage politics. Additional questions also remain about the wider political effects of CCTs on individuals and communities. In this paper, I address both of these questions and find evidence (1) that CCTs make citizens more likely to participate in political life and (2) this effect on participation may be mediated only weakly through non-programmatic, clientelistic engagements. Using multi-level modeling, matching, and a regression discontinuity design with data from Latin America, I also show CCTs are robustly associated with higher levels of participation broadly conceived. Results also demonstrate gender of the recipient and institutional features such as enforced conditions and compulsory voting can attenuate the positive effects of transfers, depending on the form of participation considered.

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While welfare programs in Europe and OECD countries are more institutionalized and long-standing, welfare programs in the developing world face a double-sided problem of higher rates of poverty and fewer resources for governments to address that poverty (Iversen and Cusack 2000; Allan and Scruggs 2004; Starke 2006; Rudra 2002). Given this, in recent years conditional cash transfer programs (CCTs), a form of targeted social assistance, have become popular welfare programs to alleviate poverty poverty and boost social capital (Das, Do and Özler 2005; Witvorapong and Foshanji 2016; de Janvry et al. 2006). On the whole, these programs distribute operate by cash transfers to poor citizens conditional on recipients fulfilling basic health and education requirements (Fiszbein and Schady 2009). What role then do these programs have in influencing political behavior, and more specifically political participation?

Previous work has shown that government institutions, such as welfare programs, can be key predictors of whether or not individuals participate in political institutions at large (Soss 1999; Mettler and Soss 2004). With regard to CCTs, there has also been a great deal of work specifically examining the effects of cash transfer programs on support for the incumbent (Layton and Smith 2015; Hunter and Power 2007; Corrêa and Cheibub 2016; Bruhn 1996). Additionally, the technical design of these welfare institutions has a key part in influencing the propensity of individuals to participate. Specifically, more efficient organizations with greater positive impacts on the personal lives of the population are more likely to increase citizen participation.

Continuing with these lines of scholarship, this study presents evidence relying on cross-national survey data from Latin America and demonstrates that receipt of transfers from targeted social assistance programs are linked with increased levels of all forms of participation. Involvement in these programs is expected to increase participation because of (1) an increase in basic resources and (2) a change in the networks of involvement for those who receive transfers. These reasons roughly correspond to the facets of poverty reduction and human capital building that are part of the mission of the CCT programs. I also expect the effect on participation to be strongest for women because of how transfers are targeted towards women. However, this expectation is only partially supported in my results, with gendered effects of cash transfers affecting different dimensions of participation to varying degrees.

Furthermore, I investigate the role of institutions, specifically condition enforcement in CCTs and compulsory voting, in countries with targeted assistance programs with a series of multi-level models. On the former, I find surprising evidence of conditions enforcement largely attenuating the increases in participation associated with transfers. With regard to compulsory voting, using multi-level models and a regression discontinuity design I find some evidence of compulsory voting limiting participatory increases associated with CCT program recipients.

While there has been evidence of benefits for the poor from CCTs (Barrientos and Santibáñez 2009;

Adato and Hoddinott 2010; Leite et al. 2011; Lindert et al. 2007), some critics have purported CCTs promote fraud and clientelism. Concerning the second criticism of clientelism, this study approaches the relationship with clientelism as a more complicated dimension that is neither wholly positive or negative. Despite transfers possibly increasing instances of clientelistic behavior, clientelism may play a part in the networks of involvement for participants in CCTs. Although transfer recipients may be more likely to be involved in clientelism, clientelistic networks may in turn their help to boost their involvement in political life. This makes sense given the institutional connection provided by these welfare programs, as well as the capacity for local officials to become involved in distribution of goods and services. Results from a series of mediation models demonstrate a only a weak clientelistic dimension that does contribute statistically significant effects on all forms of participation, but is overall relatively small in magnitude when compared to the direct effects of the transfer.

Social Assistance and Behavior

The connections between welfare policy and public response have been supported well in the literature. Previous work has drawn connections between welfare policy offerings and effects on preference for incumbents and different parties. Multiple studies also discuss the electoral repercussions of cutting welfare programs and related political outcomes can be conditional on the party in power (Allan and Scruggs 2004; Giger and Nelson 2011; Schumacher, Vis and Van Kersbergen 2013). Electorally, these welfare programs can help or hurt re-election chances, depending on if a left or right coalition controls the government. Issues of welfare expansion and retrenchment are also often highly dependent upon not just individual characteristics, but societal arrangements, such as how class is structured within a country (Margalit 2013).

Much of the literature on the welfare state has been primarily focused on comparing welfare systems in the developed world, especially in Europe. However, multiple works discuss the common features and development of social assistance in Latin America (see Adato and Hoddinott 2010; Barrientos and Santibáñez 2009; Cecchini and Madariaga 2011; Moore 2010). For one, Barrientos and Santibáñez (2009) link liberalization of Latin American economies to the rise of transfer programs. From this account, social assistance for the poor in Latin America was relatively inconsistent in its coverage prior to the 1990s. With the opening of Latin American countries to the world economy in this time period, welfare programs became more prevalent.

The scholarship on blame attribution and issue ownership is consistent with welfare policy in Latin America. Indeed, electoral success in Latin America is of course linked to policy choice and policy success (Ames 1990). Following this trend in the literature, there exists a good deal of research on the effects of assistance and conditional transfer programs on voting behavior in Latin America (Baez et al. 2012; De La O

2013; Licio, Rennó and Castro 2009; Linos 2013; Manacorda, Miguel and Vigorito 2011; Zucco 2013). Most of this scholarship focuses on the effects of CCTs on the electoral success of incumbents, especially those who are responsible for the welfare policy (Sewall 2008; Baez et al. 2012). Attempting to understand these electoral repercussions has thus been a key thread in political science studies on CCTs.

Studies from individual countries in Latin America have shown that CCTs impact turnout, while also aiding parties that are associated with CCT legislation (Radcliff 1992). Using a discontinuity in program eligibility, Baez et al. (2012) show that participation in Familias in Acción in Colombia increases the likelihood of turning out to vote, especially for younger women. Additionally, the same study also shows that those that turned out are also more likely to support the party that implemented and expanded the program.

In Honduras, the PRAF (Programa de Asignación Familiar), which awards $18 a year per capita, increased local mayors electoral probabilities by 39%, although the PRAF is actually administered nationally (Linos 2013). Evidence from Peru have also linked welfare expenditures with electoral fortunes (Graham and Kane 1998). Continuing with the Peruvian case, Schady (2000) finds evidence of funds from the Peruvian Social Fund (FONCODES) being allocated with the intent of increasing the vote shares of the ruling party.

Of specific countries in Latin America, Brazil is a great example of how conditional cash transfer programs can come to be synonymous with a particular party and even candidate. When President Lula Da Silva came into power in 2003 and combined the Bolsa Escola and Bolsa Alimentacão programs, the resulting Bolsa Familía program became associated in the media and in scholarship with him and the Worker's Party (PT) (Hellmannn 2015). This again shows the power of issue ownership and responsibility that can be associated with welfare programs, even in the developing world. The Bolsa Família also became a standard for what a conditional cash transfer system looks like, providing transfers to poor citizens with the condition that they invest in human capital through school attendance for their children and healthcare programs for the family (Das, Do and Özler 2005; Lindert et al. 2007).

Indeed, much of the research on CCTs focuses on Brazil, with the Bolsa Família being the most-well known CCT program. In a study by Licio, Rennó and Castro (2009), AmericasBarometer data from Brazil shows that being a recipient of Bolsa Família increases support for Lula and his administration. Further, some scholars even attribute Lula's 2006 re-election amid corruption scandals to poorer voters that benefit from Bolsa Família (Hunter and Sugiyama 2009). Effects of Bolsa Família have also been shown at both the municipal and aggregate level to affect incumbent vote share (Zucco 2015). The question in Brazil is especially interesting considering the relationship between personalistic politics and the party, as Bolsa Família was often associated more with Lula rather than with the PT, with the PT actually losing seats in the national congress in 2006 as Lula won re-election (Fenwick 2009).

In cross-national work on Latin America and conditional cash transfer programs, receipt of welfare is

also associated with support for the incumbent (Layton and Smith 2015). However, some research has also shown that there can also be negative effects for incumbents (Corrêa and Cheibub 2016). Still others have found mixed effects on incumbent performance. For instance, Bruhn (1996) shows mixed effects of solidarity by Mexican state on incumbent performance, suggesting a limited ability of CCTs to affect the vote.

The relationship between governments program and public support belies the fact that policy and mass political behavior do not occur in separate and isolated spheres. Instead, as Campbell (2012) describes in a review article: "policy makes mass politics". Policy can either promote or depress political participation depending on the details of the policy and the people that are reacting to the specifics of the policy. For example, when looking at the development of the American welfare state, Campbell (2003) describes how expansion of social security is associated with the rise in political participation of older Americans. Additionally, Kumlin and Rothstein (2005) describe how more inclusive, universal welfare programs tend to increase social capital in European democracies.

Scholars have also investigated the effects of welfare transfers on attitudes and behaviors beyond support for the incumbent, such as social trust and partisan identification. For instance, cash transfers have been shown to be associated with greater trust in local government (Evans, Holtemeyer and Kosec 2019). In a study using regression discontinuity of targeted social spending in Romania, results indicate increase trust for the local governments, rather than the central governments (Pop-Eleches, Pop-Eleches and Others 2012). Due to subnational variation in Latin America, especially in larger, federal countries such as Brazil and Argentina, effects of social assistance may vary (Niedzwiecki 2018). Using municipal-level data, (Zucco 2013) shows that CCTs in Brazil are associated with support for incumbents of different parties, but are not associated with party re-identification of recipients. The overall conclusion made by Zucco (2013) is that while CCTs can influence party politics in the short-term, long-term shifts in party alignment are less likely.

Expectations

So far, works have largely focused on the changes to the party system, incumbent vote choice, and public opinion. However, there has been less research done on how welfare systems are associated with overall levels political participation. For one finding, receipt of transfers has been shown to be associated with a decreased propensity to abstain from voting (Layton and Smith 2015). Outside of political science, much of the work that has covered conditional cash transfer programs has focused mainly on the institutions, or when it is behavior focused, on health and education related outcomes (Lindert et al. 2007; Maluccio and Flores 2005; Leite et al. 2011). Overall, these individual findings on CCTs have shown that they are generally effective at improving quality of life for recipients on most standard metrics of well-being. Building on these healthcare and education related progressions, I also expect the same to carry over to political life. This leads

to my first set of main hypotheses:

> **Hypothesis 1a:** Involvement in conditional cash transfer programs makes individuals more likely to participate in all areas of politics.
>
> **Hypothesis 1b:** Involvement in conditional cash transfer programs increases political participation more for women more than it does for men.

With regard to **Hypothesis 1a:**, the increased political participation of individuals that are involved in CCTs stems from two sets of factors. First, the increased resources available to the citizens involved in CCTs increases, allowing them more opportunity to participate in politics through the removal of critical resource barriers. Second, the networks of engagement for these individuals also change, increasing the likelihood of political participation.

Regarding **Hypothesis 1b**, women are especially participate more because they are specifically targeted by CCT programs (Adato and Roopnaraine 2010). Additionally, just as in other places in the world, women in Latin America are likely to be excluded from political life and participate at lower levels than men (Desposato and Norrander 2009). The transfers from these CCTs should therefore have the largest net effect for recipients that are women. This relationship has been previously theorized and tested by Baez et al. (2012) using a regression discontinuity design based on program eligibility in Colombia, with results showing greater effects on turnout for women that receive program funds. Additionally, previous research has shown that women often are more likely to support protecting government expenditures because they can help women and protect "women's interests" (Funk and Gathmann 2006).

Beyond moderating effects of the gender of recipients, the institutional conditions in the country would also have an effect on the participation of citizens. As noted in Layton and Smith (2015), although conditional cash transfer programs in developing countries have many commonalities, there are key differences that would affect the impact of the receipt of a transfer on behavior. Because human capital building is a key effort of CCT programs, effects of CCTs on participation, and especially participation that is community-based, should be strongest when there are conditions that emphasize human-capital. As such, my next hypothesis reflects this:

> **Hypothesis 2a:** Enforced program conditions improve the positive effect of involvement in conditional cash transfer programs on participation, especially community-oriented participation.

If program conditions are enforced, then the human capital capabilities of the conditional cash transfers will be much stronger. Although prior findings on enforced conditions have been inconclusive, the expectation would be that meeting program conditions would improve citizen and country-wide health outcomes and positively impact political participation (Layton and Smith 2015).

On the other hand, compulsory voting could to some degree reduce the positive effects cash transfers have on participation (Layton and Smith 2015). This is because compulsory voting would create a ceiling on participation levels and make any effects of transfers relatively smaller. Building on this, the moderating effect of compulsory voting should be larger when sanctions for non-compliance are higher. This leads to my next hypothesis:

> **Hypothesis 2b:** Compulsory voting, especially with stricter sanctions, reduces the positive effect of involvement in conditional cash transfer programs on participation, especially electoral participation.

The latter part of this **Hypothesis 2b** emphasizes higher levels of the attenuating effect of compulsory voting for voting. The expectation of compulsory voting decreasing the effect of targeted social assistance is based on the specific effects compulsory voting has on particular forms of participation. This is due to the fact that research has showed relatively smaller effects of compulsory voting on broader types of participation and consistent effects on voting (Singh 2011; Carreras 2016).

Conditional Cash Transfers and Clientelism

The resource model of participation follows that people with more resources will be more likely to participate in all forms of politics. These resources do not necessarily have to be just monetary. In their foundational piece on the resource theory of participation, Brady, Verba and Schlozman (1995) identify time, money, and civic skills as the three key resources that are necessary for political participation. Because the CCT involves a cash transfer, the money factor would by definition increase. Along with this, time would likely increase as well, given that the provision of funds will provide a lower opportunity cost to participate. It is important to note that these differences would be comparing poor individuals that did receive transfers as compared to other similarly poor individuals that did not receive any transfer. When comparing to a higher class citizen, the time and money factors would still be incredibly low. For a poor citizen though, both of these first two would increase, making all forms of political participation more likely. Additionally, with the increase in resources, recipients will want to keep these programs in place, creating a further incentive to participate (Layton and Smith 2015).

Civic skills is the category of the three provided by Brady, Verba and Schlozman (1995) that is most difficult to pin down with regard to CCTs. In most cases, CCTs would generally increase the civic skills of individuals that participate in these programs. I base this on the nature of the institutional program itself and on the terms and conditions of CCTs. Because many of the individuals who are involved in CCTs are among the poorest in society, participation in this program which requires personal management and consistent

contact with state institutions would likely increase the participants' civic skills. Additionally, due to the heavy emphasis on building human capital and investments in the community, CCTs would bolster civic skills, especially for citizens who are long-term participants.

The second main theoretical reason for an increase in participation relates in many ways to some of the points brought up when discussing civic skills. For similar reasons that individuals that are involved in CCTs will increase civic skills through contacts with institutions and the building of human capital, they are also likely to become more engaged in different networks of individuals that increase their propensity to participate in politics. This has been previously noted with regard to welfare programs, with Campbell (2006) highlighting how social networks inherent in schools and communities are a key factor for citizens to become engaged in politics and civics.

Further on this point, Soss (1999) argues that welfare participants in the United States can see their participation in a welfare program as indicative of the larger functioning of the government bureaucracy. In his work, he aims to examine why welfare participants are a more "quiscient" group that is likely to participate in the politics of the United States. Soss (1999) attributes these participatory gaps to the aforementioned lack of civic resources, passivity and dependence, tempering political demands, and finally political learning effects associated with the welfare institution.

With the fourth point Soss (1999) holds that the arrangement of the welfare program affects how citizens engage not just with the welfare program, but with politics as a whole. In the US case, the SSDI (Social Security Disability Insurance) is responsive to the public and encourages participation, while the now defunct AFDC (Aid to Families with Dependant Children) was difficult to navigate and depressed participation. Mettler and Soss (2004) reinforce the value of political learning through institutions by noting that government policy, such as welfare, develops citizens and norms of citizenship based on political cohesive effects. Furthermore, they summarize an institution's relationship with public opinion and action as such:

> Scholars working in the political tradition argue, for instance, that beliefs about government responsiveness and one's capacity to influence political outcomes (i.e., political efficacy) are not just socialized psychological orientations. They depend on mass appeals by political organizations, and they are shaped by direct experiences of government institutions.

They note that individuals are "organized into the political process". For many citizens, welfare programs are the primary contact with institutions of the state. As such, welfare institutions can provide a major force for political organization, networking citizens into political life.

Given this, when others around an individual are involved in activities that involve dealing with the government and engaging in civic life positively, even if for a welfare program, it is far more likely these

citizens will also participate. Although social networks are most often the realm of sociology, political science research based on social networks has been expanding (Campbell 2006; Schmitt-Beck and Mackenrodt 2010). Indeed, MacLean (2011) finds evidence that citizens in Africa who interact with schools and health clinics are more likely to practice their democratic citizenship. Robust social networks have also been connected to higher levels of social capital, which is often linked to political engagement in societies (La Due Lake and Huckfeldt 1998). Social networks have also been theorized to be information gathering hubs that increase the likelihood of participation (Mcclurg 2003).

For further evidence of the importance of networks on participation, Campbell (2003) describes how the growth of the welfare state in America encouraged senior citizens to participate more in politics. While in the 1950s the political participation of senior citizens was rather low, the advent of widespread social security encouraged seniors to participate. This also corresponds with the increasing power of other interest groups such as AARP (Association for the Advancement of Retired Persons) becoming more active and advocating for senior interests. In an earlier piece, Campbell (2002) also notes that as income increases, seniors are less likely to participate in political life.

On the other hand, social assistance programs have been previously critiqued as possible avenues for vote-buying and/or clientelism (Bruhn 1996; Hevia 2011; Calvo and Murillo 2004; Graham and Kane 1998; Penfold-Becerra 2007; Schady 2000). Additionally, critics argue that CCT programs may "subvert formal institutions" (Layton and Smith 2015), even if designed to prevent clientelism and vote-buying (Fiszbein and Schady 2009; Hunter and Sugiyama 2009). Hall (2008) notes that while Brazil's Familía helps millions of poor people in Brazil, it could also negatively affect patronage networks in Brazil. Some scholars note that CCTs could also lead to reductions in spending in areas such as housing, education, and sanitation, with unconditional spending could promote development better (Hevia 2011). In Venezuela, evidence has shown that Hugo Chavez used funds from *misiones* to not only reach poor populations, but also create a political advantage in a increasingly electorally competitive environment (Penfold-Becerra 2007), with similar findings from Mexico and Brazil (Sewall 2008).

Despite these concerns, some research on CCTs have not as strongly linked these programs with more corruption and worse governance. For instance, in a study on Mexico, little evidence of a positive relationship between CCTs and corruption is found (Grimes and Wängnerud 2010). However, in the same study, scant evidence is also found that shows positive effects of CCTs on civil society organizations and empowerment of women (Grimes and Wängnerud 2010). Similarly, Fried (2012) do not find that political criteria impact distribution of funds from Bolsa Familía in Brazil. Given this, it is possible these programs not make the sweeping changes to political linkages predicted by some scholars.

Regardless of the theoretical support for the idea that social networks would positively impact political

participation, I posit that the relationship between CCTs and participation is more nuanced. While these more "horizontal" networks previously described would be critical pieces in boosting participation, other more "vertical" networks also likely play a role in promoting participation. By a "vertical" network I refer to a relationship predicated on power asymmetry, such as the unequal and possibly damaging exchanges described by Soss (1999) in the AFDC. Through involvement in CCTs, the citizen will develop resources and horizontal networks of their peers, but they are also more likely to be in contact with governmental officials that also hold power over their financial situations. These officials may also have the power to distribute goods to the citizens, such as the funds for the CCT or other goods and resources. Because of this situation, involvement in CCTs is also associated with clientelism. This leads to my third hypothesis:

> **Hypothesis 3:** Involvement in CCTs has a positive effect on political participation through clientelistic engagements.

Clientelism is defined here as a quid pro quo distribution of goods or services with electoral incentives in mind (Boix and Stokes 2007). Usually, clientelism is seen to work through a patron-client relationship where patrons offer the goods to their clients in exchange for their electoral or general political support (Stokes et al. 2013). Clientelism is also noted to be built around hierarchy (Hicken 2011). This fits into the discussion of cash transfers and the networks created that impact political participation. Elites may target mobilization efforts at individuals who are well-connected to social and community networks (Cox, Rosenbluth and Thies 1998).

It is again important to note here that the relationship between CCTs and clientelism is disputed in the literature, with some pointing to ties between clientelism and CCTS while others actually emphasize that social assistance may actually *reduce* clientelistic practices. However, I argue that in the case of **Hypothesis 3**, the access provided by the CCT to clients for patrons can add another dimension to the effect on participation. This is because the participatory relationships can also work through clientelistic engagements to promote political participation. Social networks have been previously theorized as important for maintaining strong clientelistic organizations and relationships between patrons and their clients, providing easier monitoring opportunities and access to more clients (Abers 1998; Szwarcberg 2015). CCTs provide both of these things to the patron, making clientelism a practice that can possibly be reinforced by CCTs. It has been noted that when clientelism is explicitly curtailed in program design and operation, social benefits for poor citizens may suffer. With regard to Brazil, Sugiyama (2016) concludes that while central monitoring of CCTs "has advantages for insulating social policy from variations in local capacity and reducing the risks of local clientelism, it has also had the effect of dampening local dialogue with the poor." From this, citizens experiencing poverty may benefit from clientelistic relationships related to CCTs in that participation in politics may be boosted.

Data and Measurement

I rely on data from the AmericasBarometer survey (2010-2019) from 19 countries and 77 individual surveys to test my expectations (LAPOP 2019).[1][2][3] This data source allows me access to a wide array of individual-level questions about political participation, as well as involvement in transfer programs across multiple countries in Latin America. It is important to note that the wording of the transfer and clientelism questions can be slightly different by survey wave.[4] However, countries all have national-level conditional cash transfer programs that were in place at some point in the time period of 2010-2019.

The outcome of interest in my design is individual-level *political participation*. Political participation is a concept that has multiple related facets and trying to capture it with any single measure provides a considerable set of challenges. For instance, an individual may consistently vote but never decide to attend a political protest or to sign a petition. Nonetheless, all of these forms of engagement are behaviors that can theoretically be affected by cash transfer programs. Given this, to avoid relying on any one area of political participation, I draw from sets of questions related to various dimensions of political participation. These questions ask about voting, attending community meetings, solving community problems and issues, attending meetings of political parties, and involvement in protest. From there, I predict a single participation score using factor analysis for each respondent. Creating this variable also affords the advantage of estimating a linear model, as the dependent variable is continuous. The full questions used to create the participation variable are reported in the Appendix in Table A2 and their respective factor loadings in Table A3. I also estimate the effects of receiving a transfer on each of the components of the participation variable.

The main independent variable is if the respondent is the *recipient of funds in the form of a cash transfer*, operationalized with a question asking whether anyone in the respondent's household has received a cash transfer. Although conditional cash transfer programs are generally targeted at women, the funds will still be distributed through the household. Given this, asking about anyone in the household receiving the funds is appropriate. When applicable, the question also refers to the name of the overarching CCT program in the respective country (Bolsa Família in Brazil, Oportunidades in Mexico, etc.).[5]

Control variables are included at the individual level to account for any omitted factors and reduce the chance for bias. These include standard SES controls like *education*, *age* in years, and *income*. More education and a higher income make individuals far less likely to receive funds from a CCT, but also far more likely to

[1] Overall, this covers 5 waves of the AmericasBarometer survey.

[2] The countries included are Argentina, Bolivia, Brazil, Chile, Colombia, Costa Rica, Dominican Republic, Ecuador, El Salvador, Guatemala, Honduras, Jamaica, Mexico, Panama, Paraguay, Peru, Uruguay, and Venezuela.

[3] https://www.vanderbilt.edu/lapop/about-americasbarometer.php

[4] Descriptive statistics for each survey-year can be found in Table 1 in the Appendix.

[5] Later surveys ask "Now, talking specifically about *[program name]*, are you or someone in your house a beneficiary of this programme?"

participate. On the other hand, older individuals will have a higher chance of being beneficiaries of CCTs, while being more likely to participate. *Education* is measured as the number of years of schooling, while *income* is measured by a question asking the respondent to which monthly income bracket they belonged.

I also control for number of *children* in a household, given that this increases the likelihood of receiving transfer funds as well as the extent and types of political participation which individuals would be engaged. For instance, people without children would be less likely to be involved in some forms of participation that have high resource costs, but may also be more likely to engage in the local community because it would benefit their children. This is especially critical to account for given the emphasis on pushing children of recipients to have better education and health outcomes. Due to the data structure, I estimate a multilevel model with random-intercepts to account for country-based heterogeneity.

As a robustness check and to further evaluate the causality of the relationship between transfers and participation, I also conduct a matching analysis. Because matching attempts to isolate the effects of a treatment through matching different observations to create a co-variate balance, an average treatment effect can be calculated. However, due to the fact that recipients of cash transfers are clustered in countries, not accounting for this hierarchical data structure would lead to bias in interpreting the results of a standard matching algorithm. Therefore, I use cluster matching to lower the likelihood of bias in my coefficients.[6]

Instead of matching on covariates across the entire range of data points, cluster matching restricts the matching algorithm to match with other observations within the same cluster. However, as strict matching within a survey cluster can be problematic with a large difference in sample size and variation in certain clusters, I opt to use preferential matching rather than only matching within each country. Preferential matching first searches for matches within cluster and then finds matches outside the cluster if none are found within cluster. After attempting both procedures, preferential clustering resulted in a far better co-variate balance between the treated and untreated groups. This makes sense given the variations in the efficiency and scale of targeting of poor populations for the conditional cash transfers across the countries in the sample. Some countries, such as Brazil, have reputations for good targeting of recipients, while others do not administer their programs as rigorously. I then estimate a regression using OLS with weights estimated from the matching procedure.

I match based on the factors used as control variables in the multilevel model, namely sex, income, age, number of children, and education. Balance tables showing the differences between treatment and control conditions before and after the matching procedure are reported in the Appendix in Table A5. As can be seen from Table A5, covariates appear balanced, with no statistical difference between any variables in after matching.

[6] I use the CMatching library in R. This is an extension of the Matching library

In my second hypothesis, I echo the propositions of Layton and Smith (2015) and evaluate the moderating effects of program condition enforcement and compulsory voting. Therefore, I also estimate another series of models to evaluate the conditioning effects of both enforced program compulsory voting has on political participation. In both cases I use multi-level models as they allow for the estimating of effects of country-level predictors on individual-level outcomes (Gelman and Hill 2006). This strategy has been used consistently to assess effects of compulsory voting, and is used additionally in prior pieces on conditional cash transfers (Singh 2015, 2016; Layton and Smith 2015).

In these models, I again treat the survey as the highest form of clustering in order to account for both country and year heterogeneity, but also include control variables at the survey-level that may confound the identification of the effects of interest (Williams, Snipes and Singh 2020). To begin with, I include an indicator for *GDP per capita (logged)* and *Polity score*, as countries that are wealthier and more democratic could plausibly be more likely to enforce conditions of social programs. Additionally, compulsory voting is often adopted in countries as a corrective institution in certain developing countries and these two variables would help capture that. Both are also associated with citizens political participation.

At the survey-level, I also include controls for the degree of *presidentialism*, as measured using an indicator from the Varieties of Democracy (V-Dem) project that captures the degree of presidential power, with higher values indicating a stronger executive (Coppedge et al. 2021). Presidentialism is frequently related to voter turnout and participation, as well as compulsory voting. In many cases, targeted assistance programs can also be associated with a strong and visible executive, such as with Lula da Silva in Brazil (Hellmannn 2015). I also include an indicator for types of electoral *linkages* sourced from V-Dem (Coppedge et al. 2021). Parties' electoral strategies can be associated with compulsory voting and welfare politics, as discussed in this piece and shown in previous research (Singh 2018).

To evaluate the effects of enforced conditions of CCTs, I rely on the binary coding scheme used by Layton and Smith (2015). For compulsory voting, I make use of the categorizations from the Varieties of Democracy (Coppedge et al. 2021). These are labelled the baseline category of no compulsory voting, no sanctions, and weak sanctions. How each country fits into both variables is shown in Table 1. For the regression discontinuity design I implement for compulsory voting, Table 1 also shows relevant age thresholds by country. The countries that have age thresholds for compulsory voting are Argentina, Bolivia, Brazil, Ecuador and Peru.

In the second part of the analysis, I conduct a set of mediation analyses. The mediating variable in this analysis is *involvement in clientelism*. Clientelism, akin to related fuzzy concepts like corruption, can be difficult to accurately capture for a number of reasons. For one, understanding of this idea can vary from culture to culture and country to country. Therefore, objective evaluation of clientelism and when it

is harmful can be difficult. However, with the definition of clientelism previously discussed in the theory section, involving a quid pro quo with electoral incentives, I believe survey data can provide an adequate working proxy for this concept (Stokes 2005). I thus operationalize clientelism with a question from the AmericasBarometer survey that asks: *"In recent years and thinking about election campaigns, has a candidate or someone from a political party offered you something, like a favor, food, or any other benefit or thing in return for your vote or support? Has this happened often, sometimes or never?"*.[7]

Table 1: Summary Table of Countries in Analysis

Country	Transfer	Client	Conditions Enforced	Compulsory Voting	Threshold
Argentina	0.22	0.11	Yes	No Sanctions	Age 70
Bolivia	0.58	0.19	No	Weak Sanctions	Age 70
Brazil	0.23	0.14	Yes	Weak Sanctions	Age 70
Chile	0.10	0.04	Yes	No Sanctions (2010), No Compulsory Voting (2012, 2014, 2017)	
Colombia	0.26	0.13	Yes	No Compulsory Voting	
Costa Rica	0.13	0.02	Yes	No Sanctions	
Dominican Republic	0.39	0.24	Yes	No Compulsory Voting	
Ecuador	0.22	0.09	No	Weak Sanctions	Age 65
El Salvador	0.08	0.06	Yes	No Compulsory Voting	
Guatemala	0.09	0.19	Yes	No Compulsory Voting	
Honduras	0.14	0.20	No	No Sanctions	
Jamaica	0.26	0.08	Yes	No Compulsory Voting	
Mexico	0.21	0.16	Yes	No Sanctions	
Panama	0.11	0.06	No	No Sanctions	
Paraguay	0.10	0.14	No	No Sanctions	
Peru	0.11	0.10	No	Weak Sanctions	Age 70
Trinidad and Tobago	0.12		No	No Compulsory Voting	
Uruguay	0.23	0.05	Yes	Weak Sanctions	
Venezuela	0.12	0.12	No	No Compulsory Voting	

Note: Numbers in the Transfer and Client column refer to the proportion of respondents that report receiving cash transfers or involvement in clientelism, respectively.

Countries included in the analysis, as well as key statistics for each country, are reported in Table 1.[8] As can be seen in Table 1, there are clear variations in the proportion of respondents reporting receiving transfers. For instance, 39% of respondents in the Dominican Republic reported receiving a transfer, while only 8% of respondents in El Salvador reported the same. Of all countries, Bolivia has the highest proportion of respondents reporting a transfer (58%), with countries that have a strong history of CCTs, such as Brazil and Mexico, also showing relatively high transfer rates in the sample (23% and 21% respectively). Furthermore, in countries where there is higher general proportion of respondents reporting receiving a transfer, the proportion of respondents reporting clientelistic behavior also tends to be high.

[7]Note that the wording of this question has some slight variation over the course of the waves of the Americas-Barometer. However, these differences are not large enough to throw out the data from the sample. In later surveys this question is worded as "Thinking of the last general elections, any candidate or political party offered a favor, gift, or other benefit to a person whom you know in exchange for that person's support or vote?".

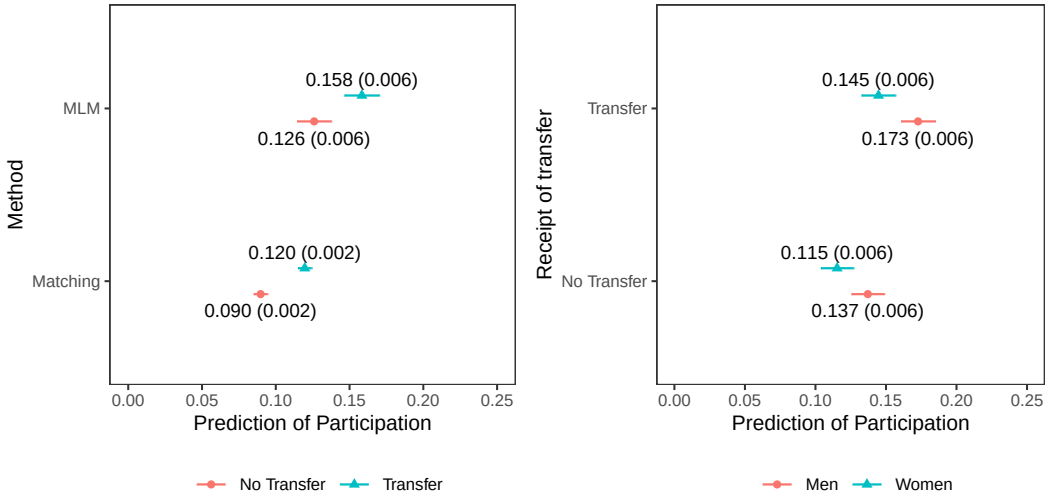
[8]For a breakdown of all variables used in the analysis by country and by year, refer to Table A1 in the Appendix.

Results

The first hypothesis predicts predicts higher levels of political participation for recipients of cash transfers, especially for women. Overall, I find strong evidence of a clear positive effect of participation in conditional cash transfer programs on political participation. However, contrary to my expectations, I find only mixed evidence that this boost in expected levels of participation is actually larger for women.

Figure 1 shows the effects of transfer status on predicted value of participation. The plot on the left shows the value of participation by method of analysis, with the one on the right the multi-level model described previously in the text. The result on the left of this plot is from the cluster matching algorithm which prioritizes matches with observations from the same country cluster. I estimate this additional matching model as a robustness check to the main multi-level model.[9]

Figure 1: Predicted value of participation by receipt of transfer, method, and gender



Note: Data are from the 2010-2019 AmericasBarometer survey. Receipt of a cash transfer is from a question that asks: "Do you or someone in your household receive monthly support in money or products from the national government, like for example from: [program(s) name]?" Predicted values in the left plot are from a matching model and a multilevel regression model, with the right plot including an interaction between transfer status and gender (estimated with MLM), as well as several control variables described in the text. The figure is created from the first column in Table A6 and Table A7, as well as Table A4, which are detailed in the Appendix. The constant is allowed to vary randomly across surveys, of which there are 66. The number of observations in the multilevel models are 86,106, and 23,168 in the matching model. Lines represent 95% confidence intervals.

Through both estimation strategies the main result is quite similar. In both cases, the predicted value of participation increases substantially when respondents report receiving a CCT transfer. In the case of the multilevel model, there is a difference of 0.32 in the expected participation level between transfer and no transfer. For the matching approach, there is a similar difference of 0.030 between the predicted value of participation in the transfer and no-transfer categories. In both cases, the difference is statistically significant

[9]For full results of all models refer to Table A6, Table A7, and Table A4, . Balance tables for the matching procedure are reported in the Appendix in Table A5.

($p < .05$).

The second part of the first hypotheses is tested with an interaction between transfer status and sex in a multilevel model. This result is shown in the plot on the right side of Figure 1. Unsurprisingly, women tend to participate in most dimensions generally less than men. However, the expected boost in participation from CCT transfers is actually comparatively smaller for women. The gap in predicted participation between men and women who receive transfers is actually slightly larger than for those that did not receive transfers. (0.022 for no receipt of transfer vs. 0.028 for those who received transfers), and the difference is statistically distinct from 0 ($p < .05$). These results run counter to the theoretical expectations regarding CCTs being especially positive programs for integrating women into sociopolitical life.

I also estimate similar multilevel models for each of the constituent forms of participation in the composite participation variable. Table 2 shows the results by each each model specification and form of participation. Note that the vertical columns labelled with each participation type indicate a different dependent variable. Models estimated for *Vote*, *Meeting*, and *Protest* are multi-level logistic regression models. On other hand, I treat the *Community* and *Party Meeting* variables as continuous, and estimate linear models for these two indicators and the *Participation* measure.

As can be seen in the table, recipient of a transfer positively affects all forms of participation, with varying effects sizes. Notably, the effect on propensity to vote is among the largest, following previous literature on the effects of CCTs on the vote (Layton and Smith 2015). Indeed, recipients of targeted social assistance are 25.2% more likely to vote and 34.7% to attend a meeting than those that did not receive a transfer, holding all else equal. Again, compared to those did not receive transfers and all else equal, transfer recipients are 22.3% more likely to protest. These findings suggests an increase in participation beyond even conventional politics into more demanding modes of engagement.

The second model of Table 2 shows the effects from the interaction between transfer status and sex. As opposed to the first set of general predicted results discussed from Figure 1, there are diverging results based on participation type. The results show that while all transfer recipients are likely to participate in all dimensions of politics, women that are involved in these programs are more likely to vote, while men that receive transfers are more likely to attend party meetings and to protest. More specifically, women involved in transfer programs are 14.1% more likely than men in these programs to vote, while on the other hand, women are also 12.3% less likely than men to protest, holding all else equal. Additionally, for the participation variable previously mentioned and for party meeting, we see smaller negative effects. On the other hand, for the *Meeting* and *Community* variables, the interaction is neither statistically nor substantively significant.

These results suggest interesting findings regarding cash transfer programs and participation at the level

Table 2: Results for each form of participation based on model type

	Participation	Vote	Meeting	Community	Party Meeting	Protest
Model 1						
Transfer	**0.032***	**0.225***	**0.298***	**0.111***	**0.089***	**0.202***
	(0.001)	(0.022)	(0.028)	(0.007)	(0.005)	(0.029)
Number of respondents	86106	102085	90045	103855	95376	104053
Number of surveys	66	76	67	77	71	77
var(survey-level constants)	0.001	0.411	0.172	0.034	0.013	0.248
sd(survey-level constants	0.035	0.641	0.415	0.185	0.113	0.498
AIC	-74345.165	96072.188	60105.565	247274.832	167563.52	59489.76
Model 2						
Transfer	0.036*	0.156*	0.291*	0.118*	0.109*	0.264*
	(0.002)	(0.030)	(0.037)	(0.009)	(0.007)	(0.039)
Female	−0.022*	0.078*	−0.244*	−0.071*	−0.069*	−0.173*
	(0.001)	(0.018)	(0.025)	(0.006)	(0.004)	(0.025)
Transfer × Female	**−0.006***	**0.132***	0.013	−0.012	**−0.038***	**−0.131***
	(0.003)	(0.041)	(0.052)	(0.012)	(0.009)	(0.053)
Number of respondents	86106	102085	90045	103855	95376	104053
Number of surveys	66	76	67	77	71	77
var(survey-level constants)	0.001	0.411	0.172	0.034	0.013	0.247
sd(survey-level constants	0.035	0.641	0.415	0.185	0.113	0.497
AIC	-74338.675	96063.642	60107.497	247282.796	167556.844	59485.72
Model 3						
Transfer	0.048*	0.155*	0.353*	0.175*	0.143*	0.404*
	(0.006)	(0.051)	(0.055)	(0.020)	(0.021)	(0.086)
Conditions enforced	0.002	0.306*	−0.061	−0.073	0.044	0.042
	(0.010)	(0.146)	(0.127)	(0.040)	(0.035)	(0.184)
Transfer × Conditions enforced	**−0.022***	0.066	−0.093	**−0.091***	**−0.076***	**−0.282***
	(0.007)	(0.063)	(0.073)	(0.025)	(0.026)	(0.111)
Number of respondents	86106	102085	90045	103855	95376	104053
Number of surveys	66	76	67	77	71	77
var(survey-level constants)	0.001	0.15	0.096	0.018	0.011	0.247
sd(survey-level constants	0.024	0.025	0.393	0.17	0.304	0.175
var(transfer random coefficients)	0.001	0.15	0.031	0.008	0.009	0.142
sd(transfer random coefficients)	0.025	0.17	0.175	0.087	0.096	0.377
AIC	-74519.61	95985.99	60073.918	247103.134	167348.376	59402.812
Model 4						
Transfer	0.046*	0.316*	0.447*	0.147*	0.133*	0.341*
	(0.006)	(0.045)	(0.052)	(0.021)	(0.021)	(0.095)
No sanctions	−0.014	0.365*	−0.311*	−0.093*	−0.021	0.219
	(0.008)	(0.124)	(0.108)	(0.036)	(0.030)	(0.161)
Weak sanctions	0.001	1.365*	−0.066	−0.046	−0.022	0.446*
	(0.009)	(0.143)	(0.122)	(0.040)	(0.034)	(0.181)
Transfer × No sanctions	−0.012	**−0.127***	**−0.223***	−0.016	−0.037	−0.052
	(0.008)	(0.064)	(0.082)	(0.030)	(0.031)	(0.131)
Transfer × Weak sanctions	**−0.024***	**−0.260***	**−0.264***	**−0.078***	**−0.082***	**−0.271***
	(0.009)	(0.068)	(0.077)	(0.031)	(0.032)	(0.135)
Number of respondents	86106	102085	90045	103855	95376	104053
Number of surveys	66	76	67	77	71	77
var(survey-level constants)	0.001	0.147	0.093	0.018	0.012	0.248
sd(survey-level constants	0.024	0.026	0.391	0.137	0.305	0.132
var(transfer random coefficients)	0.001	0.147	0.017	0.009	0.01	0.144
sd(transfer random coefficients)	0.026	0.137	0.132	0.093	0.099	0.38
AIC	-74508.751	95975.763	60064.994	247115.78	167356.776	59406.66

Note: *p<0.05

Note: Data are from the 2010-2019 AmericasBarometer survey. Columns display form of participation and rows show the main models for Hypotheses 1 and 2. Highlighted and bolded numbers show statistically significant values for predictors of interest. Numbers in parentheses are standard errors. Note that *Participation*, *Community*, and *Party Meeting* are linear multilevel models, while *Vote*, *Meeting*, and *Protest* are logistic multilevel models. For full tables with all covariates refer to the Model Results section in the Appendix.

of the individual. Overwhelmingly, results point to program involvement boosting political participation as examined in multiple different ways and through a variety of approaches, including both multi-level modeling and matching methods. Additionally, each dimension of the composite measure of participation demonstrates the expected increase in participation for respondents involved in these programs. Despite this, when analyzing the gendered component of this relationship, findings fail to support any one simple takeaway. For the composite participation variable, there is a statistically significant difference between women and men who receive transfers, with men receiving a stronger boost in participation. This finding holds for attending a party meeting and protesting. On the other hand, women who receive transfers are far more likely to vote than men. Given this, as previously supported in the literature by Baez et al. (2012), these results help provide evidence that women are especially likely to turnout after receiving this type of government aid.

In order to analyze the moderating effects of a country's institutions I again employ a strategy of multi-level modeling. To evaluate enforcement conditions for social assistance programs, I include an interaction between transfer status and if conditions are enforced. Results for each form of participation are shown in Table 2 under **Model 3**. In the composite participation variable, and every other indicator except for *Vote*, the interaction term is negative. However, in the case of voting and meeting the effect is not statistically significant. Overall, these results indicate that when program conditions are enforced individuals tend to broadly participate less. Indeed, these negative effects are so strong for that interaction term that it is similar in effect size to the base effects of transfers in **Model 1**. For instance, when conditions are enforced, citizens are 32.5% less likely to protest as compared to those receiving transfers when conditions are not enforced, holding all else equal.

Similar to the conditions model, I also interact transfer with the different categories of compulsory voting and display the results in Table 2 under **Model 4**. Overall, results show that compulsory voting is associated with a decreased positive effect of transfers across participation. As expected, for each form of participation when there are sanctions for compulsory voting, the interaction terms are negative and statistically significant. These effects in the weak sanctions category are furthermore of notable magnitude. Holding all else equal, program recipients living in compulsory voting systems with weak sanctions are 22.9% less likely to vote, 23.2% less likely to attend a community meeting, and 23.7% to protest as compared to recipients in systems with no compulsory voting.

To build on this evidence, I also evaluate the effects of compulsory voting and transfers using a sharp regression discontinuity design. A sharp regression discontinuity design views units on either side of the treatment as the same except for the external variable of interest (Jacob et al. 2012). In this case, respondents to either side of 70 years old (or 65 in Ecuador) are treated as essentially the same on all other covariates other

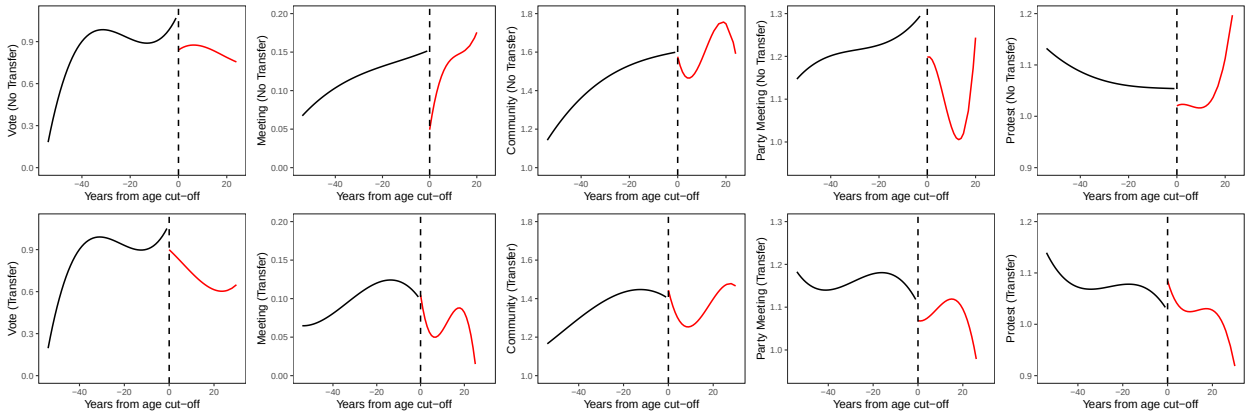
than compulsory voting. Indeed, previous work has investigated age thresholds for compulsory voting on a number of outcomes such as voting and satisfaction with democracy (Cepaluni and Hidalgo 2016; Singh 2016). For instance, Cepaluni and Hidalgo (2016) find reported turnout drops after voting becomes voluntary in Brazil, but also shows that there are meaningful differences in this decrease by level of education. Given this sharp regression discontinuity design, I analyze participation at the cut-off using a non-parametric local strategy as it is more likely to lower bias in estimates (Jacob et al. 2012; Skovron and Titiunik 2015).

Figure 2 shows the level of each form of participation by the years from the age-cut off, in years. The top row shows the data plotted from respondents who did not receive a transfer, while the bottom row is those who did receive a transfer. The first row shows mean reported levels of the vote by years from the cutout, with lines drawn to fit the data on each side of the threshold. As expected, there is a clear drop-off after voting becomes voluntary for both those that received and did not receive a transfer. However, this difference does not appear clearly different between the the two decreases in turnout.

This lack of statistically significant difference in turnout at the threshold between those who received a transfer and those who did not receive a transfer is better illustrated in Figure 3. This figure shows the effect sizes from the non-parametric local regression depending on the bandwidth, which in this case is the years around the age cut-off. Regardless of bandwidth selection, there is no statistically distinct difference in effects sizes for voting, despite a stronger negative effect closer to the age threshold.

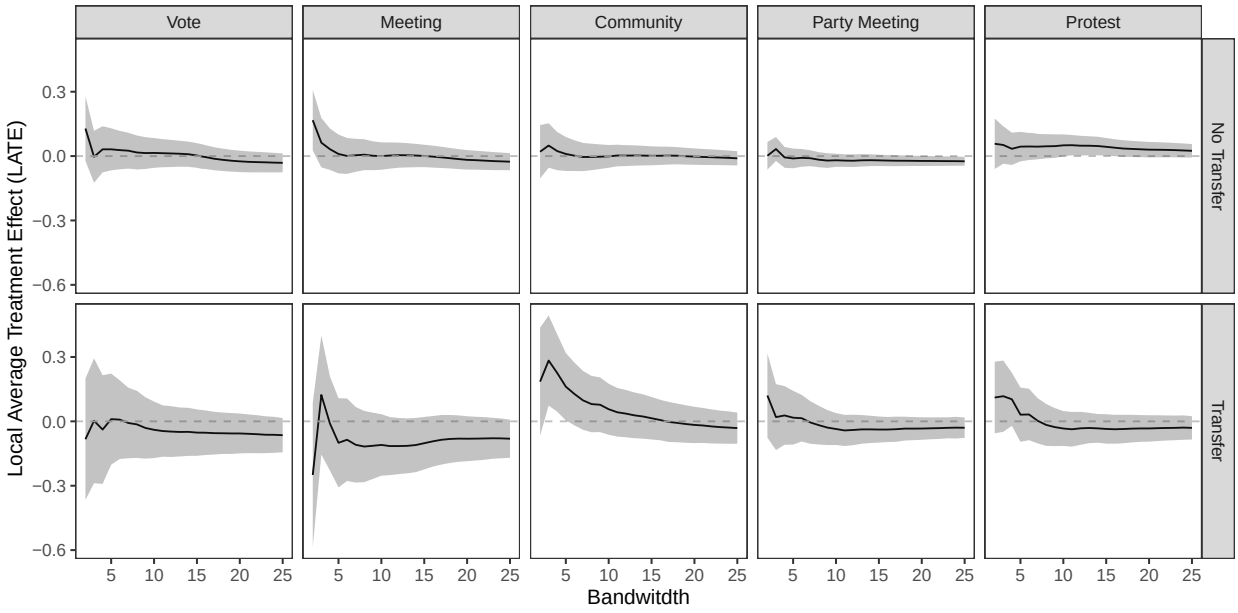
This lack of difference is observed for all other forms of participation, as evidenced from trends in both Figure 2 and Figure 3. Despite this, of interest to possible future work is that there are some small increases in propensity to solve a problem in the community after the age threshold. However, again there not any clear differences by transfer status. Results from a robustness check using data from the Brazilian Election Study largely corroborate these findings and can be seen in Figure A1 in the Appendix. In all, these results further present a murkier image of how compulsory voting might impact the relationship between targeted social assistance and participation.

Figure 2: Age, participation, and transfer status above and below compulsory voting age thresholds



Note: Data are from the 2010-2019 AmericasBarometer survey. The left column displays the results for people who did not receive a transfer, and the right column those who did receive a transfer. Results are from smoothed localized polynomial regressions estimated on both sides of the age cut-off.

Figure 3: Local Average Treatment Effect by Transfer Status and Participation Type



Note: Data are from the 2010-2019 AmericasBarometer survey. The left column displays the results for people who did not receive a transfer, and the right column those who did receive a transfer. Type of participation is given by the text in the grid rows. Results show Local Average Treatment Effect (LATE) by bandwidth selection. Sensitivity results taken from non-parametric models.

Clientelism Mediation Results

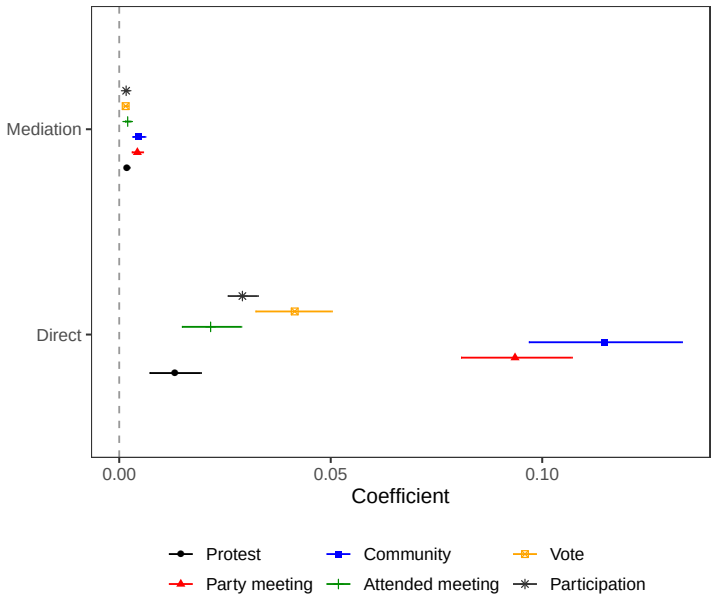
While clientelism has been purported by some to be associated with CCTs, clientelistic engagement is also associated with political participation and especially voter turnout (Stokes et al. 2013). Given this, I conduct a set of mediation analyses in order to test my third hypothesis, which suggests clientelistic engagement as a path through which involvement in CCTs affect political participation. In order to again to evaluate the effects of these mediated pathways on different areas of participation, mediation analyses are also done on each dimension of participation that forms the composite participation variable. To specify the mediation analysis, variables previously mentioned in the text are used as controls. Additionally, country and year dummies are also included to account for any country-level differences that may affect precise estimation of both direct and indirect effects..

The results from the mediation model are presented in Figure 4. The analysis of the composite participation measure shows limited evidence of clientelism mediating the effect of cash transfers on participation. As shown in Figure 4, while there are statistically significant indirect effects of clientelism, the direct effects of the CCT itself are of comparatively larger magnitudes. With regard to the composite participation variable, although both the direct and indirect effects are statistically significant, there is a direct effect size of 0.031 and an indirect effect of only 0.001.[10]

As previously mentioned, I also conduct mediation analyses on the separate dimensions of participation that make up the composite participation measure. The results of these analyses, also shown in Figure 4, suggest similar takeaways as the overall effect on participation. All of the results point to direct effects that are of a magnitude much larger than the mediation effects. Although there is some variation in effect size, with a larger mediation effect on *solving community problems* and *attending party meetings*, even these indirect, mediation effects of larger magnitude are proportionally only a fraction of the direct effects. Sensitivity analyses detailed in the Appendix and displayed in Figure 4 reinforce the relative small size of the mediation effects shown here. Given the overall small size of mediated effects and the sensitivity results, it appears clientelism only plays a minor part in the participatory effects of transfers.

[10]Note that given these statistically significant indirect effects, cash transfers are overwhelmingly positively associated with clientelism, as shown in Table A11. However, the relationship between cash transfers and clientelism is not necessarily causal.

Figure 4: Mediation results for each form of political participation



Note: Data are from the 2010-2019 AmericasBarometer survey. The figure is created from models in Table A11, which are both detailed in the Appendix, with the final results also displayed in Table A10.

Discussion and Conclusion

What can we now say about the link between conditional cash transfers and political participation? Using both multi-level modeling and a matching procedure, I find that program beneficiaries are more likely to participate in all forms of politics. Beyond this, results are mixed regarding whether these boosts in participation are larger for men or women. Therefore, the effects by the sex of participants in these programs may be more complicated than originally expected.

Upon breaking down participation into multiple dimensions, some forms of participation did not have statistically significant differences in participatory increases for program recipients by sex. Despite this, when interacted with the sex of respondents, both protest and voting did yield statistically significant results, but of opposite signs. Women who received transfers were more likely to vote than their male counterparts, and men more likely to protest. These two results present some interesting implications and avenues for future research on the gendered aspects of welfare and participation.

These results on voting also support the work of Baez et al. (2012) in Colombia, where women who received funds are more likely to turnout. Returning to the theorizing by Funk and Gathmann (2006), women could be especially likely to turnout because of gendered differences in voter preference for welfare programs, with women having a higher propensity to support welfare programs. Similarly, the result concerning the increased likelihood of men involved in CCTs to protest, suggests an even clearer gendered

aspect in political expression for citizens involved in CCT programs, with men gravitating more towards the less institutionalized and more contentious form of political participation.

Additionally, using multi-level modeling and a regression discontinuity design, the results continue to complicate the role institutions play in affecting the effects of transfers on participation. Surprisingly, sanctions are associated almost exclusively with depressed levels of every type of participation other than voting. This is a noteworthy result and should be further investigated with studies into how sanctions and compliance interact with recipient behavior. We may wonder if sanctions for welfare programs may have negative effects in other areas of public life. This is especially important when thinking about how to design programs and treating non-compliance among program participants. With regard to compulsory voting, although there are some clear negative effects from the multi-level models especially when sanctions exist, the variations in effect and the inconclusive findings from the regression discontinuity results suggest additional research needs to be done on how compulsory voting impacts behavior beyond the ballot box.

The mediation results also tell an interesting and complex story of the relationship between cash transfers and participation. The mediation analyses suggest both a direct and mediated effect through clientelism of cash transfers on participation. However, the direct effect is of a considerably larger magnitude than indirect effects. Upon running sensitivity analyses, these effects seem to generally hold up, but suggest there could be some situations when indirect effects will be negligible, given their overall smaller size. Despite this, the potential relationship between cash transfers, clientelism, and political participation requires further investigation, especially as more recent research has found clientelism can decrease when targeted social assistance programs are in place (Frey 2019). Additional work may attempt to further understand what about these programs boosts participation, and why we may observe varying levels of participatory gaps by sex and institutional design in a given context.

What are the implications of these results? Conditional cash transfers are programs that have been widely popular in the developing world, so research about them is interesting in and of itself, but the findings from this study have applications into wider literatures. For one, beyond just resources provided by the welfare program, the idea that these types of programs can deeply impact politics through a more dynamic social sphere is an intriguing consideration. Additionally, the effect that this would have on evaluations and considerations towards policy are also worth keeping in mind. Understanding why and when individuals react to welfare programs such as these is critical in understanding the durability and persistence of certain policy programs (Campbell 2012). This is because these policies create a strong base that does not just support the policy, but also becomes more active in other areas of public life. The expansion of the recipients of the transfers into other areas of public life gives them a larger voice that would most likely decrease the likelihood of the policy being retrenched, as already been shown in previous work on similar topics

(Layton and Smith 2015; Layton, Donaghy and Rennó 2017). Given this, the relationships between CCT involvement and political participation appears strong enough to warrant further research into other possible consequences of these types of welfare programs on broader sets of political behavior.

Appendix

Descriptive Statistics

Table A1: Descriptive statistics by country and year

Country	Year	Transfer	Client	Female	Age	Education	Income	Children	Participation	Vote	Meeting	Community	Party Meeting	Protest	Conditions enforced	Compulsory voting	Polity score	GDP per capita (logged)	Presidentialism	Linkages
Argentina	2010	0.21	0.18	0.52	35.54	10.39	3.09	1.30	-0.10	0.75	0.06	0.24	0.20	0.15	1.00	No Sanctions	8.00	9.85	0.39	3.00
Argentina	2012	0.19	0.13	0.50	41.93	10.46	6.29	1.78	-0.20	0.89	0.04	0.15	0.14	0.08	1.00	No Sanctions	8.00	9.88	0.40	3.00
Argentina	2014	0.19	0.02	0.52	42.13	10.26	10.20	1.77	-0.23	0.77	0.05	0.13	0.12	0.09	1.00	No Sanctions	8.00	9.86	0.39	3.00
Argentina	2017	0.25		0.51	42.00	10.69	5.86	1.92		0.84		0.18	0.14	0.13	1.00	No Sanctions	9.00	9.86	0.31	3.00
Argentina	2019	0.27		0.50	41.87	11.13	7.73	0.86		0.82		0.20		0.14	1.00	No Sanctions	9.00	9.20	0.34	3.00
Bolivia	2010	0.60	0.19	0.50	37.31	10.24	4.37	2.43	0.19	0.90	0.12	0.69	0.29	0.11	0.00	Weak Sanctions	7.00	8.55	0.65	3.00
Bolivia	2012	0.55		0.50	37.37	9.86	9.74	2.16	0.20	0.83	0.10	0.86	0.17	0.17	0.00	Weak Sanctions	7.00	8.61	0.66	3.00
Brazil	2010	0.23	0.16	0.52	38.93	8.11	2.71	2.11	-0.18	0.84	0.08	0.21	0.11	0.06	1.00	Weak Sanctions	8.00	9.56	0.10	3.00
Brazil	2012	0.19		0.50	37.84	8.98	11.99	1.62	-0.19	0.89	0.06	0.21	0.12	0.05	1.00	Weak Sanctions	8.00	9.61	0.09	3.00
Brazil	2014	0.25	0.11	0.50	39.65	8.20	8.52	1.89	-0.14	0.77	0.09	0.26	0.13	0.08	1.00	Weak Sanctions	8.00	9.63	0.08	3.00
Brazil	2017	0.27		0.50	38.60	8.79	6.52	1.91	0.15	0.79	0.18	0.36	0.37	0.14	1.00	Weak Sanctions	8.00	9.54	0.20	3.00
Brazil	2019	0.24		0.50	39.15	8.93	7.73	0.81		0.77		0.32	0.34	0.10	1.00	Weak Sanctions	8.00	9.07	0.26	3.00
Chile	2010	0.11	0.06	0.62	47.25	10.42	4.52	2.14	-0.18	0.94	0.04	0.32	0.06	0.04	1.00	No Sanctions	10.00	9.85	0.06	3.00
Chile	2012	0.13		0.63	46.77	10.41	9.72	2.01	-0.20	0.74	0.04	0.34	0.04	0.09	1.00	No Compulsory Voting	10.00	9.93	0.06	3.00
Chile	2014	0.06	0.02	0.67	49.39	10.49	8.54	2.16	-0.19	0.72	0.07	0.37	0.05	0.04	1.00	No Compulsory Voting	10.00	9.97	0.04	3.00
Chile	2017	0.10		0.51	42.48	11.01	8.29	1.97		0.55		0.43	0.04	0.10	1.00	No Compulsory Voting	10.00	9.98	0.04	3.00
Colombia	2012	0.20	0.24	0.50	36.96	9.58	8.07	1.65	-0.09	0.65	0.10	0.32	0.20	0.09	1.00	No Compulsory Voting	7.00	9.40	0.16	1.00
Colombia	2014	0.30	0.08	0.50	37.93	9.67	9.06	1.83	-0.01	0.61	0.10	0.40	0.28	0.10	1.00	No Compulsory Voting	7.00	9.47	0.19	1.00
Colombia	2016	0.27		0.50	39.50	9.85	8.03	1.82	0.04	0.61	0.11	0.48	0.24	0.14	1.00	No Compulsory Voting	7.00	9.50	0.19	1.00
Colombia	2018	0.26	0.08	0.50	40.32	9.91	8.04	1.83	0.06	0.67	0.10	0.51	0.29	0.11	1.00	No Compulsory Voting	7.00	8.81	0.21	1.00
Costa Rica	2012	0.13		0.51	43.49	8.66	7.20	2.14	-0.27	0.66	0.05	0.25	0.04	0.05	1.00	No Sanctions	10.00	9.46	0.06	3.00
Costa Rica	2014	0.08	0.02	0.51	42.69	8.70	7.21	2.07	-0.14	0.72	0.07	0.30	0.15	0.07	1.00	No Sanctions	10.00	9.49	0.06	3.00
Costa Rica	2016	0.14		0.50	40.78	9.08	9.17	2.00	-0.17	0.65	0.07	0.33	0.10	0.08	1.00	No Sanctions	10.00	9.55	0.07	3.00
Costa Rica	2018	0.18		0.50	40.47	9.53	7.90	0.80		0.73		0.33		0.10	1.00	No Sanctions	10.00	9.59	0.07	3.00
Dominican Republic	2012	0.31	0.24	0.50	39.45	9.45	8.79	2.43	0.33	0.71	0.18	0.57	0.62	0.08	1.00	No Compulsory Voting	8.00	9.38	0.69	1.00
Dominican Republic	2014	0.38	0.24	0.50	40.45	9.50	8.72	2.35	0.22	0.79	0.11	0.65	0.45	0.09	1.00	No Compulsory Voting	8.00	9.47	0.68	1.00
Dominican Republic	2016	0.42		0.50	39.82	9.56	7.70	2.41	0.32	0.81	0.21	0.67	0.43	0.12	1.00	No Compulsory Voting	7.00	9.58	0.70	1.00
Dominican Republic	2019	0.44	0.23	0.50	40.09	9.68	8.16	1.04		0.71		0.67		0.08	1.00	No Compulsory Voting	8.00	9.02	0.68	1.00
Ecuador	2010	0.25	0.08	0.51	39.44	10.12	4.29	2.49	-0.05	0.93	0.09	0.43	0.11	0.08	0.00	Weak Sanctions	5.00	9.14	0.81	1.00
Ecuador	2012	0.21		0.50	39.03	10.54	8.42	2.13	-0.05	0.89	0.11	0.42	0.12	0.07	0.00	Weak Sanctions	5.00	9.25	0.82	1.00
Ecuador	2014	0.30	0.09	0.51	39.46	10.68	8.04	2.24	0.14	0.92	0.13	0.58	0.32	0.07	0.00	Weak Sanctions	5.00	9.31	0.85	1.00
Ecuador	2016	0.18		0.50	38.63	11.43	7.28	2.14	0.04	0.86	0.14	0.54	0.18	0.05	0.00	Weak Sanctions	5.00	9.26	0.85	1.00
Ecuador	2019	0.17		0.50	38.20	11.47	7.79	1.16		0.88		0.55		0.08	0.00	Weak Sanctions	5.00	8.73	0.54	1.00
El Salvador	2012	0.11		0.50	40.30	7.48	6.64	1.97	-0.08	0.69	0.12	0.32	0.21	0.04	1.00	No Compulsory Voting	8.00	8.95	0.38	3.00
El Salvador	2014	0.08	0.04	0.55	40.56	8.52	8.66	2.23	-0.10	0.75	0.06	0.36	0.21	0.03	1.00	No Compulsory Voting	8.00	8.98	0.36	3.00
El Salvador	2016	0.08	0.08	0.50	40.18	8.91	7.89	2.19	-0.08	0.72	0.09	0.39	0.19	0.03	1.00	No Compulsory Voting	8.00	9.02	0.38	3.00
El Salvador	2018	0.05	0.05	0.50	39.97	9.02	6.46	1.01	-0.07	0.67	0.08	0.41	0.25	0.03	1.00	No Compulsory Voting	8.00	8.31	0.43	3.00
Guatemala	2012	0.08	0.35	0.49	38.73	6.90	5.07	2.68	0.15	0.79	0.15	0.76	0.17	0.07	1.00	No Compulsory Voting	8.00	8.81	0.39	1.00
Guatemala	2014	0.17	0.12	0.50	39.33	6.27	6.60	2.92	0.04	0.72	0.09	0.68	0.22	0.02	1.00	No Compulsory Voting	8.00	8.85	0.28	1.00
Guatemala	2017	0.07		0.50	38.11	8.07	7.11	2.49	0.22	0.76	0.17	0.86	0.21	0.09	1.00	No Compulsory Voting	8.00	8.90	0.27	1.00
Guatemala	2019	0.04	0.12	0.51	38.02	8.09	7.33	1.40	0.13	0.63	0.15	0.69	0.21	0.10	1.00	No Compulsory Voting	8.00	8.44	0.34	1.00
Honduras	2012	0.05		0.50	39.22	6.75	6.74	2.46	-0.02	0.51	0.14	0.46	0.20	0.06	0.00	No Sanctions	7.00	8.42	0.59	1.00
Honduras	2014	0.21	0.18	0.50	39.25	7.41	6.14	2.93	0.03	0.75	0.10	0.45	0.32	0.05	0.00	No Sanctions	7.00	8.44	0.56	1.00
Honduras	2016	0.15		0.51	38.24	7.62	7.42	2.70	0.12	0.62	0.15	0.57	0.33	0.08	0.00	No Sanctions	7.00	8.48	0.63	1.00
Honduras	2018	0.15	0.22	0.50	38.15	7.56	6.09	1.44	0.09	0.72	0.16	0.54	0.23	0.08	0.00	No Sanctions	7.00	7.83	0.60	1.00
Jamaica	2012	0.20		0.50	40.19	10.23	7.07	2.16	-0.06	0.62	0.11	0.39	0.24	0.02	1.00	No Compulsory Voting	9.00	8.86	0.15	1.00
Jamaica	2014	0.28	0.06	0.50	39.85	10.24	7.17	2.23	-0.10	0.57	0.08	0.40	0.22	0.03	1.00	No Compulsory Voting	9.00	8.85	0.18	1.00
Jamaica	2017	0.29		0.50	39.41	10.28	8.28	2.11	-0.04	0.53	0.12	0.43	0.27	0.03	1.00	No Compulsory Voting	9.00	8.87	0.14	2.00
Jamaica	2019	0.29	0.09	0.50	40.08	10.32	7.41	1.10	-0.08	0.54	0.08	0.43	0.24	0.03	1.00	No Compulsory Voting	9.00	8.63	0.14	2.00
Mexico	2010	0.20	0.17	0.50	39.43	8.94	4.28	2.38	-0.05	0.71	0.09	0.40	0.22	0.06	1.00	No Sanctions	8.00	9.60	0.29	3.00
Mexico	2012	0.17		0.51	40.00	8.77	7.23	2.03	-0.15	0.67	0.07	0.34	0.15	0.04	1.00	No Sanctions	8.00	9.66	0.30	3.00
Mexico	2014	0.24	0.15	0.50	40.85	9.20	8.58	2.23	-0.08	0.75	0.10	0.40	0.17	0.04	1.00	No Sanctions	8.00	9.67	0.35	3.00
Mexico	2016	0.24		0.49	40.58	9.39	7.34	2.12	-0.02	0.69	0.09	0.43	0.24	0.09	1.00	No Sanctions	8.00	9.69	0.36	3.00
Mexico	2019	0.23	0.17	0.51	41.96	9.86	7.96	1.04	0.01	0.80	0.10	0.46	0.23	0.07	1.00	No Sanctions	8.00	9.20	0.37	3.00
Panama	2012	0.09		0.50	38.84	10.47	5.59	2.19	-0.25	0.70	0.06	0.20	0.11	0.04	0.00	No Sanctions	9.00	9.80	0.42	1.00
Panama	2014	0.13	0.06	0.50	38.64	11.20	7.97	1.64	0.12	0.70	0.06	0.57	0.51	0.07	0.00	No Sanctions	9.00	9.90	0.47	1.00
Panama	2017	0.12		0.50	40.10	10.91	9.06	2.03	-0.12	0.69	0.07	0.35	0.19	0.06	0.00	No Sanctions	9.00	10.01	0.41	1.00
Panama	2018	0.11		0.50	39.55	10.65	7.26	1.19		0.67		0.47		0.09	0.00	No Sanctions	9.00	10.03	0.41	1.00
Paraguay	2012	0.08	0.14	0.49	36.64	9.22	8.57	2.65	0.10	0.61	0.09	0.58	0.32	0.13	0.00	No Sanctions	8.00	8.88	0.30	1.00
Paraguay	2014	0.08	0.12	0.50	37.51	9.92	7.88	2.23	0.22	0.74	0.12	0.67	0.36	0.15	0.00	No Sanctions	9.00	9.03	0.46	1.00
Paraguay	2016	0.12	0.15	0.50	39.81	9.12	7.14	2.42	0.22	0.68	0.11	0.68	0.38	0.14	0.00	No Sanctions	9.00	9.08	0.48	1.00
Paraguay	2019	0.14	0.15	0.50	40.04	9.64	7.41	1.14	0.21	0.73	0.11	0.70	0.40	0.09	0.00	No Sanctions	9.00	8.60	0.45	1.00
Peru	2010	0.22	0.12	0.49	39.06	11.07	5.07	2.24			0.12	0.58	0.18	0.12	0.00	Weak Sanctions	9.00	9.16	0.21	1.00
Peru	2012	0.04		0.50	39.38	11.01	7.84	2.10	0.02	0.91	0.12	0.52	0.10	0.13	0.00	Weak Sanctions	9.00	9.26	0.21	1.00
Peru	2014	0.05	0.06	0.50	39.91	10.96	8.93	1.95	-0.00	0.85	0.11	0.55	0.10	0.09	0.00	Weak Sanctions	9.00	9.32	0.20	1.00
Peru	2017	0.13		0.50	38.74	11.18	7.72	2.13	0.20	0.89	0.14	0.68	0.26	0.14	0.00	Weak Sanctions	9.00	9.39	0.21	1.00
Peru	2019	0.11	0.11	0.50	38.80	11.69	7.43	1.13	0.15	0.82	0.12	0.64	0.24	0.14	0.00	Weak Sanctions	9.00	8.85	0.22	1.00
Trinidad and Tobago	2012	0.19		0.50	39.39	10.32	8.38	1.53	-0.04	0.69	0.14	0.31	0.22	0.08	0.00	No Compulsory Voting	10.00	10.31	0.23	3.00
Trinidad and Tobago	2014	0.10		0.50	39.92	10.89	9.18	1.63	-0.12	0.68	0.09	0.24	0.25	0.06	0.00	No Compulsory Voting	10.00	10.34	0.23	3.00
Uruguay	2010	0.31	0.06	0.53	44.68	9.52	5.34	2.10	-0.02	0.94	0.08	0.22	0.30	0.11	1.00	Weak Sanctions	10.00	9.71	0.11	4.00
Uruguay	2012	0.17		0.50	45.59	9.59	9.95	2.00	-0.17	0.90	0.09	0.17	0.11	0.08	1.00	Weak Sanctions	10.00	9.79	0.11	4.00
Uruguay	2014	0.11	0.03	0.53	46.50	9.70	9.87	2.02	-0.13	0.85	0.09	0.19	0.17	0.08	1.00	Weak Sanctions	10.00	9.86	0.11	4.00
Uruguay	2017	0.27		0.53	46.56	9.92	8.14	2.07		0.89		0.24	0.14	0.12	1.00	Weak Sanctions	10.00	9.90	0.10	4.00
Uruguay	2019	0.27		0.51	46.21	9.66	7.59	0.68		0.84		0.26		0.11	1.00	Weak Sanctions	10.00	9.69	0.11	4.00
Venezuela	2010	0.11	0.12	0.51	39.41	10.50	4.22	2.14	0.09	0.69	0.15	0.56	0.24	0.08	0.00	No Compulsory Voting	-3.00	9.75	0.97	0.00
Venezuela	2012	0.08		0.50	40.36	10.59	6.91	2.00	0.02	0.82	0.13	0.47	0.18	0.04	0.00	No Compulsory Voting	-3.00	9.83	0.97	0.00
Venezuela	2016	0.16		0.50	40.30	11.17	9.54	2.09	0.46	0.82	0.22	0.88	0.36	0.16	0.00	No Compulsory Voting	4.00	9.63	0.98	0.00

Note: Data are from the 2010-2019 AmericasBarometer survey.

Political Participation Variable

Table A2: Questions and responses used for participation variable and as dependent variables in this analysis.

Question	Responses	AmericasBarometer Code
Did you vote in the last presidential elections of (year of last presidential elections)?	Voted or did not vote	vb2
Have you attended a town meeting, city council meeting or other meeting in the past 12 months?	Yes or no	np1
Have you sought assistance from or presented a request to any office, official or councilperson of the municipality within the past 12 months?	Yes or no	cp8
Meetings of a political party or political organization? Do you attend them...	Once a week, once or twice a month, once or twice a year or never in last 12 months	cp13
In the last 12 months, have you participated in a demonstration or protest march?	Yes or no	prot3

Note: Data are from the 2010-2019 AmericasBarometer survey.

Table A3: Factor loadings for participation variable

Participation Type	Factor 1	Uniquenesses
Vote (vb2)	0.15	0.98
Town meeting (np1)	0.47	0.78
Community meeting (cp8)	0.52	0.73
Political meeting (cp13)	0.50	0.75
Protest (prot3)	0.30	0.91

Note: Data are from the 2010-2019 AmericasBarometer survey. Results are from an exploratory factor analysis which shows one meaningful dimension of participation. For full question wordings for the participation questions refer to Table A2.

Model Results

Table A4: Matching results with standard error

	Participation
Transfer	0.035*
	(0.001)
Female	−0.025*
	(0.002)
Income	−0.020*
	(0.003)
Age	0.076*
	(0.007)
Children	0.006*
	(0.001)
Education	0.056*
	(0.004)
Constant	0.087*
	(0.003)
Observations	49,846
R^2	0.026
Adjusted R^2	0.025
Residual Std. Error	0.112 (df = 49839)
F Statistic	217.837* (df = 6; 49839)
Note:	*p<0.05

Note: Data are from the 2010-2019 AmericasBarometer survey. Results are from a cluster matching procedure with receipt of a CCT transfer as the treatment. Standard errors in parantheses.

Table A5: Balance table for cluster matching model

	Before Matching	After Matching
Female mean treatment	0.5401	0.56319
Female mean control	0.48947	0.56319
std mean diff	10.158	0
Client mean treatment	0.15973	0.056868
Client mean control	0.1236	0.056868
std mean diff	9.8605	0
Income mean treatment	0.34238	0.34562
Income mean control	0.46862	0.34562
std mean diff	-51.017	0
Age mean treatment	0.26304	0.20165
Age mean control	0.28758	0.20151
std mean diff	-13.866	0.089929
Children mean treatment	2.4588	1.5896
Children mean control	1.8992	1.5896
std mean diff	24.187	0
Education mean treatment	0.44435	0.50365
Education mean control	0.53325	0.50365
std mean diff	-40.038	0

Note: Data are from the 2010-2019 AmericasBarometer survey. The results of the matching model are shown in Model 3 in Table A4.

Table A6: Results from base transfer models

	Participation	Vote	Meeting	Community	Party Meeting	Protest
	(1)	(2)	(3)	(4)	(5)	(6)
Transfer	0.032*	0.225*	0.298*	0.111*	0.089*	0.202*
	(0.001)	(0.022)	(0.028)	(0.007)	(0.005)	(0.029)
Female	−0.023*	0.103*	−0.241*	−0.073*	−0.077*	−0.202*
	(0.001)	(0.016)	(0.022)	(0.005)	(0.004)	(0.023)
Income	−0.029*	0.199*	−0.357*	−0.139*	−0.076*	0.113*
	(0.002)	(0.033)	(0.045)	(0.010)	(0.008)	(0.046)
Age	0.077*	4.988*	0.687*	0.332*	0.078*	−0.187*
	(0.004)	(0.067)	(0.078)	(0.017)	(0.014)	(0.080)
Children	0.004*	0.088*	0.050*	0.019*	0.004*	0.007
	(0.0003)	(0.006)	(0.006)	(0.001)	(0.001)	(0.007)
Education	0.061*	1.425*	0.726*	0.112*	0.125*	1.472*
	(0.003)	(0.044)	(0.055)	(0.013)	(0.010)	(0.058)
Constant	0.091*	−0.990*	−2.622*	1.357*	1.193*	−3.266*
	(0.005)	(0.081)	(0.067)	(0.023)	(0.015)	(0.073)
Number of respondents	86106	102085	90045	103855	95376	104053
Number of surveys	66	76	67	77	71	77
var(survey-level constants)	0.001	0.411	0.172	0.034	0.013	0.248
sd(survey-level constants	0.035	0.641	0.415	0.185	0.113	0.498
AIC	-74345.165	96072.188	60105.565	247274.832	167563.52	59489.76

*p<0.05

Table A7: Results from sex interactive models

	Participation	Vote	Meeting	Community	Party Meeting	Protest
	(1)	(2)	(3)	(4)	(5)	(6)
Transfer	0.036*	0.156*	0.291*	0.118*	0.109*	0.264*
	(0.002)	(0.030)	(0.037)	(0.009)	(0.007)	(0.039)
Female	−0.022*	0.078*	−0.244*	−0.071*	−0.069*	−0.173*
	(0.001)	(0.018)	(0.025)	(0.006)	(0.004)	(0.025)
Transfer × Female	−0.006*	0.132*	0.013	−0.012	−0.038*	−0.131*
	(0.003)	(0.041)	(0.052)	(0.012)	(0.009)	(0.053)
Income	−0.029*	0.200*	−0.357*	−0.139*	−0.075*	0.113*
	(0.002)	(0.033)	(0.045)	(0.010)	(0.008)	(0.046)
Age	0.077*	4.993*	0.687*	0.332*	0.076*	−0.193*
	(0.004)	(0.067)	(0.078)	(0.017)	(0.014)	(0.080)
Children	0.004*	0.088*	0.050*	0.019*	0.004*	0.007
	(0.0003)	(0.006)	(0.006)	(0.001)	(0.001)	(0.007)
Education	0.061*	1.426*	0.726*	0.112*	0.125*	1.471*
	(0.003)	(0.044)	(0.055)	(0.013)	(0.010)	(0.058)
Constant	0.091*	−0.978*	−2.621*	1.356*	1.189*	−3.277*
	(0.005)	(0.081)	(0.067)	(0.023)	(0.015)	(0.073)
Number of respondents	86106	102085	90045	103855	95376	104053
Number of surveys	66	76	67	77	71	77
var(survey-level constants)	0.001	0.411	0.172	0.034	0.013	0.247
sd(survey-level constants	0.035	0.641	0.415	0.185	0.113	0.497
AIC	-74338.675	96063.642	60107.497	247282.796	167556.844	59485.72

Note: *p<0.05

Table A8: Results from conditions enforced interactive models

	Participation	Vote	Meeting	Community	Party Meeting	Protest
	(1)	(2)	(3)	(4)	(5)	(6)
Transfer	0.048*	0.155*	0.353*	0.175*	0.143*	0.404*
	(0.006)	(0.051)	(0.055)	(0.020)	(0.021)	(0.086)
Conditions enforced	0.002	0.306*	−0.061	−0.073	0.044	0.042
	(0.010)	(0.146)	(0.127)	(0.040)	(0.035)	(0.184)
Transfer × Conditions enforced	−0.022*	0.066	−0.093	−0.091*	−0.076*	−0.282*
	(0.007)	(0.063)	(0.073)	(0.025)	(0.026)	(0.111)
Female	−0.023*	0.103*	−0.240*	−0.071*	−0.076*	−0.202*
	(0.001)	(0.016)	(0.022)	(0.005)	(0.004)	(0.023)
Income	−0.029*	0.197*	−0.352*	−0.140*	−0.073*	0.104*
	(0.002)	(0.033)	(0.045)	(0.010)	(0.008)	(0.046)
Age	0.075*	4.986*	0.682*	0.320*	0.068*	−0.252*
	(0.004)	(0.067)	(0.078)	(0.017)	(0.014)	(0.081)
Children	0.004*	0.089*	0.050*	0.020*	0.005*	0.010
	(0.0003)	(0.006)	(0.006)	(0.001)	(0.001)	(0.007)
Education	0.061*	1.423*	0.719*	0.113*	0.124*	1.468*
	(0.003)	(0.044)	(0.055)	(0.013)	(0.010)	(0.058)
No sanctions	−0.012	0.309*	−0.349*	−0.088*	−0.012	0.214
	(0.008)	(0.122)	(0.113)	(0.031)	(0.029)	(0.152)
Weak sanctions	0.006	1.243*	−0.110	0.001	0.0003	0.350
	(0.009)	(0.145)	(0.133)	(0.035)	(0.032)	(0.179)
Polity_score	0.086*	0.362	0.278	0.227*	0.361*	0.635
	(0.027)	(0.406)	(0.360)	(0.106)	(0.101)	(0.504)
GDP per capita (logged)	−0.033*	0.202	−0.447*	−0.303*	−0.047	0.281
	(0.015)	(0.235)	(0.208)	(0.059)	(0.056)	(0.290)
Presidentialism	0.072*	1.094*	0.458	0.122	0.288*	0.169
	(0.019)	(0.287)	(0.259)	(0.074)	(0.072)	(0.357)
Linkages	−0.042*	−0.110	−0.397	−0.157*	−0.092	−0.442
	(0.016)	(0.253)	(0.215)	(0.063)	(0.059)	(0.312)
Constant	0.035	−2.370*	−2.374*	1.461*	0.844*	−3.999*
	(0.031)	(0.459)	(0.418)	(0.120)	(0.116)	(0.575)
Number of respondents	86106	102085	90045	103855	95376	104053
Number of surveys	66	76	67	77	71	77
var(survey-level constants)	0.001	0.15	0.096	0.018	0.011	0.247
sd(survey-level constants	0.024	0.025	0.393	0.17	0.304	0.175
var(transfer random coefficients)	0.001	0.15	0.031	0.008	0.009	0.142
sd(transfer random coefficients)	0.025	0.17	0.175	0.087	0.096	0.377
AIC	-74519.61	95985.99	60073.918	247103.134	167348.376	59402.812

Note: *p<0.05

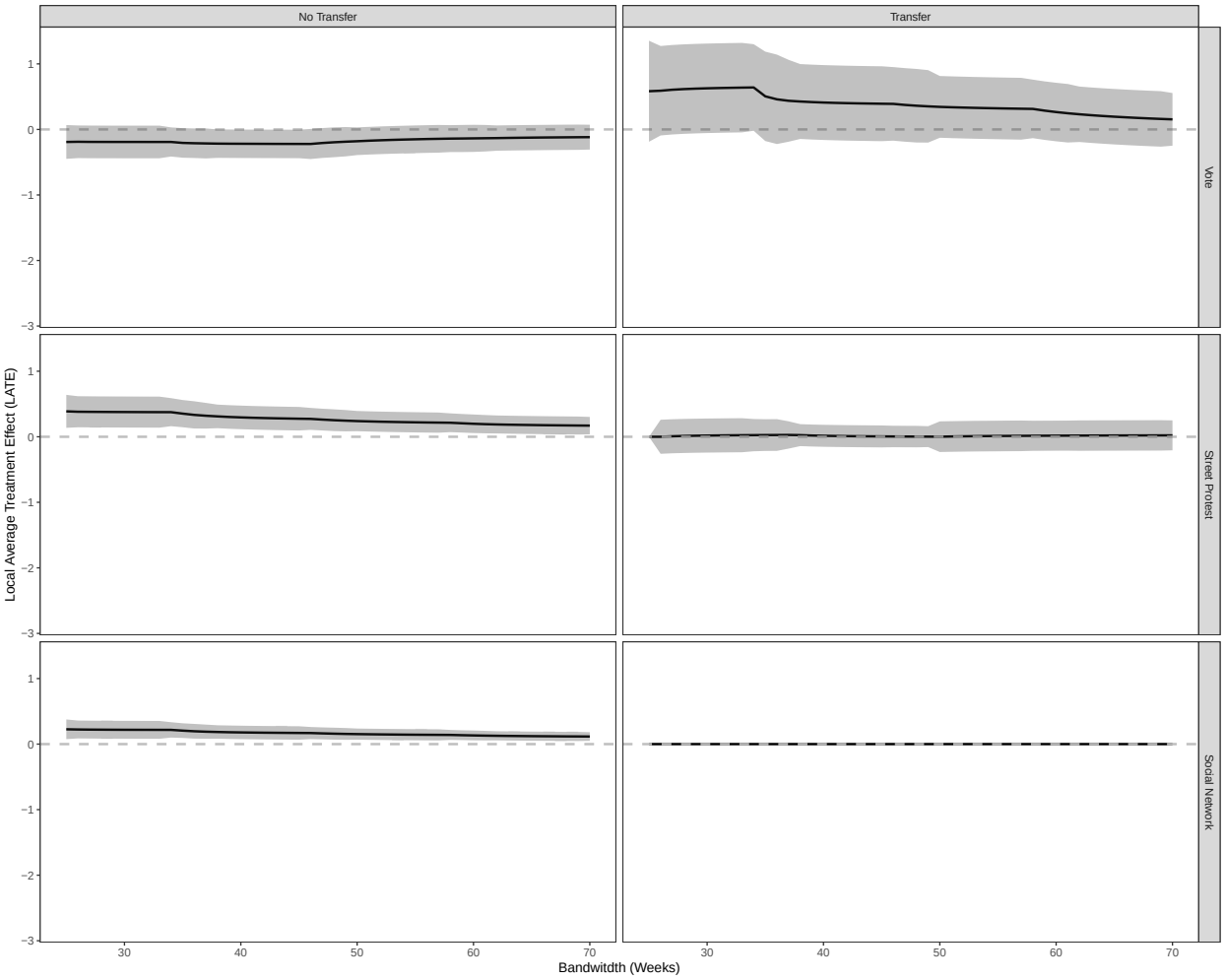
Table A9: Results from compulsory voting interactive models

	Participation	Vote	Meeting	Community	Party Meeting	Protest
	(1)	(2)	(3)	(4)	(5)	(6)
Transfer	0.046*	0.316*	0.447*	0.147*	0.133*	0.341*
	(0.006)	(0.045)	(0.052)	(0.021)	(0.021)	(0.095)
No sanctions	−0.014	0.365*	−0.311*	−0.093*	−0.021	0.219
	(0.008)	(0.124)	(0.108)	(0.036)	(0.030)	(0.161)
Weak sanctions	0.001	1.365*	−0.066	−0.046	−0.022	0.446*
	(0.009)	(0.143)	(0.122)	(0.040)	(0.034)	(0.181)
Transfer × No sanctions	−0.012	−0.127*	−0.223*	−0.016	−0.037	−0.052
	(0.008)	(0.064)	(0.082)	(0.030)	(0.031)	(0.131)
Transfer × Weak sanctions	−0.024*	−0.260*	−0.264*	−0.078*	−0.082*	−0.271*
	(0.009)	(0.068)	(0.077)	(0.031)	(0.032)	(0.135)
Female	−0.023*	0.103*	−0.240*	−0.071*	−0.076*	−0.202*
	(0.001)	(0.016)	(0.022)	(0.005)	(0.004)	(0.023)
Income	−0.029*	0.195*	−0.353*	−0.139*	−0.073*	0.104*
	(0.002)	(0.033)	(0.045)	(0.010)	(0.008)	(0.046)
Age	0.075*	4.980*	0.680*	0.320*	0.068*	−0.251*
	(0.004)	(0.067)	(0.078)	(0.017)	(0.014)	(0.081)
Children	0.004*	0.090*	0.050*	0.020*	0.005*	0.010
	(0.0003)	(0.006)	(0.006)	(0.001)	(0.001)	(0.007)
Education	0.061*	1.422*	0.716*	0.112*	0.123*	1.467*
	(0.003)	(0.044)	(0.055)	(0.013)	(0.010)	(0.058)
Conditions enforced	0.007	0.327*	−0.098	−0.014	0.062	−0.109
	(0.009)	(0.142)	(0.128)	(0.036)	(0.035)	(0.183)
Polity score	0.086*	0.311	0.246	0.228*	0.360*	0.624
	(0.027)	(0.407)	(0.355)	(0.105)	(0.101)	(0.506)
GDP per capita (logged)	−0.032*	0.180	−0.446*	−0.292*	−0.045	0.254
	(0.015)	(0.237)	(0.205)	(0.059)	(0.056)	(0.289)
Presidentialism	0.071*	1.085*	0.457	0.114	0.285*	0.161
	(0.020)	(0.290)	(0.255)	(0.073)	(0.073)	(0.358)
Linkages	−0.043*	−0.070	−0.365	−0.174*	−0.096	−0.400
	(0.016)	(0.252)	(0.211)	(0.063)	(0.060)	(0.310)
Constant	0.035	−2.396*	−2.365*	1.443*	0.844*	−3.929*
	(0.031)	(0.464)	(0.410)	(0.120)	(0.116)	(0.580)
Number of respondents	86106	102085	90045	103855	95376	104053
Number of surveys	66	76	67	77	71	77
var(survey-level constants)	0.001	0.147	0.093	0.018	0.012	0.248
sd(survey-level constants	0.024	0.026	0.391	0.137	0.305	0.132
var(transfer random coefficients)	0.001	0.147	0.017	0.009	0.01	0.144
sd(transfer random coefficients)	0.026	0.137	0.132	0.093	0.099	0.38
AIC	-74508.751	95975.763	60064.994	247115.78	167356.776	59406.66

Note: *p<0.05

BES Regression Discontinuity

Figure A1: Local Average Treatment Effect by Transfer Status and Participation Type (Brazilian Election Study)



Note: Data are from the 2014 Brazilian Election Study (BES). The left column displays the results for people who did not receive a transfer, and the right column those who did receive a transfer. Type of participation is given by the text in the grid rows. Results show Local Average Treatment Effect (LATE) by bandwidth selection in months. Sensitivity results taken from non-parametric models.

Mediation

Table A10: Direct and mediated effects of transfers on each participation outcome

	Type	Effect	Lower	Upper	Outcome
1	Mediation	0.002	0.001	0.002	Participation
2	Direct	0.029	0.025	0.033	Participation
3	Mediation	0.002	0.001	0.002	Vote
4	Direct	0.041	0.032	0.050	Vote
5	Mediation	0.002	0.001	0.003	Attended meeting
6	Direct	0.021	0.015	0.028	Attended meeting
7	Mediation	0.005	0.003	0.006	Community
8	Direct	0.115	0.098	0.132	Community
9	Mediation	0.004	0.003	0.006	Party meeting
10	Direct	0.093	0.079	0.108	Party meeting
11	Mediation	0.002	0.001	0.002	Protest
12	Direct	0.013	0.007	0.019	Protest

Note: Mediation and Direct results from each form of participation.

Table A11: Constituent models for mediation analyses

	Client	Participation	Client	Vote	Client	Attended Meeting	Client	Community	Client	Party Meeting	Client	Protest
	probit	*OLS*	*probit*	*OLS*	*probit*	*OLS*	*probit*	*OLS*	*probit*	*OLS*	*probit*	*OLS*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Transfer_cct	0.146*	0.029*	0.139*	0.041*	0.143*	0.021*	0.138*	0.115*	0.142*	0.093*	0.138*	0.013*
	(0.019)	(0.002)	(0.018)	(0.005)	(0.018)	(0.003)	(0.018)	(0.009)	(0.018)	(0.007)	(0.018)	(0.003)
Female	−0.107*	−0.024*	−0.111*	0.012*	−0.104*	−0.022*	−0.112*	−0.091*	−0.104*	−0.079*	−0.112*	−0.019*
	(0.015)	(0.001)	(0.014)	(0.003)	(0.015)	(0.003)	(0.014)	(0.007)	(0.015)	(0.005)	(0.014)	(0.002)
Client		0.054*		0.053*		0.069*		0.162*		0.147*		0.064*
		(0.002)		(0.005)		(0.004)		(0.010)		(0.008)		(0.004)
Income	−0.114*	−0.032*	−0.111*	0.019*	−0.122*	−0.030*	−0.107*	−0.178*	−0.118*	−0.088*	−0.108*	0.003
	(0.033)	(0.003)	(0.032)	(0.008)	(0.032)	(0.006)	(0.031)	(0.015)	(0.032)	(0.012)	(0.031)	(0.005)
Age	−0.601*	0.094*	−0.595*	0.710*	−0.608*	0.089*	−0.608*	0.379*	−0.607*	0.132*	−0.598*	−0.001
	(0.056)	(0.005)	(0.053)	(0.012)	(0.054)	(0.009)	(0.053)	(0.025)	(0.054)	(0.019)	(0.052)	(0.008)
Children	0.026*	0.003*	0.026*	0.009*	0.028*	0.003*	0.028*	0.017*	0.028*	0.003	0.027*	−0.0002
	(0.004)	(0.0004)	(0.004)	(0.001)	(0.004)	(0.001)	(0.004)	(0.002)	(0.004)	(0.002)	(0.004)	(0.001)
Education	0.143*	0.066*	0.139*	0.203*	0.140*	0.072*	0.125*	0.119*	0.139*	0.156*	0.133*	0.103*
	(0.038)	(0.004)	(0.037)	(0.009)	(0.037)	(0.007)	(0.036)	(0.018)	(0.037)	(0.013)	(0.036)	(0.006)
CountryBolivia	0.235*	0.063*	0.150*	0.068*	0.213*	0.059*	0.143*	0.481*	0.169*	0.070*	0.137*	−0.024*
	(0.048)	(0.005)	(0.046)	(0.012)	(0.047)	(0.009)	(0.046)	(0.024)	(0.046)	(0.018)	(0.046)	(0.008)
CountryBrazil	0.224*	0.001	0.148*	0.026*	0.215*	0.040*	0.143*	0.043*	0.168*	−0.052*	0.141*	−0.046*
	(0.043)	(0.004)	(0.041)	(0.010)	(0.042)	(0.008)	(0.041)	(0.021)	(0.042)	(0.015)	(0.041)	(0.007)
CountryChile	−0.378*	−0.007	−0.459*	−0.059*	−0.354*	0.001	−0.426*	0.176*	−0.404*	−0.101*	−0.426*	−0.080*
	(0.059)	(0.005)	(0.058)	(0.011)	(0.054)	(0.008)	(0.053)	(0.022)	(0.053)	(0.016)	(0.053)	(0.007)
CountryColombia	0.065	0.029*	0.013	−0.134*	0.062	0.035*	0.014	0.203*	0.027	0.085*	0.015	−0.015*
	(0.043)	(0.004)	(0.042)	(0.010)	(0.043)	(0.008)	(0.042)	(0.021)	(0.042)	(0.015)	(0.042)	(0.007)
CountryCosta Rica	−0.529*	0.006	−0.593*	−0.052*	−0.538*	0.023*	−0.593*	0.092*	−0.576*	−0.027	−0.586*	−0.025*
	(0.090)	(0.006)	(0.089)	(0.014)	(0.089)	(0.011)	(0.089)	(0.029)	(0.089)	(0.022)	(0.089)	(0.010)
CountryDominican Republic	0.490*	0.087*	0.457*	−0.065*	0.489*	0.074*	0.455*	0.394*	0.456*	0.354*	0.459*	−0.036*
	(0.043)	(0.004)	(0.039)	(0.010)	(0.042)	(0.008)	(0.038)	(0.020)	(0.041)	(0.016)	(0.038)	(0.007)
CountryEcuador	−0.108*	0.033*	−0.181*	0.107*	−0.118*	0.054*	−0.182*	0.278*	−0.161*	−0.002	−0.189*	−0.049*
	(0.045)	(0.004)	(0.043)	(0.010)	(0.044)	(0.008)	(0.043)	(0.020)	(0.043)	(0.015)	(0.043)	(0.007)
CountryEl Salvador	−0.260*	0.020*	−0.295*	−0.048*	−0.242*	0.028*	−0.307*	0.177*	−0.285*	0.075*	−0.293*	−0.076*
	(0.052)	(0.004)	(0.050)	(0.011)	(0.051)	(0.008)	(0.050)	(0.022)	(0.050)	(0.017)	(0.050)	(0.008)
CountryGuatemala	0.376*	0.061*	0.325*	−0.039*	0.377*	0.083*	0.321*	0.513*	0.341*	0.021	0.324*	−0.031*
	(0.041)	(0.004)	(0.040)	(0.010)	(0.041)	(0.008)	(0.040)	(0.021)	(0.040)	(0.015)	(0.040)	(0.007)
CountryHonduras	0.566*	0.039*	0.516*	−0.018	0.571*	0.062*	0.516*	0.207*	0.529*	0.104*	0.514*	−0.037*
	(0.047)	(0.005)	(0.046)	(0.012)	(0.047)	(0.009)	(0.046)	(0.024)	(0.046)	(0.018)	(0.046)	(0.008)
CountryJamaica	−0.096	0.010*	−0.152*	−0.226*	−0.087	0.028*	−0.156*	0.206*	−0.129*	0.039*	−0.150*	−0.088*
	(0.055)	(0.005)	(0.053)	(0.012)	(0.054)	(0.009)	(0.053)	(0.024)	(0.053)	(0.018)	(0.052)	(0.008)
CountryMexico	0.356*	0.022*	0.281*	−0.047*	0.350*	0.044*	0.278*	0.211*	0.305*	0.022	0.278*	−0.059*
	(0.042)	(0.004)	(0.040)	(0.010)	(0.041)	(0.008)	(0.040)	(0.020)	(0.040)	(0.015)	(0.040)	(0.007)
CountryPanama	−0.161*	0.060*	−0.186*	−0.078*	−0.132*	0.006	−0.181*	0.378*	−0.180*	0.326*	−0.195*	−0.051*
	(0.066)	(0.005)	(0.063)	(0.013)	(0.064)	(0.010)	(0.063)	(0.027)	(0.064)	(0.020)	(0.064)	(0.009)
CountryParaguay	0.167*	0.079*	0.104*	−0.058*	0.167*	0.050*	0.102*	0.486*	0.129*	0.205*	0.108*	0.013
	(0.042)	(0.004)	(0.041)	(0.010)	(0.041)	(0.008)	(0.041)	(0.020)	(0.041)	(0.015)	(0.040)	(0.007)
CountryPeru	−0.027	0.051*	−0.090	0.051*	−0.008	0.068*	−0.074	0.403*	−0.048	−0.005	−0.076	0.001
	(0.052)	(0.005)	(0.050)	(0.011)	(0.044)	(0.008)	(0.042)	(0.021)	(0.043)	(0.015)	(0.042)	(0.007)
CountryUruguay	−0.353*	0.019*	−0.424*	0.047*	−0.359*	0.035*	−0.426*	0.021	−0.408*	0.085*	−0.431*	−0.016*
	(0.054)	(0.004)	(0.053)	(0.011)	(0.053)	(0.008)	(0.053)	(0.022)	(0.053)	(0.016)	(0.053)	(0.007)
CountryVenezuela	0.042	0.054*	−0.054	−0.130*	0.016	0.101*	−0.054	0.386*	−0.029	0.067*	−0.065	−0.042*
	(0.059)	(0.006)	(0.057)	(0.014)	(0.058)	(0.011)	(0.057)	(0.028)	(0.057)	(0.021)	(0.057)	(0.009)
Year2012	0.286*	0.009*	0.232*	−0.005	0.265*	0.034*	0.229*	0.031	0.245*	0.015	0.221*	−0.020*
	(0.035)	(0.003)	(0.034)	(0.009)	(0.033)	(0.006)	(0.033)	(0.017)	(0.033)	(0.013)	(0.033)	(0.006)
Year2014	−0.163*	0.012*	−0.205*	−0.038*	−0.189*	0.019*	−0.212*	0.086*	−0.200*	0.036*	−0.217*	−0.015*
	(0.028)	(0.003)	(0.028)	(0.006)	(0.026)	(0.004)	(0.026)	(0.012)	(0.026)	(0.009)	(0.026)	(0.004)
Year2016	0.121*	0.014*	0.061	−0.059*	0.080	0.029*	0.066	0.095*	0.068	0.023	0.048	−0.001
	(0.051)	(0.005)	(0.050)	(0.012)	(0.048)	(0.009)	(0.049)	(0.023)	(0.049)	(0.017)	(0.048)	(0.008)
Year2018	−0.099*	0.025*	−0.138*	−0.046*	−0.120*	0.039*	−0.146*	0.178*	−0.131*	0.032*	−0.152*	0.0003
	(0.045)	(0.004)	(0.044)	(0.010)	(0.043)	(0.008)	(0.043)	(0.020)	(0.043)	(0.015)	(0.043)	(0.007)
Year2019	−0.020	0.021*	−0.044	−0.049*	−0.043	0.028*	−0.048	0.115*	−0.056	0.071*	−0.055	−0.009
	(0.036)	(0.003)	(0.034)	(0.008)	(0.032)	(0.006)	(0.031)	(0.015)	(0.032)	(0.012)	(0.031)	(0.005)
Constant	−1.160*	0.036*	−1.063*	0.495*	−1.135*	−0.018*	−1.051*	1.036*	−1.085*	1.074*	−1.051*	1.069*
	(0.047)	(0.004)	(0.044)	(0.011)	(0.045)	(0.008)	(0.044)	(0.022)	(0.044)	(0.016)	(0.044)	(0.007)
Observations	50,441	50,441	53,087	53,087	53,279	53,279	54,711	54,711	53,322	53,322	54,759	54,759
R^2		0.069		0.123		0.020		0.062		0.050		0.028
Adjusted R^2		0.068		0.123		0.019		0.062		0.050		0.028
Log Likelihood	−17,992.170		−19,299.210		−19,009.460		−19,833.400		−19,070.690		−19,857.050	
AIC	36,042.340		38,656.420		38,076.920		39,724.800		38,199.390		39,772.090	
Residual Std. Error		0.156 (df = 50411)		0.394 (df = 53057)		0.298 (df = 53249)		0.803 (df = 54681)		0.597 (df = 53292)		0.271 (df = 54729)
F Statistic		128.724* (df = 29; 50411)		257.046* (df = 29; 53057)		37.400* (df = 29; 53249)		125.678* (df = 29; 54681)		97.614* (df = 29; 53292)		55.067* (df = 29; 54729)
Note:												*p<0.05

Note: Results are from the two models estimated prior to the mediation analyses. The first model regresses clientelism on transfer status and control variables, including country and year dummies. The second model regresses form of participation on both transfer status and clientelism.

Mediation Sensitivity Analyses

Although mediation analyses are useful tools that can aid in evaluating the pathways between variables, they are still subject to a number of limitations and assumptions that must be kept in mind. As detailed in multiple works on causal mediation, one of the most important assumptions when conducting a mediation analysis is the assumption of sequential ignorability (Imai, Keele and Tingley 2010; Imai, Keele and Yamamoto 2010). Sequential ignorability assumes that mediation status is "effectively randomly assigned" given treatment status and values of the main predictor variable.

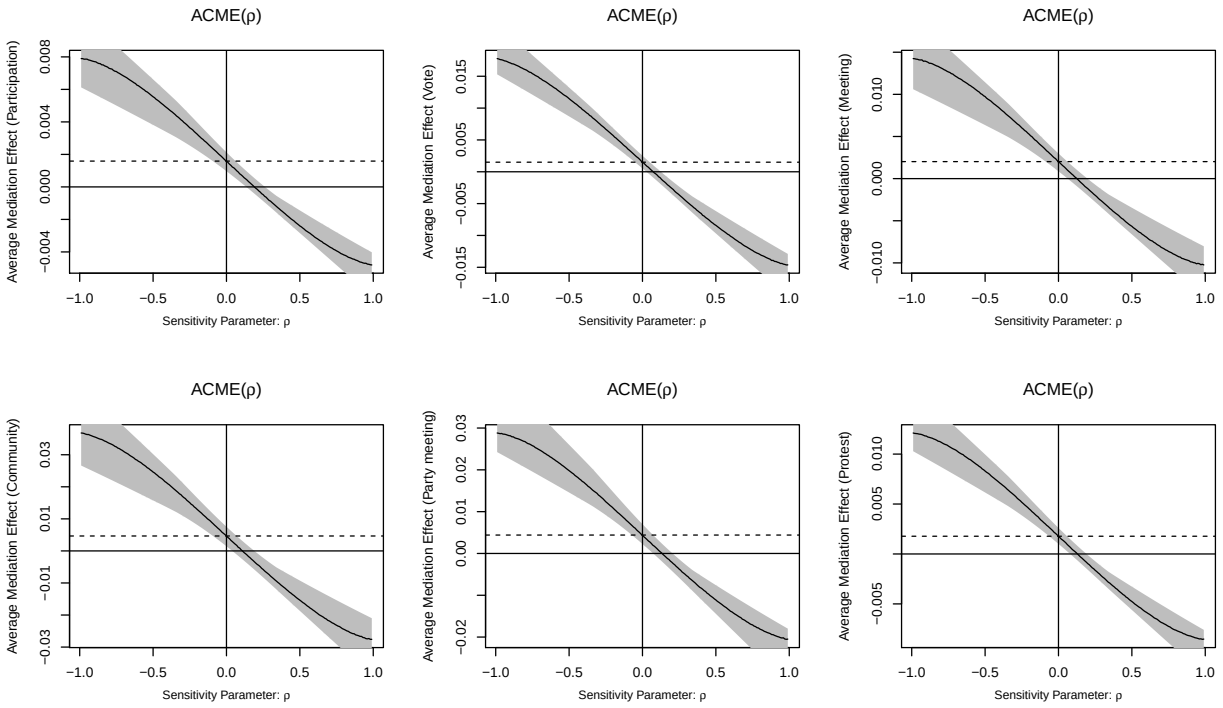
Sequential ignorability is a critical assumption given the possibility of a situation where the mediator variable and treatment variable are confounded, which can often be the case in practical research. Further, this assumption can still be violated even when the treatment is randomly assigned, because it assumes a lack of confounding with the mediator in both the pre-treatment and post-treatment. In the context of transfers and participation, estimates of the indirect effects of clientelism would be biased if both transfer status and clientelistic engagement are affected by an omitted variable, such as the partisan relationships in the surrounding community that may affect distribution, among other possible confounders.

However, because I use a non-parametric approach developed by Imai, Keele and Tingley (2010) and Imai, Keele and Yamamoto (2010) to estimate the mediation effects for each part of the participation outcome, I am able to use sensitivity analyses to better evaluate the degree of sensitivity of these results and test the sequential ignorability assumption. Given this, I implement these sensitivity analyses for each mediation model from Figure 4 and graphically show results in Figure A2.

By manipulating the correlations in the error terms in the constituent models that inform the mediation analysis, I probe, and thus evaluate, the change and robustness in the effect sizes of the mediator (Clientelism). Overall, these results suggest a moderate sensitivity in these mediation analyses. Beyond this though, there is still a degree of consistency in that over the majority of the range of possible correlations, effects remain positive and statistically significant. Note that this method does not address possibility of post-treatment confounders which can bring bias. However, due to this, I still include the same covariates from the main model specifications in the constituent mediation models, as well as the country and year dummies.

From these sensitivity results in Figure A2, mediation effects appear moderately sensitive to differences in the sensitivity parameter ρ, which is the correlation between the error terms of the constituent models in each mediation procedure. Despite this, overall effects for each variable tends to be positive across the majority of values of ρ. For instance, for the composite participation variable, the ρ value when the effect size becomes 0 is 0.19. This indicates that for 69% of the possible values of ρ, the mediated effects are positive. On the other hand, some of the particular participatory dimensions of the composite measure appear to be more sensitive than participation on the whole. For voting, the ρ is comparatively only 0.07, solving a community

Figure A2: Sensitivity with respect to error correlation for each participation dimension



Note: Data are from the 2010-2019 AmericasBarometer survey. The figure shows the average mediation effect for each type of participation, including the overall participation variable. As can be seen from these figures, while there is some variation in the sensitivity parameter ρ, for most values of ρ the effects for each variable is positive. However, with regard to the scale of the y-axes, note that the effect size varies based on the dimension of participation.

problem is 0.11, and protest is 0.13. While all of these still suggest for the most part positive effects, this means that for a participatory dimension like voting, only 57% of possible values of ρ is the mediating effect positive.

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