Policy Responsiveness in Developing Democracies: Evidence from Latin America

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Abstract

The literature on government responsiveness to public preferences has tended to treat responsiveness as a unitary phenomenon, asking whether or to what degree governments respond. In this paper, I argue that government responsiveness is likely to be unevenly distributed geographically. First, government responsiveness is absent in places controlled by non-state actors. Second, responsiveness decreases with voters' abstention. I provide empirical support for my arguments using administrative data from over 1,000 municipalities and more than four million administrative records of a CCT program in Colombia. I also examine responsiveness at the country level. I show that, in Latin American countries where the electorate is at the left side of the government's ideological position, governments redistribute more. My findings on unequal responsiveness have implications for subnational socioeconomic inequalities.

Keywords: responsiveness, redistribution, Latin America, administrative data

Word count: 8780

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Introduction

There is substantial evidence of government responsiveness in developing democracies (Golden and Min, 2013; Grossman and Slough, 2022). Existing scholarship shows that governments adjust their policies in response to electoral competition (Schady, 2000; Garay, 2016; Fairfield and Garay, 2017), citizens' needs (Besley and Burgess, 2002), public policy preferences (Roberts and Kim, 2011), political party alignment at the subnational level (Khemani, 2007; Brollo and Nannicini, 2012), or reelection motives (Gonschorek, Schulze and Sjahrir, 2018). Although these studies provide valuable insights into government responsiveness, they say little about the *conditions* under which we observe responsiveness. There are reasons to expect heterogeneous responsiveness within countries.

In developing democracies, states have an uneven capacity over the territory (Migdal, 1988; Scott, 1998; Herbst, 2000). Recent studies show that non-state actors provide socioeconomic order in places where the state is absent (Blattman et al., 2021; Peñaranda Currie, Otero-Bahamon and Uribe, 2021). I argue that in these contexts, non-state governance substitutes state governance and breaks the chain of policy responsiveness because it limits the government institutional capacity to respond to public preferences. Specifically, I posit that governments are unresponsive to citizens preferences in places where the state is absent and non-state actors provide socioeconomic order.

Likewise, there is heterogeneous voter turnout at the subnational level in developing democracies (Besley and Burgess, 2002; Lehoucq and Wall, 2004). Because elections are the mechanism that allows citizens to express their preferences, I argue that places with low voter turnout have citizens with weaker policy preferences and generate fewer incentives for governments to respond. In consequence, governments are less responsive to citizens in places with low voter turnout.

In this paper, I test for these two forms of heterogeneity by examining how governments adjust

policy in response to citizens' preferences as expressed in election results. I rely on the ideological distinction between left and right to operationalize such preferences. As opposed to the right, the left has been historically associated with redistribution and social spending. I use municipal-level vote shares of left-wing candidates competing in national elections as a proxy of preference for redistribution. With these data, I build a measure of electorate distance from the central government. The electorate distance captures how far to the left the electorate is at the municipal level, compared to the central government ideological position. My measure of electorate distance is sensitive to ideological variation among left-wing candidates. Therefore, the vote share of a far-left candidate should lead to a higher response than the same vote share of a center-left candidate. I operationalize policy responsiveness as the *additional* proportion of government transfers a municipality receives. A particular feature of my analysis is that it distinguishes between nondiscretionary and discretionary transfers. The former is based on specific criteria; the latter is conditional on citizens' socioeconomic characteristics and budget availability.

I exploit sub-national variation in the preference for redistribution in Colombia to empirically test my hypotheses. Although data on preferences for redistribution is scant at the subnational level, I use elections data to measure redistributive preferences. In Colombia, people directly elect the president every four years. There is variety in the ideological positions of presidential candidates, and the long history of armed conflict makes the left-right distinction particularly salient. Additionally, the state has an uneven control over the territory, and abstention in presidential elections varies across municipalities. In the early 2000s, Colombia implemented nondiscretionary central government transfers to municipalities. Such transfers are redistributive by design. In addition, the Colombian government started *Familias en Acción*, a (discretionary) conditional cash transfer (CCT) program to alleviate poverty. I use data on the 2006, 2010, 2014, and 2018 presidential elections, 13 years of central government transfers from over 1,000 municipalities, and more

than four million administrative records of a CCT program in Colombia to empirically test my arguments. To the best of my knowledge, no previous study has used these data.

I employ two-way fixed effects panel regression models and find that governments respond to public preferences for redistribution. Specifically, I find that municipalities, where the electorate is one standard deviation to the left of the government ideological position, receive around 2.8 percent higher nondiscretionary transfers per capita than municipalities where the electorate does not deviate from the government ideological position. Moreover, I find that the number of CCT beneficiaries in such municipalities increases by around 20 percent. However, I find heterogeneous effects: there is no responsiveness in municipalities historically controlled by non-state actors. Furthermore, the effect of the leftist support for redistribution decreases with voter abstention. I also examine responsiveness at the country level. I show that, in Latin American countries where the electorate is at the left side of the government ideological position, governments redistribute more.

My study makes important empirical and theoretical contributions to our understanding of policy responsiveness. A relevant empirical contribution is the fine-grained administrative data at the subnational level to measure policy preferences, responsiveness, and the mechanisms that condition it. Importantly, these data allow me to investigate responsiveness through nondiscretionary and discretionary transfers. Previous studies focus on discretionary transfers to assess responsiveness. I show that governments might also respond by using nondiscretionary transfers. This is important because it suggests that transfers are unlikely to be based only on technical criteria; nondiscretionary transfers can be adjusted by using the information provided by citizens' preferences.

Under which conditions does policy responsiveness hold? I make a relevant theoretical contribution by proposing that responsiveness varies by degree of state control and levels of citizen

participation. This heterogeneous responsiveness has implications for subnational inequality in developing democracies. Specifically, it implies that low levels of state control and citizen participation can further exacerbate subnational socioeconomic disparities. In order to understand why some subnational units lag behind others, it is important to consider whether the conditions for governments to respond to citizens' preferences are present.

Responsiveness

Research on government responsiveness in developing democracies shows that governments adjust their policies in response to several factors. Studies that focus on the politics of redistribution emphasize how electoral competition led to the implementation or reform of social policies. For example, Schady (2000) shows that in Peru, former president Fujimori increased spending on a discretionary social fund in provinces where he had a larger vote share. In contrast, Garay (2016) argues that it is the electoral competition to obtain votes from the informal sector, rural workers, and unemployed that explains the expansion of social policies in Argentina, Brazil, Chile, and Mexico in the 1990s and 2000s. Similarly, Fairfield and Garay (2017) qualitatively show how electoral competition and social mobilizations motivated the expansion of redistributive policies under right-wing governments in Chile and Mexico.

Another strand of the literature on policy responsiveness analyzes the determinants of discretionary transfers allocation at the subnational level and consistently shows that the allocation of transfers is politically motivated. Examples include Brollo and Nannicini (2012) for Brazil and Porto and Sanguinetti (2001) for Argentina.

However, these studies do not explore heterogeneous responses to electoral competition, ponder the ideological distance among parties competing in elections, or consider nondiscretionary trans-

fers as a mechanism to respond to voters¹. I build on the premise that election results express the demands and preferences of the public (Powell, 2000) and propose a theory that addresses these gaps.

Historically, left-wing parties have aimed at more equal societies (Bobbio, 2004) and represented low-income people and working-class interests (Korpi, 1983). According to power resource theorists, unions and left parties contributed to the expansion of welfare policies (Korpi, 1983, 2006; Esping-Andersen, 1990). More recent evidence shows that left-wing parties tend to implement redistributive policies to enhance the economic conditions of their electorate (Morgan and Kelly, 2013). Likewise, left-wing governments are associated with a reduction of income inequality and this association holds even in the context of globalization (Ha, 2012). In the lower part of the income distribution, leftist governments redistribute via unemployment compensation, higher minimum wages, and strengthening the bargaining power of unskilled labor (Rueda, 2007). In the upper part, they do so through progressive taxes (Pontusson, Rueda and Way, 2002).

The distinction between Left and Right is pivotal for my argument. Different studies argue that the increasing concern with issues of recognition, identity, and liberalism has kept workers from voting for the left and, as a consequence, the left moving toward more conservative policies (Kriesi et al., 2008; Beramendi et al., 2015). However, these studies focus on developed countries. As opposed to these post-materialist concerns, Stokes (2001) and Campello and Zucco Jr (2016) claim that economic voting is of central relevance in the electoral competition in developing countries. Additionally, Debs, Helmke et al. (2010) show that economic inequality defines the electoral fortunes of left-wing candidates in Latin American countries.

Given that election results express the demands and preferences of the public, the vote share of

¹Although Garay (2016) treats CCTs as nondiscretionary transfers, the expansion of CCTs is under the discretion of governments and it is usually tied to budget availability. What is nondiscretionary is the criteria to select the beneficiaries of the transfers.

left-wing parties raises the salience of inequality and redistribution. However, left-wing candidates in multiparty competition vary in terms of ideological position and electoral strength.

To illustrate this point, imagine a theoretical presidential election with five candidates, two of them from the left. Although both left-wing candidates are expected to advance redistributive economic policies, one candidate might propose more radical policies (candidate l_r) than the other one (candidate l_m) to appeal to different demands of the public. Under this setting, l_r is to the left of l_m . Likewise, the ideological distance between l_r and a centrist candidate will be longer than the distance between l_m and the same centrist candidate. The electoral performance of the left-wing candidates might be different as well. If the elected candidate is a non-leftist candidate and responds to the leftist support for redistribution, the elected candidate must weigh such support. To do so, the elected candidate can use the ideological position of l_r and l_m and their vote shares to know how radical and strong the supported redistribution was and respond accordingly.

The literature tends to focus on discretionary transfers as a mechanism of government responsiveness (Porto and Sanguinetti, 2001; Brollo and Nannicini, 2012). Yet, Litschig (2012) provides evidence on the political manipulation of nondiscretionary transfers in Brazil. The *Fundo de Participação dos Municípios* (Municipal Participation Fund) is a nondiscretionary transfer program that aims at redistributing income from rich to poor municipalities. Based on an allocation formula, the government shares revenue with local governments. However, Litschig (2012) finds a political manipulation of the population estimates in the formula in 1991, with swing-voter municipalities having a fictitious population increase. The fictitious population increase allowed the government to allocate more transfers to swing-voter municipalities. Likewise, Rozo and Vargas (2021) recently found that migration shocks increase central government nondiscretionary transfers to municipalities in Colombia.

Given that election results express the preferences of the public, left-wing parties vote shares

indicate different levels of support for redistribution, and governments can respond to public preferences through nondiscretionary and discretionary transfers, I hypothesize the following:

H1 Governments increase nondiscretionary transfers as the public moves to the left side of the government ideological position.

H2 Governments increase discretionary transfers as the public moves to the left side of the government ideological position.

Heterogeneous Responsiveness

A relevant question is whether governments responsiveness is geographically heterogeneous. In developing countries, states have an uneven capacity over the territory (Migdal, 1988; Scott, 1998; Herbst, 2000) and voter turnout is heterogeneous at the subnational level (Besley and Burgess, 2002; Lehoucq and Wall, 2004). I argue that these factors condition government responsiveness.

Different studies show that the state capacity to collect taxes, deliver services, and provide institutional order varies at the subnational level (Ch et al., 2018; Garfias et al., 2018; Sanchez de la Sierra, 2020; Blattman et al., 2021; Peñaranda Currie, Otero-Bahamon and Uribe, 2021). While in some cases citizens rely on brokers and informal connections to compensate for the limited state capacity (Berenschot, 2019), there are other contexts in which non-state actors fill the vacuum of state order (Blattman et al., 2021; Peñaranda Currie, Otero-Bahamon and Uribe, 2021). My focus is on the latter.

Non-state actors with control over a territory and long-term goals provide socioeconomic order (Arjona, 2016; Sanchez de la Sierra, 2020) and undertake governance activities (Blattman et al., 2021). The illicit economic activities non-state actors rely upon involve the local population and have well-defined labor relations (Mejía and Rico Valencia, 2010). Non-state actors usually regulate

the production, processing, and transportation of illicit goods produced in these territories. In addition, these actors collect taxes and provide public goods (Arjona, 2016; Sanchez de la Sierra, 2020).

Because the government is absent in such territories, non-state governance substitutes state governance. The order provided by non-state actors in these places breaks the chain of policy responsiveness. Although citizens express their preferences through voting, governments have limited capacity to adjust their nondiscretionary and discretionary policies in response to them.

H3 Governments' adjustment of nondiscretionary transfers is unresponsive to leftist support for redistribution in places where the government is absent.

H4 Governments' adjustment of discretionary transfers is unresponsive to leftist support for redistribution in places where the government is absent

Elections are a mechanism that allows citizens to express their policy preferences. During elections, citizens can vote or abstain from voting. When a citizen decides to vote, she shows a policy preference. In contrast, if a citizen chooses not to vote, she refrains from expressing a policy preference. Politicians cannot access ballots to learn about what voters want. In addition, politicians are limited in knowing the intensity of citizens' policy preferences. Regardless of how intense a citizens' preference is, her vote is one. However, at the aggregated level, politicians know if there are enough expressed preferences to respond. Places with low voter turnout are places where there are more indifferent voters in terms of policy preferences. Because politicians rely on votes to win elections and because low voter turnout implies more indifferent citizens in terms of political preferences, politicians have fewer incentives to respond to the electorate in places where voter turnout is low. Existing studies show that higher electoral participation leads governments to increase social spending and transfers (Besley and Burgess, 2002; Boix et al., 2003). I draw on

these findings and hypothesize the following:

H5 Government responsiveness to leftist support for redistribution through nondiscretionary transfers decreases with higher levels of voters abstention.

H6 Government responsiveness to leftist support for redistribution through discretionary transfers decreases with higher levels of voters abstention.

Research Design: The Colombian Case

In this paper, I use data on the 2006, 2010, 2014, and 2018 presidential elections, over 12 years of central government transfers from 1,122 municipalities, and more than four million administrative records of a CCT program in Colombia to empirically test the policy responsiveness argument. There are several features of this case that make it an ideal setting for testing policy responsiveness.

Colombia is a unitary state divided between 32 departments (administrative states) that group over 1000 municipalities. There are democratically elected officials at each level of political authority. The people directly elect the president every four years through general elections. A candidate wins the presidential race with half plus one of the ballots, or in a runoff election between the two most voted candidates if any of them reached the 50% plus one vote in the first round. Since the Political Constitution of 1991, the electoral system opened to political competition beyond the two traditional parties. This democratic openness derived into a multiparty system, allowed new political forces to compete for office and provided voters with a more broad set of ideological options. Although no left-wing candidate has won office so far, former left-wing guerrillas combatants have been frontrunner candidates in past elections, thus generating a salient left-right political cleavage.

At the sub-national level, there are regional elections where people elect governors and mayors, who then take office at the departmental and municipal levels, respectively. The presidential

election takes place one year before (or three years after) the regional elections. Although municipalities can implement and collect taxes within their jurisdiction, most of their revenue comes from the central government transfers. In 2001, the government established a transfer system with a redistributive goal, to close the social and economic gaps between municipalities. Since 2002, municipalities receive annual government transfers based on their population, poverty level, and demand for schooling and health. Around 80% of the transfers are assigned to education and health at the municipal level.

In addition to the transfer system, the government created *Familias en Accion* in 2001, a CCT program aimed at alleviating and reducing poverty in 350 municipalities. The program targets families in poverty or extreme poverty as well as families displaced by the armed conflict. The cash transfer is conditional on recurrent children's medical checks and school attendance. While the central government transfers to municipalities rely on the central government revenue, the CCT targets individuals and is funded by the government, the World Bank, and the Inter-American Development Bank. In 2012, the government expanded the CCT program to all municipalities (DPS, 2019). Although there are specific selection criteria of potential beneficiaries of *Familias en Accion*, the number of potential beneficiaries is under the government discretion.

Colombia provides geographical heterogeneity in levels of state control over the territory and political abstention. Colombia has territories with coca crops that have been historically controlled by non-state actors (Peceny and Durnan, 2006). Because coca is an illegal crop, the order necessary for its economy to work is provided by non-state armed actors. As Peñaranda Currie, Otero-Bahamon and Uribe (2021) point out, the expansion of the coca economy generated a gap of institutional order that was filled by the Colombian Revolutionary Armed Forces (FARC in Spanish). The peace agreement between the Colombian government and the FARC in 2016 highlighted the state abandonment in territories with coca crops. Moreover, there is noncompulsory voting in

Colombia. The level of abstention in presidential elections varies across municipalities and over time (see Figure S6).

The heterogeneous political ideologies of presidential candidates, the cross-municipal variation in government transfers, and the different state presence and level of electoral abstention over the territory provide variation on the key features or the policy responsiveness argument.

I aim to investigate whether the central government responds to the leftist support for redistribution. To do so, I focus on central government transfers to education and public health at the municipal level. Likewise, I use the number of beneficiaries of the CCT program by municipality to test for responsiveness through a discretionary mechanism.

The central government transfers are redistributive by design and provide more benefits to the poor, who are more likely to use public education and health. Although municipal authorities implement the transfers, they have no control over the allocations, it is the central government that delineates and oversees the spending. While transfers are expected to rely on specific criteria, there is qualitative evidence on the flexible use the government makes from such transfers (see Bonet and Galvis, 2016; El Tiempo, 2017). Moreover, Rozo and Vargas (2021) recently found that migration shocks increase central government transfers to municipalities.

Personal interviews with experts suggest that the government has room to modify transfers. Experts confirmed that specific criteria determine transfers, being population and level of unsatisfied basic needs the most relevant ones. However, they affirmed that the government makes minor methodological adjustments to the formula that determines transfers. In addition, experts from the National Planning Department maintained that the government adjusts transfers for the year t in December of year $t - 1$ based on budget availability. Moreover, experts explained that it is the government that provides the data to estimate the transfers. Likewise, the government, rather

than an independent agency, sends the transfers to municipalities².

Similar to my research design, Archibong et al. (2021) use central government transfers to subnational entities as a measure of policy responsiveness. They find that protests increase the amount of transfers the central government sends to subnational entities.

Empirical Strategy: Data and Models

Main Outcomes

To test whether there is policy responsiveness, I collected two sets of administrative data. First, I collected data at the municipal-year level on central government transfers to education and public health between 2006 and 2019 to evaluate responsiveness through nondiscretionary transfers. Governments can respond to the electorate distance after being elected ($t + 1$) or before the next presidential election ($t + 3$). Because nondiscretionary transfers growth over time depends on central government revenue, I expect that governments respond during the first year they can adjust transfers ($t + 1$) given the uncertainty about future available funds to respond to the electorate distance. I use central transfers at $t + 1$ in constant values as the dependent variable. The data on transfers come from the National Department of Planning, and it is publicly available information.

The second dataset includes the number of beneficiaries of the CCT program at the municipal-quarter level between the last quarter of 2012 and the last quarter of 2018. This dataset allows me to test whether there is responsiveness through discretionary transfers. Because an elected government takes office in August, I aggregated the number of beneficiaries enrolled during the last five months of 2014 and the last five months of 2018 in each municipality. Since the last available

²Online interview with author, 2 July 2021.

data is for December 2018, I focus on this time frame to compare equivalent periods. I use the aggregated data as the dependent variable. The data on beneficiaries come from the Department of Social Prosperity and is publicly available. I log-transformed both outcome variables to reduce the skewness of the distribution.

Main Explanatory Variable

In my analysis, I build a measure of electorate distance from the government ideological position to assess the leftist support for redistribution. The electorate distance is the municipal-level deviation of the electorate to the left of the central government ideology. Put another way, it is a standardized measure of how far to the left the electorate is at the municipal level, compared to the president's ideology. My measure captures the relative weight of the candidates by multiplying their ideological position with their vote shares within municipalities. The electorate distance is:

$$\left(\sum_{c=1}^n Ideology_c * Voteshare_c\right) - \text{Ruling Party Ideology}, \quad (1)$$

where c is the subscript for candidates. I use Baker and Greene's data (2011) to measure candidates' ideology. The ideological scores go from 1 (far left) to 20 (far right), then positive values imply an electorate to the right of the ruling party ideological position. I invert the variable scale in the analysis to interpret results in terms of left-leaning electorates. Baker and Green rely on Wiesehomeier and Benoit (2009), Pop-Eleches (2008), Coppedge (1998), and surveys of experts to measure and standardize the ideology of parties and candidates in Latin American countries between 1995 and 2015. The main advantage of this dataset is that it not only measures the ideology of presidents but also presidential candidates. However, the ideological position of

candidates is fixed over time. Candidates that run in several elections are assumed to keep their ideological positions over time. Therefore, the variation in my analyses comes from the different electorate distances in municipalities. To my knowledge, this is the only dataset on Latin American politicians that measures the ideology of presidential candidates.

Control Variables

In the regressions where the dependent variable is central government transfers per capita, I use as control variables the criteria the government employs to determine the amount of transfers to education and health: total population, education coverage, education enrollment, unsatisfied basic needs, unvaccinated population, and a rurality index (DNP, 2019). These data are also reported by the National Department of Planning.

Because the Colombian armed conflict and its legacies may affect how people vote in the national elections and the criteria to assign transfers, I control for demobilized guerrilla combatants, displaced population, violent threats, presence of coca crops, and the number of homicides. I collected these data from CEDE. Additionally, I use income growth per capita, a fiscal performance index, and the number of people in tertiary education as additional controls to improve the efficiency of my estimates.

The CCT program *Familias en Acción* targets low-income families and requires that municipalities have the infrastructure to provide health and education. Therefore, regressions estimating the effect of the electorate distance on the number of CCT beneficiaries control for the number of households registered in the System of Identification of Social Program Beneficiaries (SISBEN), the number of schools, and the number of health centers. In addition, I control for conflict-related variables, population size, income growth per capita, students in primary school, the share of the population living in rural areas, fiscal performance, and unsatisfied basic needs.

Empirical Model

Since the electoral distance is measured on election years and stays constant over the presidential term, I use municipality-election year as unit of analysis. Although an annual periodization in the panel would increase the statistical power of the analysis, the literature suggests that an election-year periodization is more suitable for partisan models (Schmitt, 2016)³. To test the effect of the electorate distance on central government transfers, I use this periodization and estimate an OLS two-way fixed effects (TWFE) regression with the following specification:

$$y_{it+1} = \alpha_i + \zeta_t + \beta D_{it} + \lambda X_{it-1} + \epsilon_{it}, \quad (2)$$

where y are transfers to public health and education per capita in municipality i and t indicates the election year. α_i are municipality fixed effects, ζ_t are year fixed effects, and X_{it-1} is the matrix of time-varying covariates measured the year before the election. The term D_{it} is the variable of interest, the electorate distance. Based on the policy responsiveness argument, I expect the coefficient β to be positive. My identifying assumption with this strategy is that electorate distance is conditionally independent of transfers within presidential terms. To test the effect of the electorate distance on the number of CCT beneficiaries, I follow a similar strategy, but I set y at t rather than $t + 1$ given the lack of data for 2019.

Recent methodological research shows that estimates from TWFE models are likely to be biased when the treatment is implemented in multiple periods. In these settings, treatment effects can change over time within treated units, but TWFE is not robust to such heterogeneity (Goodman-Bacon, 2021; Callaway and Sant’Anna, 2020). However, TWFE works when the treatment effect does not change across different periods. I employ Jakiela’s (2021) diagnostics and prove that the

³Recent work follows this approach. See Schmitt and Zohlnhöfer (2019) and Garritzmman and Seng (2020).

effect of electorate distance does not change across elections (see TWFE Diagnostics in SI.).

Results

Government Transfers

Figure 1 presents my findings of the effect of electorate distance on central government transfers. All models include control variables and municipality and year fixed effects. In line with the policy responsiveness argument and consistent with my first hypothesis, the effect of electorate distance on government transfers is positive. As expected, the regression coefficient of the electorate distance is positive and statistically different from 0 at $t + 1$ but not in other periods. Results indicate that municipalities that are one standard deviation to the left of the government ideological position receive higher transfers from the central government. Specifically, figure 1 shows that central government transfers are 2.8 percent higher at $t + 1$ in municipalities that are one standard deviation to the left of the government ideological position.

Discretionary Transfers

A particular feature of my analysis is that it distinguishes between nondiscretionary and discretionary government transfers. While the government has less room to modify the former, it can expand the latter at discretion. I explore whether there is responsiveness through discretionary transfers. In particular, I test if the number of CCT beneficiaries increases in municipalities that are at the left of the government ideological position.

Consistent with my second hypothesis, figure 2 shows that there is responsiveness through discretionary transfers. Models 1 and 2 include municipality and year fixed effects. Model 1

Figure 1: Log Government Transfers Per Capita.

Note. OLS estimates with 95% CI regressing logged government transfers per capita on electorate distance. The models include municipality and year fixed effects using standard errors clustered at the municipal level. Regressions reported in table S4, in SI.

reports the effect of electorate distance on the log number of CCT beneficiaries without control variables. The effect is positive and holds when I include controls: a one-unit increase in the electorate distance is associated with a 20.7 percent increase in the number of CCT beneficiaries (Model 2)⁴. A relevant determinant of discretionary transfers is the level of unsatisfied basic needs. The confounding effect of such a variable is absorbed by the municipality fixed effects in models 1 and 2. I remove municipality fixed effects in Model 3 and include unsatisfied basic needs as a control. As expected, the effect is positive and statistically different from 0.

Government Transfers - Heterogeneous Effects

In developing countries, the state has an uneven presence over the territory (Herbst, 2000; Ch et al., 2018). Colombia, in particular, has territories with coca crops that have been historically controlled by non-state actors (Peceny and Durnan, 2006). The characteristics of coca crops

⁴In Supplementary Information S1, I provide evidence for the robustness of these results.

Figure 2: Log CCT Beneficiaries.

Note. OLS estimates with 95% CI regressing logged CCT beneficiaries on electorate distance. Models 1 and 2 include municipality and year fixed effects. Model 3 includes year fixed effects. All models use standard errors clustered at the municipal level. Regressions reported in table S6, in SI.

allow inhabitants of such territories to develop economies with well-defined labor relations and high amounts of money (Mejía and Rico Valencia, 2010). Because coca is an illegal crop, the order necessary for its economy to work is provided by non-state actors. Non-state actors usually regulate the production, processing, and transportation of coca. They structure and organize labor, collect taxes, and provide public goods (Arjona, 2016). As Peñaranda Currie, Otero-Bahamon and Uribe (2021) point out, the expansion of the coca economy generated a gap of institutional order that was filled by the Colombian Revolutionary Armed Forces (FARC in Spanish). The peace agreement between the Colombian government and the FARC in 2016 highlighted the state abandonment in territories with coca crops.

A relevant question is whether the government is more or less responsive to the electorate distance in municipalities with coca crops. Figure 3 shows the estimated effect of electorate distance by coca crops. I use the same specification as in Figure 1 and split the data into two groups:

municipalities with and without coca crops. Consistent with my third hypothesis, the effect of electorate distance is concentrated in municipalities without coca crops and is null in municipalities with coca crops. While the differences between both groups of municipalities are estimated under several controls, I maintain that such differences cannot be associated with municipality- or time-invariant characteristics.

Figure 3: Heterogeneous Effect of Electorate Distance.

Note. Estimated effect of electorate distance on the logged government transfers per capita, by presence of coca crops. The regression includes municipality and year fixed effects using standard errors clustered at the municipal level. Regression reported in table S5 in SI.

Another source of heterogeneous responsiveness is abstention. Because citizens express their preferences through elections, the government will be less responsive in municipalities with higher levels of abstention. Figure 4 shows that there is empirical support for my fifth hypothesis: The marginal effect of the electorate distance on central government transfers decreases with the level of abstention.

Figure 4: Heterogeneous Effect of Electorate Distance.

Note. Estimated effect of electorate distance on the logged government transfers per capita, by level of electoral abstention. The regression includes municipality and year fixed effects using standard errors clustered at the municipal level. Regression reported in table S5 in SI.

Discretionary Transfers - Heterogeneous Effects

In addition, I estimate whether there is heterogeneous responsiveness to the electorate distance.

As with the central government transfers, responsiveness is heterogeneous: responsiveness is null in municipalities with coca crops (5) and responsiveness decreases with the level of abstention (6).

Both of these findings provide empirical support for my hypotheses 4 and 6, respectively.

Responsiveness in Latin America

I investigate whether there is policy responsiveness at the country level using data from over 90 national elections in 18 Latin American countries between 1995 and 2018 ⁵. There are several reasons to use this sample. First, in 1995 the different dictatorships that had been ruling countries like Argentina, Bolivia, Chile, and Uruguay already ended and the subsequent introduction of

⁵Summary statistics and countries are reported in the SI.

Figure 5: Heterogeneous Effect of Electorate Distance.

Note. Estimated effect of electorate distance on the logged CCT beneficiaries, by presence of coca crops. The regression includes municipality and year fixed effects using standard errors clustered at the municipal level. Regression reported in table S7 in SI.

elections allowed voters to express their political preferences (Hagopian and Mainwaring, 2005). Second, left-wing parties became electorally stronger, particularly in the first decade of this century (Baker and Greene, 2011; Debs, Helmke et al., 2010). Finally, during this period most of these countries experienced a decline in the level of inequality (López-Calva and Lustig, 2010). In short, this sample provides cross-country and over time variation at both sides of the equation.

To examine responsiveness at the country level, I rely on two features of my previous analysis, the ideological difference across presidential candidates and the redistributive nature of the nondiscretionary transfers. Although not all countries have unitary states and data on government transfers are scarce at the country level, I lean on the redistributive design of central government transfers in Colombia. Therefore, I use average redistribution⁶ by presidential term rather than transfers to test the policy responsiveness argument across Latin American countries. Redistribu-

⁶I use average redistribution rather than redistribution at $t + 1$ because redistribution takes time to vary.

Figure 6: Heterogeneous Effect of Electorate Distance.

Note. Estimated effect of electorate distance on the logged CCT beneficiaries, by level of electoral abstention. The regression includes municipality and year fixed effects using standard errors clustered at the municipal level. Regression reported in table S7 in SI.

tion is estimated as the difference between the Gini of market income and the Gini of disposable income. Given that disposable income includes state transfers, I can have a good proxy of redistribution via policies. I use the standardized measures of Gini of the market and disposable incomes computed by Solt (2016) to define the redistribution variable. In this way, higher values mean more redistribution.

For the electorate distance calculation, I use Baker and Greene’s data (2011) and follow the same approach as in the Colombian case. A distinctive feature of my analysis is that it looks for the effect of the electorate distance controlling for the government’s ideology to test whether this effect operates independently of who holds office. Therefore, in the regressions I have controlled by government ideology, as measured by Baker and Greene (2011). Additionally, my regressions included a set of lagged controls that may affect electorate distance. By using lags, I can be certain that these variables are potential causes rather than consequences of the electorate distance since

they are measured at $t - 1$ and the electorate distance is measured at t . The lagged controls are terms of trade, inflation, unemployment, education, GDP per capita, the proportion of elderly, Gini of market income, legal opposition to the executive power, economic growth, and external debt.

Because higher terms of trade relax economic constraints, fiscal discipline, and opposition to redistribution (Campello, 2015), left-wing parties may increase their vote share when the terms of trade rise. I control for terms of trade as measured by UNCTADstat to discount the effect of the commodity boom experienced in Latin America in the first decade of this century. Because hyperinflation has been usually associated with left-wing governments and jeopardized the electoral fortunes of left-wing parties (Sader, 2011), I control for inflation using data from CepalStat. Left-wing parties generally include unemployment compensations in their policy agendas (Rueda, 2007), consequently, higher rates of unemployment may increase the left vote. I use data from the International Labor Organization (ILO) to control for unemployment. Provided that the level of education may affect the vote for the left, I control for education using data from Barro and Lee (2013). I include the log-transformed GDP per capita as routine control to discount the effect that economic development may have on the left vote share as well as on redistribution. I employ data from the World Bank to measure GDP per capita.

In the regressions, I also included the lagged value of trade openness as a proxy of globalization. Although the electorate distance may not depend on this variable, globalization might be correlated with my dependent variable. Philips, Souza and Whitten (2020) find that globalization affects the distribution of income across quintiles depending on the relative factor endowment of each country. In labor-rich countries such as the Latin American ones, globalization increases the income share of the lower quintiles at the expense of the top quintile, thus affecting the overall income distribution. I collected the data on trade openness from the World Bank database. I also control for the

proportion of the elderly population as measured by the World Bank since a higher proportion of the elderly may increase redistribution. I control for inequality at the market level to discount the Meltzer-Richard prediction that more inequality will be associated with more redistribution (Meltzer and Richard, 1981). I employ the measure of Gini of market income, as measured by Solt (2016). Finally, I include a measure of legal opposition to government and external debt as calculated by Varieties of Democracies (Coppedge et al., 2019) and CepalStat, respectively. Opposition to the government might limit the redistributive policies that it plans to implement. On the other hand, economic growth may allow governments to advance redistributive agendas.

Figure 7: Residual Plot - Country-level data.

Note. Greater values in the x-axis mean electorates more to the left of the president's ideology.

As figure 7 illustrates, electorate distance is associated with more redistribution. Table 1 presents the results of OLS regressions on average redistribution. I used bootstrapped simulations to obtain standard errors clustered by country. This approach provides a more accurate inference than the usual cluster-robust standard errors when there is a low number of clusters (Cameron, Gelbach and Miller, 2008). The extended regression tables are reported in the supplementary

information.

Table 1 indicates that there is policy responsiveness in Latin American countries. Column 1 shows that one standard deviation to the left of the government ideology increases average redistribution by 0.014 points. Column 2 presents the results using country-year units of analysis. This approach allows me to include the year as a running variable in the models and estimate an OLS two-way fixed effects, thus discounting idiosyncratic and time trend effects on the associations of interest⁷. The result is consistent with the country-election regression.

Table 1: Electorate Distance Effect on Average Redistribution. Country-level Panel

	1	2
Electorate Distance	0.014*	0.010*
	(0.005)	(0.004)
Country FE	Yes	Yes
Year FE	Yes	No
Num. Obs.	92	435

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$. OLS estimates.
Bootstrapped SE's clustered by country.

Robustness Tests

Empirical research with observational data implies several challenges and limitations. One of the main concerns in these analyses is the omitted variable bias. Although my regressions include several controls, there are unobserved variables that might be affecting the relation between electorate distance and the outcome variables. I follow Cinelli and Hazlett (2020) and conduct sensitivity analyses to assess the robustness of my findings on Colombia to omitted variables. The results from the sensitivity analyses are reported in the supplementary information and indicate that my results are robust to omitted variables (see Robustness Checks). Additionally, I show that the

⁷Because the electorate distance in a given national election does not vary until the next election, I set all variables following the time structure in the country-election panel, but now I let t to be a running variable.

effect of electorate distance on nondiscretionary transfers holds after removing from the analysis the municipalities with extreme values in reported transfers (see S9 in SI.).

My analysis of responsiveness through nondiscretionary transfers relies on completed presidential terms. Importantly, TWFE diagnostic shows that the effect of electorate distance is homogeneous over time (see TWFE Diagnostics). However, when I expand the analysis to include available data for the current government (2018-2022), I find that the effect of electorate distance on nondiscretionary transfers is statistically indistinguishable from 0. I posit that there is a plausible explanation for this. In 2018, Gustavo Petro, a former guerrilla member, advanced to the second round of the presidential elections as the left-wing candidate. It was the first time in Colombian history the establishment faced a credible electoral threat from the left. I contend that under this scenario, a government strategy to penalize leftist support for redistribution substitutes policy responsiveness. Therefore, a TWFE estimation of the effect of the electorate distance is inaccurate as this effect might vary in 2018. Specifically, the inclusion of 2018 in the TWFE estimation washes out the average electorate distance effect found in previous elections. If my assumption about 2018 is plausible, I should observe a negative effect of the electorate distance on nondiscretionary transfers in 2018. Table S10 shows that the effect of electorate distance is negative and significant in 2018.

Conclusions

Existing studies for developing democracies show that governments adjust their policies in response to factors like electoral competition and public policy preferences. For example, governments implement or expand social programs when they face electoral competition. In this paper, I show that governments are responsive to voters ideological preferences as well. However, responsiveness

is heterogeneous. Indeed, I advance and empirically test an argument that builds on the ideological difference between left- and right-wing presidential candidates and accounts for heterogeneous responsiveness. I argue that elected officials learn about the distribution of voters ideological preferences and respond accordingly. Specifically, elected officials increase redistributive transfers in places where the electorate leans to the left of the ideological spectrum. However, responsiveness is conditional on state control of the territory and electoral abstention. Responsiveness is null in places where non-state actors control the territory. Likewise, responsiveness decreases with voters abstention.

To empirically test my theory, I take the case of Colombia, a developing democracy where the left-right political distinction is salient. Using a two-way fixed-effects estimation and administrative data on government transfers, I find that governments increase nondiscretionary and discretionary transfers as the leftist support for redistribution increases. Importantly, I show that responsiveness is heterogeneous: there is no government responsiveness in places controlled by non-state actors, and responsiveness decreases with voters abstention. Using a panel of Latin American countries, I further find that in countries where the electorate is at the left side of the government ideological position, governments redistribute more.

My findings are robust to a series of additional tests, and they can inform the renewed interest in government responsiveness (see Grossman and Slough, 2022) by pointing to the conditions under which policy responsiveness holds as well as the mechanisms government use to respond. Importantly, my findings have implications for subnational socioeconomic inequalities as unequal responsiveness might further exacerbate subnational disparities.

My extended analysis of the Colombian case suggests that the direction of the government responsiveness might change. In the presence of a credible leftist threat, the elected government might choose to penalize the leftist electorate. Further research must explore when and how gov-

ernments change the direction of the response. Likewise, future research ought to assess to what extent runoff elections with a credible leftist electoral threat break the chain of responsiveness. Another important avenue for future research is inequality in accountability. For instance, what does unequal responsiveness imply about how democratic accountability operates at the subnational level?

While I address external validity concerns with the country-level analysis, the heterogeneous responsiveness within countries must be further researched. There are reasons to expect similar findings in other Latin American countries given that uneven state presence over the territory is a shared characteristic. However, it is unclear whether the effect of voters abstention will hold in countries with mandatory voting. Likewise, future research must explore other factors that might as well condition responsiveness. For instance, social protests and ideological ties with local politicians might attenuate government responsiveness.

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Policy Responsiveness in Developing Democracies: Evidence from Latin America: Supplementary Information*

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*All replication material, including R code and data, will be made available via Harvard University's Dataverse.

S1 Colombia

Data

In this section, I present the summary statistics of the variables used in the regression analyses. In addition, I display plots with the distribution of logged government transfers per capita (figure S1), the evolution of the average government transfers per capita over time (figure S2), and the distribution of the logged CCT beneficiaries (figure S3). Likewise, I present the distribution of the electorate distance across elections (figure S4) and municipalities (figure S5), and the distribution of voter abstention (figure S6).

Although figure S1 suggests the presence of some outliers, my results are robust to the exclusion of municipalities with extreme values of government transfers (see table S9).

Table S1: Colombia - Summary Statistics

Statistic	N	Mean	St. Dev.
Gov. Transfers Per Capita (millions)	3,358	556,776.4	682,920.4
Beneficiaries CCT	2,130	46.2	110.7
Electorate Distance	3,355	-0.3	0.8
Population	3,358	41,654.6	254,755.0
Educ. Coverage	3,291	85.5	18.0
Educ. Enrollment	3,358	6,966.9	28,416.1
Unsatisfied Basic Needs	3,358	48.2	21.3
Unvacc. Population	3,358	1,523.1	8,244.8
Rurality Index	3,358	0.6	0.2
Ex-guerrilla	2,236	1.7	7.0
Displaced Pop. (Arrived)	3,358	308.9	1,595.0
Income Growth Per Capita	2,220	9.7	39.0
Fiscal Performance Index	3,281	65.2	8.4
Homicides	3,358	14.8	73.6
Students	3,348	8,402.4	45,674.5
School Admin Staff	2,791	156.6	1,156.4
Professors	3,348	394.8	2,206.3
Abstention	3,355	0.6	0.2
Coca Crops	3,358	0.2	0.4

Figure S1: Distribution of log Government Transfers

Figure S2: Evolution of Central Government Transfers

Note. Transfers to education and public health at constant 2015 COP.

Figure S3: Distribution of log CCT beneficiaries

Figure S4: Distribution of Electorate Distance - Colombia

Note. Ideological distribution of the electorate in four national elections. Negative and positive values in the x-axis refer to left and right-wing ideology, respectively.

Figure S5: Municipal-level Variation of Electorate Distance - Colombia

Note. Cross-municipal variation of electorate distance in four national elections. Negative/positive values indicate municipalities located to the right/left of the government ideological position.

Figure S6: Distribution Abstention

S1.1 TWFE Diagnostics

In a recent paper, Jakiela (2021) suggests diagnostics tests for the feasibility of TWFE. Jakiela argues that an observable implication of the treatment effect homogeneity and common trends is a similar slope between treatment and control groups. I regress the residualized outcome variable (central government transfers) on the interaction between treatment group and residualized treatment (electorate distance). Because my treatment is a continuous variable, I classify those municipalities with a score above the mean of the electorate distance as treated units. Column 1 in table S2 shows that there is no significant difference between the slope of control and treatment groups. Column 2 provides further evidence of the homogeneity assumption. As expected, the interaction between the treatment variable and year yields null results. Importantly, the model reported in column 2 includes interacted controls. I conduct a similar diagnosis for discretionary transfers. Results are reported in table S3. As in government transfers, the homogeneity assumption holds.

Table S2: Government Transfers - Diagnostics for TWFE

	1	2
Treatment group X residualized treatment	-0.016 (0.014)	
Electorate distance X 2010		-0.011 (0.009)
Electorate distance X 2014		0.014 (0.014)
Num. obs.	3287	3287

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table S3: Discretionary Transfers - Diagnostics for TWFE

	1	2
Treatment group X residualized treatment	0.029 (0.044)	
Electorate distance X 2010		0.050 (0.069)
Num. obs.	1829	1829

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

S1.2 Regression Tables

S1.2.1 Government Transfers

Table S4: Electorate Distance Effect on Government Transfers Per Capita

	1	2	3
Electorate Distance	0.064*** (0.010)	0.027*** (0.007)	0.028** (0.009)
Log Population		0.178*** (0.043)	0.309*** (0.090)
Educ. Coverage		0.107*** (0.012)	0.109*** (0.020)
Log Educ. Enrollment		0.149*** (0.038)	0.135** (0.041)
Unsatisfied Basic Needs		0.117*** (0.015)	0.103** (0.039)
Log Unvacc. Population		-0.043** (0.014)	-0.038 (0.021)
Rurality Index		0.052 (0.081)	-0.100 (0.128)
Log Ex-Guerrilla			-0.042 (0.036)
Log Arrived Displaced Pop.			-0.009 (0.006)
Inc. Growth Per Capita			-0.001 (0.002)
Fiscal Performance Index			-0.042*** (0.013)
Log Homicides			0.006 (0.007)
Log Students			-0.026 (0.081)
Log School Admin Staff			0.002 (0.012)
Log Professors			0.015 (0.064)
Num. obs.	4471	3287	2730
TWFE	Yes	Yes	Yes

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$. Control variables in lagged values. Standard errors clustered at the municipal level

Table S5: Marginal Effects of Electorate Distance on Government Transfers

	1	2	3
Electorate Distance	0.027* (0.011)	-0.003 (0.020)	0.060* (0.028)
Abstention	0.332** (0.104)	0.044 (0.101)	0.304*** (0.090)
Log Population	0.112*** (0.026)	0.116*** (0.033)	0.110*** (0.020)
Educ. Coverage	0.152** (0.051)	0.055 (0.069)	0.135*** (0.040)
Log Educ. Enrollment	0.064 (0.053)	0.219*** (0.056)	0.103** (0.039)
Unsatisfied Basic Needs	-0.046 (0.025)	-0.009 (0.040)	-0.034 (0.022)
Log Unvacc. Population	-0.171 (0.161)	0.097 (0.139)	-0.099 (0.128)
Rurality Index	-0.018 (0.045)	-0.065 (0.068)	-0.041 (0.036)
Log Ex-Guerrilla	-0.009 (0.007)	-0.012 (0.019)	-0.009 (0.006)
Log Arrived Displaced Pop.	-0.000 (0.003)	-0.001 (0.002)	-0.001 (0.002)
Inc. Growth Per Capita	-0.047** (0.015)	-0.039 (0.028)	-0.043*** (0.013)
Fiscal Performance Index	0.004 (0.007)	0.011 (0.016)	0.006 (0.007)
Log Homicides	-0.069 (0.098)	0.073 (0.124)	-0.029 (0.082)
Log Students	-0.004 (0.013)	0.009 (0.025)	0.001 (0.012)
Log School Admin Staff	0.098 (0.059)	-0.279 (0.217)	0.015 (0.063)
Log Professors			0.018 (0.056)
Electorate Distance X Abstention			-0.051 (0.038)
Coca Crops	No	Yes	Yes
TWFE	Yes	Yes	Yes
Num. obs.	2258	472	2730

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$. Control variables in lagged values. Standard errors clustered at the municipal level

S1.2.2 Discretionary Transfers

Table S6: Electorate Distance Effect on log Beneficiaries CCT

	1	2	3
Electorate Distance	0.179*** (0.031)	0.207*** (0.033)	0.119*** (0.024)
Log Population		-0.834 (1.325)	-1.133*** (0.156)
Inc. Growth Per Capita		0.010 (0.016)	0.026* (0.012)
Log Arrived Displaced Pop.		0.070* (0.028)	0.091*** (0.017)
Log Students - Primary		-0.111 (1.050)	1.034*** (0.148)
Log Schools		0.487* (0.196)	0.264*** (0.046)
Log Students		0.042 (0.131)	0.343*** (0.059)
Log Health Centers		-0.048 (0.097)	-0.042 (0.033)
Log Sisben - Households		-1.367*** (0.406)	0.151** (0.058)
Log Displaced Pop.		-0.017 (0.037)	0.058** (0.020)
Log Threats		0.116*** (0.032)	0.008 (0.021)
Rurality Index		0.861 (0.773)	-0.087** (0.027)
Log Homicides		0.051 (0.043)	-0.013 (0.023)
Fiscal Performance Index		0.087 (0.047)	0.051* (0.023)
Unsatisfied Basic Needs			0.081*** (0.024)
Year FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	No
Num. obs.	2130	1829	1829

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$. Control variables in lagged values. Standard errors clustered at the municipal level

Table S7: Marginal Effects of Electorate Distance on log Beneficiaries CCT

	1	2	3
Electorate Distance	0.306*** (0.037)	-0.033 (0.070)	0.510*** (0.138)
Abstention	1.401 (1.638)	-3.081 (2.675)	-0.385 (1.342)
Log Population	0.044** (0.015)	-0.139 (0.142)	0.011 (0.017)
Inc. Growth Per Capita	0.059 (0.033)	0.033 (0.075)	0.075** (0.028)
Log Arrived Displaced Pop.	-2.073 (1.246)	3.148 (2.207)	-0.460 (1.061)
Log Students - Primary	0.341 (0.175)	2.091** (0.755)	0.406* (0.198)
Log Schools	-0.181 (0.175)	0.304 (0.253)	0.080 (0.136)
Log Students	0.088 (0.120)	-0.125 (0.110)	-0.053 (0.096)
Log Health Centers	-0.950* (0.419)	-2.526** (0.959)	-1.217** (0.411)
Log Sisben - Households	-0.026 (0.040)	0.134 (0.128)	-0.018 (0.037)
Log Displaced Pop.	0.084* (0.039)	0.082 (0.067)	0.122*** (0.033)
Log Threats	-0.008 (0.981)	1.493 (1.084)	0.913 (0.765)
Rurality Index	0.087 (0.052)	-0.057 (0.081)	0.040 (0.044)
Log Homicides	0.106 (0.055)	0.054 (0.088)	0.088 (0.046)
Fiscal Performance Index			0.466* (0.236)
Electorate Distance X Abstention			-0.668* (0.278)
Coca Crops	No	Yes	Yes
TWFE	Yes	Yes	Yes
Num. obs.	1476	353	1829

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$. All independent variables but log-transformed ones are standardized $N \sim (0, 1)$. Control variables in lagged values. Standard errors clustered at the municipal level

S1.3 Robustness Checks

S1.3.1 Government Transfers

Omitted variable bias is one of the main concerns in observational studies. To cause a confounding effect in my setting, omitted variables must vary over time and across municipalities. While I control for potential confounders with such characteristics, there might be variables that I did not consider or observe. To determine how serious the omitted variables should be to alter my results, I conduct a sensitivity analysis following Cinelli and Hazlett 2020. Table S8 shows the regression results reported in Figure ?? and additional information that details the sensitivity of the results to omitted confounding variables. The robustness value ($RV_{q=1}$) of 6.72 percent means that hypothetical confounders as strong as the combination of the main institutional determinants of transfers (population and unsatisfied basic needs) would need to explain at least 6.72 percent of the residual variance of both electorate distance and transfers per capita at $t+1$ to wash out the observed effect. In addition, the robustness value for testing the null hypothesis ($RV_{q=1, \alpha=0.05}$) is 2 percent. This means that unobserved confounders that do not explain at least 2 percent of the residual variance in both electorate distance and transfers per capita are not sufficiently strong

for the estimate to lose statistical significance at the 0.05 level. The values of $R^2_{Y \sim Z|X,D} = 3.4$ percent and $R^2_{D \sim Z|X} = 0.1$ percent provide bounds on confoundings as strong as the combination of the main institutional determinants of transfers. Because both values are below $RV_{q=1}$, confounders as strong as the institutional determinants are not sufficient to wipe out the observed estimate.

Table S8: Sensitivity Analysis
Outcome: *Log Government Transfers Per Capita*

Treatment:	Est.	S.E.	t-value	$R^2_{Y \sim D X}$	$RV_{q=1}$	$RV_{q=1, \alpha=0.05}$
<i>Electorate Distance</i>	0.028	0.01	2.798	0.5%	6.7%	2.1%
df = 1616	<i>Bound (1x Inst. Drivers): $R^2_{Y \sim Z X,D} = 3.4\%$, $R^2_{D \sim Z X} = 0.1\%$</i>

Figure S7: Effect Estimates at Different Levels of Assumed Confounding.

Table S9 presents results after removing municipalities with extreme reported values in government transfers per capita. The effect of the electorate distance holds.

Table S9: Electorate Distance Effect on Government Transfers Per Capita

	1
Electorate Distance	0.027*** (0.008)
Log Population	0.122 (0.064)
Educ. Coverage	0.103*** (0.016)
Log Educ. Enrollment	0.065* (0.029)
Unsatisfied Basic Needs	0.110*** (0.024)
Log Unvacc. Population	-0.036* (0.018)
Rurality Index	0.013 (0.101)
Log Ex-Guerrilla	0.000 (0.018)
Log Arrived Displaced Pop.	-0.006 (0.005)
Inc. Growth Per Capita	0.001 (0.001)
Fiscal Performance Index	-0.020* (0.008)
Log Homicides	-0.000 (0.006)
Log Students	-0.112 (0.060)
Log School Admin Staff	0.010 (0.008)
Log Professors	0.057 (0.043)
Num. obs.	2632
TWFE	Yes

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$. Control variables in lagged values. Standard errors clustered at the municipal level

As mentioned in the discussion section, the inclusion of the 2018 elections in the analysis wipes out the effect of the electorate distance on central government transfers. Table S10 suggests that the null effect is driven by 2018. Column 1 presents the null result of electorate distance on government transfers. However, column 2 shows that the effect of electorate distance is negative and significant in 2018.

Table S10: Electorate Distance Effect on Government Transfers Per Capita

	1	2
Electorate distance	−0.003 (0.004)	0.013 (0.007)
Electorate distance X 2010		−0.013 (0.009)
Electorate distance X 2014		0.003 (0.011)
Electorate distance X 2018		−0.034*** (0.009)
Num. obs.	4384	4384
TWFE	Yes	Yes

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$. Control variables in lagged values. Control variables in column 2 are interacted with year. Standard errors clustered at the municipal level

S1.3.2 Discretionary Transfers

Table S11 and figure S8 show the results of the sensitivity analysis for the logged CCT beneficiaries. In this analysis, I use logged displaced population that arrives at a municipality and logged households registered in the System of Identification of Social Program Beneficiaries (SISBEN) as the benchmark covariate. The CCT program targets both of these groups (DPS, 2019). In this case, the robustness value ($RV_{q=1}$) is 21.3 and the robustness value for testing the null hypothesis ($RV_{q=1, \alpha=0.05}$) is 15.9 percent. The latter means that unobserved confounders that do not explain at least 15.9 percent of the residual variance in electorate distance and logged CCT beneficiaries are not sufficiently strong for the estimate to lose statistical significance at the 0.05 level.

Table S11: Sensitivity Analysis - CCT beneficiaries
Outcome: *Log CCT beneficiaries*

Treatment:	Est.	S.E.	t-value	$R^2_{Y \sim D \mathbf{X}}$	$RV_{q=1}$	$RV_{q=1, \alpha=0.05}$
<i>Electorate Distance</i>	0.207	0.029	7.107	5.4%	21.3%	15.9%
df = 878	<i>Bound (1x OVs): $R^2_{Y \sim Z \mathbf{X}, D} = 2.5\%$, $R^2_{D \sim Z \mathbf{X}} = 1.6\%$</i>

Figure S8: Effect Estimates at Different Levels of Assumed Confounding.

S2 Cross-country Results

List of countries: Argentina, Bolivia, Brazil, Chile, Colombia, Costa Rica, Dominican Republic, Ecuador, El Salvador, Guatemala, Honduras, Mexico, Nicaragua, Panama, Paraguay, Peru, Uruguay, Venezuela.

Table S12: Country-level Panel - Summary Statistics

Statistic	N	Mean	St. Dev.
Average Redistribution	92	0.03	0.03
Electorate Distance	97	-0.04	1.0
President Ideology	97	11.4	5.3
Log GDP Per Capita	97	8.5	0.7
Proportion Elderly	97	10.2	3.4
Gini Market	97	0.5	0.04
Legal Opposition	97	1.1	1.3
Inflation	97	231.8	1,489.5
Unemployment	97	7.1	3.5
Education	97	2.1	0.7
Terms of Trade	97	131.3	62.1
Trade Openness	97	62.1	27.4
Economic Growth	97	3.9	3.2
External Debt	97	44.2	32.4

Table S13: Electorate Distance Effect on Average Redistribution. Country-level Panel

	1	2
Electorate Distance	0.014*	0.010*
	(0.005)	(0.004)
President Ideology	−0.017*	−0.002*
	(0.006)	(0.001)
Log GDP Per Capita (lag)	−0.036	−0.018
	(0.024)	(0.027)
Proportion Elderly (lag)	0.008*	0.010*
	(0.004)	(0.004)
Gini Market (lag)	0.128	0.098
	(0.118)	(0.146)
Legal Opposition	0.001	−0.001
	(0.003)	(0.002)
Inflation (lag)	−0.000	0.000
	(0.000)	(0.000)
Unemployment (lag)	−0.001	−0.001
	(0.001)	(0.001)
Education (lag)	−0.007	−0.002
	(0.010)	(0.009)
Terms of Trade (lag)	0.000	−0.000
	(0.000)	(0.000)
Trade Openness (lag)	0.000	0.000
	(0.000)	(0.000)
Economic Growth (lag)	−0.001	−0.000
	(0.000)	(0.000)
External Debt (lag)	−0.000	−0.000
	(0.000)	(0.000)
Country FE	Yes	Yes
Year FE	Yes	No
Num. Obs.	92	435

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$. Electorate distance and president ideology are standardized $N \sim (0, 1)$. Bootstrapped standard errors clustered by country.

References

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