Social Transfers and Conditionalities under Different Regime Types

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Abstract

This paper examines the implications of political factors for social policy choices. Specifically, we explore the link between regime type and adoption of unconditional transfers versus transfers conditioned on beneficiaries' investments in human capital. Due to the direct nature of benefits, unconditional transfers are more likely to be used to buy off opposition and prevent social unrest. As transfers that are conditioned on education and health pay off only in a relatively distant future, they are rarely initiated for political motives and rather defined by interests of long-term development and human capital accumulation. Using the new dataset on Non-Contributory Social Transfer Programs (NSTP) in developing countries, we find that transfers are indeed chosen so as to be unconditional under less democratic regimes. There is some evidence that conditional transfers are more likely to be adopted in democracies. In particular, democracies tend to increase the number of conditional schemes once any social transfer program is introduced.

Keywords

Social transfers, conditional programs, pro-poor redistribution, democracy/non-democracy, political regime

1 Introduction

Social protection programs are fundamental for poverty alleviation as they help to raise the living standards of the poor. Since the beginning of the 1990s, antipoverty transfer programs have been broadly extended in developing countries. However, policymakers may adopt social policies not only to combat poverty and inequality but also to please voters in case of upcoming elections, to increase office value, or to pursue rent-seeking. These political motives compromise the effectiveness of social policies. In addition, political institutions themselves, such as regime type or electoral rule, create biases for the optimal design of social transfer programs. Specific elements of the design of pro-poor programs might be manipulated for a variety of political ends. Further, in developing countries—where high inequality and poverty problems are far from uncommon—political leaders have to base their agendas on redistribution and antipoverty policies. Therefore, a key challenge is to understand what role political constraints play in the adoption of pro-poor programs in developing countries. This paper focuses on one characteristic of the design of social transfer programs that might be chosen strategically, namely the conditionality of transfers. In the literature, there is no theoretical foundation or conclusive empirical evidence on the politics of assigning conditions. Therefore, in this paper we examine whether the introduction of conditions is solely based on pro-poor considerations or whether political motives also affect such policy decisions. Specifically, we explore whether the regime type matters for the design of a transfer program.

Unconditional transfers include all cash and in-kind transfers that do not require the beneficiary to comply with any conditions apart from meeting the eligibility criteria. This group of transfers is aimed at current consumption increase and short-term, income-smoothing effects. Old-age grants, family support schemes, and other social pensions are good examples of such transfers that are paid to every (poor) household or to informal workers. In the case of conditional transfers, the poor are required to comply with certain behavioral rules in order to receive the benefits, for example regular health check-ups and/or school attendance by their children. Hence, conditional transfers are costlier in implementation in the short run but lead to higher returns in the long run if the poor would underinvest in health and education otherwise. A particular feature of such schemes is that they incentivize human capital accumulation and thereby promote the long-term prospects of poverty reduction being sustainable and successful.

Several strategic motives to assign conditions that are not related to pro-poor concerns can be distinguished. On the one hand, conditional transfers help to facilitate targeting and to increase public support for redistribution by benefiting only the "deserving poor"—in other words, those who comply with the conditions. Hence, they may be used strategically to target certain population groups or they may be preferred in societies with a higher degree of paternalism. In contrast, unconditional transfers may be favored as they allow leaders to contribute to the welfare of the masses in the short run and can thus be used to reward political supporters or reduce public unrest.

The dominating motives to adopt conditions might differ systematically between political regimes. In non-democratic countries, the primary strategic reason to redistribute is to prevent uprisings and maintain the ruling elite's power. Non-democracies may therefore prefer unconditional transfers. Democratic countries rely on public support which is likely to increase redistribution in general and public spending on education and health in particular. In addition, democracies are considered as higher accountable to the public and care more about the long term prospects of poverty reduction. Consequently, they may be more likely to adopt conditional transfers that also give benefits in a relatively distant future. We differentiate democracies and non-democracies using the polity score from Marshall and Jaggers (2015) and the binary measure of democracy from Boix, Miller, and Rosato (2013). Whereas the former rather focuses on constitutional constraints, the latter reflects the implementation of democracy in practice.

While the political economy of redistribution in developed countries has already been broadly explored in the literature, few studies have focused on developing countries. Moreover, existing studies consider proxies for redistribution such as tax revenue, government expenditure, or health and education spending, which are inadequate in developing countries. We propose to proxy redistribution with modern social transfer programs as they are the main redistributive policy in developing countries.

The new database by Dodlova, Giolbas, and Lay (2017) on non-contributory social transfer programs in developing countries, makes it possible to test the existence of systematic differences in the use of conditions between regime types. We compare 163 unconditional and conditional non-contributory transfer schemes in 99 countries in a panel form from 1960 until 2015. Controlling for economic development, demographic structure, inequality and other characteristics we explore how the regime type influences the type of

social transfers. The empirical strategy includes using not only standard specifications with country- and time-fixed effects but also models with instrumental variables. We adopt two approaches to deal with the endogeneity of the democracy variable: democratic capital in neighboring countries and regional democratization waves.

This paper, first, finds ambiguous evidence on the relationship between the regime type and the probability of having a transfer program of any type. This can be partly explained by our second – very robust – finding. We show that non-democratic countries are more likely to adopt unconditional transfers. Third, there is some evidence that democracies tend to promote more conditional transfer programs. Yet, this finding is robust when we look at the share of conditional programs in all transfer programs in a sub-sample of countries that have adopted at least one social transfer program.

The paper proceeds as follows. The next section reviews the related literature. Section 3 presents the empirical strategy and Section 4 describes the data. Section 5 discusses the empirical results as well as robustness checks. The final section concludes.

2 Related Literature

This paper relates to several strands of literature. First, the analysis of determinants of a type of transfer is related to the debate on regime type and redistribution in general. For democratic countries, the seminal model by Meltzer and Richard (1981) assumes that because the income distribution is always right-skewed the median voter has a below-mean income, and so favors redistribution. Further, higher inequality leads to greater redistribution—as the median voter shifts more to the left of the income distribution (Alesina and Rodrik, 1994). However, recent studies cannot confirm the implications of median voter theory and propose explanations for lower rates of redistribution in democratic and/or unequal societies (Albertus and Menaldo, 2013; Acemoglu et al., 2011, 2014b; Przeworski, 2016). In particular, Przeworski (2016) argues that the median voter theorem does not allow for political power to vary between individuals belonging to different income groups. Acemoglu et al. (2011) state that a democratic but inefficient structure of government based on patronage leads to a rich–bureaucratic coalition and promotes high rents and less redistribution. Keeping fiscal capacity at low levels is a way to constrain future redistribution. Acemoglu et al. (2014b) find a robust

and quantitatively significant effect of democracy on tax revenues and secondary school investments that supports the conclusion of higher redistribution and human capital investments in democracies. However, they find no effect of democracy on inequality.

In non-democratic regimes, the leader alone or the ruling elite decides on social policy. Redistribution might be used to appease the poor and prevent a revolution that could result in democratization (Mejia and Posada, 2007). Further, by targeting specific groups redistribution may help to increase the number of supporters and thus contribute to the political survival of autocrats (Knutsen and Rasmussen, 2014). Or, on the contrary, redistribution may be used by the leader to reduce the wealth of some groups so as to limit their future political power (Leon, 2014; Acemoglu and Robinson, 2006). A similar argument is made by Solt (2008), who asserts that in autocracies political leaders may redistribute less because higher inequality depresses political participation. Hence, redistribution can play a strategic role too.

The main contribution of this paper is on the politics of social transfers. We provide new evidence on how the regime type may affect the choice made in favor of or against conditions. While the aim of conditions is to increase household investment in education and health, critics argue that they are costly and unnecessary. Regarding the differences between types of transfer, the efficiency and effectiveness of unconditional versus conditional transfers have indeed already been broadly discussed in the literature. Both types of schemes are found to contribute to poverty reduction (Barrientos, 2013; Browne, 2015). Scholars confirm that conditional cash transfers (CCTs) with explicit education conditions and penalties for non-compliance have a stronger effect on school achievements than unconditional transfers do (Baird et al., 2013; De Brauw and Hoddinott, 2011). Interestingly, Benhassine et al. (2015) find improvements in educational service for transfers with non-enforceable conditions—in other words, that merely label benefits for educational use. Studies that focus on CCTs also provide evidence of increased health service use and improved health outcomes (Fiszbein and Shady, 2009; Ranganathan and Lagarde, 2012). Recent studies that compare conditional and unconditional transfers suggest that health conditions do indeed matter (Attanasio et al., 2015; Robertson et al., 2015).

Regarding the political motives that affect the choice of social transfer programs, conditional transfers might be preferred in societies where the poor are only perceived as deserving of assistance if they make an effort—in other words, if they comply with conditions

(Fiszbein and Schady, 2009). A related argument is that conditions facilitate the targeting of poor children who cannot be held responsible for their economic situation. Moreover, by the examples of different conditional cash transfer programs it has been shown that social benefits can be strategically used to gain or to reward political supporters (e.g. Manacorda et al., 2011; De La O, 2013; Labonne, 2013). Some other political factors which shape social security systems are reviewed in Galasso and Profeta (2002). However, to the best of our knowledge there are no comparative studies that focus on the political motives behind introducing conditions. Systematic differences in assigning conditions between democracies and non-democracies would indicate that it is not only efficiency and effectiveness considerations that determine the choice of transfer programs.

Our paper is also related to the literature on the incentives of leaders in different political regimes to invest in human capital development. Klomp and de Haan (2013) show that democracies typically invest more in human capital, while unstable and non-democratic regimes limit such investments. The authors apply a combined approach, and use a large set of indicators on human capital. Many more studies proxy human capital investments through government expenditure on education and health. A number of studies confirm that government expenditure on education and health, as well as indicators of human capital, such as life expectancy and education levels, are higher in democracies compared to non-democracies (Acemoglu et al., 2014a; Baum and Lake, 2003; Brown and Hunter, 2004; Stasavage, 2005). In addition, Miller (2015) examines more deeply the differences across autocratic types and finds that regimes with multiparty autocratic elections promote human development more than other autocratic regimes do. Higher human capital investments in more democratic countries may be explained by the fact that democracies rely on a broad voter base that benefits from public education and health systems and hence holds political leaders accountable for the services. Non-democracies are generally supported by the rich who often prefer private education and health providers. Another argument is that non-democratic regimes may be threatened by an educated population (Feng, 2003). Finally, the regime type may affect public health and education spending through its impact on income and governance (Rajkumar and Swaroop, 2008). Democracies are generally richer and better governed and this in turn affects the efficiency of public services. As we suggest to use an alternative proxy for human capital investments – assigning conditions in social transfer schemes – our findings also complement this literature.

Finally, using data on social transfer programs to capture progressive redistribution enables us to shed light on controversial empirical findings on the triple relationship between regime type, redistribution, and inequality. Previous ambiguous empirical findings may be partially explained by the lack of reliable data in developing countries, which causes limitations in related methodological approaches. Typically, tax revenues and government expenditures are used as proxies for redistribution. For example, Mulligan et al. (2004) and Ansell and Samuels (2014) find no correlation between tax revenues and democracy in a cross-section sample. Mulligan, Gil, and Sala-i-Martin (2010) even confirm the opposite—that democracies spend a little less of their GDP on social security. Profeta, Puglisi, and Scabrosetti (2013) find no evidence for a link between separate tax revenues and political variables like civil liberties and democratic institutions, when controlling for country-fixed effects. Nevertheless, there are many studies that do find a certain positive relationship between democracy and redistribution. In particular, Aidt and Jensen (2008) show a positive effect of suffrage on government expenditures as a share of GDP and tax revenues as a share of GDP. Other studies find evidence that democracy has positive effects on government expenditures as well as on social security spending as a share of GDP (e.g. Acemoglu et al., 2014b; Lindert, 2004). Being a democracy also affects taxation structure and patterns (Hettich and Winer, 1999; Kenny and Winer, 2006). However, in developing countries the data on taxes and social spending is highly imperfect because of high tax evasion rates, non-transparency, and weak state capacity. The strength of the NSTP dataset by Dodlova, Giolbas, and Lay (2017) is that it captures progressive redistribution to the poor. This dataset significantly expands and updates the work by Barrientos, Niño-Zarazúa, and Maitrot (2010) in terms of both the time period covered and the number of programs included.

The literature review thus leads us to the following assertions that we test empirically below. (1) Democracies engage more in redistribution and are therefore more likely to adopt a social transfer program. (2) We expect that non-democracies have a higher probability to adopt unconditional transfer programs as the literature ascribes the retention of power as the main motive to redistribute in such countries. Unconditional transfers require less bureaucratic effort and state capacity and provide immediate poverty relief. They are therefore assumed to be a more operative tool for politically motivated redistribution than conditional transfers. (3) We expect a higher probability to adopt conditional programs in democratic countries since politicians in democracies are disciplined through reelection and therefore, they seek public support by increasing spending on social welfare policies and on

education and health. Moreover, democracies are considered as higher accountable, especially if they are characterized by free and fair elections, and so they care more about long run development. As conditional programs imply investments in education and health they have been shown to be more effective at enhancing human capital and thus self-sustained development than unconditional programs.

3 Econometric Specification

In this section, we present our empirical strategy to investigate the relationship between regimes and types of transfer. First, we estimate a canonical panel data model with country-fixed effects and year dummies. Second, we carry out an instrumental variable estimation to deal with time-varying omitted variables that may simultaneously affect the likelihood of regime change and adoption of social transfer programs.

The first econometric specification, fixed-effects model, is the following:

$$T_{it} = \beta_0 + \beta_1 D_{it-1} + \Sigma \gamma_k X^k_{it-1} + \nu_i + \delta_t + \varepsilon_{it} \qquad (1)$$

where T_{it} is the outcome of interest, which is either a dummy that indicates if a country has (1) any transfer or (2) a certain type of transfer (conditional or unconditional transfer program) or (3) a share of the respective type of transfer in the total number of transfer programs in a country in a particular year. In the first case, countries without any transfer program are included in the sample, while in the second case the analysis is restricted to countries with at least one transfer program in operation. As some countries focus their resources on large flagship programs while others have several schemes, often conditional as well as unconditional ones, the share of a type of transfer captures its relative importance.

The main independent variable D_{it-1} is the level of democracy measured by either the polity score from Marshall and Jaggers (2015) or the binary democracy score from Boix, Miller, and Rosato (2013). The vector of all other control variables is X^k_{it-1} and includes economic development indicators, population structure measures, and other characteristics of countries. We use the logarithm of all control variables to facilitate the interpretation and allow the impact of democracy to be proportional to the baseline level. In addition, we control for the overall number of social transfer programs within the region to capture social policy

diffusion (Gilardi, 2010). All independent variables are lagged to take into account that current social policy is determined by earlier levels of the political, economic, and demographic indicators. The error term that captures all omitted variables and random errors is ε_{it}, the country-fixed effects refer to ν_i, and time effects are denoted by δ_t. The latter effects capture common shocks and time trends for all countries. We thus control for two sources of potential bias. First, we allow democracies to be different from non-democracies in many permanent non-observed characteristics that also affect the adoption of social transfer programs. Second, we take into account the time trend of the broad expansion of social transfer programs, especially in recent years.

The effect of democracy in the OLS estimations is likely to be biased if there are time-varying omitted variables that simultaneously affect the likelihood of transitioning to democracy and the adoption of social transfer programs. For example, state capacity, higher citizens' demand for redistribution or human capital endowments might influence both democratization and the adoption of a transfer program in a country. The direction of the bias in the OLS estimates, however, is not clear as there might be several omitted variables whose effects might offset each other. Omitted variables are only one of the causes of endogeneity. Measurement error in the democracy variable may also increase the bias of the OLS estimates. Finally, the inverse relationship between a type of transfer and democracy may double the problem. A specific type of transfer being adopted can change the preferences and income of the poor and affect their demand for democratization.

We follow two IV strategies to deal with the endogeneity problem. First, following Persson and Tabellini (2009), we instrument democracy with the average of a country's neighbors' democratic scores. The idea is to control for endogenous transitions through democratic capital accumulated in the country's neighborhood. The corresponding two-stage least squares (2SLS) model we estimate is given by:

$$T_{it} = \beta_0 + \beta_1 D_{it-1} + \Sigma \gamma_k X^k_{it-1} + \nu_i + \delta_t + \varepsilon_{it} \qquad (2)$$

$$D_{it-1} = \beta_0 + \pi Z_{it-1} + \Sigma \gamma_k X^k_{it-1} + \mu_i + \lambda_t + u_{it}$$

where $Z_{it-1} = \frac{1}{N}\sum_{j\neq i}^{N} D_{jt-1}$ is an average of democratic scores in neighboring countries[1]. Our key assumption of exclusion restriction, $E(\varepsilon_{it}|Z_{it-1}, v_i, \delta_t)$=0, is that democratic regimes in neighbor countries do not have a direct impact on the adoption of a transfer program in a particular country.

The second strategy is based on regional waves of democratization as a source of exogenous variation. We rely on Acemoglu et al. (2014b) and Huntington (1991), who argue that democratization occurs in regional waves that are not only explained by economic trends. Their approach differs from Persson and Tabellini (2009), as it exploits regional waves rather than an individual country's democratic score regressed on its neighbors' democratic ones. They support the reasonable hypothesis that the demand for democracy spreads within a region where countries typically have a similar history, close culture, and informational ties (Acemoglu et al., 2014b). We have the same 2SLS specification as in (2), but now the instrument is defined by the jack-knifed average of democratic scores in the region including only countries that had a similar democratic score as the considered country in the initial period[2]:

$$Z'_{it-1} = \frac{1}{N_r-1}\sum_{j\neq i}^{N_r} D_{jt-1} \qquad (3)$$

We assume that the exclusion restriction is true $E(\varepsilon_{it}|Z'_{it-1}, v_i, \delta_t)$=0. Hence, our identification also relies on the assumption that democratic waves have no direct effect on the adoption of a social transfer program in a particular country.

One threat to the validity of our instrument is that democratic capital and democratization waves influence the probability of having a transfer program in neighboring countries or in the region and that this affects the probability to adopt a transfer program in a given country. We exclude this channel by allowing for social policy diffusion. In all specifications, we include the variable that indicates the number of transfer programs in the region in the previous year.

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[1] Persson and Tabellini (2009) take the average of polity scores weighted by the inverse distance between capitals of neighboring countries. However, we believe that the democratic capital spreads out across-the-board and the simple average of polity scores also proves to be a valid instrument.

[2] In our case, the initial period was 1960.

4 Data

The new NSTP dataset constructed by Dodlova, Giolbas, and Lay (2017) allows to distinguish between different types of transfer programs. This database provides detailed information on non-contributory social transfer schemes in developing countries, including types of transfer, targeting mechanisms, duration, mode of delivery, and—where available—budget and coverage of programs. The dataset covers the most prominent and important programs, and hence reflects the main characteristics and trends of social policies in different countries.

Dodlova, Giolbas, and Lay (2017) distinguish between unconditional family support schemes, social pension schemes, conditional cash transfers (CCTs), and public works programs. They define unconditional family support schemes as transfers targeted to low-income households, or specifically to children, that are not tied to investments in asset accumulation or human capital. They range from a basic safety net for those below the poverty line to (universal) child support grants. Under social pension schemes the authors subsume transfers to the elderly that are independent of a history of contributions by the beneficiary or his/her employer. We combine family support and social pension schemes into our category of unconditional transfers and contrast them with CCTs, which link the receipt of a transfer to investments in human capital. We also distinguish between conditions attached to improving education outcomes, health status, or both and check our results with different types of conditions. Typically, health conditions help to improve child and/or maternal health service usage. Education conditions are designed to increase school enrollment and achievements of children from low-income households.

The most well-known example of a conditional program is Mexico's Prospera (earlier Opportunidades), which provides income transfers to poor households if their children are regularly enrolled at school and they regularly report their children's health status. The strictness of conditions ranges from clearly defined behaviors that are verified, and whose noncompliance is sanctioned, to softer conditions that are not enforced. A good example is the conditional scheme introduced in the early 1990s in Honduras, the Programa de Asignacion Familiar (PRAF), or Family Allowances Program. The program was designed to distribute cash subsidies, first, to families with children aged from 6 to 12 who were enrolled

in primary school and regularly attended classes and, second, to families with children under 3 and pregnant mothers who regularly visited health centers. In this particular case, school enrollment but not attendance was enforced as a condition of the payments, while no transfers were suspended because of no visits to health centers.

The sample includes 163 social transfers in 99 developing countries, covering the period up to 2015. In 2015 there are 70 unconditional family support programs, 64 CCTs, and 43 social pensions. Of all the CCTs, 23 require education and 8 health investments; 33 are conditional upon investments in both education and health.

We conduct our main analysis on a country-year panel where the dependent variable is either a dummy that captures whether at least one of a certain type of social transfer program is in operation or a share of a certain type of program in the overall number of transfer programs in a country in a particular year. We merge this data on social transfers with other databases on socioeconomic indicators and political features of countries. As a result, we have a panel dataset that combines both major characteristics of social transfer programs and other indicators by country and year from 1960 until 2015. All countries included in the sample are considered as "low income," "lower-middle income," and "middle income" during a period of at least ten years since 1960 according to the World Bank's classification.

The data on social transfers are merged with the polity score from the Center for Systemic Peace's Polity IV database by Marshall and Jaggers (2015), and Boix, Miller, and Rosato's (2013) binary measure of democracy updated up until 2010. We focus on distinguishing between the two democratic indices as the former is based on constitutional constraints whereas the latter represents an empirical measure of democracy incorporating the elements of contestation and participation. In comparison with Polity IV, Boix et al.'s index has a minimal suffrage requirement for democracy which is important in a historical perspective[3].

The data on economic development and governance indicators – including GDP per capita, natural resource rents, total population, the share of urban population, the share of women in the total population and in the labor force, age dependency ratio, the share of agricultural value added, tax revenues as a share of GDP, government expenditure, and

[3] Munck and Verkuilen (2002) review alternative methodologies of constructing democracy indices and conclude that many of them suffer from important drawbacks. However, Polity IV and Boix et al.'s indices are considered as most representative ones in many studies.

primary and secondary school enrollment – are taken from the World Development Indicators (WDIs) dataset of the World Bank.

We control for the degree of revolutionary threat with a dummy that indicates if at least one protest event occurred in a country in a particular year. The information on protests is extracted from the SPEED Project Civil Unrest Event Data of the Cline Center for Democracy. Personal autonomy rights and political participation rights taken from Welzel (2013) are employed as alternative measures of paternalism.

Table 1 presents the summary statistics for all variables separately for democracies and non-democracies. The distinction between regimes is based on the polity score threshold of 6. We report means, standard deviations, and the total number of observations. The summary statistics show that we have more country-year observations for non-democracies than for democracies. This share is explained by the fact that many countries were non-democratic in the 1960s and then experienced democratization in the 1980s—for example, African and Latin American countries. Democracies tend to be more developed than non-democracies are, with much lower infant mortality rates. Democracies have higher population numbers, with a greater share of an urban population and a lower one of young people under 15—as well as higher school enrollment rates. Total natural resources and agricultural income constitute a larger part of GDP in non-democracies than in democracies. The net official development assistance and official aid received is also higher in non-democracies. The mean value of GDP per capita for all years is similar in both regimes, but its standard deviation is significantly higher for non-democracies. Tax revenues also are comparable across regimes, but the data is quite limited and cannot be seen as a complete picture of tax redistribution in developing countries—especially in non-democracies, where only one-quarter of all country-year statistics are available.

Figure 1 displays the significant expansion of social transfer programs across political regimes over time that was much more pronounced in democracies. It also highlights that in democracies relatively more conditional programs were adopted than in non-democracies. In 2014 about 70 percent of programs in democracies are conditional (66 unconditional programs versus 47 conditional programs), while in non-democracies only about 40 percent of the programs are conditional (34 versus 14).

	Non-Democracies			Democracies		
	Mean	Sd	N	Mean	Sd	N
GDP per capita (constant 2010 US$)	4281	10132	3617	4059	3476	1753
Total population (in million)	30.7	122	4521	48	155	1805
Urban population (% of total)	40.10	23.27	4524	48.39	21.78	1797
Female population (% of total)	49.55	3.16	4524	50.31	1.04	1799
Age dependency ratio, young (% of working-age population)	73.69	20.30	4521	61.05	20.86	1799
Age dependency ratio, old (% of working-age population)	7.02	2.94	4521	9.25	4.87	1799
Mortality rate, infant (per 1,000 live births)	78.69	48.54	4204	44.42	33.35	1777
Total natural resources rents (% of GDP)	15.19	16.38	3128	7.14	9.10	1555
Agriculture, value added (% of GDP)	26.29	16.38	3090	17.39	12.62	1566
Protest events (SPEED)	0.44	0.50	4528	0.31	0.46	1805
GINI index of market income inequality (Solt, 2016)	40.65	8.79	1845	42.51	8.86	1105
Net official development assistance and official aid received (constant 2013 US$ million)	519	811	4140	542	855	1702
Tax revenue (% of GDP)	14.02	8.00	864	16.70	7.78	855
General government final consumption expenditure (% of GDP)	15.23	7.49	3387	14.95	9.02	1615
Female labor force (% of total labor force)	38.35	12.28	1863	40.16	6.91	1799
School enrollment, primary (gross), gender parity index (GPI)	0.85	0.18	2800	0.96	0.11	1287
School enrollment, secondary (gross), gender parity index (GPI)	0.77	0.28	2082	0.97	0.21	1053
Personal autonomy rights (QoG and Welzel, 2013)	0.39	0.18	2160	0.64	0.18	1119
Political participation rights (QoG and Welzel, 2013)	0.31	0.21	2174	0.76	0.16	1121

Table 1. Summary statistics for democracies and non-democracies.
Notes: The upper panel shows variables used in the main specification while variables in the lower panel are used for robustness checks. The sources of the data are presented in the parentheses after the variable definition. If the source is not indicated, then the data is extracted from the World Development Indicators (WDIs) dataset of the World Bank. The other sources of the data are the following: SPEED refers to the Civil Unrest Event Data of the Cline Center for Democracy; QoG indicates the Quality of Government Institute database of the University of Gothenburg.

Figure 2 illustrates the detailed distribution of countries with an unconditional and a conditional program along the polity scale taken from the Polity IV data by Marshall and Jaggers (2015). We can confirm that democracies adopt more transfer programs of any type than non-democracies do, as for both types of transfer the distributions are skewed upward and the median polity scores are higher than 5[4]. Further, the median is higher for conditional than for unconditional transfer: it is 8 in the first case, and 6 in the second. Conditional transfer programs are thus more often adopted in more democratic countries. In addition, the

[4] Countries with a polity score above 6 are classified as democratic.

box is much smaller for conditional transfers. Hence, while conditional schemes are only prevalent in countries with a high polity score, unconditional schemes are widespread across regimes with another peak in the distribution of such schemes in very authoritarian regimes.

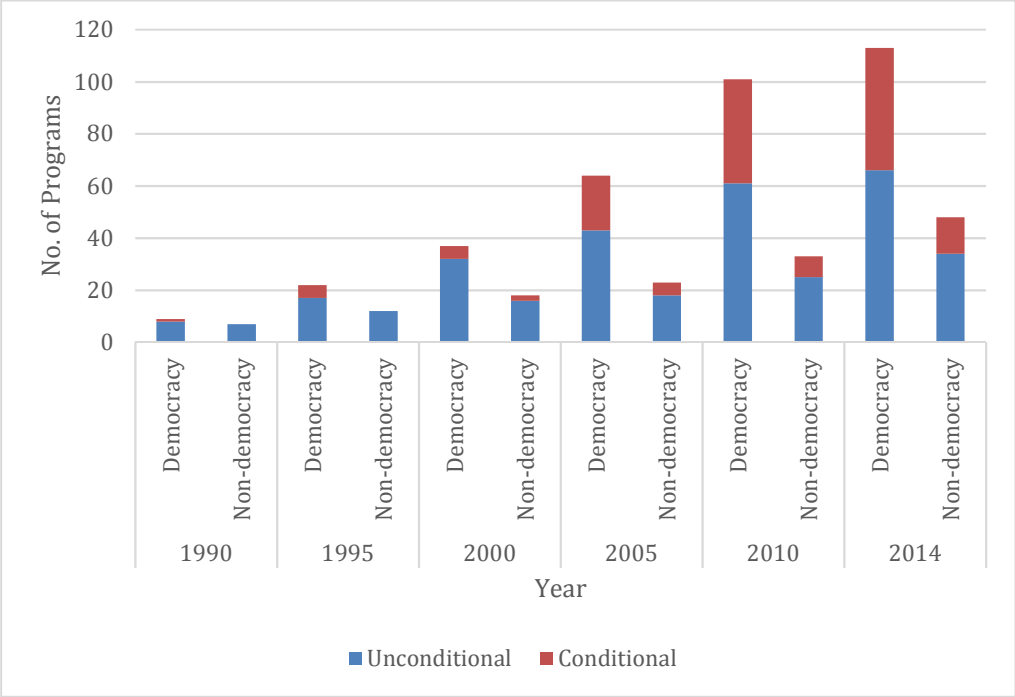

Figure 1. Conditional and Unconditional Transfers in Democracies and Non-Democracies, 1990–2014.

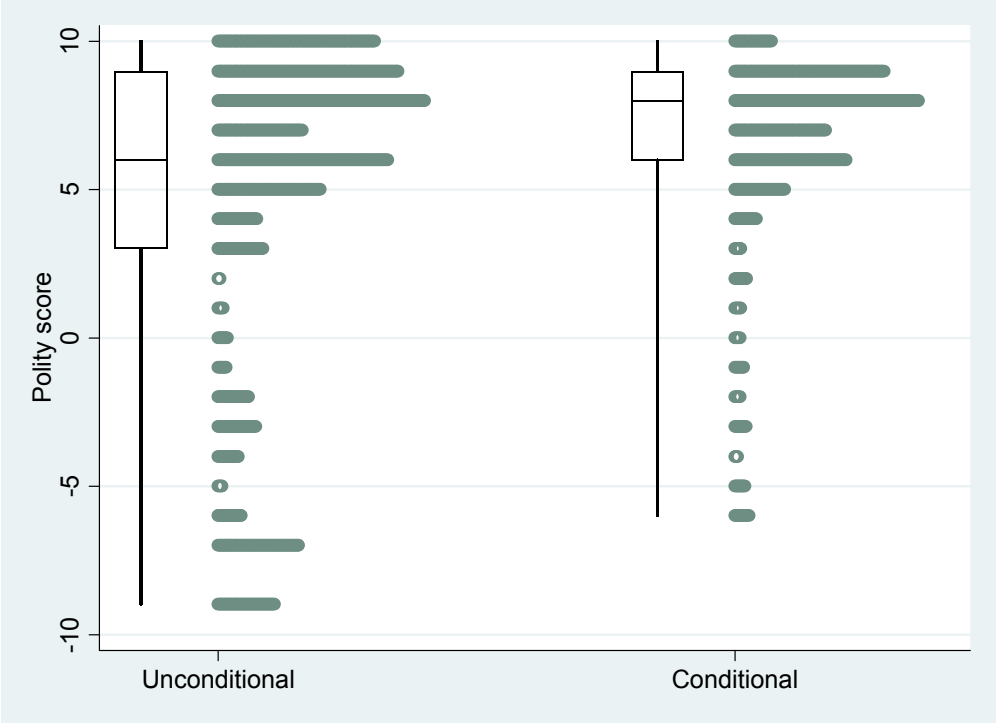

Figure 2. Transfer Program Types According to Polity Score, 1990–2014.

5 Empirical Evidence

In this section, the empirical results of the analysis on the link between regime type and the probability of adopting any social transfer program or a particular type of program are presented. In addition, the effects of inequality, tax redistribution, paternalism, and other variables on the choice of transfer program types are studied.

5.1 Regime Types and any Social Transfer

First, we report the estimates for adopting any transfer program. In Table 2 the dependent variable is a dummy that equals 1 if a country has at least one unconditional or conditional redistributive transfer program in a particular year and 0 if it has none. Columns 1-2 present the OLS results with the polity score from Marshall and Jaggers (2015) and the binary democratic index from Boix, Miller, and Rosato (2013) as main explanatory variables, respectively. Both regime type variables are positive and strongly significant in the OLS specifications. The coefficient on the polity score is lower because it ranges from -10 (institutionalized autocracy) to 10 (institutionalized democracy), whereas the democracy variable is a dummy that equals 1 if a regime is democratic. These preliminary results suggest that more democratic countries are more likely to adopt a transfer program of any type.

However, the estimates from OLS on the pooled data with country- and year-fixed effects suffer from endogeneity caused by measurement error, or by time-varying omitted variables that might be external drivers for the simultaneous change in regime type and the probability of having a transfer program. Columns 3-5 report results from the IV estimations. In Column 3, following Persson and Tabellini (2009) the polity score is instrumented through neighbors' democratic capital. In Columns 4-5, the polity score and the binary democratic index are instrumented through democratization waves, respectively.

Tests for the goodness-of-fit of the IV specifications reveal that the first instrument of neighbors' democratic capital is relevant only for the polity variable and is not for the binary democratic index. This is very probably explained by smaller country coverage and timespan for the democratic index as compared to the polity score. As we calculate the democratic capital of neighboring countries without water borders, the sample size is sharply reduced and the instrument is thus not reliable for the binary democratic index. Since the second instrument, democratization waves, is calculated on the basis of the democratic scores of all countries in the region and not only neighboring ones, there are much fewer missing values

and the instrument is relevant and valid for both the polity score and the binary democratic index. We present *F* statistics of excluded instruments for the first stage regressions for all specifications.

The result using neighbors' democratic capital as an instrument for democracy (Column 3) confirms the conclusion of the OLS specifications. A change in one score toward a higher level of democracy leads to an almost 3 percentage point increase in the probability of having a transfer program. OLS is hence downward biased. Surprisingly, the 2SLS estimates using democratization waves based on the polity score or binary democracy index (Columns 4 and 5) do not confirm a significant effect of democracy on the adoption of transfer programs. Hence, we find cautious evidence that democracies have a higher probability of launching a transfer program, although the result is not robust to the second IV identification strategy.

The identifying assumption for the IV specification is that the democratic capital and democratization waves affect the democratic score in a particular country but that once regime change is controlled for they have no independent effect on the adoption of a transfer program. The instruments account for up to 48 percent of the variation in regime change depending on the specification[5].

In all specifications, we control for the number of social transfer programs in the region, to exclude the possibility that democratic capital or democratization waves also capture the expansion of social transfer policies within a region. This social policy diffusion effect is strongly significant, and equals about 1-1.5 percentage points. At the same time, it is clear that the correlation between regime type and the likelihood of introducing a transfer program is not crowded out by a policy diffusion effect.

The other control variables demonstrate the expected signs. The coefficient on the lagged GDP per capita is positive, while the coefficient on the lagged squared term of GDP per capita is negative. They are both highly significant. This supports the nonlinear effect of economic development on adopting a transfer program. Richer countries apply more transfer programs in general.

[5] The first-stage regressions are available upon request.

	(1) any transfer OLS	(2) any transfer OLS	(3) any transfer IV capital	(4) any transfer IV waves	(5) any transfer IV waves
polity score	0.008***		0.027*	-0.009	
	(0.001)		(0.014)	(0.005)	
democratic index		0.162***			-0.215
		(0.016)			(0.231)
infant mortality	-0.076***	-0.133***	-0.057*	-0.277***	-0.290***
	(0.026)	(0.028)	(0.032)	(0.036)	(0.039)
female population	0.857***	1.514***	1.334***	3.492***	4.071***
	(0.268)	(0.339)	(0.381)	(0.712)	(0.637)
protest	-0.031***	-0.027**	-0.019	-0.012	-0.029*
	(0.011)	(0.011)	(0.012)	(0.014)	(0.017)
GDP	1.137***	1.058***	1.100***	0.875***	0.862***
	(0.113)	(0.132)	(0.150)	(0.163)	(0.193)
GDP squared	-0.061***	-0.056***	-0.054***	-0.043***	-0.042***
	(0.008)	(0.009)	(0.011)	(0.011)	(0.013)
total population	-0.001	0.106	0.065	0.069	-0.019
	(0.059)	(0.066)	(0.115)	(0.096)	(0.199)
urban population	-0.179***	-0.095**	-0.197***	-0.153**	-0.148**
	(0.037)	(0.041)	(0.041)	(0.061)	(0.063)
agedepend old	0.147***	0.128**	0.153***	0.178**	0.111
	(0.055)	(0.056)	(0.058)	(0.076)	(0.116)
agedepend young	-0.073	-0.098*	-0.241**	0.031	0.045
	(0.049)	(0.054)	(0.099)	(0.067)	(0.107)
natural resources	-0.034***	-0.027***	-0.031***	-0.033***	-0.032**
	(0.009)	(0.009)	(0.010)	(0.011)	(0.013)
agricultural value	-0.113***	-0.082***	-0.097***	-0.101***	-0.106***
	(0.020)	(0.021)	(0.024)	(0.026)	(0.033)
regional diffusion	0.011***	0.014***	0.014***	0.012***	0.015***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Constant	-6.999***	-10.627***	-9.594***	-16.627***	-17.362***
	(1.596)	(1.852)	(2.858)	(3.434)	(4.269)
Observations	3,924	3,779	3,268	2,516	2,304
R-squared	0.727	0.735	0.706	0.710	0.677
F first			21.05	224.57	14.76
Country FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Table 2. Political Regimes and all Transfer Programs.

Notes: Robust standard errors are in parentheses *** p<0.01, ** p<0.05, * p<0.1. All estimations are made for the whole sample, where the unit of analysis is country–year. The time period is 1960–2014. The dependent variables are dummies for any transfer program. The methods employed are OLS (Columns 1–2), IV based on democratic capital (Column 3), IV based on democratization waves (Columns 4–5). All specifications include a full set of country- and year-fixed effects. The main explanatory variables are one period lagged democratic scores from Polity IV by Marshall and Jaggers (2015) and Boix, Miller, and Rosato (2013). Other control variables listed in order of appearance (all in one-year lags and continuous ones in logs) include infant mortality rate, share of female population, dummy for protest events, GDP per capita, GDP per capita squared, total population, share of urban population, age dependency ratio of the old and young, natural resource rents as a percentage of GDP, agricultural value as a percentage of GDP, the total number of transfer programs in a particular year in the region.

The infant mortality rate is negatively correlated with the probability of having a transfer program. This correlation may be explained by reverse causality or omitted variable bias. Infant mortality may be falling in countries where social transfers, more so conditional ones, are in place. Further, we may not sufficiently capture weak state capacity, which may explain both the lack of transfer adoption and high infant mortality because of deficient health service delivery. Likewise, the positive correlation of high infant mortality with the lack of social transfers may also be rationalized by taking high infant mortality as a general sign of neglecting social services for the poor.

Demographics seems important as well. Total population is not significant but the share of urban population demonstrates a significantly negative effect on the probability of having a transfer program. A larger share of old people to working age people leads to more transfer programs being in operation although this result is not robust across all IV specifications. The lagged share of women in the total population is highly significant and positive in all specifications. Resource-rich countries are about 3 percentage points less likely to adopt a transfer program. The ruling elite in countries with resource abundance may have more opportunities for repression and other ways of pleasing the population than by adopting social transfers. The revolutionary threat measured by a dummy on at least one protest event in the previous year is significant and negative in Columns 1, 2, and 5. In Column 3 it is significant only at the 20 percent level.

5.2 Regime Types and Unconditional Social Transfers

This section presents insights on the effect of regime type on an unconditional social transfer program in place. We run two specifications: first, the dependent variable is a dummy that equals 1 if a country has at least one unconditional transfer program in a particular year and 0 if it has none (see Table 3). Then, Table 4 reports the results from regressions where the dependent variable is the share of unconditional programs in the total number of programs based on a sub-sample of countries excluding those without a program of any type. This second specification thus captures the effects of democracy (and other explanatory variables) on the adoption of unconditional programs given any social transfer policy in place.

The OLS estimates of the effect of democracy on the probability of having an unconditional transfer program as well as on the share of unconditional programs are positive and significant (Columns 1-2 in Tables 3 and 4). However, the point estimates become

negative and strongly significant in the 2SLS specifications (Columns 3-5 in Tables 3 and 4). The upward bias in the OLS estimations is likely to be due to omitted variables, for example state capacity. Similar to the argument made above for child mortality, state capacity is likely to be correlated with the polity score and will, at the same time, affect the ability and thus likelihood of adopting a transfer program. The 2SLS estimations confirm that non-democratic countries tend to choose unconditional transfer programs. This effect persists for both the dummy and share of unconditional programs, as well as for both measures of democracy and for both IV strategies (Tables 3 and 4).

A one point increase on the polity scale results in a 2-3 percentage point reduction of the probability of having an unconditional transfer and of the share of unconditional transfers. Hence, the effect is quite large: a regime change from democracy to non-democracy may result in a 40-60 percentage point increase of the probability of an unconditional transfer scheme being introduced (Columns 3-5 in Table 3). The share of unconditional transfers after a regime change may increase by 70–98 percentage points if we reduce our sample to countries with at least one transfer program regardless its type (Columns 3-5 in Table 4).

In all specifications, we again control for the number of unconditional social transfer programs in the region in the previous period. An additional unconditional program in the region increases the likelihood of adopting the same type of program in a particular country by 1.5–2 percentage points. The other control variables have effects that are similar to those in the above specifications with any transfer program. Interestingly, natural resource abundance and agricultural value added are not significant for unconditional transfers. Whereas the occurrence of at least one protest event becomes strongly significant: periods without any protest are likely characterized by at least one unconditional social transfer program in place. This suggests that unconditional transfers may be used to prevent civil unrest as this type of transfer provides urgent assistance.

	(1) unconditional transfer OLS	(2) unconditional transfer OLS	(3) unconditional transfer IV capital	(4) unconditional transfer IV waves	(5) unconditional transfer IV waves
polity score	0.006***		-0.034**	-0.022***	
	(0.001)		(0.017)	(0.006)	
democratic index		0.115***			-0.616**
		(0.015)			(0.253)
infant mortality	-0.073***	-0.120***	-0.117***	-0.353***	-0.277***
	(0.027)	(0.029)	(0.039)	(0.038)	(0.050)
female population	0.049	0.709**	-0.838*	2.471***	2.301***
	(0.280)	(0.325)	(0.441)	(0.647)	(0.700)
protest	-0.024**	-0.017*	-0.031**	-0.026**	-0.039**
	(0.011)	(0.010)	(0.014)	(0.013)	(0.019)
GDP	1.179***	1.000***	1.343***	0.610***	0.639***
	(0.116)	(0.132)	(0.162)	(0.162)	(0.207)
GDP squared	-0.063***	-0.051***	-0.078***	-0.028**	-0.034**
	(0.008)	(0.009)	(0.012)	(0.011)	(0.014)
total population	-0.169***	-0.147**	-0.567***	-0.179*	-0.769***
	(0.064)	(0.067)	(0.126)	(0.101)	(0.225)
urban population	-0.156***	-0.084**	-0.138***	-0.013	-0.037
	(0.039)	(0.042)	(0.043)	(0.063)	(0.076)
agedepend old	0.008	-0.063	-0.035	0.116	-0.148
	(0.055)	(0.055)	(0.066)	(0.079)	(0.131)
agedepend young	0.042	0.014	0.178*	0.266***	0.395***
	(0.051)	(0.053)	(0.099)	(0.069)	(0.123)
natural resources	-0.027***	-0.020**	-0.016	-0.004	-0.012
	(0.009)	(0.009)	(0.012)	(0.011)	(0.014)
agricultural value	-0.050**	-0.031	-0.042	-0.014	-0.042
	(0.020)	(0.020)	(0.026)	(0.026)	(0.036)
regional diffusion	0.008***	0.009***	0.016***	0.007**	-0.002
	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)
Observations	3,924	3,779	3,268	2,516	2,304
R-squared	0.719	0.739	0.617	0.677	0.510
F first			16.48	204.47	17.19
Country FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Table 3. Political Regimes and Unconditional Transfer Programs.

Notes: Robust standard errors are in parentheses *** p<0.01, ** p<0.05, * p<0.1. All estimations are made for the whole sample, where the unit of analysis is country–year. The time period is 1960–2014. The dependent variable is a dummy for at least one unconditional transfer program in a country in a particular year. The methods employed are OLS (Columns 1–2), IV based on democratic capital (Column 3), IV based on democratization waves (Columns 4–5). All specifications include a full set of country- and year-fixed effects. The main explanatory variables are one period lagged democratic scores from Polity IV by Marshall and Jaggers (2015) and Boix, Miller, and Rosato (2013). Other control variables listed in order of appearance (all in one-year lags and continuous ones in logs) include infant mortality rate, share of female population, dummy for protest events, GDP per capita, GDP per capita squared, total population, share of urban population, age dependency ratio of the old and young, natural resource rents as a percentage of GDP, agricultural value as a percentage of GDP, the total number of transfer programs in a particular year in the region.

	(1) unconditional transfer share OLS	(2) unconditional transfer share OLS	(3) unconditional transfer share IV capital	(5) unconditional transfer share IV waves	(6) unconditional transfer share IV waves
polity score	0.004***		-0.069**	-0.024***	
	(0.001)		(0.028)	(0.005)	
democratic index		0.085***			-0.985***
		(0.016)			(0.320)
infant mortality	-0.090***	-0.149***	-0.265***	-0.485***	-0.426***
	(0.033)	(0.036)	(0.088)	(0.041)	(0.076)
female population	-0.235	-0.050	-2.977***	2.842**	0.214
	(0.385)	(0.383)	(1.135)	(1.210)	(2.375)
protest	0.001	-0.003	-0.041*	-0.014	-0.068**
	(0.012)	(0.012)	(0.025)	(0.016)	(0.031)
GDP	1.059***	0.948***	1.475***	0.604***	0.757**
	(0.137)	(0.155)	(0.256)	(0.185)	(0.297)
GDP squared	-0.057***	-0.047***	-0.091***	-0.029**	-0.051**
	(0.010)	(0.011)	(0.019)	(0.013)	(0.020)
total population	-0.058	-0.098	-0.605***	0.170*	-0.700**
	(0.066)	(0.074)	(0.192)	(0.097)	(0.304)
urban population	-0.087**	-0.038	-0.059	0.232***	0.274**
	(0.042)	(0.048)	(0.062)	(0.072)	(0.134)
agedepend old	-0.323***	-0.332***	-0.471***	-0.093	-0.289*
	(0.059)	(0.062)	(0.090)	(0.078)	(0.157)
agedepend young	-0.206***	-0.121*	0.087	-0.056	0.262
	(0.058)	(0.064)	(0.183)	(0.075)	(0.182)
natural resources	-0.040***	-0.035***	-0.019	-0.014	-0.025
	(0.009)	(0.010)	(0.015)	(0.011)	(0.019)
agricultural value	-0.056**	-0.025	0.022	-0.034	-0.063
	(0.024)	(0.025)	(0.039)	(0.032)	(0.054)
regional diffusion	0.013***	0.014***	0.026***	0.016***	0.008
	(0.002)	(0.003)	(0.004)	(0.003)	(0.005)
Observations	3,133	2,903	2,529	2,004	1,826
R-squared	0.703	0.721	0.331	0.668	0.170
F first			9.38	187.51	13.81
Country FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Table 4 Political Regimes and Shares of Unconditional Transfer Programs.

Notes: Robust standard errors are in parentheses *** p<0.01, ** p<0.05, * p<0.1. All estimations are made for the sample of countries with at least one transfer program in operation. The unit of analysis is country–year. The time period is 1960–2014. The dependent variable is a share of unconditional transfer programs in a country in particular year. The methods employed are OLS (Columns 1–2), IV based on democratic capital (Column 3), IV based on democratization waves (Columns 4–5). All specifications include a full set of country- and year-fixed effects. The main explanatory variables are one period lagged democratic scores from Polity IV by Marshall and Jaggers (2015) and Boix, Miller, and Rosato (2013). Other control variables listed in order of appearance (all in one-year lags and continuous ones in logs) include infant mortality rate, share of female population, dummy for protest events, GDP per capita, GDP per capita squared, total population, share of urban population, age dependency ratio of the old and young, natural resource rents as a percentage of GDP, agricultural value as a percentage of GDP, the total number of transfer programs in a particular year in the region.

5.3 Regime Types and Conditional Social Transfers

In this section, we discuss the link between regime types and conditional transfers. The specifications are similar to the above subsection. In the specifications of Table 5 the dependent variable is a dummy that equals 1 if a country has at least one conditional transfer program in operation in a particular year, and 0 if none. In Table 6, we report the results for the share of conditional schemes in the total number of programs in a country in a particular year. Again, we limit the sample to countries with at least one program of any type in operation.

The coefficients on the democracy variables are positive and significant in the OLS specifications (Columns 1–2 in Tables 5 and 6). Implementing the IV strategies supports this finding when the dependent variable is the conditional transfer dummy and we instrument the binary democratic index with democratization waves (Column 5 in Table 5) but not in the IV specifications with the polity score as main explanatory variable (Columns 3–4 of Table 5). However, when the dependent variable is the share of conditional transfers, the IV specifications show a significantly positive and robust effect of democracy (Columns 3-5 in Table 6). Once social transfer policies are put in place, being a democratic regime increases the number of pro-poor conditional programs. Democracies thus enhance the relative importance of conditional cash transfers that imply investments in education and health.

The results in Tables 5 and 6 demonstrate a downward bias in the OLS specifications. This bias may stem from omitted variables, which may again be related to weak state capacity that explains both levels of democracy and specific program adoption. In addition, omitted human capital endowment may play a role: societies with high human capital endowment are more likely to be democratic but tend to adopt fewer conditional programs as investment in human capital accumulation is not their priority.

As above, the estimated effects are large: a transition from non-democracy to democracy increases the share of conditional transfer programs by 20-40 percentage points according to the estimations based on the polity score (Columns 3-4 in Table 6), and by about 50 percentage points according to the estimations based on the binary index (Column 5 in Table 6). The magnitude of the effect is lower for the polity variable than for the binary democratic index, but nevertheless large.

	(1) conditional transfer OLS	(2) conditional transfer OLS	(3) conditional transfer IV capital	(4) conditional transfer IV waves	(5) conditional transfer IV waves
polity score	0.004***		-0.004	-0.000	
	(0.001)		(0.010)	(0.004)	
democratic index		0.072***			0.398*
		(0.013)			(0.232)
infant mortality	-0.131***	-0.129***	-0.198***	-0.066**	-0.077**
	(0.018)	(0.019)	(0.023)	(0.028)	(0.034)
female population	0.693***	0.835***	0.660**	0.725*	1.300**
	(0.204)	(0.219)	(0.318)	(0.436)	(0.535)
protest	-0.025***	-0.021***	-0.021**	-0.016	-0.000
	(0.008)	(0.007)	(0.009)	(0.010)	(0.014)
GDP	0.362***	0.365***	0.315***	0.394***	0.283*
	(0.089)	(0.094)	(0.112)	(0.123)	(0.166)
GDP squared	-0.023***	-0.024***	-0.019**	-0.022***	-0.009
	(0.006)	(0.007)	(0.008)	(0.008)	(0.010)
total population	0.205***	0.271***	0.074	-0.064	0.433**
	(0.052)	(0.056)	(0.099)	(0.087)	(0.203)
urban population	-0.003	0.057**	-0.045*	-0.135***	-0.120**
	(0.026)	(0.026)	(0.027)	(0.049)	(0.057)
agedepend old	0.297***	0.318***	0.375***	0.086*	0.261**
	(0.036)	(0.038)	(0.041)	(0.045)	(0.103)
agedepend young	0.121***	0.086**	0.236***	0.262***	0.075
	(0.035)	(0.035)	(0.075)	(0.049)	(0.101)
natural resources	-0.001	0.005	-0.012*	-0.018**	-0.005
	(0.006)	(0.006)	(0.007)	(0.009)	(0.010)
agricultural value	-0.038***	-0.019	-0.070***	-0.048**	-0.003
	(0.014)	(0.014)	(0.017)	(0.022)	(0.026)
regional diffusion	0.029***	0.032***	0.025***	0.030***	0.034***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
Observations	3,924	3,779	3,268	2,516	2,304
R-squared	0.650	0.613	0.646	0.649	0.516
F first			19.27	212.63	15.80
Country FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Table 5. Political Regimes and Conditional Transfer Programs.

Notes: Robust standard errors are in parentheses *** p<0.01, ** p<0.05, * p<0.1. All estimations are made for the whole sample, where the unit of analysis is country–year. The time period is 1960–2014. The dependent variable is a dummy for at least one conditional transfer program in a country in a particular year. The methods employed are OLS (Columns 1–2), IV based on democratic capital (Column 3), IV based on democratization waves (Columns 4–5). All specifications include a full set of country- and year-fixed effects. The main explanatory variables are one period lagged democratic scores from Polity IV by Marshall and Jaggers (2015) and Boix, Miller, and Rosato (2013). Other control variables listed in order of appearance (all in one-year lags and continuous ones in logs) include infant mortality rate, share of female population, dummy for protest events, GDP per capita, GDP per capita squared, total population, share of urban population, age dependency ratio of the old and young, natural resource rents as a percentage of GDP, agricultural value as a percentage of GDP, the total number of transfer programs in a particular year in the region.

	(1) conditional transfer share OLS	(2) conditional transfer share OLS	(3) conditional transfer share IV capital	(5) conditional transfer share IV waves	(6) conditional transfer share IV waves
polity score	0.002**		0.021*	0.014***	
	(0.001)		(0.013)	(0.004)	
democratic index		0.042***			0.734***
		(0.012)			(0.253)
infant mortality	-0.046**	-0.047*	-0.058	0.120***	0.030
	(0.022)	(0.024)	(0.042)	(0.031)	(0.057)
female population	1.162***	0.937***	2.051***	3.100***	5.180***
	(0.211)	(0.232)	(0.559)	(0.795)	(1.603)
protest	-0.010	-0.011	0.006	0.009	0.036
	(0.008)	(0.008)	(0.012)	(0.011)	(0.023)
GDP	0.186**	0.212**	0.071	0.589***	0.441**
	(0.082)	(0.088)	(0.124)	(0.126)	(0.220)
GDP squared	-0.013**	-0.014**	-0.003	-0.033***	-0.016
	(0.006)	(0.006)	(0.009)	(0.008)	(0.014)
total population	0.239***	0.267***	0.349***	0.165**	0.853***
	(0.049)	(0.058)	(0.105)	(0.081)	(0.237)
urban population	0.004	0.044*	-0.017	-0.168***	-0.148*
	(0.023)	(0.024)	(0.027)	(0.042)	(0.092)
agedepend old	0.147***	0.204***	0.220***	-0.031	0.240**
	(0.033)	(0.037)	(0.043)	(0.046)	(0.112)
agedepend young	-0.028	-0.035	-0.135	0.001	-0.246*
	(0.035)	(0.036)	(0.098)	(0.048)	(0.128)
natural resources	-0.013**	-0.010*	-0.023***	-0.028***	-0.010
	(0.006)	(0.006)	(0.007)	(0.008)	(0.013)
agricultural value	-0.032**	-0.012	-0.057***	-0.051*	-0.022
	(0.016)	(0.016)	(0.020)	(0.026)	(0.036)
regional diffusion	0.021***	0.024***	0.019***	0.024***	0.026***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.003)
Observations	3,133	2,903	2,529	2,004	1,826
R-squared	0.602	0.570	0.516	0.602	0.005
F first			11.52	182.39	13.58
Country FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Table 6 Political Regimes and Shares of Conditional Transfer Programs.

Notes: Robust standard errors are in parentheses *** p<0.01, ** p<0.05, * p<0.1. All estimations are made for the sample of countries with at least one transfer program in operation. The unit of analysis is country–year. The time period is 1960–2014. The dependent variable is a share of conditional transfer programs in a country in particular year. The methods employed are OLS (Columns 1–2), IV based on democratic capital (Column 3), IV based on democratization waves (Columns 4–5). All specifications include a full set of country- and year-fixed effects. The main explanatory variables are one period lagged democratic scores from Polity IV by Marshall and Jaggers (2015) and Boix, Miller, and Rosato (2013). Other control variables listed in order of appearance (all in one-year lags and continuous ones in logs) include infant mortality rate, share of female population, dummy for protest events, GDP per capita, GDP per capita squared, total population, share of urban population, age dependency ratio of the old and young, natural resource rents as a percentage of GDP, agricultural value as a percentage of GDP, the total number of transfer programs in a particular year in the region.

We do not discuss the effects of control variables in detail as they tend to be similar as in the case of any transfer. One can only notice that total population demonstrates a robust positive association with the conditional transfer dummy. Further, a higher female share results in more conditional transfers. This might be related to women's empowerment ensured often through conditional social transfer programs. Countries with high natural resources rents and agricultural value added tend to have less conditional programs. Regional diffusion also matters for conditional transfers. The effect is larger than for unconditional transfers and varies between 2.5 and 3 percentage points.

5.4 Additional Results and Robustness Checks

This section presents results on the effect of inequality on the adoption of a transfer program type as well as several sensitivity checks to the main findings. The robustness tests consist of specification changes, the inclusion of additional control variables, running regressions for different timespans, and controlling for lags of one, three, or five years. We also check the findings using alternative data on social transfer programs.

The effect of inequality is considered separately in sub-samples of democratic and non-democratic countries, as it can be assumed that political regimes have structural differences in social policymaking while fighting inequality. We use gross income inequality from the standardized database by Solt (2016)[6]. The models where the dependent variable is a dummy that at least one program of a certain type is in operation do not show any significant results for inequality[7]. However, the share of conditional transfers significantly decreases with higher inequality in democracies. The effect is not large but consistent with the logic that higher inequality may increase civil unrest and that unconditional transfers may be adopted to prevent civil unrest.

As robustness checks, we include additional control variables in our benchmark specifications; in particular, the level of paternalism, taxes and government expenditure, development assistance, the share of women in the labor force, and school enrollment rates. Inclusion of these variables reduces the sample by more than half, even if the time period is

[6] The data is multiply imputed but has a high country–year coverage. We also extract the inequality data from the World Income Inequality Database (WIID3.3) by UNU-WIDER (2015) and can confirm the findings using that data for a five-year sample.

[7] The results discussed in this section are available from the authors upon request.

limited to 1990 to 2015. However, these results should still be a part of the analysis, as social transfer policies may be affected by these control variables.

We explore how the level of paternalism in a society influences the choice of social transfer programs. This is particularly interesting for conditional transfers as conditions may be adopted to facilitate targeting of the "deserving poor", which would be more likely in societies with higher levels of paternalism. We use two alternative measures of paternalism: personal autonomy rights and political participation rights from Welzel (2013). The indicators measure to what extent a country enacts personal autonomy or political participation rights by law, and respects them in practice. Both variables have a positive effect on the adoption of conditional schemes. Hence, lower – not higher – paternalism is associated with more conditional transfer programs. For unconditional transfers, the effect of paternalism is rather the opposite. While countries with higher personal autonomy rights are less likely to have an unconditional transfer, political participation rights have no significant effect. In the analysis of any type of social transfer, political participation rights are positive and strongly significant implying that social policies are more widely used in countries where citizens' political support matters.

The literature has already pointed out that political regime type might affect levels of taxation and government spending (Hettich and Winer, 1999; Kenny and Winer, 2006; Profeta, Puglisi, and Scabrosetti, 2013; Acemoglu et al., 2014b). Therefore, we change the specifications to control for change in government tax redistribution policy and expenditure across different regime types. Taxation and public spending are not that significant in any of the regressions, meaning that government strategies on tax redistribution are not strongly correlated with the choice in favor of any type of social transfer program. This might be explained by the fact that we consider non-contributory social transfer schemes that are not part of contributory social security systems based on tax redistribution. Moreover, our main results remain unchanged. We confirm that in developing countries government tax redistribution does not crowd out the impact of political regime type on the choice of social transfer programs.

The lagged official development assistance and aid as a share of GDP prove not significant. It appears that foreign aid does not influence the adoption of social transfer programs.

The share of women in the labor force is significant and negative; fewer women in the labor force are associated with adopting more social transfer programs of any type. If fewer working women are a sign of a poorer society, this may explain a higher demand for poverty alleviation policies. Moreover, fewer women in the labor force may lead to more women's empowerment policies through social assistance. Indeed, many social transfer programs have women as their main target group.

Initial levels of educational achievement matter for social transfer policies, and especially for the choice of conditional transfer programs. Enrollment in primary education is significantly positive, whereas that in secondary education is significantly negative. Countries with already high levels of secondary education may not see the need to incentivize household investments in education.

Further, we examine whether political regime type affects the choice of social transfers with conditions in a particular sphere: education or health. We create separate dummies on the types of condition, such as regular medical investigations or school attendance, and run the same regressions as in Tables 2–6. We can confirm that the type of condition does not matter for the results in this paper. However, for health conditions results are somewhat less significant and it seems as if regime type is of greater importance for schemes with education conditions.

The main specifications above use annual panel data. To minimize serial correlation a five-year panel from 1960 onwards that takes an observation every five years is constructed. According to Acemoglu et al. (2014b), this approach is more appropriate than taking averages over a five-year period since the latter could keep serial correlation and render estimates inconsistent. In addition, we run specifications using different lag structures, with lags higher than one year—three and five years—on the annual panel. All results remain unchanged, except for the case of the share of unconditional transfers—where the democratic score becomes insignificant. All other control variables are slightly less significant, but do not change the signs of the effects. Furthermore, in view of the binary nature of the dependent variable, we also run the logit and probit models with and without control variables. The results concerning the link between democracy and the probability of having a transfer program remain robust. As the count models have limitations in using fixed effects, we use OLS regressions as baseline models.

In this paper, we use a new dataset on social transfer policies that was constructed for this research purpose. We check the validity of the results by applying the same analysis to the only available alternative data at the cross-country level—that of Barrientos, Niño-Zarazúa, and Maitrot (2010). The results hold true when using this earlier data.

6 Conclusion

Do political institutions and motives have an impact on the choice of social policies? This question is a critical one, especially for developing countries—where poverty alleviation and social security issues are at the forefront of the political agenda. Developing countries also suffer from poor governance and weak state capacity, which create biases in the formulation and implementation of social policies. In this paper, we investigate how politics influences the types of non-contributory social transfer programs that have been broadly applied in the developing world.

Using the new and unique NSTP dataset collected by Dodlova, Giolbas, and Lay (2017), we contrast unconditional pro-poor transfers directly paid to the poor with conditional transfers, which are available to the beneficiaries under special stipulations such as school attendance and/or health check-ups. Our empirical analysis thus assesses the effects of the regime type on the probability to adopt (1) any type of social transfers, (2) unconditional and/or (3) conditional transfer programs. We apply two IV strategies to test the causal effects of democracy on the choice of transfer types. We also run regressions using shares of unconditional (and conditional) programs in all transfer programs based on a sub-sample of social transfer policy adopters.

We find, first, ambiguous evidence on the relationship between the regime type and the probability of having a transfer program of any type. This ambiguity may mainly be explained by the fact that social transfer programs are being implemented out of different motives that are likely to systematically differ between regime types. Further, there may be non-linearities that our approach may not be able to detect. For example, very repressive authoritarian regimes may not see the "need" to use social transfer programs to appease their citizens.

Second, our analysis shows a robust effect of being a non-democracy on adopting unconditional transfers. It can be assumed that non-democracies introduce more unconditional transfer programs because they provide quicker short-term effects that help to maintain power and prevent civil unrest. Unconditional transfers can more easily be used and manipulated to target the insurgent poor or buy-off potential opposition groups. Third, our analysis suggests that democracies are more likely to promote conditional transfers. In particular, democracies enhance the relative importance of conditional cash transfers once countries have adopted some transfer policy. Hence, of those countries that have a social transfer program, democracies seem to invest more in human capital and thus long-term self-sustained development. At the same time, this may indicate that the effect of democracy is conditional on the capacity of the state to deliver such programs.

State capacity is likely to play an important role in explaining and conditioning the politics of social transfers. As there are multiple channels through which state capacity affects the regime type and vice versa, it may be hard to empirically pin down this role in the analysis – also given the fact that state capacity is not straightforward to operationalize empirically. We attempt to deal with this omitted variable problem by controlling for the imperfect proxies of tax revenues and other types of redistribution – without major implications for our findings.

The role of state capacity is only one aspect that – in our view – merits further research in the study of the politics of social policies. The motivations of democracies to choose more conditional transfers and of non-democracies to potentially use social transfers to suppress social unrest are interesting further areas of inquiry. Work using within-country variation is promising to shed light on the channels through which the political motives guide social policymaking, and which elements of the political structure are most important for effective and efficient poverty alleviation policies.

The interesting finding by Benhassine et al. (2015) that a cash transfer labelled as an education program yields a large improvement in school enrolment may generate a discussion that the distinction between conditional and unconditional transfers might be fuzzy and not that important from the policy perspective. However, other studies show that explicit and enforced conditions do indeed matter (Fiszbein and Schady, 2009). In particular, De Brauw and Hoddinott (2011) demonstrate that non-enforcement of a condition significantly reduces the likelihood that children attend school with this effect most pronounced when children are

transitioning to lower secondary school[8]. Whether strict conditions should be applied or whether nudging or labeling do the trick may be context specific. Also, this question is crucial in view of the fact that enforcing conditions may be expensive.

The fact that there are systematic differences between regime types regarding the likelihood of adopting social transfer programs and in the use of conditions highlights the importance of political motives in the formulation and implementation of social policies. It is therefore crucial to have an understanding of the effects of political systems and potential biases when designing and supporting social policies in developing country contexts. This paper enhances our knowledge of political distortions in social policy, even though it is hard to identify the exact political motivations behind choosing certain types of social policy in a cross-country analysis. Our findings demonstrate that social policy might be designed based on the motives not related to pro-poor ones and these motives are likely to compromise the effectiveness of transfer schemes as instruments for poverty reduction.

Our evidence should be added to the list of arguments against considering such programs as a panacea to poverty reduction. While introducing new pro-poor policy the policymakers should try to anticipate and offset distortive political effects by using the elements of the policy design such as program type, selection basis, targeting mechanism and others. All political constraints for social policy should be taken into account. For example, the international donors and funds might provide more support in favor of conditional cash transfers implying investments in education and health or limit the fungibility of aid to reduce rent-seeking.

[8] This refers to the absence of the forms needed to monitor school attendance in the case of Mexico's flagship program PROGRESA.

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Appendix A: country coverage

Albania	Haiti	Paraguay
Argentina	Honduras	Peru
Armenia	Hungary	Philippines
Azerbaijan	India	Qatar
Bahrain	Indonesia	Romania
Bangladesh	Iraq	Rwanda
Belarus	Jamaica	Senegal
Belize	Jordan	Serbia
Bolivia	Kazakhstan	Sierra Leone
Botswana	Kenya	Singapore
Brazil	Kosovo	South Africa
Brunei	Kyrgyzstan	South Sudan
Bulgaria	Lesotho	Sri Lanka
Burkina Faso	Liberia	Sudan
Cape Verde	Macedonia	Suriname
Chile	Malawi	Swaziland
China	Malaysia	Tajikistan
Colombia	Maldives	Tanzania
Congo	Mali	Thailand
Costa Rica	Mauritius	Timor-Leste
Cote d'Ivoire	Mexico	Togo
Cuba	Moldova	Trinidad and Tobago
Dominican Republic	Mongolia	Tunisia
Ecuador	Montenegro	Turkey
Egypt	Morocco	Uganda
El Salvador	Mozambique	Ukraine
Ethiopia	Myanmar	Uruguay
Fiji	Namibia	Uzbekistan
Georgia	Nepal	Venezuela
Ghana	Niger	Vietnam
Grenada	Nigeria	Yemen
Guatemala	Pakistan	Zambia
Guyana	Panama	Zimbabwe