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Do dictatorships redistribute more?

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Abstract: This paper seeks to examine the effect of the political regime on fiscal redistribution for a maximum of 144 developed and developing countries between 1960 and 2010. Using data on Gini coefficients before and after government intervention allows us to apply a measure of fiscal redistribution which reflects the effect of taxes and transfers on income inequality. We find that dictatorial regimes redistribute more than democracies through taxes and transfers. Our empirical findings remain robust across several different specifications and estimation techniques. Subsequently, we employ fiscal policy data in an attempt to enlighten this puzzling - at a first glance - empirical finding. Our results indicate that democracies and dictatorships actually follow different patterns of redistribution. Dictatorships redistribute income mostly through cash transfers, whereas democratic regimes basically rely on public good services (such as health and education) and consequently redistribute income mostly through in-kind public services. We interpret our empirical findings in the context of a simple theoretical framework that builds upon McGuire and Olson (1996).

JEL: P16, H5

Keywords: democracy, fiscal redistribution, public good provision

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1. Introduction

Governing authorities can affect the distribution of income through a wide range of policy instruments, but most directly through implemented fiscal redistribution (i.e., cash transfers to households and taxes collected from them). Since the political system is a crucial determinant for every governmental policy, a large number of theoretical and empirical studies investigate the interplay between political institutions and fiscal redistribution (see Acemoglu and Robinson, 2006; Boix, 2003; Lizzeri and Persico, 2004).

According to a strand of the theoretical literature, political institutions that concentrate political power within a narrow segment of the population (i.e., non-democratic regimes) generate less fiscal redistribution and greater inequality, while in contrast democratic regimes redistribute more and produce more egalitarian outcomes (see, e.g., Acemoglu and Robinson, 2006; Boix, 2003).[1] Another strand of the theoretical literature investigates the impact of political institutions on the allocation of government budget between public goods and cash transfers (see, e.g., Bueno de Mesquita et al., 2003; Deacon, 2009; Lizzeri and Persico, 2004; McGuire and Olson, 1996). According to these studies, democracies favor spending on public goods (such as spending on education, health etc.) and consequently redistribute income mostly through in-kind public services, whereas dictatorships favor spending on cash transfers targeted to politically influential groups.[2]

Starting from Lindert (1994) a number of empirical studies have tested the relationship between political institutions and fiscal policy outcomes. Some studies have employed historical data to investigate the effect of democratization on government spending (e.g., Aidt et al., 2006; Aidt and Jensen, 2013; Boix 2003; Lindert, 1994; 2004) and taxation (Aidt and Jensen 2009a; 2009b), whereas others rely on modern data in order to examine the relationship under

[1] The driving force behind this result is the mechanism highlighted by Romer (1975), Roberts (1977) and Meltzer and Richard (1981), according to which the lower the income of the median voter, relative to the average income, the higher the demand for fiscal redistribution. Therefore, since in democracy the voting franchise is extended to poorer segments of the population, increasing the distance between the income of the median voter and the average income, the demand for redistribution increases.

[2] The intuition behind this theoretical result goes as follows. In non-democratic regimes the political influence is more concentrated and therefore the rational leader will spend the public budget mainly on transfers targeted to politically powerful groups. Spending on a nonexclusive public good does not make sense in such a framework mostly because public good's benefits spills over to non-influential outsiders. In contrast, in democracies the electorate (and the required winning coalition) increases and therefore spending on public good appears to be much more attractive due to the economies of scale inherent in supplying a public good to a larger population (see, e.g., Bueno de Mesquita, 2003; Deacon, 2009; North et al., 2009). For an excellent review of this literature see Deacon and Saha (2006).

consideration (see, e.g., Acemoglu et al., 2015; Baum and Lake, 2001; Bueno de Mesquita et al., 2003; Mulligan et al., 2004; Profeta et al., 2013).

Interestingly, the empirical evidence concerning the relationship between the political regime and total tax revenues appears to be mixed. Specifically, Mulligan et al., (2004) and Profeta et al., (2013) fail to provide evidence in favor of a clear-cut link between political institutions and tax policy, whereas Acemoglu et al., (2015) suggest that there is a positive and robust effect of democracy on the size of total tax revenues. Moreover, according to historical studies the extension of the voting franchise that took place in the late 19th and early 20th century in Western European countries mostly affected the composition – rather than the size - of tax revenues in favor of direct taxation (see, e.g., Aidt and Jensen, 2009b; Aidt and Jensen, 2013). A clearer pattern appears in the relationship between political institutions and government spending. In particular, democracy seems to exert a positive and significant impact on those government spending accounts that could be viewed as redistributive (e.g., spending on health and education), as well as on specific education and health outcomes (see, e.g., Ansell, 2010; Baum and Lake, 2001; Bueno de Mesquita, 2003; Gallego, 2010; Lindert, 2004).[3] However, according to Mulligan et al., (2010) non-democratic regimes spend more of their GDP on social security, and redistribute more income -though payroll taxation- compared to democracies.[4]

Obviously, both the theoretical and the empirical literature conclude that linking fiscal policy choices to variations in political institutions is a highly complicated research issue. If, on top of that, the research question attempts to address the effect of political institutions on redistribution, as a result of the implemented fiscal policies, then the task becomes even more ambitious.[5] This is because political institutions influence many different aspects of the

[3] To the best of our knowledge the only study that provides evidence in favor of a positive effect of totalitarian regimes on education spending is Lott (1999). Following a similar rationale, Ross (2006) suggests that although democracies spend more money on education and health than non-democracies these benefits are mostly directed to middle -and upper- income groups.

[4] More precisely, Mulligan et al., (2010) mostly highlight the importance of economic and demographic factors on social security policies, providing only weak evidence for the effect of political institutions. However, they suggest that if there is any observed difference between democracies and non-democracies, it is that the latter spend a little more of their GDP on social security, and moreover they redistribute more -through payroll taxes- to lower income groups. Recently, Knutsen and Rasmussen (2014) conclude that single-party autocracies, which are characterized by larger winning coalitions compared to monarchies and military regimes (see e.g. Fjelde, 2010), provide specific type of social policies (such as pension programs) in order to increase their probability of survival.

[5] The empirical literature investigating the relationship between political regime and income inequality fails to provide any straightforward result. Specifically, Scheve and Stasavage (2009), Timmons (2010) and Knutsen (2015) fail to provide any relationship between democracy and economic inequality, whereas Li et al. (1998) verify a negative and significant relationship between civil liberties and income inequality. More recently, Acemoglu et al.

implemented fiscal policy (i.e., the size as well as the composition of the government budget), and moreover income inequality can be affected through many alternative fiscal policy channels. The feature that distinguishes our analysis from the rest of the literature is that our preferred measure to capture the extend of fiscal redistribution is an outcome - not a fiscal - variable that isolates the most direct fiscal policy channel through which income is redistributed; namely through taxes and cash transfers. More specifically, our main dependent variable in this study equals to the difference between Gini coefficients before taxes and transfers, which reflects the actual effect of taxes and transfers on income inequality.[6] Our preferred data are obtained from the Standardized World Income Inequality Database (SWIID) as developed by Frederick Solt (Solt, 2009).[7] It is worth noting that a similar measure of fiscal redistribution has been applied by other researchers in the past, to address though different research questions (see, e.g., Iversen and Soscise, 2006; Milanovic, 2000).

In turn, we categorize political regimes as democratic or dictatorial, based on three alternative measures developed by Boix et al. (2012), Cheibub et al. (2010) and Marshall and Jaggers (2010) (i.e., *POLITY2*), in order to examine the effect of institutions on fiscal redistribution for a maximum of 144 developed and developing countries over the period of 1960-2010. Our findings provide strong evidence that dictatorial regimes redistribute more than democracies through taxes and cash transfers. This result remains robust across several different specifications and estimation techniques. Among our robustness checks we present instrumental variables estimates that rely on the "democratization in waves" concept developed by Huntington (1993), as well as the "foreign democratic capital" theory suggested by Persson and Tabellini (2009), to account for concerns about reverse causality.

Our second contribution in the literature is that in the second part of our empirical analysis we attempt to further illuminate our findings on actual fiscal redistribution by investigating the effect of the political institutions on specific fiscal policy variables. The merit

(2015), using an extensive panel dataset of 128 countries, over the period 1960-2010 provide weak evidence of a negative relationship between democratic institutions and gross income inequality (i.e. Gini coefficient before taxes and transfers), whereas they fail to establish any kind of relationship with net income inequality (i.e. Gini coefficient after taxes and transfers).

[6] Although a large number of fiscal policy choices (e.g., spending on health, education etc.) may affect income inequality, the only two fiscal policy instruments that by definition affect the difference between gross income inequality and net income inequality are apparently the taxes and the cash transfers that are mediating between market income distribution and net income distribution.

[7] There has been much debate recently on the quality of the SWIID (see, e.g., Jenkins, 2015; Solt, 2015). We postpone the discussion of this to the next section.

of this strategy is twofold. First, we can enlighten the exact fiscal policy channel through which political regimes redistribute income. Second, and more importantly, the combination of findings in both empirical sections allows us to provide some insights for the contradicting findings of the existing literature. To this end, we investigate the impact of political institutions on fiscal revenues as well as on the allocation of government budget between public goods and cash transfers. Our analysis fails to provide evidence in favor of a relationship between political institutions and total tax revenues.[8] In contrast, other empirical findings suggest that democracy exerts a positive and significant impact on government spending on education and health, whereas non-democratic regimes rely heavier on cash transfers. These findings are in accordance with a strand of the theoretical literature which suggests that democracies and dictatorships actually follow different patterns of government spending (see, e.g., Bueno de Mesquita et al., 2003; Deacon, 2009; Lizzeri and Persico, 2004). Moreover, these results allow us to clarify the puzzling -at a first glance- empirical result of a positive and robust relationship between dictatorship and actual fiscal redistribution. Since democracies favor in-kind public services (education, health) their fiscal policies choices mostly affect gross income inequality (i.e., Gini coefficient before taxes and transfers). On the other hand, non-democracies rely more heavily on cash transfers that are expected to affect in a direct way net income inequality (i.e., Gini coefficient after taxes and transfers).[9] Therefore actual fiscal redistribution (that equals the difference between gross income inequality and net income inequality) is expected to be higher in non-democratic regimes.

[8] Our analysis also suggests that political institutions do not affect the composition of the public budget between direct and indirect tax revenues.

[9] A big issue in the relevant literature is whether non-democracies direct cash transfers to the poorer segments of the society in a manner that reduces income inequality. A large number of theoretical studies suggest that since in autocratic regimes citizens have no voting rights, transfers are directed to the politically powerful elites (that compose the so-called minimum winning coalition) increasing, therefore, income inequality. This winning coalition may consist of land owners, soldiers, workers in vital industrial sectors (see e.g. Haggard and Kaufman, 2008; Knutsen and Rasmussen, 2014) or even urban industrial workers (Ansell and Samuels, 2014). Obviously, available macro data on transfers fail to illuminate the potential targeting of transfers to specific groups of agents. However, we know that a large part of transfers consists of old-age benefits programs that are directed to the elderly, which, according to income distribution statistics, belong to the poorer segments of the population even in developed economies (see e.g. OECD, 2015). This is one example of progressive redistribution through cash transfers. Along these lines, Mulligan et al., (2010) suggest that non-democratic regimes spend more of their GDP on social security compared to democracies, whereas Knutsen and Rasmussen (2014) develop a detailed framework that investigates how formalized and stable social policies (such as old-age pension schemes) may act as a survival tool that reduces the probability of an autocratic breakdown. At the same time, it has been shown that dictators follow policies that redistribute income in favor of politically powerful elites, not through in cash payments, but mostly through market interventions and rent seeking activities (see e.g. Giuliano et al., 2010).

Motivated by this evidence, in section 4 we present a simple theoretical framework in which we interpret our empirical findings. More precisely, building upon Olson (1993; 2000) and McGuire and Olson (1996), we consider an endogenous growth model where the ruler - whether democratically elected or not- decides both the level of the tax rate and the share of the tax revenues directed to public production services. Our results are in line with those obtained by McGuire and Olson (1996). Rulers that are characterized by a lower encompassing interest in the private consumption of the citizens (and consequently in the productivity of the whole society), direct a lower share of the tax revenues to public production services. In contrast, governments that do care for the function of the private markets direct a larger amount of resources to public production services and extract less from the public funds.[10] According to Olson (1993), non-democratic regimes are usually characterized by a lower encompassing interest in the function of the private markets and consequently in the productivity of the economy.[11] Following this rationale our theoretical framework suggests that non-democratic regimes direct a lower share of the tax revenues to public production services compared to democracies.

The remainder of the paper is organized as follows: Section 2 illustrates the data and the econometric techniques employed; Section 3 discusses the empirical results. Section 4 introduces a theoretical framework that helps us to formalize the testable implications of the relevant literature. Finally, Section 5 summarizes the main points.

2. Data and Empirical Specification.

2.1 The Data

Investigating the effect of the political regime on the redistribution of income that takes place through fiscal policies appears to be an extremely ambitious and complicated research question. This is because political institutions influence many different aspects of implemented fiscal policy, and moreover income inequality is affected through many alternative fiscal policy

[10] Following the rationale of Olson (1993; 2000) and McGuire and Olson (1996) in democracies the prospective majority that is required to win the national elections earns a significant amount of its income in private markets.

[11] It must be noted that this view for democracies cannot be taken as a panacea. There are numerous historical examples of dictatorships that followed pro-market policies such as the dictator Augusto Pinochet in Chile, Chung Hee Park and Doo-Hwan Chan in South Korea, Chiang Kai-shek and his son in Taiwan and Deng Xiaoping in China. Moreover, Olson (1982) suggests that in many cases democratically elected governments (mostly in mature democratic regimes) are not characterized by a high encompassing interest in the function of the private markets due to the increased political influence of specific interest groups, lobbies and cartels. Following a similar rationale De Luca et al., (2015) show that capital-rich dictators follow policies that generate higher growth rates than the ones obtained under democracy.

channels. This study focuses on the most direct fiscal policy channel through which authorities redistribute income, namely on fiscal redistribution that takes place through taxes and cash transfers. Following Iversen and Soscise (2006) and Milanovic (2000), among others, we isolate this channel by taking the difference between gross income inequality (i.e., income inequality before taxes and transfers) and net income inequality (i.e., income inequality after taxes and transfers):

$$fiscal\ redistribution_{it} = pretax\ and\ transfers\ Gini_{it} - posttax\ and\ transfers\ Gini_{it} \quad (1)$$

Our data are obtained by the SWIID, developed by Frederick Solt (Solt, 2009). More precisely, Solt (2009) uses a systematic method, a "custom missing-data algorithm" to address the non-comparability of the various surveys (e.g., Luxembourg Income Study, Socio-Economic Database for Latin America, World Income Inequality database etc.) that underlie the data. The SWIID maximizes the comparability of available income inequality datasets for the broadest possible sample of countries and years; namely for 174 countries for as many years as possible from1960 to 2013. The fact that the SWIID is the most comprehensive attempt at developing a cross-nationally comparable database of Gini indices across time, has made its applications very frequent in recent studies (see, e.g., Ostry et al., 2014; Acemoglu et. al., 2015; Brueckner et al., 2015; De Haan and Sturm, 2015). Despite the popularity of the SWIID, there has been much debate recently on the quality of its data mostly because of potential problems raised by the aforementioned algorithm that assembles inequality statistics from different sources (see, e.g., Jenkins, 2015; Solt, 2015). For this reason, through our empirical strategy and robustness checks care is taken to ensure that reliability of our estimates.

In order to provide some simple descriptive statistics of the variable *fiscal redistribution*, we note that its mean value in our sample is 5.25 (the standard deviation is equal to 5.58), with higher values indicating a higher level of *fiscal redistribution*. Moreover, the descriptive statistics indicate that Denmark and Sweden are amongst the countries that achieve the maximum fiscal redistribution over the period examined with values that exceed 24 points,

while, in sharp contrast, Thailand present regressive fiscal redistribution that for some years exceeds -10 points.[12]

For the main explanatory variable of our study, we use the three alternative variables which allow us to distinct democratic from dictatorial regimes. First, we employ the dichotomous variable developed by Cheibub et al. (2010, henceforth *CGV*) that classifies regimes as democratic or dictatorial for 202 countries over the period 1946 to 2008. The key political factors that *CGV* takes into account in order to codify a period as democratic are: (i) popular elections of the executive and legislature, (ii) multiple parties competing in the election and (iii) unconsolidated incumbent advantage. Second, we use the dichotomous measure developed by Boix et al. (2012, henceforth *BMR*) that provides information for 219 distinct countries from 1800 to 2007. The *BMR* dichotomous measure qualifies a country as democratic if, in addition to the factors that were taken into account by *CGV*, at least half of the male electorate is enfranchised.[13] Finally, we rely on the continuous measure of political institutions *POLITY2* as obtained from the Polity IV database (Marshall and Jaggers, 2010). This index has been applied as a tool to classify political regimes (democracy versus autocracy) in number of studies (see e.g., Mulligan et al., (2004); Haber and Menaldo, 2011), though a closer look at it suggests that it mainly focuses on the institutional side of political competition (see, Vanhanen, 2000). However, it offers the advantage of varying from -10 (extreme autocracy) to +10 (perfect democracy), thus allowing for more within-country variation in the sample. For consistency with the other two measures, the *POLITY2* index is reversed and normalised to run from 0 to 1 with higher values indicating more authoritarianism.

To ensure robust econometric identification, our analysis employs a number of covariates that are expected to affect *fiscal redistribution*. In particular, we control for the level of economic development by employing the log of real GDP per capita (denoted as *GDP per capita*) obtained from the Penn World Tables. According to Wagner's law, we expect richer countries to have larger public sectors, which in turn may affect the extent of fiscal redistribution. Moreover, given that a number of studies have shown a direct effect of democratization on economic growth (see,

[12] It is worth noting that Botswana, Fiji, Philippines, Sri Lanka, Thailand and Zambia are the only other countries in our sample that present significant negative values of *fiscal redistribution*. Interestingly, these negative values do not exclusively refer to dictatorial regimes, whereas if they are excluded from the empirical analysis the results presented in section 3 remain essentially the same.

[13] Both, the *CGV* and the *BMR* datasets, are different updates and revisions of the well-established dichotomous classification of regimes introduced in Alvarez et al. (1996) and Przeworski et al. (2000).

e.g., Papaioannou and Siourounis, 2008; Acemoglu et al., 2014) controlling for *GDP per capita* reduces the potential omitted variable bias in our empirical specification. Our next control variable is the dependency ratio of the population (denoted as *age dependency*). It is measured as the percentage of the population younger than 15 years or older than 64 to the number of people of working age between 15 and 64 years. According to a number of studies, demographic factors consist a basic driving force behind the design of fiscal policy (see, e.g., Lindert, 1994; Mulligan et al., 2004; Mulligan et al., 2010). Finally, our analysis takes into account the effects of international market integration by including the ratio of imports plus exports to GDP (denoted as *openness*). It is well established in the literature that the demand for spending, especially for income transfer programmes, varies positively with the degree of globalization as a safety net against the exposure to the terms of trade risk (see, e.g., Rodrik, 1997; 1998).

It is worth noting that we have attempted to include in our model a series of other variables, such as the urbanization rate, the population size, the average years of schooling and many others. However, none of these variables had a significant effect on our dependent variable, and due to other concerns as well (correlation of control variables, reduction of sample size), we do not include them in our estimations.[14] Our unbalanced cross-country time series dataset includes observations for a maximum of 144 countries over the period of 1960-2010.[15] A complete list of all variables used in our estimations is provided in the Appendix A.

2.2 Econometric Model

To analyse the influence of political institutions on fiscal redistribution, we formulate the following empirical model:

$$Y_{it} = \alpha_1 Dictatorship_{it-1} + \beta X_{it-1} + \gamma_i + \delta_t + \varepsilon_{it} \qquad (2)$$

where Y_{it} denotes the dependent variable *fiscal redistribution*, in country *i* and year *t*. The variable $Dictatorship_{it-1}$ classifies the political regime at year *t-1*, according to the *CGV*, *BMR*, and *POLITY2* variables described above. Moreover, X_{it-1} includes the additional covariates that

[14] All these empirical findings are available upon request.

[15] Although we begin with all the countries from the World Bank's World Development Indicators, we exclude from our sample non-independent territories and very small-states (e.g., Andorra, Monaco, Puerto Rico, Timor-Leste, etc.). Subsequently, the sample size was restricted by the availability of the income inequality data.

are expected to affect *fiscal redistribution*. Finally γ_i and δ_t correspond to country and time fixed effects, respectively, and ε_{it} is the error term. In this specification year *t* represents the last observation of each 5 year sub-period (1965, 1970,.., 2010) of our sample, whereas year *t-1* the first observation of each sub-period (1960, 1966,..,2006) (see also Acemoglu et al., 2015). We follow this specification for three reasons. First, the lagged value of the variable *Dictatorship* is preferred because we expect its effect not to be contemporaneous.[16] Second, this approach allows us to mitigate concerns of reverse causality running form the explanatory variables to *fiscal redistribution*. Finally, as already mentioned, Solt (2009) employed a custom missing-data algorithm in order to standardize Gini estimates from all major existing resources of inequality data. In order to minimize reliance on problematic assumptions, Solt (2009) uses as much information as possible from proximate years within the same economy to estimate missing country-year observations. Our empirical specification that uses one observation of each 5 year sub-period of our sample aims to reduce, to the degree possible, the problems from data imputation from observations within the same country.

The model could be dynamic due to the persistence in inequality and fiscal commitments that carry over from one year to the next. To capture this persistence, previous empirical studies have applied dynamic panel specifications (see, e.g., Aidt and Jensen, 2013; Amendola et al., 2013). Following the rationale of this literature, we include a lagged dependent variable in our model estimating the following equation:

$$Y_{it} = a_1 Y_{it-1} + \alpha_2 Dictatorship_{it-1} + \beta X_{it-1} + \gamma_i + \delta_t + \varepsilon_{it} \quad (3)$$

We seek a robust method to identify the extent of fiscal redistribution between democratic and dictatorial regimes. To establish baseline results, we estimate equation (2) using the standard within estimator. This method guarantees that our estimates are not contaminated by aggregate shocks and trends common to all countries or by time invariant country-specific characteristics.

In order to estimate equation (3), we cannot rely on a dynamic Fixed Effects (FE) model, since the inclusion of a lagged dependent variable on the right hand side of the estimated equation introduces a potential bias by not satisfying the strict exogeneity assumption of the error

[16] It should be stressed that alternative empirical strategies, where the variable *Dictatorship* and/or the controls are entered contemporaneously in the specification, produce the same qualitative results to those discussed in section 3.

term ε_{it}. As shown in the literature, the estimated bias of this formulation is of order *1/T*, where *T* is the time length of the panel, even as the number of countries becomes large (see, among others, Kiviet, 1995; Nickell, 1981). The time series length of our panel is on average below 7 observations per country and, hence, the bias is not negligible. To address this issue we rely on the generalized method of moments (GMM) for dynamic panel models, as proposed by Holtz-Eakin et al. (1988) and Arellano and Bond (1991). This econometric technique removes fixed effects using either first-differencing or forward orthogonal deviations. In our case, we apply the forward orthogonal deviations as proposed by Arellano and Bover (1995) as follows:

$$\Delta Y_{it} = a_1 \Delta Y_{it-1} + \alpha_2 \Delta Dictatorship_{it-1} + \beta \Delta X_{it-1} + \Delta \delta_t + \Delta \varepsilon_{it} \quad (4)$$

This transformation method essentially subtracts the mean of future observations available in the sample from the first observations, and its main advantage is that it preserves sample size in panels with gaps. Although the model given by equation (4) solves some major econometric problems, it introduces a correlation between the new error term and the lagged dependent variable. To address this issue, Arellano and Bond (1991) suggest the use of lagged values of the explanatory variables in levels as instruments.[17] Therefore, the lagged endogenous regressor is instrumented with second and further lags of the dependent variable, whereas all the other covariates are considered as exogenous.

Although the use of lagged values of the variable *Dictatorship* in empirical specifications (2) and (4) mitigate concerns of endogeneity to some extent, it does not resolve the issue. To further dispel these concerns, in section 3.2.3 we adopt a 2SLS identification strategy. The challenge in our case is to find an external instrument that affects *fiscal redistribution* only through its effect on the political regime. Along these lines, we consider regional democratic diffusion as an attractive source of exogenous variation for the determination of the domestic political regime (see also Acemoglu et al., 2015).

[17] An alternative to the difference-GMM is the Blundell and Bond's (2000) system-GMM estimator, which maintains the differenced equation to which it adds an equation in levels with an additional set of instruments. We prefer the difference-GMM over the system-GMM estimator for two reasons. First and foremost, the additional identification assumption required by the system-GMM, namely that *fiscal redistribution* is uncorrelated with time-invariant country characteristics, is untestable and may be difficult to defend; raising instrument validity concerns (see also Acemoglu et al. (2015)). Second, related to the first point, recent research has challenged the perceived superiority of system-GMM in contexts with weak internal instruments. Bun and Windmeijer (2010) find that system-GMM may not be as robust to weak instrument bias as previously thought.

3. Results

3.1 Baseline Results

Our baseline results are reported in Table 1. In columns (1) to (3) of Table 1 we report the estimates of equation (2) where each time the variable *fiscal redistribution* is regressed on one of the three alternative measures of *Dictatorship*, as well as on the additional covariates. As can be seen, in all alternative specifications *Dictatorship* bears a positive and highly significant coefficient highlighting the positive effect of non-democratic political institutions on fiscal redistribution. It should be stressed that focusing on the within-country effects of the political regime enables us to account for time-invariant country characteristics such as geography and social norms that may affect *fiscal redistribution*. However, it is also true that our estimates rely on countries for which the type of the political regime changes over time. For this reason, in Appendix B we provide a list of political regime changes according to the *CGV* and *BMR* dichotomous variables for which within-country variation in is significantly lower in comparison to the continuous index *POLITY2*. As far as the rest of the covariates are concerned, we observe that their sign and statistical significance is consistent with our theoretical priors.

[Insert Table 1, here]

Moving one step forward, in columns (4) to (9) of Table 1 we add the lagged dependent variable into the set of controls. In columns (5), (7) and (9), we use the GMM estimator as described in equation (4), whereas in columns (4), (6) and (8) we report the Dynamic FE estimates for comparison reasons. The first thing to notice is that the lagged dependent variable enters in all regressions with a positive and statistically significant coefficient. Moreover, as expected, the coefficient of the lagged dependent variable in the GMM estimates is higher than those in the FE estimates. Regarding the main variable of interest, as can be easily verified, all three measures of *Dictatorship*, namely *CGV*, *BMR* and *POLITY2*, retain their positive and statistically significant effect on *fiscal redistribution*. As far as the rest of the covariates are concerned, our empirical findings suggest that in the presence of the lagged dependent variable, only the a*ge dependency* retains its statistically significant effect on *fiscal redistribution*. The consistency of the GMM estimator depends on the validity of the assumption of no serial

correlation in the error term (i.e., no second-order autocorrelation in the differenced idiosyncratic errors) and on the validity of the instruments. The Arellano–Bond test of second order serial correlation indicates that there is no second-order serial correlation among the differenced residuals, and the Hansen test of over-identifying restrictions suggests that our instruments are valid. Hence, although the lagged dependent variable is highly significant in all alternative specifications, illustrating that there is a considerable degree of persistence in the redistributive mechanisms, the positive relationship between the variables *Dictatorship* and *fiscal redistribution* remains unaffected in all specifications.

Concerning the magnitude of the long-run effect of the variable *Dictatorship,* according to the static specification in columns (1) to (3) of Table 1, this lies between 1.3 and 2.54 points. To obtain the long-run effect in the dynamic specification, the coefficient of the variable *Dictatorship* is divided by (1 – *fiscal redistribution*$_{t-1}$). According to the GMM estimates, the long-run effect is comparable to the static specification lying between 1.96 and 2.26 points. Given that the mean value of *absolute fiscal redistribution* in the sample is 5.25 points (with a standard deviation of 5.58), it is clear that this effect I s quantitatively sizable.

3.2 Sensitivity analysis

In this sub-section, we explore the robustness of our baseline empirical findings presented in Table 1. First, we check if our results are influenced by outlier observations. Second, we consider the possibility that our estimates are driven by the most noisy/unreliable inequality data. Third, we test if our results survive when ex-communist countries are dropped from the estimates. Fourth, we add into the set of the control variables gross income inequality. In that way, we can exclude the possibility that our results are driven by differences in market inequality between democratic and dictatorial regimes. Finally, we take an instrumental variables approach in order to mitigate further concerns for potential endogeneity and omitted variable bias in our results.

3.2.1. Testing for outliers

Our first step in the sensitivity analysis is to ensure that our findings are not influenced by outlier observations. For this reason, we re-estimate equations (2) and (4) without countries with a

standardized residual above 1.96 or below -1.96.[18] More precisely, in columns (1) to (3) of Table 2 we replicate the static FE estimates of Table 1, whereas in columns (4) to (6) we replicate the GMM estimates of Table 1. By repeating the regressions without the identified outlier observations we drop up to 34% of our sample. As can be easily verified, *Dictatorship* bears again a positive and highly significant coefficient. However, we note that the implied long-run effect of the variable *Dictatorship* on *fiscal redistribution* is much lower in comparison to Table 1. Moreover, as expected, the R-squared of the FE estimates has significantly been improved by the exclusion of the outliers. Regarding the rest of the control variables, in Table 2 our empirical findings are in line to those depicted in Table 1.

[Insert Table 2, here]

3.2.2. Income inequality estimates

As already mentioned, the SWIID takes stock simultaneously from a large number of income inequality datasets, through a multiple imputation procedure, in order to maximize comparability and data coverage of Gini indices. Despite the popularity and wide coverage of the SWIID, an intense discussion has arisen lately for its suitability on cross-country analysis of income inequality (see, Jenkins, 2015; Solt, 2015). As Jenkins (2015) argues, the aforementioned imputation procedure that assembles inequality data from different sources raises serious issues for the comparability and harmonization of Gini indices across countries and time. His critique follows in Atkinson and Brandolini's (2001, 2009) footsteps, who review the pitfalls encountered in the utilization of secondary income inequality datasets. However, Solt (2015) objects that '*the SWIID incorporates Atkinson and Brandolini's recommendations to provide the most comparable data available for those engaged in broadly cross-national research on income inequality*'.

An additional advantage of the SWIID is the provision of both gross and net Gini indices that enables the calculation of the variable *fiscal redistribution* which is at the centre of this particular research project. However, as shown by Atkinson and Brandolini (2001) and Jenkins (2015), econometric results based on inequality statistics are likely to be affected by the choice

[18] We prefer this cut-off point, instead of the standard textbook way where standardized residuals have an absolute value greater than 3 (see, e.g., Maddala, 2001), in order to ensure further the precision of our results.

of the dataset. They recommend checking the robustness of the empirical results using data from alternative sources, as well as mentioning the potential drawbacks related to the choice of a specific dataset. An alternative source in our case could have been the World Income Inequality Database (UNO-WIDER, 2014) that lately increased substantially the coverage of gross and net Gini indices, thus enabling the calculation of a similar measure of *fiscal redistribution*. However, the increased coverage does not particularly apply to developing economies, where a considerable amount of within-country variation in the type of the political regime is observed in our sample. Therefore, in our case we cannot use any alternative data source which is comparable to the SWIID in terms of coverage, quality and comparability.

To this end, our empirical strategy, as described in section 2.2, was adjusted to incorporate issues related to the custom missing algorithm employed by Solt (2009), whereas in this subsection we perform three checks in order to ensure the reliability of our estimates. First, although the SWIID maximizes the comparability of available income inequality data, incomparability remains and it is reflected in the standard errors reported for the available observations contained in the database. The provision of standard errors is an additional advantage of the SWIID, because a large part of the uncertainty of the inequality estimates can be taken into account. Therefore, in order to increase the reliability of our results, we drop from regressions 10% of the observations that the variable *redist* is associated with the higher standard errors.[19] Second, we drop from our sample all Sub-Saharan Africa countries, because we expect the accuracy of the inequality data to be thinner in comparison to the rest of our sample. Third, for the same reason, we drop from our estimates the first two decades of our sample.

[Insert Table 3, here]

As shown in panels A, B and C of Table 3, our results regarding the effect of the political regime on *fiscal redistribution* are very similar to those obtained in Table 1.

3.2.3. Dropping ex-communist countries

[19] To incorporate the uncertainty of both components of the dependent variable, we aggregate the standard error of both Gini indices to construct a standard error estimate for the variable *fiscal redistribution*. This strategy, though, has the drawback of entailing the strong assumption that the errors of the two Gini indices are independent.

Our next test is to estimate our preferred specifications after excluding the ex-communistic countries from the estimates. The purpose of this test is twofold. First, such dictatorships may appear more 'redistributive' just on account of the scale of state involvement in the economy. Hence, this group of countries, which move towards pro-market policies and less state involvement after democratisation, might be crucial for the positive impact of dictatorial regimes on *fiscal redistribution*. Second, we drop Eastern Europe and former Soviet Union countries, because the dynamics of inequality following the fall of the communist rule are probably different from other democratizations observed in our sample. Related to this point, an additional concern is that the sharp changes of Gini estimates in these countries might reflect to greater extent measurement problems of market inequality, rather than realised changes on income inequality. As can be seen in Table 4, our qualitative results are in line to those depicted in Table 1.

[Insert Table 4, here]

3.2.4. Adding gross income inequality in the set of control variables

Our next robustness check is to add in the set of the control variables gross income inequality. According to Meltzer and Richard (1981) higher levels of income inequality (i.e., larger distance between the median and the average income) lead to increased demand for fiscal redistribution. Therefore, gross income inequality is expected to exert a positive impact on fiscal redistribution. Moreover, controlling for gross income inequality our analysis seeks to isolate a large number of potential fiscal policy channels through which political institutions may affect income redistribution. As we have already discussed, democracy exerts a positive impact on specific government spending accounts (such as government spending on health and education) that are expected to affect directly gross income inequality (see, e.g., Ansell, 2010; Baum and Lake, 2001; Gallego, 2010). Therefore, by including gross income inequality in our set of controls, our analysis mitigates a large number of potential fiscal policy channels that may introduce significant noise to the obtained empirical findings.

[Insert Table 5, here]

In Table 5 we replicate the static FE estimates and the GMM estimates of Table 1. As can be seen, the empirical results regarding the variable *Dictatorship* remain qualitatively identical to those presented in Table 1. Moreover, *gross income inequality* enters with a positive and statistically significant coefficient in columns (1) to (3). This finding is in accordance with the rationale developed by Meltzer and Richard (1981), which suggests that more unequal countries are expected to redistribute more. However, this result becomes statistically insignificant in columns (4) to (6) when the lagged dependent variable enters in the specification with a positive and statistically significant coefficient.[20] Regarding the rest of the controls variables, once again *age dependency* bears a positive and statistically coefficient in all specifications highlighting the robust effect of demographic factors on fiscal redistribution.

3.2.5 The 2SLS identification strategy

The empirical strategy with the lagged dependent variable on the right hand side of the estimated equation, in addition to the full set of country and time fixed effects, rules out certain types of contaminating factors for our results. However, one could still argue that our results can be affected by potential reverse causality running from fiscal redistribution to the political regime, by the measurement error of the alternative regime-type variables that we use in our empirical analysis as well as potential omitted variable bias. To deal with these concerns, in this sub-section we follow a 2SLS identification strategy.

The challenge in our case is to find an instrument that is adequately correlated with the regime within the country, while it remains uncorrelated with the unobserved time-varying component that affects fiscal redistribution. In other words, we need a variable that affects fiscal redistribution only through its effect on the regime within the country. Following the "democratization in waves" concept developed by Huntington (1993), as well as the "foreign democratic capital" theory suggested by Persson and Tabellini (2009), we conclude that regional democratic diffusion appears to be an attractive source of exogenous variation in the determination of the domestic regime. To this end, we apply the inverse distance weighting formula in order to develop the variable *Democracy abroad* for country *i* in year *t* as follows:

[20] An additional check is to interact income inequality with the variable *Dictatorship*, in order to check whether the result is particularly driven by very unequal dictatorships. However, our estimates- available upon request- indicate that the interaction term appears to be negative and statistically insignificant.

$$Z_{it} = \frac{\sum_{j \neq i} W_{ij} D_{jt}}{\sum_{j \neq i} W_{ij}} \quad (5)$$

where D_{jt} classifies the political regime in country j (different from i) according to the *CGV*, *BMR*, and *POLITY2* variables described above. Moreover, W_{ij} is the inverse distance in kilometres between the capitals of country i and j. Therefore, our instrument Z_{it} takes values between 0 and 1, with higher values indicating that a country has more democratic countries in the geographic neighbourhood. More than that, a wave of democratization that takes place within a geographic neighbourhood is expected to increase the value of Z_{it} through time. It is worth that Acemoglu et al. (2015) have applied a similar instrument in their study to tackle the aforementioned econometric issues, whereas Ansell (2010) and Aidt and Jensen (2013), as in our case, add the lagged value of the instrumented variable in the vector of instruments.

In Table 6 we re-estimate our basic specification as described in equation (2), with and without the inclusion of the variable *gross income inequality*. More precisely, in columns (1) to (3) of Table 6 we re-estimate our basic specification presented in columns (1) to (3) of Table 1, whereas in columns (4) to (6) we add in the set of the control variables *gross income inequality*. We abstain from employing a dynamic specification, since the inclusion of a lagged dependent variable on the right hand side of the equation introduces a potential bias in our estimates (see, e.g., Nickell, 1981). The first-stage results are reported in the lower part of the Table 6.

As can be easily verified, the positive effect of the variable *Dictatorship* continues to hold in all specifications. Moreover, the coefficient of the variable *Democracy Abroad* bears the expected negative sign and it is statistically significant in 4 out of 6 regressions. The consistency of the 2SLS model requires that the instruments are strong enough and valid to predict the endogenous variable *Dictatorship*. For this reason, first we refer to the first stage F-statistics of the excluded instruments. According to Staiger and Stock (1997), the first stage F-statistic should be at least 10 for weak identification not to be a problem. As can be seen, the first-stage F statistics in Table 6 are high enough to guard against the problem of weak instruments. Second, since the number of excluded instruments exceeds the number of endogenous variables, a Hansen test statistic can be calculated to test the validity of the overidentifying restrictions. The null hypothesis is that the instruments are valid and thus uncorrelated with the error term. In all

cases the overidentification test does not reject the null hypothesis, giving some confidence in the overall set of instruments.

[Insert Table 6, here]

The results reported in Table 6 verify once again the positive effect of the variable *Dictatorship* on *fiscal redistribution*. However, one could still argue that the exclusion restriction can be violated if the regime type abroad relates to redistribution abroad, and the latter has direct spillover effects on the dependent variable. To exclude this possibility, we control in the 2SLS models for *Redistribution abroad*. Even in that case our results –available upon request- are similar to those obtained in Table 6. It is worth noting that the 2SLS coefficients of the variable *Dictatorship* are higher than those obtained with the within estimator. We interpret the larger coefficients in the 2SLS estimates as a possible measurement error problem in the right hand side endogenous variable, which leads to an attenuation bias in the OLS estimates (see Angrist and Krueger, 1999).

3.3 Fiscal Policy Channels

Having established a positive and robust relationship between dictatorial regimes and actual fiscal redistribution, in this section our analysis seeks to investigate the impact of political institutions on fiscal revenues as well as on the allocation of government budget between public goods and cash transfers. This allows us to place the spotlight on the potential fiscal policy channels though which redistribution takes place in different political regimes, and therefore to further clarify the puzzling -at a first glance- empirical findings presented in the previous sections. To this end, our analysis relies on three alternative databases (described below in detail), and employs as dependent variables a number of fiscal policy measures that reflect the level as well as the composition of fiscal policy.

First, our analysis employs data from the ICTD Government Revenue Dataset (ICTD). ICTD covers 188 countries over the period 1980-2013 and it has been compiled by sources like the IMF Government Finance Statistics (GFS) and the IMF Article IV Reports. This is a new and high quality source for internationally comparable disaggregated tax data that draws both on central and general government data as appropriate in order to provide the most accurate possible

picture of national revenue collection (see, Prichard et al., 2014). Most researchers dealing with developing countries have historically focused on central government data only, in order to maximize data coverage. The contribution of this dataset is that it provides data at the general government level - when available - which allows researchers to avoid the underestimation of revenue collection in federal states. Second, we employ data from the Economic Freedom of the World project (EFW) that reports measures for the size of the general government every five years since 1970, and annually since 2000, until 2012 for a maximum of 153 countries.

These two databases use as one of their primary sources the GFS for fiscal data before and after 1990. Therefore, both face the same issue of comparability of data before and after this period. Although for data until 2000 financial information was calculated according to the Government Finance Statistics Manual 1986 (GFSM 1986) classification, since then the Government Finance Statistics Manual 2001 (GFSM 2001) framework has been used. The new classification has been applied retrospectively to data from 1990 onwards. However, it is difficult to bridge the two frameworks since fiscal variables are measured on a 'cash' basis in the GFSM 1986 and on an 'accrual' basis in the GFSM 2001 classification.[21] For this reason we also employ data from the Global Development Network Growth Database (GDNGD), which is a reliable source for disaggregated fiscal revenue and expenditure data for 123 countries over the period 1972-2000. Its primary source is GFS and it covers consolidated central government accounts based entirely in the GFSM1986 classification.

Concerning the fiscal revenues side, we employ in our analysis measures which allow us to capture the size of the tax system. More precisely, we obtain from the ICTD the variables total revenues and total tax revenues (denoted as *revenues_ICTD* and *tax revenues_ICTD*, respectively), both scaled by GDP and expressed as percentages.[22] We obtain the respective variables from the GDNGD, which are denoted as *revenues_GDNGD* and *tax_revenues_GDNGD*. According to the standard Meltzer and Richard (1981) argument, we expect all these alternative variables to be negatively affected by non-democratic institutions. This is because in democracy the voting rights are extended to poorer segments of the population, which in turn increase the distance between the income of the median voter and the

[21] For more details see: www.imf.org/external/pubs/ft/gfs/manual/pdf/class.pdf

[22] An additional advantage of the ICTD is that it flags the observations that are not credible for international comparisons. Using this information we exclude from the analysis observations for which the variables *prob1*, *prob2* and *prob3* take the value of 1. For details see pp. 30-32 in Prichard et al. (2014).

average income and thus the demand for fiscal redistribution (see, e.g., Boix, 2003; Acemoglu and Robinson, 2006).[23]

Concerning the expenditures side, we use the following four variables. First, the fiscal variable social security and welfare affairs and services as a share of GDP (denoted as *social _services_GDNGD*) obtained from the GDNGD. This measure includes central government's payments, both in cash and in kind, which intend to compensate for reduction or loss of income or inadequate earning capacity.[24] Second, we employ the variable subsidies and transfers as a share of GDP (denoted as *services_subsidies_EFW*) obtained from the EFW database, which includes subsidies and social benefits in cash and in kind of the general government. Third, we construct a similar variable from the GDNGD by summing the variables *subsidies* and *transfers to households and nonprofit institutions* (denoted as *transfers_subsidies_GDNGD*). This variable concerns the central government and it has the advantage of including only in cash payments.[25] Finally, we sum health and education expenditures as a share of GDP from the GDNGD in order to construct the variable *health_education_GDNGD*. According to a strand of the relevant theoretical literature democracies favor spending on public goods services (such as on health and education) and consequently redistribute income mostly through in-kind public services. In contrast, dictatorships rely heavier on cash transfers (see, e.g., Bueno de Mesquita et al., 2003; Deacon, 2009; Lizzeri and Persico, 2004).

In the analysis that follows we modify the estimated equation (2) of section 3.2 as follows:

$$Y_{it,t+4} = \alpha_1 Dictatorship_{it} + \beta X_{it,t+4} + \gamma_i + \delta_t + \varepsilon_{it} \qquad (6)$$

where $Y_{it,t+4}$ represents a fiscal variable in country *i* over a five year period. The variable $Dictatorship_{it}$ classifies the political regime at year *t*, according to the *CGV*, *BMR*, and *POLITY2* variables described above. Moreover, $X_{it,t+4}$ is the vector of socio-economic variables,

[23] However, the empirical evidence is mixed. Specifically, Mulligan et al., (2004) and Profeta et al., (2013) fail to provide evidence in favor of any link between political institutions and tax policy, whereas Acemoglu et al., (2015) suggest that there is a positive and robust relationship between democracy and total tax revenues.

[24] For more information regarding the expenditure categories that compose the fiscal variable social security and welfare affairs and services see page 46 in the following link:
https://www.imf.org/external/pubs/ft/gfs/manual/1986/eng/pdf/ch4a.pdf

[25] Unfortunately, we cannot exclude the transfers to non-profit institutions from the calculation because the database does not provide a separate classification for transfers to households and transfers to non-profit institutions.

as described in section 3.1, in country *i* over a five year period. Finally, γ_i and δ_t correspond to country and time fixed effects, respectively, and ε_{it} is the error term.

We prefer the specification of equation (6) in this section for two reasons. First and foremost, as mentioned in section 2.2, one of the reasons that we chose to take one observation of our dependent variable for each 5-year sub-period of our sample is the custom missing algorithm employed by Solt (2009), which uses as much information as possible from proximate years within the same economy to estimate missing observations. Therefore, the strategy adopted in section 3 allows us to reduce, to the degree possible, the problems from data imputation from observations within the same country. Given that we do not face this issue with fiscal data, we resort to non-overlapping 5-year averages so as to smooth over some of the cyclical features of the data (see, e.g., Kneller et al., 1999). Second, given that fiscal data have missing observations and gaps, taking five-year averages, instead of one observation for each 5-year period, allows us to maximize the available number of observations. It is worth mentioning that we do not estimate a dynamic specification in this section, because introducing a lagged dependent variable either does not affect our results or it reduces our sample so significantly that makes its use irrelevant. Moreover, given that EFW database provides one observation every five years until 2000, in specifications that we employ the variable *transfers_EFW*, we use the first observation of each 5-year sub-period of our sample in both sides of the estimated equation.

The results for the revenue variables are presented in Table 7, whereas those for the expenditure variables in Table 8. As can be seen in Table 7, *Dictatorship* bears a non-significant coefficient in all alternative specifications. Therefore, based on the results presented in columns (1)-(12), our analysis fails to provide evidence that political institutions influence the level of fiscal revenues. These empirical findings are in line with previous studies suggesting that political institutions do not play an important role in the design of tax policy (see, e.g., Mulligan et al., 2004; Profeta et al., 2013; Scheve and Stasavage, 2012). Related to these results, it is worth mentioning that for brevity we do not report estimates that concern the composition of tax revenues. However, in specifications that we use direct and indirect tax revenues as dependent variables, once again, the variable *Dictatorship* is statistically insignificant.[26] Regarding the rest of the covariates, as expected, the variable *GDP per capita* is positive and significantly related to

[26] Results are available upon request.

total revenues and the variable *tax_revenues_GDNGD*. The variable *openness* is statistically insignificant in all specifications, while the variable *age_dependency* enters with a negative and significant coefficient in some of the empirical specifications.

[Insert Table 7, here]

In Table 8 our analysis investigates whether political institutions influence the allocation of government budget between specific type of public goods and cash transfers. As can be easily verified in columns (1)-(6), the variable *Dictatorship* enters with a non-significant coefficient in all alternative specifications. Therefore our analysis fails to provide any clear cut relationship between political regime and social spending accounts that include both in cash and in kind transfers (i.e., *social_services_GDNGD* and *services_subsidies_EFW*).

In contrast, the variable *Dictatorship* is positive and statistically significant in columns (8) and (9), where the dependent variable *transfers_GDNGD* includes only in cash transfers to the population. A potential issue with these estimates is that the number of observations drops significantly making more difficult to identify correctly the effect of the political regime in FE estimations. However, it should be noted that even in yearly panels, with and without the inclusion of a lagged dependent variable, the qualitative effect of the political regime on *transfers_GDNGD* remains unaffected. Moreover, according to the results in columns (10)-(12) dictatorial regimes are negatively related to health and education spending. Therefore, our empirical findings suggest that democracies and dictatorships actually follow different patterns of redistribution through the implemented fiscal policy. More precisely, dictatorial regimes redistribute income mostly through cash transfers, whereas democratic regimes basically rely on public good services (such as health and education) and consequently redistribute income mostly through in-kind public services. To the best of our knowledge the only other study that has provided similar evidence regarding the effect of the political regime on the composition of public spending is by Kaufman and Segura-Ubiergo (2001) for a sample of Latin American countries over the period 1973-1997. Regarding the negative effect of *Dictatorship* on health and education spending, our results are in line with many previous empirical studies that have provided similar evidence for these specific spending accounts (see, e.g., Ansell, 2010; Baum

and Lake, 2001; Bueno de Mesquita, 2003; Gallego, 2010; Lindert, 2004). Finally, our control variables do not seem to depict any robust relationship with any of the fiscal variables in Table 8.

[Insert Table 8, here]

Summarizing, our empirical findings presented in Tables 7 and 8 suggest that political institutions do not exert any impact on fiscal revenues or their composition, but they do influence the allocation of government spending between public goods and cash transfers. These empirical findings help us to further illuminate the puzzling –at a first glance-empirical results presented in Tables 1 to 6. Democratic regimes rely heavier on in-kind public services (education, health) and their policies basically affect gross income inequality (i.e., Gini coefficient before taxes and transfers), whereas dictatorships redistribute income mostly through cash transfers. As a result, actual fiscal redistribution that takes place through cash transfers and taxes is expected to increase in non-democratic regimes.

4. A theoretical framework along the lines of McGuire and Olson (1996)

Motivated by the empirical evidence presented above, this section investigates theoretically why political regimes follow different patterns of fiscal policy. To this end, our analysis presents a simple theoretical model that builds upon Olson (1993; 2000) and McGuire and Olson (1996), which highlights the encompassing interest of the ruler for the productivity of the whole economy as a crucial factor. More precisely, we consider an endogenous growth model where the ruler (whether democratically elected or not) decides both the level of the tax rate on income and the amount of tax revenues directed to public production services. Tax revenues that are not directed to public production services remain in the discretion of the ruler and they are used for his own purposes. Thus, these resources are affecting the welfare of the ruler either directly (by increasing his own consumption) or indirectly (by increasing his ability to "buy" political support). In any case these resources are directed away from productive activities.

4.1 Households

The intertemporal utility of the representative household is:

$$U = \sum_{t=0}^{\infty} \beta^t (\log c_t) \tag{7}$$

where c_t is the private consumption at time t, and $0 < \beta < 1$ is the discount rate.

At each time t, the household rents its predetermined capital, k_t, to the firm and receives $r_t k_t$, where r_t is the return to capital. It also supplies inelastically one unit of labor services per time-period so that labor income is w_t. Further, it receives profits made by firms, π_t. Thus, the household's budget constraint is:

$$k_{t+1} + c_t = (1-\theta_t)(r_t k_t + w_t + \pi_t) \tag{8}$$

where k_{t+1} is the end-of-period capital stock and $0 < \theta_t < 1$ is the income tax rate. For simplicity, we assume full capital depreciation. The initial capital stock, k_0, is given.

The household chooses the paths of c_t and k_{t+1} to maximize (7) subject to (8). In doing so, it acts competitively by taking prices, profits and policy variables as given. The first-order conditions of the household's problem are:

$$\frac{1}{c_t} = \beta \left[\frac{(1-\theta_{t+1}) r_{t+1}}{c_{t+1}} \right] \tag{9}$$

and the budget constraint in (8).

4.2 Firms

The representative firm maximizes the usual profit, π_t, function:

$$\pi_t \equiv y_t - r_t k_t - w_t l_t \tag{10}$$

As in the literature introduced by Barro (1990), we assume that public services provide production externalities to private firms. We also assume that technology at the firm's level takes a Cobb-Douglas form. Thus, the firm's production function is:

$$y_t = Ak_t^{\alpha} l_t^{1-\alpha} G_t^{1-\alpha} \tag{11}$$

where y_t is output at t, l_t is the labor input at t, g_t is public production services at t, $A>0$ and $0<\alpha<1$.

The firm chooses k_t and l_t. In doing so, it acts competitively by taking prices and policy variables as given. The first-order conditions of the firm's problem are:

$$r_t = \frac{\alpha y_t}{k_t} \tag{12a}$$

$$w_t = \frac{(1-\alpha) y_t}{l_t} \tag{12b}$$

4.3 Government budget constraint

To finance the public good the ruler taxes the household's income at a rate $0<\theta_t<1$. Thus,

$$R_t + G_t = \theta_t \left(r_t k_t + w_t + \pi_t \right) \tag{13a}$$

Without loss of generality, we assume that a share $0<b_t<1$ of total tax revenues finances public production services, G_t, and the rest $0<(1-b_t)<1$ is used by the ruler for his own purposes. Thus, these resources are used by the ruler either to finance his own consumption or to finance other non-productive activities (e.g., to "buy" political support). Thus, (13a) is decomposed into:

$$G_t = b_t \theta_t \left(r_t k_t + w_t + \pi_t \right) \tag{13b}$$

$$R_t = \left(1-b_t\right) \theta_t \left(r_t k_t + w_t + \pi_t \right) \tag{13c}$$

where inspection of (13a)-(13c) reveals that θ_t and b_t can summarize fiscal policy at t.

4.4 Competitive decentralized equilibrium (for given economic policy)

Given the paths of the policy instruments $\{\theta_t, b_t\}_{t=0}^{\infty}$, a competitive decentralized equilibrium (CDE) is defined to be a sequence of allocations $\{y_t, c_t, k_{t+1}, G_t, R_t\}_{t=0}^{\infty}$ and prices $\{r_t,\ w_t\}_{t=0}^{\infty}$ such that: (i) households maximize utility and firms maximize profits by taking prices, policy and public services as given; (ii) all budget constraints are satisfied; (iii) all markets clear.[27] This CDE is summarized by the following equations that give the paths of output, private consumption, private capital accumulation:

$$y_t = A^{\frac{1}{\alpha}} (b_t\theta_t)^{\frac{1-\alpha}{\alpha}} k_t \tag{14a}$$

$$c_t = (1-\alpha\beta)A^{\frac{1}{\alpha}}(1-\theta_t)(b_t\theta_t)^{\frac{1-\alpha}{\alpha}} k_t \tag{14b}$$

$$k_{t+1} = \alpha\beta A^{\frac{1}{\alpha}}(1-\theta_t)(b_t\theta_t)^{\frac{1-\alpha}{\alpha}} k_t \tag{14c}$$

$$G_t = b_t\theta_t A^{\frac{1}{\alpha}} (b_t\theta_t)^{\frac{1-\alpha}{\alpha}} k_t \tag{14d}$$

$$R_t = (1-b_t)\theta_t A^{\frac{1}{\alpha}} (b_t\theta_t)^{\frac{1-\alpha}{\alpha}} k_t \tag{14e}$$

In this solution, y_t, c_t, k_{t+1}, G_t and R_t depend on the beginning-of-period capital stock and the current value of the policy instruments only.[28]

4.5 Optimal Fiscal Policy

We now endogenize policy by assuming that the ruler chooses the paths of θ_t and b_t in order to maximize his own well-being (we specify ruler's objective function in Equation (9) below). In doing so the ruler takes into account the CDE as summarized by (14a)-(14e).

4.5.1 The ruler's problem

[27] In the labor market, the market-clearing condition is $l_t = 1$.

[28] As is known, the model specification (logarithmic preferences and Cobb-Douglas constraints with full depreciation) allows us to obtain a closed-form solution at the level of competitive decentralized equilibrium (CDE). In this equilibrium, private consumption-saving decisions are proportional to current output, and the degree of proportionality depends on the current policy instruments only.

Following McGuire and Olson (1996) we assume that the ruler (whether democratically elected or not) maximizes the following intertemporal objective function:

$$W = \sum_{t=0}^{\infty} \beta^t \left(F \log c_t + (1-F) \log R_t \right) \tag{15}$$

where $0 < \beta < 1$ is the discount rate of the ruler and $0 < F < 1$ is a parameter that captures the degree of the encompassing interest of the ruler in private consumption of the citizens and consequently in the productivity of the whole society.[29] Obviously, the second term of the objective function captures the incentive of the ruler to extract the maximum amount of resources from the public funds and to use it for his own purposes. As can be easily verified when parameter *F* tends to zero the ruler gains utility solely through rent extraction.[30] In contrast, when *F* is larger than zero, the ruler also cares for the welfare of their citizens (who earn a significant amount of their income in private markets) and this is the case of the "redistributive democracy" as defined by McGuire and Olson (1996).[31]

We will use dynamic programming to solve the ruler's problem. From the governor's point of view, the state at any time *t* is the predetermined economy-wide capital stock, k_t. Then $V(k_t)$ denote the value function at *t*. This function must satisfy the Bellman equation:

$$V(k_t) = \max_{\theta_t, b_t} [F \log c_t + (1-F) \log R_t + \beta V(k_{t+1})] \tag{16}$$

where c_t, k_{t+1} and R_t follows (14b), (14c) and (14e) respectively.

Inspection of the above problem reveals that the value function in (16) is expected to be of the log-linear form $V(k_t)=u_0+u_1 \log k_t$ where u_0 and u_1 are undetermined coefficients. Using this

[29] Olson (1993) suggests that in democratic regimes candidates need a majority to win and they might be able to "buy" a majority by transferring income from the population to this prospective majority. However, the competition for vote buying will not generate that large distortion of incentives through taxation as in autocracies. This is because in democracies the majority earns a significant share of the market income of the society and this gives to the democratically elected government a more encompassing interest the function of the private markets and consequently in the productivity of the economy.

[30] According to McGuire and Olson (1996) this is the case of a "pure autocracy".

[31] Though essentially ad hoc, this characterization of policy-makers' preferences is a convenient way of encompassing a wide range of possibilities by supposing that policy makers are neither wholly benevolent nor wholly self-serving Leviathan (see, e.g., Edwards and Keen, 1996 for more details on this).

conjecture for the value function into (16), the first order conditions for θ_t and b_t are respectively:[32]

$$\theta_t = 1 - a\beta - a(1-\beta)F \tag{17a}$$

$$b_t = \frac{1-a}{1-a\beta-a(1-\beta)F} \tag{17b}$$

As can be easily verified, the chosen policy instruments are independent of the state of the economy k_t and they are constant over time $\theta_t=\theta$ and $b_t=b$ for all t. Moreover, we note that $\frac{\partial \theta_t}{\partial F} < 0$ and $\frac{\partial b_t}{\partial F} > 0$. Thus a higher encompassing interest of the ruler in private consumption and consequently in the productivity of the private markets leads: (i) to lower level of tax rates and (ii) to higher share of tax revenues used to finance public production services relative to rents' extraction. It is worth noted that higher tax rates do not necessarily induce higher tax revenues. This is because in this model national income (i.e., the tax base) apparently is endogenous to the implemented fiscal policy.

Our results are in line to those obtained by McGuire and Olson (1996). Rulers that are characterized by a lower encompassing interest in the welfare of the citizens -and consequently in the productivity of the whole society- direct a lower share of the tax revenues to public production services and they impose higher tax rates. In contrast, governments that do care for the function of the private markets direct a larger amount of resources to public production services and extract less from the public funds. Following the rationale of Olson (1993) and McGuire and Olson (1996) autocracies are characterized by a lower encompassing interest in the function of the private markets. Therefore, autocracies direct a lower share of the tax revenues to public production services and extract more from the public funds for political economy purposes.

[32] Using the conjecture $V(k_t)=u_0+u_1\log k_t$ into (16) and equating coefficients on both sides of the Bellman, we get $u_1=1/(1-\beta)>0$. Plugging this into the first order conditions for θ_t and b_t we obtain (17a) and (17b). This also confirms the conjecture for the value function in (16).

5. Conclusions

Our analysis examines the relationship between political institutions and fiscal redistribution for a maximum of 144 developed and developing countries between 1960 and 2010. Backed by strong empirical findings, obtained from several different specifications and robustness checks, we suggest that dictatorial regimes redistribute more than democracies through taxes and cash transfers. Subsequently, our analysis provides some insights about this empirical finding. Focusing on the potential fiscal policy channels though which redistribution takes place, we conclude that democracies and dictatorships follow different patterns to redistribute income. More precisely, dictatorships redistribute income mostly through cash transfers, whereas democratic regimes rely more on public good services (e.g. health, education).

To the best of our knowledge, this is the first study that employs a measure of actual fiscal redistribution, whereas at the same time attempts to enlighten the fiscal policy channels through which political regimes redistribute income. In this sense, our findings contribute to the well-established agenda studying the interplay between political institutions and fiscal redistribution (see Acemoglu and Robinson, 2006; Acemoglu et al., 2015; Aidt and Jensen, 2013; Boix, 2003). However, since investigating the influence of the political regime on income redistribution that takes place through fiscal policies is a highly complicated and ambitious research agenda, these empirical findings call for a deeper understanding of the specific inter- and intra-country mechanisms that create these patterns and this is an issue that definitely warrants future research.

Appendix A: Definitions, data sources and descriptive statistics

Variable	Description	Obs.	Mean	SD	Min	Max	Source
fiscal redistribution	Difference of Gini coefficients before and after the fiscal redistribution (i.e., before and after transfers and taxes)	849	5.255	5.584	-10.263	34.714	Solt (2009), Standardized World Income Inequality Database (SWIID).
gross income inequality	Gini coefficient before and after transfers and taxes	849	43.838	8.747	22.619	77.463	SWIID
Dictatorship (CGV)	Dummy variable that equals to one whenever a political regime is characterized as dictatorial and 0 otherwise	1227	0.523	0.500	0	1	Cheibub et al. (2010)
Dictatorship (BMR)	Dummy variable that equals to one whenever a political regime is characterized as dictatorial and 0 otherwise	1222	0.521	0.500	0	1	Boix et al. (2013)
Dictatorship (POLITY2)	Index variable that ranges from 0 to 1, with higher values indicating a more authoritarianism.	1167	0.421	0.366	0	1	Marshall and Jaggers (2010)
Democracy abroad (CGV)	Measure of democratic diffusion from abroad as defined in section 3.2.3	1233	0.431	0.178	0.043	0.872	Cheibub et al. (2010)
Democracy abroad (BMR)	Measure of democratic diffusion from abroad as defined in section 3.2.3	1233	0.431	0.167	0.049	0.870	Boix et al. (2013)
Democracy abroad (POLITY2)	Measure of democratic diffusion from abroad as defined in section 3.2.3	1233	0.525	0.145	0.113	0.846	Marshall and Jaggers (2010)
total_revenues_ICTD	Total revenues as a share of GDP (%)	806	22.714	11.104	1.015	89.077	ICTD Government Revenue Dataset (ICTD)
tax_revenues_ICTD	Total tax revenues as a share of GDP (%)	832	16.064	8.459	0.486	47.098	ICTD
total_revenues_GDNGD	Total revenues as a share of GDP (%)	500	25.770	10.842	1.785	77.397	Global Development Network Growth Database (GDNGD)
tax_revenues_GDNGD	Total tax revenues as a share of GDP (%)	501	20.773	9.611	0.832	47.325	Global Development Network Growth Database (GDNGD)
social_services_GDNGD	Social security and welfare affairs and services of the central government both in cash and in kind as a share of GDP (%)	459	10.732	9.170	0	40.292	Global Development Network Growth Database (GDNGD)
services_subsides_EFW	Subsidies and social benefits of the general government both in cash and in kind as a share of GDP (%)	835	9.051	8.174	0	37.200	Economic Freedom of the World (EFW)
transfers_subsides_GDNGD	Subsidies and transfers payments in cash to households and nonprofit institutions of the central government as a share of GDP (%)	231	9.524	7.938	0	30.427	Global Development Network Growth Database (GDNGD)
health_education_GDNGD	Health and education expenditures of the central government as a share of GDP (%)	435	5.6132	2.865	0.357	16.651	Global Development Network Growth Database (GDNGD)
GDP per capita	Log of GDP per capita	1303	8.308	1.213	5.371	10.946	Penn World tables 8.0 (PWT)
age dependency	Share of the population younger than 15 years or older than 64 to the number of people of working age (%)	1435	72.661	19.397	36.409	119.008	World Banks' World Development Indicators (WDI)
openness	International trade volume as a share of GDP (%)	1189	71.200	45.904	5.992	400.200	World Banks' World Development Indicators (WDI)

Appendix B: Regime changes

Data source:	Cheibub et al. (2010)		Boix et al. (2012)	
Status:	**Democratisation**	**Reversals to Dictatorship**	**Democratisation**	**Reversals to Dictatorship**
Albania	1991		1997	1996
Argentina	1973, 1983	1976	1963, 1973, 1983	1976
Bangladesh	1986	2007	1986	2007
Bhutan	2007			
Brazil	1985		1979	
Bulgaria	1990		1990	
Burundi	1993, 2005	1996	2005	
Belarus			1994	
Central African Republic	1993	2003	1993	2003
Sri Lanka	1989		1991	
Chile	1990		1990	
Croatia			2000	
Comoros	2004			
Ecuador	2002	2000	2003	2000
El Salvador	1984		1984	
Fiji	1992	2000		1987
Georgia	2004		2004	
Gambia				1994
Ghana	1993		1997	
Greece	1974		1974	
Guatemala	1986	1982	1986	1982
Indonesia	1999		1999	
Kenya	1998		2002	
Korea	1988		1988	
Lesotho			2002	
Kyrgyz Republic	2005			
Liberia	2006		2006	
Madagascar	1993		1993	
Malawi	1994		1994	
Mali	1992		1992	
Mauritania	2007	2008		
Mexico	2000		2000	
Mozambique				2004
Nepal	1990, 2008	2002	1991	2002
Niger	1993, 2000	1996	1993, 1999	1996
Nigeria	1999	1983		1983
Pakistan	1972, 1988, 2008	1999	1972, 1988	1999
Panama	1989		1991	
Paraguay			2003	

Peru	2001	1962, 1990	2001	1962, 1990
Philippines	1986	1965	1986	1965
Portugal	1976		1976	
Guinea-Bissau	2000, 2004	2003	1994	1998
Romania	1990		1991	
Russia				1999
Senegal	2000		2000	
Sierra Leone	1996, 1998	1967, 1997	2002	1967
South Africa			1994	
Thailand	1975, 1979, 1992, 2008	1976, 1991, 2006	1975, 1983, 1992	1976, 1991, 2006
Turkey	1983	1980	1983	1980
Uruguay	1985		1985	
Venezuela				2005

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Table 1. Political regime and fiscal redistribution: Baseline Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	FE	FE	FE	FE	GMM	FE	GMM	FE	GMM
Political variable:	**CGV**	**BMR**	**POLITY2**	**CGV**	**CGV**	**BMR**	**BMR**	**POLITY2**	**POLITY2**
Dictatorship	1.304***	1.608***	2.538***	1.019***	0.959***	1.087***	1.016***	1.225**	1.126**
	(0.496)	(0.533)	(0.867)	(0.343)	(0.338)	(0.352)	(0.348)	(0.487)	(0.542)
***fiscal redistribution*$_{t-1}$**				0.464***	0.510***	0.461***	0.507***	0.474***	0.502***
				(0.070)	(0.078)	(0.070)	(0.081)	(0.069)	(0.086)
GDP per capita	1.431***	1.532***	1.393***	-0.001	-0.065	0.048	-0.026	-0.130	-0.171
	(0.383)	(0.384)	(0.375)	(0.370)	(0.378)	(0.374)	(0.388)	(0.369)	(0.388)
age dependency	0.110***	0.109***	0.109***	0.074***	0.068***	0.074***	0.068***	0.070***	0.067***
	(0.024)	(0.024)	(0.024)	(0.016)	(0.014)	(0.016)	(0.014)	(0.015)	(0.014)
openness	0.011*	0.010*	0.009	-0.001	-0.001	-0.001	-0.001	-0.002	-0.002
	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)	(0.005)	(0.006)	(0.006)
R2	0.095	0.104	0.105	0.351		0.353		0.364	
Observations	761	758	739	665	530	662	528	649	520
Number of countries	144	143	136	135	126	134	125	129	121
Number of instruments					49		49		49
Hansen (p-value)					0.458		0.480		0.291
AR(2) (p-value)					0.305		0.290		0.328

Notes: In all specifications we control for a full set of country and year fixed effects. In the GMM the variable *fiscal redistribution*$_{t-1}$ is instrumented with second and further lags, whereas all other covariates are treated as exogenous. The Hansen statistic is a test of overidentifying restrictions, under the null that overidentifying restrictions are valid. The AR(2) is a test for second-order serial correlation in the differenced residuals, under the null of no serial correlation. Robust standard errors, clustered by country are reported in parentheses. *** denotes significance at 1% level, ** denotes significance at 5% level and * denotes significance at 10% level.

Table 2. Regime and fiscal redistribution: Testing for outliers

	(1)	(2)	(3)	(4)	(5)	(6)
	FE	FE	FE	GMM	GMM	GMM
Political variable:	**CGV**	**BMR**	**POLITY2**	**CGV**	**BMR**	**POLITY2**
Dictatorship	0.686**	0.924**	1.048*	0.783**	0.888**	1.069*
	(0.311)	(0.400)	(0.551)	(0.331)	(0.375)	(0.585)
***fiscal redistribution*$_{t-1}$**				0.298**	0.292**	0.287**
				(0.133)	(0.133)	(0.134)
GDP per capita	1.527***	1.562***	1.675***	0.342	0.394	0.316
	(0.361)	(0.366)	(0.360)	(0.366)	(0.380)	(0.379)
age dependency	0.086***	0.089***	0.087***	0.071***	0.071***	0.071***
	(0.017)	(0.017)	(0.018)	(0.019)	(0.019)	(0.019)
openness	0.008	0.010	0.008	-0.001	-0.001	-0.002
	(0.008)	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)
R2	0.200	0.206	0.198			
Observations	569	572	563	447	445	444
Number of countries	119	119	114	112	111	109
Number of instruments				49	49	49
Hansen (p-value)				0.327	0.331	0.394
AR(2) (p-value)				0.749	0.640	0.711

Notes: In all specifications we control for a full set of country and year fixed effects. In all estimations we remove countries with standardized residuals above 1.96 or below -1.96. In the GMM the variable *fiscal redistribution*$_{t-1\ 1}$ is instrumented with second and further lags, whereas all other covariates are treated as exogenous. The Hansen statistic is a test of overidentifying restrictions, under the null that overidentifying restrictions are valid. The AR(2) is a test for second-order serial correlation in the differenced residuals, under the null of no serial correlation. Robust standard errors, clustered by country are reported in parentheses. *** denotes significance at 1% level, ** denotes significance at 5% level and * denotes significance at 10% level.

Table 3. Regime and fiscal redistribution: Quality of Inequality data

	(1)	(2)	(3)	(4)	(5)	(6)
	FE	FE	FE	GMM	GMM	GMM
Political variable:	**CGV**	**BMR**	**POLITY2**	**CGV**	**BMR**	**POLITY2**
Panel A: Dropping most noisy Inequality data						
Dictatorship	1.318***	1.484***	2.519***	0.634**	0.957**	1.287**
	(0.462)	(0.512)	(0.813)	(0.321)	(0.430)	(0.614)
R2	0.103	0.110	0.118			
Observations	685	683	666	467	465	458
Number of countries	140	139	134	120	119	115
Number of instruments				49	49	49
Hansen (p-value)				0.317	0.373	0.302
AR(2) (p-value)				0.289	0.265	0.293
Panel B: Dropping Sub-Saharan Africa countries						
Dictatorship	1.475**	1.860***	3.031**	1.128**	1.265***	1.574**
	(0.686)	(0.685)	(1.181)	(0.470)	(0.463)	(0.735)
R2	0.114	0.126	0.129			
Observations	602	599	580	441	439	431
Number of countries	104	103	96	96	95	91
Number of instruments				49	49	49
Hansen (p-value)				0.564	0.592	0.281
AR(2) (p-value)				0.297	0.274	0.321
Panel C: Reducing sample between 1980 and 2010						
Dictatorship	1.303**	1.613**	2.626**	0.713**	0.852***	1.090*
	(0.651)	(0.649)	(1.087)	(0.316)	(0.314)	(0.634)
R2	0.073	0.086	0.084			
Observations	625	622	604	446	444	436
Number of countries	144	143	136	126	125	121
Number of instruments				39	39	39
Hansen (p-value)				0.349	0.361	0.428
AR(2) (p-value)				0.345	0.329	0.362

Notes: All models control for GDP per capita, age dependency and openness, but these coefficients are not reported to save space. In all specifications we control for a full set of country and year fixed effects. In Panel A we repeat the estimates of Table 1 after dropping 10% of the observations that are associated with the higher uncertainty in the inequality data estimates. In Panel B we drop from estimates in Table 1 all Sub-Saharan Africa countries, whereas in Panel C we drop the first two decades of our sample. In the GMM the variable *fiscal redistribution*$_{t-1}$ is instrumented with second and further lags, whereas all other covariates are treated as exogenous. The Hansen statistic is a test of overidentifying restrictions, under the null that overidentifying restrictions are valid. The AR (2) is a test for second-order serial correlation in the differenced residuals, under the null of no serial correlation. Robust standard errors, clustered by country are reported in parentheses. *** denotes significance at 1% level, ** denotes significance at 5% level and * denotes significance at 10% level.

Table 4. Regime and fiscal redistribution: Dropping ex-Communist countries

	(1)	(2)	(3)	(4)	(5)	(6)
	FE	FE	FE	GMM	GMM	GMM
Political variable:	**CGV**	**BMR**	**POLITY2**	**CGV**	**BMR**	**POLITY2**
Dictatorship	1.339**	1.627***	2.492***	1.009***	1.021***	1.156**
	(0.529)	(0.565)	(0.922)	(0.361)	(0.360)	(0.483)
***fiscal redistribution*$_{t-1}$**				0.494***	0.493***	0.506***
				(0.087)	(0.089)	(0.084)
GDP per capita	1.606***	1.666***	1.493***	-0.286	-0.240	-0.452
	(0.426)	(0.425)	(0.420)	(0.490)	(0.496)	(0.472)
age dependency	0.115***	0.112***	0.113***	0.072***	0.071***	0.068***
	(0.024)	(0.024)	(0.024)	(0.013)	(0.014)	(0.013)
openness	0.013**	0.013*	0.012*	0.003	0.003	0.002
	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
R2	0.106	0.114	0.113			
Observations	671	671	652	468	468	460
Number of countries	116	116	109	101	101	97
Number of instruments				49	49	49
Hansen (p-value)				0.472	0.434	0.381
AR(2) (p-value)				0.264	0.253	0.283

Notes: see Table 1

Table 5. Regime and fiscal redistribution: Adding gross income inequality in the set of control variables

	(1)	(2)	(3)	(4)	(5)	(6)
	FE	FE	FE	GMM	GMM	GMM
Political variable:	**CGV**	**BMR**	**POLITY2**	**CGV**	**BMR**	**POLITY2**
Dictatorship	1.399**	1.679***	2.493**	1.103***	1.026***	1.315**
	(0.583)	(0.599)	(0.985)	(0.364)	(0.357)	(0.547)
***fiscal redistribution*$_{t-1}$**				0.566***	0.565***	0.551***
				(0.090)	(0.095)	(0.097)
gross income inequality	0.083***	0.086***	0.080***	-0.076*	-0.072	-0.067
	(0.023)	(0.024)	(0.024)	(0.045)	(0.045)	(0.045)
GDP per capita	0.480	0.576	0.380	0.020	0.060	-0.079
	(0.408)	(0.411)	(0.403)	(0.355)	(0.364)	(0.356)
age dependency	0.117***	0.115***	0.115***	0.076***	0.075***	0.073***
	(0.029)	(0.029)	(0.029)	(0.016)	(0.016)	(0.015)
openness	-0.001	-0.002	-0.004	0.002	0.001	-0.000
	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)
R2	0.135	0.145	0.144			
Observations	665	662	649	530	528	520
Number of countries	135	134	129	126	125	121
Number of instruments				50	50	50
Hansen (p-value)				0.483	0.503	0.303
AR(2) (p-value)				0.304	0.282	0.326

Notes: see Table 1

Table 6. Regime and fiscal redistribution: Instrumental variables approach (IV)

	(1)	(2)	(3)	(4)	(5)	(6)
	FE-IV	FE-IV	FE-IV	FE-IV	FE-IV	FE-IV
Political variable:	**CGV**	**BMR**	**POLITY2**	**CGV**	**BMR**	**POLITY2**
Dictatorship	2.548**	2.668**	2.378	3.543***	3.795***	2.729*
	(1.176)	(1.176)	(1.659)	(1.271)	(1.156)	(1.590)
gross income inequality				0.062**	0.071***	0.073***
				(0.027)	(0.025)	(0.027)
GDP per capita	1.349***	1.514***	1.245***	0.393	0.585	0.077
	(0.433)	(0.451)	(0.436)	(0.521)	(0.557)	(0.458)
age dependency	0.109***	0.107***	0.110***	0.103***	0.098***	0.106***
	(0.023)	(0.024)	(0.024)	(0.028)	(0.028)	(0.028)
openness	0.009	0.007	0.004	0.000	-0.001	-0.005
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
	First-Stage Results					
Democracy Abroad	-0.892**	-0.264	-0.825**	-0.910**	-0.429	-0.966**
	(0.409)	(0.375)	(0.386)	(0.434)	(0.462)	(0.457)
Dictatorship$_{t-1}$	0.281***	0.308***	0.320***	0.284***	0.301***	0.304***
	(0.048)	(0.053)	(0.050)	(0.052)	(0.056)	(0.051)
F-stat	23.589	17.079	24.621	17.751	14.705	21.086
Overidentification test	0.352	0.131	0.462	0.535	0.141	0.765
Observations	712	709	692	630	627	615
No. of Countries	133	132	127	126	125	121

Notes: In all specifications we control for a full set of country and year fixed effects. 2SLS are estimated using the variable *Democracy Abroad* and the first lag of the variable *Dictatorship* as instruments. The F-stat is the F statistics for the explanatory power of the excluded instruments in first stage regressions, whereas the overidentification test is the p-value of the Hansen J test of the validity of the excluded instruments. Robust standard errors, clustered by country are reported in parentheses. All regressions include a full set of country and year fixed effects. *** denotes significance at 1% level, ** denotes significance at 5% level and * denotes significance at 10% level.

Table 7. Political regime and fiscal revenues

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE
Political variable:	**CGV**	**BMR**	**POLITY2**	**CGV**	**BMR**	**POLITY2**	**CGV**	**BMR**	**POLITY2**	**CGV**	**BMR**	**POLITY2**
Fiscal variable:	***revenues_ICTD***			***tax_revenues_ICTD***			***revenues_GDNGD***			***tax_revenues_GDNGD***		
Dictatorship	-0.234	0.129	-0.537	-0.296	-0.068	-0.155	0.429	0.306	1.094	0.082	-0.496	-0.056
	(0.614)	(0.537)	(0.968)	(0.453)	(0.437)	(0.658)	(0.980)	(0.920)	(1.410)	(0.840)	(0.777)	(1.174)
GDP per capita	4.063***	4.051***	3.668***	0.740	0.719	0.452	7.042***	7.037***	6.504***	5.592***	5.527***	5.155***
	(1.289)	(1.288)	(1.329)	(0.545)	(0.542)	(0.573)	(1.348)	(1.356)	(1.086)	(1.822)	(1.831)	(1.959)
age dependency	0.026	0.025	0.026	-0.030	-0.031	-0.040	0.037	0.037	0.050	0.038	0.037	0.042
	(0.053)	(0.053)	(0.056)	(0.024)	(0.024)	(0.025)	(0.044)	(0.045)	(0.045)	(0.042)	(0.042)	(0.043)
openness	0.004	0.004	0.030*	-0.004	-0.004	0.009	-0.006	-0.007	0.063**	-0.009	-0.009	0.030
	(0.016)	(0.016)	(0.015)	(0.009)	(0.009)	(0.009)	(0.039)	(0.039)	(0.026)	(0.025)	(0.025)	(0.026)
R2	0.136	0.135	0.144	0.130	0.128	0.116	0.202	0.202	0.254	0.211	0.212	0.215
Observations	700	696	651	749	745	698	477	477	451	478	478	452
Number of countries	140	139	132	140	139	132	109	109	103	109	109	103

Notes: All models are estimated according to equation (6) as described in section 5, taking five year averages for all variables except for the variable Dictatorship that we use value t of each five-year period (t, t+4) of our sample. In all specifications we control for a full set of country and year fixed effects. Robust standard errors, clustered by country are reported in parentheses. *** denotes significance at 1% level, ** denotes significance at 5% level and * denotes significance at 10% level.

Table 8. Political regime and fiscal expenditures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE
Political variable:	**CGV**	**BMR**	**POLITY2**	**CGV**	**BMR**	**POLITY2**	**CGV**	**BMR**	**POLITY2**	**CGV**	**BMR**	**POLITY2**
Fiscal variable:	***social_services_GDNGD***			***services_subsidies_EFW***			***transfers_subsidies_GDNGD***			***health_education_GDNGD***		
Dictatorship	-0.643	-0.746	-0.949	0.448	0.655	0.649	1.725	1.886*	5.354**	-0.585**	-0.698***	-1.063**
	(0.958)	(0.915)	(1.338)	(0.560)	(0.550)	(1.046)	(1.212)	(1.137)	(2.205)	(0.259)	(0.264)	(0.422)
GDP per capita	-0.560	-0.575	-0.180	1.544*	1.639**	1.574*	0.055	0.141	-0.851	-0.124	-0.142	-0.429
	(1.370)	(1.367)	(1.591)	(0.805)	(0.788)	(0.830)	(1.762)	(1.737)	(1.586)	(0.573)	(0.566)	(0.525)
age dependency	0.005	0.005	0.016	0.014	0.015	0.017	-0.066	-0.077	-0.108	-0.028	-0.029	-0.033*
	(0.030)	(0.030)	(0.031)	(0.028)	(0.027)	(0.029)	(0.082)	(0.080)	(0.087)	(0.019)	(0.019)	(0.018)
openness	-0.000	-0.001	-0.008	-0.012	-0.012	-0.009	-0.008	-0.010	-0.007	-0.022*	-0.023*	-0.004
	(0.009)	(0.009)	(0.015)	(0.008)	(0.008)	(0.009)	(0.013)	(0.013)	(0.014)	(0.012)	(0.012)	(0.009)
R2	0.109	0.110	0.109	0.115	0.117	0.116	0.150	0.164	0.238	0.158	0.165	0.116
Observations	437	437	411	808	807	768	225	225	215	416	416	390
Number of countries	107	107	101	132	131	126	84	84	80	105	105	99

Notes: In columns (1)-(3)and (7)-(12) we estimate equation (6) as described in section 5, taking five year averages for all variables except for the variable Dictatorship that we use value t of each five year period (t,t+4) of our sample. In columns (4)-(6) that EFW database provides one observation every five years until year 2000, we use the initial year *t* for both the left and right hand side variables of equation (6). In all specifications we control for a full set of country and year fixed effects. Robust standard errors, clustered by country are reported in parentheses. *** denotes significance at 1% level, ** denotes significance at 5% level and * denotes significance at 10% level.