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Do dictatorships redistribute more?

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Abstract: This paper examines the effect of political institutions on fiscal redistribution for a country-level panel from 1960-2010. Using data on Gini coefficients before and after government intervention, we apply a measure of *effective fiscal redistribution* that reflects the effect of taxes and transfers on income inequality. Our findings clearly indicate that non-democratic regimes demonstrate significantly greater direct fiscal redistribution. Subsequently, we employ fiscal data in an attempt to enlighten this puzzling empirical finding. We find that dictatorial regimes rely more heavily on cash transfers that exhibit a direct impact on net inequality and consequently on the difference between market and net inequality (i.e., effective fiscal redistribution), whereas democratic regimes devote a larger amount of resources to public inputs (health and education) that may influence market inequality but not the difference between market and net inequality *per se*. We argue that the driving force behind the observed differences within the pattern on government spending and effective fiscal redistribution is that democratic institutions lead survival-oriented leaders to care more for the private market, and thus to follow policies that enhance the productivity of the whole economy.

JEL: D7, H1, H2

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., taxes collected from households and cash transfers distributed to them. Since the political system is a crucial determinant of every governmental policy, a large number of theoretical and empirical studies have investigated the interplay between political institutions and fiscal redistribution (Boix, 2003; Lizzeri and Persico, 2004; Acemoglu and Robinson, 2006).

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) redistribute less, while an expansion of democracy should increase redistribution and produce more egalitarian outcomes (Boix, 2003; Acemoglu and Robinson, 2006).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, McGuire and Olson, 1996; Bueno de Mesquita et al., 2003; Lizzeri and Persico, 2004; Deacon, 2009). According to these studies, democracies favour spending on public goods such as in health and education, whereas dictatorships favour spending on cash transfers targeted to politically influential groups.²

Starting with Lindert (1994), a number of empirical studies have examined the effect of political institutions on the size and composition of government spending and taxation. Interestingly, the empirical evidence concerning the effect on taxation appears to be mixed. Specifically, Mulligan et al., (2004) and Profeta et al., (2013) fail to provide evidence in favour of a clear-cut link between political institutions and the size and composition of taxes, whereas Acemoglu et al., (2015) suggest a positive and robust impact of democracy on the size of tax revenues. Moreover, according to historical studies, the extension of the voting franchise in Western Europe in the late 19th and early 20th centuries affected taxation but in much more

¹ The driving force of this result is highlighted by Romer (1975), Roberts (1977), and Meltzer and Richard (1981). According to their rationale, 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, thus increasing the distance between the median and the mean income, the demand for redistribution increases.

² The intuition behind this theoretical result is as follows. In non-democratic regimes in which political power is more concentrated, the rational leader will spend the public budget mainly on transfers targeted to critically supporting groups. Spending on a non-exclusive public good does not make sense, since their benefits can spillover to non-influential outsiders. In contrast, in democracies, the electorate (and the required winning coalition) increases, making spending on public goods a more attractive option due to the economies of scale inherent in providing public goods to large groups (Bueno de Mesquita, 2003; Deacon, 2009; North et al., 2009). For an excellent review of this literature, see Deacon and Saha (2006).

complex and intriguing ways than is often assumed (Aidt and Jensen, 2009a; 2009b; 2013).³ A clearer pattern around this period appears in the relationship between democracy and government spending that seems to be positive, primarily driven by increased spending on infrastructure and internal security, since social programs until the 1930s played only a marginal role in most countries (see Lindert, 1994; Aidt et al., 2006; Aidt and Jensen, 2013). Regarding studies that use modern data, expansion of democracy seems to exert a robust positive impact on the provision of public goods (see, e.g., Kaufman and Segura-Ubiergo, 2001; Ansell, 2010).⁴ With respect to welfare spending, the effect of political institutions is ambiguous. In particular, according to Boix (2003), democracy is associated with higher subsidies and transfers, though this linkage is conditional on the level of development. In contrast, Mulligan et al., (2010) provide evidence that non-democratic regimes spend more of their GDP on social security and redistribute more income through payroll taxation.⁵ Similarly, Kaufman and Segura-Ubiergo (2001) show that democracy is positively correlated with social spending in a sample of 14 Latin American countries. Table 1 provides a concise overview of the papers discussed in this paragraph that have tested the effect of democracy on fiscal policy.

[Insert Table 1 here]

Obviously, both the theoretical and the empirical literature conclude that linking fiscal policy choices to variations in political institutions is a complicated research issue. In our case, the attempt to investigate the effect of political institutions on fiscal redistribution is even more ambitious. This is because political institutions influence many different aspects of the implemented fiscal policy, which in turn affects income inequality through a variety of alternative channels.⁶ Our first contribution that distinguishes our analysis from the rest of the literature is that

³ More precisely, Aidt and Jensen (2009a; 2013) provide evidence of a non-monotonic relationship between suffrage extension and taxation (retrenchment hypothesis), whereas Aidt and Jensen (2009b) suggest that the effect of franchise extension on the tax structure is conditional on the tax collection cost.

⁴ A related literature provides evidence for a robust positive effect of democracy on health and education outcomes (see Baum and Lake, 2001; 2003; Bueno de Mesquita, 2003; Gallego, 2010). Contrary to these findings, Ross (2006) suggests that although democracies spend more money on education and health than non-democracies do, such spending does not reduce infant and child mortality, since these benefits are mostly directed to middle- and upper-income groups.

⁵ 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.

⁶ It should be noted that the empirical literature fails to provide any straightforward association between an expansion of democracy and income inequality (see Li et al., 1998; Scheve and Stasavage, 2009; Timmons, 2010; Acemoglu et al., 2015; Knutsen, 2015).

we capture the extent of fiscal redistribution through an outcome variable. Specifically, we employ a measure of “effective fiscal redistribution” calculated as the difference between Gini coefficients before and after taxes and transfers.⁷ We obtain our data from the Standardized World Income Inequality Database (SWIID) (Solt, 2009), which provides market and net Gini indices for the broadest possible sample of countries and years. It is worth noting that researchers have applied a similar measure of fiscal redistribution in the past, though they address different research questions (see, e.g., Milanovic, 2000; Iversen and Soskice, 2006; Grundler and Kollner, 2017). Using a panel of countries from 1960 to 2010, we provide strong evidence that dictatorial regimes redistribute more than democracies through taxes and cash transfers. This result remains robust to alternative codings of political regimes, across several different specifications and estimation techniques. Among our robustness checks, we present Two Stage Least Squares (2SLS) estimates that rely on the theory of “regional democratisation in waves” developed by Huntington (1993).

Our second contribution in the literature is that we use data on the size and composition of the public sector in an attempt to uncover the channels through which political institutions affect effective fiscal redistribution. The merit of this strategy is that we can provide some insights for the contradicting findings in the existing literature. Interestingly, we do not find any association between political institutions and taxation. Nevertheless, democratic institutions are positively associated with health and education expenses, whereas non-democratic regimes rely more heavily on cash transfers. These results are consistent with our findings on effective fiscal redistribution. On the one hand, dictatorial regimes rely more heavily on cash transfers, with a direct impact on net inequality (after taxes and transfers), the difference between market and net inequality, and consequently the level of effective fiscal redistribution. On the other hand, democratic regimes favour the provision of public inputs, with the potential to affect market income inequality, but not the difference between market and net inequality *per se*.

These results are in accordance with a strand of the theoretical literature which suggests that democracies and dictatorships actually follow different patterns of government spending (McGuire and Olson, 1996; Bueno de Mesquita et al., 2003; Lizzeri and Persico, 2004; Deacon, 2009). According to Olson (1993) and McGuire and Olson (1996), a crucial factor that drives this difference is that democratic leaders are in need of a majority of voters that earns a significant

⁷ Although a large number of fiscal policy choices such as spending on health and education may affect Gini coefficients, the fiscal instruments that, by definition, affect the difference between market Gini and net Gini are taxes and the cash transfers that mediate between market income distribution and net income distribution.

share of the market income of the economy. This inevitably leads democratic governments to care more for the private market and thus to spend more on public inputs that enhance the productivity of the whole economy.⁸ In other words, the interest of voters in their market earnings induce democratic leaders to follow a different pattern of government spending, thus providing more public inputs (such as education and health) and less private benefits (such as targeted cash transfers) compared to the leaders in autocratic regimes. In the latter case, spending on targeted transfers can be a more attractive way to gain political support, since rulers are in need of a narrower group of supporters. In line with the above theoretical rationale, our analysis provides evidence that democratic regimes care more for human capital that enhances the productivity of the whole economy (see, e.g., Besley and Kudamatsu, 2006). On the contrary, non-democratic regimes rely more heavily on market regulation that generates a wide range of privileged private goods (such as business licences, privileged access to credit, etc.) and prospects for rent-seeking activities (see, e.g., De Haan and Sturm, 2003; Giuliano et al., 2010).

The remainder of the paper is organized as follows. Section 2 describes the data and the econometric methodology and presents the empirical findings on effective fiscal redistribution. Section 3 attempts to illuminate the fiscal policy channels that drive our results and provides some case-study evidence to illustrate these channels. Section 4 discusses how the political institutions affect the priorities of survival-oriented leaders concerning the provision of private versus public goods and provides empirical evidence in support of these theoretical considerations. In Section 5, we summarize and conclude.

2. Political institutions and fiscal redistribution

2.1 Data

Since fiscal redistribution through taxes and transfers is in the centre of our attention, we should first of all clarify how we measure the size of this governmental policy. Our preferred measure is calculated by taking the difference between the Gini of incomes before (*market Gini*) and after

⁸ 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 for the private economy 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.

(*net Gini*) taxes and transfers (see, e.g., Milanovic, 2000; Iversen and Soskice, 2006; Grundler and Kollner, 2017):

$$fiscal\ redistribution_{it} = market\ Gini_{it} - net\ Gini_{it} \quad (1)$$

The Gini indices range between 0 and 100, where higher values indicate a more unequal income distribution. When the difference in equation (1) is positive taxes and cash transfers reduce income inequality, and as this difference increases effective *fiscal redistribution* in country i at time t is higher.

Our data are obtained by the SWIID. The main advantage of this database is that it incorporates Atkinson and Brandolini's (2001) recommendations to provide the most comparable market and net Gini indices for the broadest possible sample of countries and years- namely 174 countries for as many years as possible from 1960 to 2013. To achieve this goal, the SWIID employs market and net Ginis from the Luxembourg Income Study (LIS) as a benchmark of most reliable data to which inequality estimates from other sources are standardized. Observations from these sources are harmonized through a multiple-imputation algorithm that estimates country-years not yet covered in the LIS.⁹ To minimize reliance on problematic assumptions, the custom missing-data algorithm uses as much information as possible from proximate years within the same country. A precise description of the procedure with detailed documentation of the number of countries for which adjustments vary can be found in Solt (2015, 2016). The coverage of country-years in the SWIID far exceeds those of alternative cross-national inequality datasets, making it an appealing choice for studies based on broad panel estimation (see, e.g., Ostry et al., 2014; Acemoglu et. al., 2015; Brueckner et al., 2015; De Haan and Sturm, 2015). Nevertheless, SWIID has recently been subject to some criticism concerning the reliability of the imputation technique, especially for less developed countries that few and less reliable baseline observations are available (see Jenkins 2015). In this regard, our empirical strategy and extensive robustness checks treat the data with special care in order to establish the reliability of our estimates.

⁹ In the earlier SWIID versions, Solt (2009) used the World Income Inequality Database (UNU-WIDER, 2014, henceforth WIID) as the only source for the imputations. However, version 5.0 utilizes over ten thousand Gini coefficients adding the OECD Income Distribution Database, the Socio-Economic Database for Latin America and the Caribbean Center generated by CEDLAS and the World Bank, Eurostat, the World Bank's PovcalNet, the UN Economic Commission for Latin America and the Caribbean, the World Top Incomes Database, the University of Texas Inequality Project, national statistical offices around the world, and many other sources.

For the main explanatory variable of our study, we use data from three alternative sources that allow us to distinguish democratic from dictatorial regimes. First, we employ the dichotomous variable developed by Cheibub, Gandhi, and Vreeland (2010, henceforth *CGV*) that covers 202 countries over the period 1946 to 2008. The key political factors *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, Miller and Rosato (2013, 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.¹⁰ In both cases, the variable *dictatorship* takes the value of 1 when a country is categorized as non-democratic, and 0 otherwise. Third, we rely on the continuous measure *POLITY2* as obtained from the Polity IV database (Marshall and Jaggers, 2010). This index has been applied as a tool to classify political regimes in a number of studies (see, e.g., Mulligan et al., 2004; Haber and Menaldo, 2011), though a closer look 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 providing 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.

Finally, in all regressions we add 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 (Wagner, 1883), we expect richer countries to have larger public sectors, which in turn can affect the design of fiscal policy and fiscal redistribution. Moreover, given that a number of studies have shown a direct effect of democratisation on economic development (Papaioannou and Siourounis, 2008; Acemoglu et al., 2018), 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 working

¹⁰ Both, the *CGV* and the *BMR* datasets, are different updates and revisions of the well-established dichotomous classification of regimes introduced by Alvarez et al. (1996) and Przeworski et al. (2000).

age population. According to the literature, demographic factors consist a basic driving force for the extent of social benefits and public pensions (Lindert, 1994; Mulligan et al., 2004; Mulligan et al., 2010). Finally, our analysis takes into account the effect of international market integration by including the percentage 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 programs, varies positively with the degree of globalization as a safety net against the exposure to the terms of trade risk (see, e.g., Rodrik, 1998). Data on *age dependency* and *openness* are obtained from World Banks' World Development Indicators (WDI).¹¹

Our dataset comprises of 146 countries from 1960 through to 2010, though not all variables are available for all countries in all periods.¹² Table A1 in Appendix A provides definitions, data sources and descriptive statistics of all variables used in this study. Moreover, Table B1 in the Online Appendix provides the full list of countries with information for regime changes according to the *CGV* and *BMR* variables, as well as minimum and maximum values of the continuous index *POLITY2*.

2.2 Empirical strategy

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

$$Y_{it} = \alpha_1 \text{dictatorship}_{it-1} + \beta X_{it-1} + \gamma_i + \delta_t + \varepsilon_{it} \quad (2)$$

The dependent variable is our measure of effective *fiscal redistribution*_{it} in country *i* at time *t*. The main variable of interest *dictatorship*_{it-1}, classifies the political regime in country *i* at time *t-1*, according to the *CGV*, *BMR*, and *POLITY2* variables described above. Moreover, *X*_{it-1} includes the additional covariates of the regression. Finally, γ_i and δ_t correspond to country and time fixed effects, respectively, and ε_{it} is the error term. In this specification years *t-1* and *t* represent the first and last observations, respectively, of each subperiod (1960-65 to 2006-10) of our sample.

¹¹ It is worth noting that we have attempted to include in our model a series of other variables, such as the population and the urbanization rate. However, none of these variables had a significant effect on fiscal redistribution, and due to other concerns (correlation of covariates, reduction of sample size) we do not include them in our estimations.

¹² We exclude from our sample non-independent territories and very small countries below 500,000 citizens (e.g., Andorra, Iceland).

We adopt this strategy for three reasons. First, because we expect the effect of a change in the political regime not to be contemporaneous (see, Acemoglu et. al., 2015).¹³ Second, we attempt to reduce reverse causation from *fiscal redistribution* to *dictatorship*. Third, we aim to mitigate, to the degree possible, reliance on the data imputation technique applied by Solt (2009), who estimates missing country-year observations from proximate years within the same country. Equation (2) guarantees that our estimates are not contaminated by aggregate shocks and trends common to all countries or time-invariant local factors including geography, history and social norms.

However, due to the persistence in inequality and fiscal commitments that carry over from one year to the next, our model could be dynamic. Following the rationale of the relevant literature (see, e.g., Aidt and Jensen, 2013; Amendola et al., 2013), we include a lagged dependent variable in our model estimating the following equation:

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

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 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, Nickell, 1981; Kiviet, 1995). 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:

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

¹³ 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 Subsection 2.3.

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 the 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. Arellano and Bond (1991) suggest the use of lagged values of the explanatory variables in levels as instruments.¹⁴ To this end, our lagged endogenous regressor is instrumented with second and further lags of the dependent variable, whereas all the other covariates are considered as exogenous.

Our empirical strategy rules out certain types of contaminating factors for our results, though it does not resolve endogeneity concerns. For this reason, in Subsection 2.4.3 we take an instrumental variables approach. The challenge in our case is to find an external instrument that affects *fiscal redistribution* only through its effect in 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, Huntington, 1993; Persson and Tabellini, 2009). Finally, it should be noted that in all models throughout our analysis reported standard errors are clustered at the country level.

2.3 Baseline results

Our first results are reported in Table 2. In columns (1) to (3) we report the estimates of equation (2) where 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 cases the main variable of interest is positive and statistically significant at the 1% level highlighting that non-democratic regimes redistribute more income through taxes and transfers. The rest of the covariates are positive and statistically significant - consistent with our theoretical priors.

[Insert Table 2, here]

¹⁴ An alternative to the difference-GMM is the Blundell and Bond's (2000) system-GMM estimator, which maintains the differenced equation adding an equation in levels with another set of instruments. We prefer the difference 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.

Moving one step forward, in columns (4) to (9) of Table 2 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, its coefficient in the GMM estimates is higher to those obtained in the FE specifications as expected. The main variable of interest, *dictatorship*, remains positive and statistically significant, whereas from the additional covariates only *age dependency* retains its 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 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.

Concerning the magnitude of the long-run effect of the variable *dictatorship*, according to the static specification of Table 2, 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 $(1 - \alpha_1)$, where α_1 is the coefficient of the lagged dependent variable (i.e., $\frac{\alpha_2}{(1-\alpha_1)}$). 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 *fiscal redistribution* in the sample is 5.19 points (with a standard deviation of 5.57), it is clear that this effect is quantitatively sizable.

2.4 Sensitivity analysis

In this subsection, we report various robustness checks. First, we examine if our estimates are contaminated by the most noisy/unreliable inequality data. Second, we add market Gini into the set of the control variables, to exclude the possibility that our results are driven by differences in gross income inequality between democratic and dictatorial regimes. Third, we take an instrumental variables approach in order to alleviate concerns of endogeneity in our estimates.

2.4.1 Outliers and income inequality estimates

Despite the popularity of the SWIID among social scientists, an intense discussion has arisen for its suitability on cross-country analysis of income inequality (see, Jenkins, 2015). The main

critique follows in Atkinson and Brandolini's (2001; 2009) footsteps, who review the pitfalls encountered in the utilization of secondary income inequality datasets. The newest versions of SWIID address these pitfalls, though, some issues remain due to the wide country-year coverage of the database. In particular, for some estimates the degree of comparability across countries is still a concern, since different sources may apply different methodologies to compute the degree of inequality. However, given that our regressions include country fixed effects, the across-country comparability should not be a major concern. An additional pitfall relates to the incompatibility of inequality data within countries over time, especially in less developed countries that few and less precise sources are available.¹⁵

For this reason, our empirical strategy, as described in Subsection 2.2, was adjusted to incorporate issues related to the custom missing algorithm employed by Solt (2009), whereas in this subsection we perform five checks in order to establish the reliability of our estimates. First, the SWIID provides estimates of uncertainty for each country-year observation of the income inequality data. Therefore, to take into account a large part of the remaining incomparability in the data, we drop from our estimates 10 percent of the observations where the variable *fiscal redistribution* is associated with the higher uncertainty.¹⁶ Second, we drop from our sample the Sub-Saharan Africa countries, because fewer and less precise inequality data are available for the construction of the variable *fiscal redistribution*. Third, following a similar rationale, we also drop from our estimates the first two decades of the sample. Fourth, we drop Soviet and Soviet satellite countries. Such dictatorships may appear "more redistributive" just on account of the scale of state involvement in the economy. 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. Finally, we rerun our estimates without countries with a standardized residual above 1.96 or below -1.96.

In panels A to E of Table 3, we rerun the static FE and the GMM regressions of Table 2 for the above robustness checks. To save space, we only report the estimates for the *dictatorship*

¹⁵ A solution according to Atkinson and Brandolini (2001) is to check the robustness using data from alternative sources, as well as mentioning the potential drawbacks related to the choice of a specific dataset. Such a source in our case is the WIID that lately increased substantially the coverage of market and net Gini indices. 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.

¹⁶ To incorporate the uncertainty of both components of the dependent variable, we aggregate the standard errors of Gini indices to construct a standard error estimate for the variable *fiscal redistribution*. It should be noted though that this strategy has the drawback of entailing the strong assumption that the errors of the two Gini indices are independent.

coefficient. Once again, the results strongly support our baseline findings, suggesting that authoritarianism significantly enhances the scope of redistribution.

[Insert Table 3, here]

2.4.2 *Gross income inequality and fiscal redistribution*

Our next robustness check is to add in the set of the control variables the *market Gini* index. The most obvious reasons is that the likelihood of democracy (see, Acemoglu and Robinson, 2006; Dorsch and Maarek, 2015), but also the evolution of *fiscal redistribution* may depend on income inequality levels (see Meltzer and Richard, 1981). Moreover, democracy exerts a positive impact on the provision of public goods according to the literature (see, e.g., Kaufman and Segura-Ubiergo 2001; Ansell, 2010), which in turn may impact income inequality independently of the effect of democracy on these policy instruments. Therefore, by controlling for the *market Gini* we attempt to eliminate a large number of alternative policy channels through which political institutions may affect the income distribution.

[Insert Table 4, here]

As shown in Table 4, the variable *dictatorship* remains a highly significant and positive determinant of *fiscal redistribution*. In addition, *market Gini* enters with a positive and statistically significant coefficient in columns (1) to (3). This finding is consistent with the hypothesis developed by Meltzer and Richard (1981) that higher inequality leads to a greater demand for redistribution, which translates to an expansion of the welfare system. 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.¹⁷ Regarding the rest of the covariates, once again *age dependency* bears a positive and statistically coefficient in all specifications highlighting the robust effect of demographic factors on fiscal redistribution.

¹⁷ An additional check is to interact *market Gini* with the variable *dictatorship*, in order to examine if the effect of the political regime on *fiscal redistribution* is conditional on the level of income inequality. A concern in our case is that more unequal non-democratic regimes appear as the most redistributive. However, our estimates- available upon request- indicate that the interaction term is negative and statistically insignificant.

2.4.3 The 2SLS identification strategy

Our estimates linking political institutions and *fiscal redistribution* do not necessarily imply a causal relationship. By exploiting within country variation, we control for all time-invariant features shaping *fiscal redistribution* and the type of the political regime. However, we cannot rule out the fact that omitted time-varying country characteristics drive the correlation. One could still argue that our results can be affected by potential reverse causality running from fiscal redistribution to the political regime, or by the measurement error of the alternative regime-type variables that we use in our empirical analysis. To alleviate these concerns we pursue an instrumental variables 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*. To this end, our strategy relies on the theory of “regional democratisation in waves” developed by Huntington (1993), as well as the “foreign democratic capital” theory suggested by Persson and Tabellini (2009). In particular, we expect regional democratic diffusion in Latin America, Asia Pacific, Eastern Europe, and Sub-Saharan Africa countries during “the third wave” to be an attractive source of exogenous variation in the determination of the domestic political regime. To construct our instrument we apply the inverse distance weighting formula as follows:

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

where D_{jt} classifies if country j (different from i) is democratic according to the *CGV*, *BMR*, and *POLITY2* variables described above; and W_{ij} is the inverse distance in kilometres between the capitals of countries i and j . Our instrument, *democracy abroad*_{it}, takes values between 0 and 1 with higher values indicating that country i at time t has more democratic countries in the geographic neighbourhood. More importantly, a regional wave of democratisation increases the value of *democracy abroad*, which in turn can be diffused in the domestic political regime (see Huntington, 1993; Persson and Tabellini, 2009). It is worth noting that Acemoglu et al. (2018) have applied a similar strategy 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.

Columns (1) to (3) of Table 5 present the baseline 2SLS results, whereas in columns (4) to (6) we add in the set of the control variables the *market Gini*. 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 bias in our estimates (see, e.g., Nickell, 1981). The first-stage results are reported in the lower part of Table 5. 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 5 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, highlighting the validity of our instruments.

[Insert Table 5, here]

The second-stage results reported in Table 5 verify once again the positive effect of the variable *dictatorship* on *fiscal redistribution*. Although our instrumental variables strategy does not fully exploit random variation, accounting for the “third wave of democratisation” as an external instrument strongly supports our baseline findings. It is not possible to test directly the “exclusion restriction”, though it seems reasonable that the primary impact of regional democratic diffusion on *fiscal redistribution* is via the domestic political regime. 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*. Our results - available upon request- are similar to those obtained in Table 5. Another threat is that the impact of the regime type on redistribution (via the selection and subsequent implementation of particular fiscal policies) to come with substantial time lags, and it may thus be that the exclusion restriction

is violated by including this instrument. It should be noted that when we increase the distance between regime type and fiscal redistribution from 10 to 20 years, although we lose a significant number of observations, the effect of *dictatorship* remains giving us some confidence for the validity of our results. Finally, we observe that the 2SLS coefficients are somewhat higher than in our previous specifications. We interpret this 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. Fiscal policy channels

3.1 Political institutions and fiscal policy

In this subsection, we attempt to illuminate the fiscal policy channels that could be driving our puzzling empirical finding. Our fiscal data that reflect the level as well as the composition of fiscal policy are obtained from three alternative databases. First, we use 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, which provides information at the general government level – when available – allowing researchers to avoid the underestimation of revenue collection in federal states (see, Prichard et al., 2014). Our second source is the Economic Freedom of the World (EFW) project that reports measures for the size of the general government every five years since 1970, and annually from 2000 to 2012 for a maximum of 153 countries. Third, we obtain 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.¹⁸

Regarding our variables on the revenues side, we measure the size of the government using the variables *total revenues* and *total tax revenues* -from the ICTD and the GDNGD- both scaled

¹⁸ Using the ICTD and the EFW, we face an inevitable trade-off between coverage and comparability. In both cases, data before 1999 are based on the Government Finance Statistics Manual (GFSM) (1986) classification, whereas beyond 1999, the data are based on the GFSM (2013) framework. The new accounting practice has been backdated until 1990, though it is difficult to bridge the two classifications, since fiscal variables are measured on a *cash* basis in GFSM (1986) and on an *accrual* basis in GFSM (2013). Therefore, merging the two classifications might involve a number of inaccuracies of unknown magnitude. This is not an issue in the GDNGD, which is based entirely on the old accounting technique.

by GDP and expressed as percentages.¹⁹ Concerning the expenditures side, we use the following four variables all expressed as a percentage of GDP. First, we obtain the fiscal variable social security and welfare affairs and services (denoted as *social services*) from the GDNGD. This measure includes central government payments, both in cash and in kind, which intend to compensate for reduction or loss of income or inadequate earning capacity.²⁰ Second, we employ the variable *subsidies and transfers* from the EFW database that includes in cash and in kind subsidies and social benefits of the general government. Third, we apply the variable transfers to households and non-profit institutions from GDNGD (denoted as *cash transfers*).²¹ This variable concerns the central government and has the advantage of including only cash payments. Finally, we sum health and education expenditures from the GDNGD in order to construct the variable *health and education*. The aim is to check for changes in the provision of public goods that generate in kind services with no direct effect on effective fiscal redistribution.

In the analysis that follows, we modify equation (2) as follows:

$$Y_{it,t+4} = \alpha_1 \text{Dictatorship}_{it} + \beta X_{it,t+4} + \gamma_i + \delta_t + \varepsilon_{it} \quad (6)$$

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 time t , according to the *CGV*, *BMR*, and *POLITY2* variables. Moreover, $X_{it,t+4}$ is the vector of covariates 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) for two reasons. First and foremost, by using fiscal variables on the left side of the estimated equation, we do not face issues related to the custom missing algorithm employed by Solt (2009). Therefore, our strategy is to resort to non-overlapping 5-year averages, which has the advantage of smoothing over some of the cyclical features of the fiscal variables (see, e.g., Kneller et al., 1999). Second, given that fiscal data have gaps, taking five-year averages instead of one observation from each subperiod allows us to maximize the available number of observations. Unfortunately, we cannot apply this strategy with

¹⁹ An additional advantage of the ICTD is that it flags the observations that are not credible for international comparisons (for details, see pp. 30-32 in Prichard et al., 2014). In light of this information, we exclude these data from the analysis.

²⁰ For more information regarding the expenditure categories that compose this fiscal variable, see page 46 in the following link: <https://www.imf.org/external/pubs/ft/gfs/manual/1986/eng/pdf/ch4a.pdf>

²¹ Unfortunately, the database does not provide a separate classification for *transfers to households* and *transfers to non-profit institutions*.

the variable *subsidies and transfers* from the EFW database, since it provides one observation every five years until 2000. Thus, we opt for using only the last observation of each subperiod (1970-75 to 2006-10) of our sample in the estimations. It is worth noting that we do not estimate a dynamic specification, because introducing a lagged dependent variable either does not affect our results or it reduces our sample significantly.

The results for the revenue variables are presented in Table 6A. As can be seen, *dictatorship* bears a non-significant coefficient in all alternative specifications. This evidence is consistent with previous studies (see, e.g., Mulligan et al., 2004; Scheve and Stasavage, 2012; Profeta et al., 2013), though it contradicts conventional theory that an expansion of democracy should lead to greater tax revenues and redistribution (Meltzer and Richard, 1981).²²

[Insert Table 6A, here]

The results for the composition of spending are reported in Table 6B. As can be seen in columns (1)-(6), the variable *dictatorship* is statistically insignificant. Consequently, our analysis fails to provide evidence that the type of political regime is associated with changes in social spending that accounts for both cash and in kind transfers. Interestingly, in columns (8) and (9), that social spending includes only *cash transfers* the variable *dictatorship* is positive and statistically significant.²³ Moreover, according to the results in columns (10)-(12), the variable *dictatorship* is negatively correlated with *health and education* expenses. To the best of our knowledge, only Kaufman and Segura-Ubiergo (2001), for a panel of Latin American countries over the period 1973-1997, have provided similar evidence regarding the effect of the political regime on the composition of public spending. Regarding the positive effect of democratisation on *health and education*, our results are similar to those of important previous studies (see, e.g., Lindert, 2004; Ansell, 2010). The effect of non-democratic regimes on *cash transfers* is consistent

²² For brevity we do not report estimates concerning the relationship between political institutions and the composition of taxes. In specifications that we use direct and indirect tax revenues as dependent variables, once again, the variable *dictatorship* is statistically insignificant. This lack of significant results could be attributed to measurement error. This is because the simple measures of tax composition cannot capture the complexity of the whole tax system or provide a clear picture of the adopted tax policy. More importantly, these measures fail to reflect the redistributive nature of the tax system which is mostly related to the specific design of each tax (and the corresponding degree of tax progressivity) rather than the level of each tax category as a share of GDP.

²³ A potential issue with these estimates is that the number of observations drops significantly, making more difficult to capture within variation of the political regime. However, even in yearly panels, with and without the inclusion of a lagged dependent variable, results remain unaffected.

with the cross-sectional analysis of Mulligan et al. (2010), though the results in the literature are far from consensus (see, e.g., Boix, 2003; Profeta et al., 2013).

[Insert Table 6B, here]

Overall, these empirical findings suggest that democracies and dictatorships follow a different pattern of spending, which is consistent with the puzzling evidence presented in Tables 2-5. On the one hand, dictatorial regimes seem to rely on *cash transfers*, which directly affect net inequality (after taxes and transfers), the difference between market and net inequality, and consequently the level of effective *fiscal redistribution*. On the other hand, democratic regimes prefer higher provision of public goods with the potential to affect market income inequality, but definitely not the difference between market and net inequality or the level of effective *fiscal redistribution*.²⁴

3.2 Case studies

In this section, we examine a set of case studies that provide useful information about the shape of fiscal policy under different political institutions. During the 1970s, a wave of political liberalization took place in Southern Europe with Portugal, Spain, and Greece exiting from authoritarian military rule. Before the democratic transition, all three countries had extremely low levels of education spending by European standards (less than 2 percent of the national income) (see Ansell, 2010). The cases of Spain and Greece are the most characteristic examples concerning the political economy forces that may block educational expansion in society. In Spain, education until 1970 was under the domain of the Catholic Church, which ran fee-paying schools mostly for children of the upper and the upper-middle classes. During that period, Spanish education was extremely stratified, with only 3 percent of Spaniards attending secondary school (see McNair, 1984). Public spending on education increased substantially only after the democratisation and especially after the 1978 post-Franco Constitution that enshrined the right to secondary education and led to the development of a strong state school system (see Ansell, 2010). This reform did not take place during the authoritarian years, mostly because the Catholic Church – a core supporter

²⁴ It should be noted that we performed some ancillary regressions to investigate if *health and education* is a negative predictor of *market Gini*. We employed the specification in equation (2), and although the coefficient of the variable *health and education* is negative its effect is statistically insignificant.

of the Franco's regime – blocked such an expansion in mass education in several ways.²⁵ However, although public spending on education was considerably below its long-run trend under Franco's rule (Diebolt, 1999), public pension spending increased substantially during the same period, highlighting the preference of the ruler to large social security budgets (Mulligan et al., 2010).

Unlike Spain, Greece had an extended period of post-war democratic governments before the autocratic interlude of military rule (the so-called “Junta of the Colonels”) from 1967-1974. Three years before that dictatorial interval, the democratic government of centre-liberal Prime Minister George Papandreou attempted to introduce a modern education bill that would replace the artificial state language *Katharevousa* with the spoken Greek language in primary education. This educational reform aimed to increase the accessibility of the poor and the middle class – potential supporters of the centre-liberal party – to education. At the same time, compulsory education was extended to nine years (Kazamias, 1978). As a result of these reforms, public spending on education increased from 1.4 percent of national income in 1960 to 2.1 percent of national income in 1965 (Ansell, 2010). In 1967, Colonel George Papadopoulos seized control of government and established a military junta. The generals revoked the 1964 Educational Act and reduced the compulsory education requirement from nine to six years (see Gouvias, 1998). As a result of this counter reform, education spending decreased to 1.7 percent of the national income until the mid-1970s (see Ansell, 2010). However, compulsory education reverted to nine years, and public spending on education increased substantially (2-2.5 percent of national income) after the restoration of democracy in 1974.

Next we place the spotlight on a number of cases in the area of Latin America and especially in Brazil. According to a number of studies, authoritarian regimes in this region enacted and maintained social policy programs that distributed private benefits (i.e. targeted old-age pension programs) to groups of “critical supporters” (i.e. military, white-collar workers, some strategically situated blue-collar unions) according to the principles of state corporatism (see e.g., Haggard and Kaufman, 2008; Knutsen and Rasmussen, 2018). Some characteristic examples of targeted social policy design are the “bureaucratic authoritarian state” in Argentina during its first period (1966-1973) and the military regime in Uruguay. In Argentina, the broad social security system established by Peron was partly replaced by the bureaucratic authoritarian state, with a

²⁵ It must be noted that a similar reform that would increase the access to education was also attempted by the floundering Franco regime in the early 1970s. However, the political bargain between the dictatorial regime and the Catholic Church finally led to very little expansion (see O'Malley, 1995 for more details on this).

number of programs targeting the core supporters of the regime in the military, police, and civil services (see Mesa-Lago, 1978). Similarly, the military regime in Uruguay that came into power after 1973 did not implement deep structural changes in the previous social security system, but decided to cut budget expenditures and to restrict the generosity of social benefits mostly because of the fiscal pressures driven from the oil crisis. During the crisis, the regime allowed inflation to reduce the real value of pensions for broad categories of the population, while it took actions to support the pensions of a narrow group of agents (see Castiglioni, 2005).

For the purposes of our analysis, it is also interesting to investigate how these political regimes responded to electoral incentives in a few cases in which some kind of semi-competitive elections were allowed. To this end, we place the spotlight on the case of Brazil during the 1970s. Brazil's military dictatorship from 1964 to 1985 attached increasing importance to semi-competitive legislative elections as a means to weaken the opposition. More precisely, during the early 1960s, the authoritarian regime established a "pro-government" party *Aliança Renovadora Nacional* (ARENA) and at the same time encouraged the creation of an official opposition party the *Movimento Democrático Brasileiro* (MDB) led by "safe" political leaders. In 1971, the regime extended non-contributory old-age pensions to peasants and rural indigents. This development served the political purpose of pre-empting potential rural unrests, but also reflected specific electoral considerations. During the mid '70s, when more moderate military factions gained the control of government, clear-cut electoral motivations led to further expansion of social security programs.²⁶ A key feature of this strategy was the creation of strong patronage networks between the civilian politicians of the ARENA party and the potential voters in the rural, northeast areas (Weyland, 1996). It must be noted that, during the same period, the military regime did make some attempts to strengthen the financing of the educational system, but mostly by providing resources to the universities (instead of the primary and the secondary education) that were far more important for its core supporters (see Haggard and Kaufman, 2008). All in all, these case studies provide a view of how political institutions may affect the priorities of survival-oriented leaders on the allocation of the government budget.²⁷

²⁶ It is worth noting that, in the congressional elections that took place in 1974, MDB made a surprising show of political strength that threatened the political survival of the pro-government ARENA party, leading to substantial changes in the implemented policy.

²⁷ The cases of Poland and Hungary during the 1990s also present a number of interesting characteristics which are in line with our results. During the phase of democratic transition, national governments in both countries employed rather radical reforms to their social security systems by giving priority to the pension system. The general idea of the implemented reform was the drastic restriction of early retirements, which benefited specific groups of workers, and the establishment of a fully funded second pillar

4. Political institutions and private vs. public goods

4.1 Theoretical considerations

Motivated by the empirical and anecdotal evidence above, our final step in this study is to provide a theoretical connection between political institutions and the composition of government spending that is essential for the scope of redistribution. Following Olson (1993; 2000) and McGuire and Olson (1996), we argue that the encompassing interest of the ruler, for the productivity of the economy, is a crucial factor in explaining the different patterns of spending between dictatorships and democracies. According to this rationale, when authorities encompass the interest of the society as a whole, they direct a larger amount of revenues to public inputs (health and education), since the latter, through increases in productivity, increase the consumption of a large number of agents. In contrast, rulers characterized by lower encompassing interest prefer to keep public revenues under their discretion in order to increase their own consumption or “buy” political support through targeted transfers to specific groups of agents. According to Olson (1993), democratic leaders are usually characterized by a higher encompassing interest, since they are in need of a majority of voters that earns a significant share of the market income of the economy. This inevitably leads democratic governments to care about the performance of the private market and spend more on public inputs that enhance the productivity of the whole economy. In contrast, in non-democratic regimes in which rulers focus on a narrower group of supporters, spending on benefits like cash transfers can be a more effective way to stay in power. A simple theoretical framework along the lines of this argument is provided in the Online Appendix.

Following a similar rationale, the Selectorate Theory suggests that the size of the winning coalition increases as we move from autocratic to democratic regimes, inducing survival-oriented leaders to shift policy from targeted private goods (such as cash transfers, business, or export licenses) to public goods, in order to please the members of the winning coalition (see, e.g., Bueno de Mesquita et al., 2003). This is because democratically elected leaders lack sufficient resources to “bribe” all voters of the winning coalition with private goods in order to retain power. This can be achieved, though, through public goods that are broadly accessible to voters with inherent

that would ensure a more universal coverage for workers entering the labor market (see e.g. Haggard and Kaufman, 2008). As a result of these policies, the size of cash transfers (as a share of GDP) reduced substantially after democratization, although the coverage of the pension system became more homogeneous and universal.

economies of scale in their supply.²⁸ In contrast, in non-democratic regimes, the ruler depends on a much narrower group of supporters (i.e., small winning coalitions), making spending on private goods a much more preferable way to buy political support. Finally, along the same lines, Deacon (2009) suggests that, in dictatorial regimes in which political influence is more concentrated, a rational leader will spend the public budget mainly on transfers targeted to politically influential groups. In such an institutional context, spending on a non-exclusive public good is unwise, since much of the public good's benefits will spillover to non-influential outsiders.

Our results clearly indicate that dictatorial regimes, by relying more heavily on private goods such as targeted cash transfers, are more redistributive in comparison to democracies. However, a big issue in the relevant empirical literature is whether dictatorships direct these targeted transfers to the poorer segments of the society in a manner that reduces income inequality or in contrast allocate them to politically powerful elites (the so-called minimum winning coalition) enforcing in this way existed income inequality (see e.g. Acemoglu et al., 2015; Ross, 2006). Obviously, the available macro data fail to illuminate the precise targeting of transfers to specific groups of agents. However, we know that a large part of transfers consists of old-age pension programs that are directed to the elderly (see. e.g., Knutsen and Rasmussen, 2018), which, according to income distribution statistics, belong to the poorer segments of the population, even in developed economies (see, e.g., OECD, 2015). Following this rationale, we can infer that redistribution, which takes place through targeted cash transfers, is by its nature mostly progressive.

4.2 Additional empirical evidence

Our empirical findings in Section 3 suggest that democracies spend more on public inputs (such as health and education), whereas non-democratic regimes rely more heavily on targeted cash transfers. Here, we attempt to provide additional evidence that the differences in the pattern of government spending are driven by differences in the encompassing interest of the leaders to the performance of the private market. First, we explore the effect of the political regime on health and education outcomes. Starting from Lucas (1988), a large number of theoretical and empirical studies suggest that human capital is one of the main determinants of economic growth. So, if

²⁸ This is because public goods are non-rivalrous (the amount of the good available to be consumed is not diminished by the amount of people they consume it) and non-exclusive (once it is provided anyone can enjoy it). In contrast, private goods (such as cash transfers, business and export licenses, etc.) benefit some members of the society and not others.

democratic rulers are more concerned with the productivity of the whole economy, spending should be directed to investments in human capital and consequently to improvements of both health and education outcomes. To investigate the effect of political institutions on health outcomes, we employ two measures that are broadly employed in the relevant literature, namely *infant mortality* rate and *life expectancy* at birth. Moreover, to examine if increased spending on education is translated to higher levels of human capital, we focus on *primary and secondary school enrolment* rate. Data on health outcomes are obtained from WDI, whereas on education from Banks and Wilson (2015) aiming in both cases to maximise data availability. Our findings in panel A of Table 7 indicate a positive relationship between political institutions and health and education outcomes, which is broadly consistent with the relevant literature (see Brown, 1999; Besley and Kudamatsu, 2006; Acemoglu et al., 2018). It should be noted that these estimates rely entirely on the static FE specification of equation (2), since in many estimates our GMM instruments are proven to be weak.

[Insert Table 7, here]

Next, to further support the hypothesis that political institutions shape the priority of rulers to provide private or public goods, we attempt to measure the extent to which a country is open to market forces or in contrast prefers more heavily regulated markets (Gwartney and Lawson, 2002).²⁹ The rationale is that institutions promoting economic freedoms have been linked empirically to increased productivity (see e.g., Dawson, 1998). At the same time, increased market regulation generates a wide variety of privileged private goods (such as business licences, privileged access to credit etc.) that can be used to gain political support in institutional contexts characterized by small winning coalitions (see, e.g., Bueno de Mesquita, et al., 2003). To this end, we employ the *credit market regulations* and *starting a business* indices developed by the EFW database. The former quantifies the extent to which countries use a private banking system to allocate credit and refrain from controlling interest rates. The latter is designed to measure the extent to which regulations and bureaucratic procedures impose barriers to entry and generate prospects for rent-seeking activities. Both variables range from 0 to 10, where higher values

²⁹ It should be noted that other databases attempt to measure similar aspects of institutional quality, but we prefer the EFW since it provides better coverage.

indicate fewer regulations. As can be seen in panel B of Table 7, findings are in line with our theoretical priors as well as with a number of previous empirical studies highlighting the positive relationship between democratic institutions and economic freedom (see, e.g., De Haan and Sturm, 2003; Giavazzi and Tabellini, 2005; Giuliano et al., 2010). Moreover, our empirical findings indicate the tendency of non-democratic regimes to rely more heavily on market regulation so as to generate a variety of private goods that can be useful to dictatorial rulers in terms of political survival.

5. Conclusions

Our analysis examines the relationship between political institutions and effective fiscal redistribution, measured by the difference between Gini coefficients before and after taxes and transfers. Contrary to expectations, our empirical results suggest that dictatorial regimes are more redistributive than democracies. In an attempt to enlighten this puzzling finding, the second step of our analysis is to associate political institutions with the size and composition of fiscal policy. We find that dictatorial regimes rely more heavily on cash transfers that exhibit a direct impact on net inequality and consequently on the difference between market and net inequality (effective fiscal redistribution). On the other hand, democratic regimes direct a larger amount of resources to public inputs that may influence market income inequality but not the difference between market and net inequality *per se*. The present study suggests that the driving force behind the different pattern of government spending and effective fiscal redistribution is that democratic institutions lead survival-oriented leaders to care more for the productivity of the economy, since they are in need of a large winning coalition that earns a significant amount of its income in the private markets. In contrast, in non-democratic regimes in which the ruler focuses on a narrower group of supporters, spending on private goods can be a more effective way to remain in power.

We note, however, the complexity of investigating the influence of political institutions on income redistribution through fiscal policies. Obtained empirical findings call for a deeper understanding of the precise mechanisms that create the observed patterns. An advantageous field of future research would be to employ more detailed data at the regional level for different income/occupational groups of agents in countries that have undergone political transitions. Although institutional changes and potential outcomes are more limited than in a cross-country setting, by analysing in more depth the targeting of different (fiscal) policies towards regions or

groups of agents within regions, can reveal the distinct mechanism of income redistribution in greater detail.

Appendix A

Table A1. Definition of variables, data sources and descriptive statistics

Variable	Description	Obs.	Mean	SD	Min	Max	Source
<i>fiscal redistribution</i>	Difference between the Gini of incomes before (market Gini) and after (net Gini) taxes and transfers	828	5.192	5.573	-10.263	34.714	Solt (2009), Standardized World Income Inequality Database (SWIID)
<i>market Gini</i>	Gini coefficient before taxes and transfers	775	43.196	9.480	18.775	77.464	SWIID
<i>dictatorship (CGV)</i>	Dummy variable that takes the value of 1 when a political regime is characterized as dictatorial and 0 otherwise	1250	0.563	0.496	0.000	1.000	Cheibub et al. (2010)
<i>dictatorship (BMR)</i>	Dummy variable that takes the value of 1 when a political regime is characterized as dictatorial and 0 otherwise	1246	0.562	0.496	0.000	1.000	Boix et al. (2013)
<i>dictatorship (POLITY2)</i>	Index variable that ranges from 0 to 1, with higher values indicating more authoritarianism	1240	0.450	0.376	0.000	1.000	Marshall and Jaggers (2010)
<i>democracy abroad (CGV)</i>	Measure of regional democratic diffusion, based on CGV variable, as defined in Subsection 2.4.3	1258	0.420	0.176	0.043	0.872	Cheibub et al. (2010)
<i>democracy abroad (BMR)</i>	Measure of regional democratic diffusion, based on BMR variable, as defined in Subsection 2.4.3	1258	0.421	0.166	0.049	0.870	Boix et al. (2013)
<i>democracy abroad (POLITY2)</i>	Measure of regional democratic diffusion, based on POLITY2 index, as defined in Subsection 2.4.3	1258	0.518	0.146	0.113	0.847	Marshall and Jaggers (2010)
<i>total revenues (ICTD)</i>	General government total revenues as a percentage of GDP	825	22.524	11.016	1.015	89.078	ICTD Government Revenue Dataset (ICTD)
<i>tax revenues (ICTD)</i>	General government tax revenues as a percentage of GDP	846	16.051	8.590	0.487	54.475	ICTD
<i>total revenues (GDNGD)</i>	Central government total revenues as a percentage of GDP	475	25.566	10.935	1.785	77.397	Global Development Network Growth Database (GDNGD)
<i>tax revenues (GDNGD)</i>	Central government tax revenues as a percentage of GDP	476	20.707	9.775	0.833	47.326	GDNGD
<i>social services</i>	Central government social security and welfare affairs and services in cash and in kind as a percentage of GDP	434	10.927	9.289	0.000	40.292	GDNGD
<i>subsidies and transfers</i>	General government subsidies and social benefits in cash and in kind as a percentage of GDP	794	9.186	8.259	0.000	37.200	Economic Freedom of the World (EFW)
<i>cash transfers</i>	Central government cash transfer payments to households and non-profit institutions as a percentage of GDP	221	9.745	7.962	0.000	30.428	GDNGD
<i>health and education</i>	Central government health and education expenditures as a percentage of GDP	410	5.420	2.784	0.357	16.651	GDNGD
<i>GDP per capita</i>	Log of GDP per capita	1272	8.293	1.255	4.889	13.264	Penn World tables 8.0 (PWT)
<i>age dependency</i>	Population younger than 15 years or older than 64 as a percentage of working age population	1454	73.647	19.303	28.615	120.595	World Banks' World Development Indicators (WDI)
<i>openness</i>	International trade volume as a percentage of GDP	1141	68.215	45.596	4.983	430.358	WDI
<i>infant mortality</i>	Infant mortality rate per 1000 live births	1340	55.476	47.172	2.200	260.700	WDI
<i>life expectancy</i>	Life expectancy at birth (years)	1454	63.313	11.312	23.885	82.843	WDI
<i>primary and secondary enrolment</i>	Primary and secondary school enrolment per capita	1278	17.799	5.939	1.580	36.610	Banks and Wilson (2015)
<i>credit market regulations</i>	Index that measures the extent to which countries use a private banking system to allocate credit to private parties and refrain from controlling interest rates. Higher values indicate fewer regulations.	959	6.790	2.645	0.000	10.000	EFW
<i>starting a business</i>	Index that measures the extent to which regulations and bureaucratic procedures restrain entry and reduce competition. Higher values indicate fewer regulations.	410	7.834	1.768	0.000	9.964	EFW

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Table 1. Overview of empirical studies of the relationship between democracy and fiscal policy

Study	Influence on	Political variable(s)	Period	Sample	Method	Basic Findings
Acemoglu et al. (2015)	Tax revenues (% GDP)	Dichotomous measure of democracy	1960-2010	128 countries	Fixed effects OLS panel estimates	Positive and significant effect of democracy on tax revenues
Aidt et al. (2006)	<ul style="list-style-type: none"> Government spending (% GDP) Spending on defines, general administration, the judiciary and the police (% GDP) Spending on economic services, transportation and communication (% GDP) Spending on health, public housing, education and social security (% GDP) 	<ul style="list-style-type: none"> Economic franchise (the size of the electorate in percentage of its reference age group) Female franchise (dummy variable that equals 1 after women enfranchisement and equals 0 otherwise) 	1830-1938	12 European countries	Fixed effects OLS panel estimates	Extension of the franchise exhibits a positive association with total government spending mainly by increasing spending on infrastructure and internal security. Female suffrage exhibits a weak positive association with spending on health, education and welfare
Aidt and Jensen (2009a)	Probability of adopting the income tax	Suffrage (percentage of adult males who could vote)	1815-1939	17 countries	Logit and 2SLS panel estimates	Extension of the franchise at first reduced the probability of adoption of the income tax, but eventually as universal suffrage was approached increased the likelihood
Aidt and Jensen (2009b)	<ul style="list-style-type: none"> Government spending/taxes (% GDP) Direct taxes (% tax revenues) Revenues from customs (% tax revenues) Market taxes (% tax revenues) 	Suffrage (percentage of adult males who could vote)	1860-1938	10 European countries	Fixed effects OLS and 2SLS panel estimates	Extension of the franchise exhibits a positive association with total government spending and taxation. Moreover, the share of direct taxes is positively affected by franchise extension but only when tax collection costs are below a given threshold
Aidt and Jensen (2013)	<ul style="list-style-type: none"> Government revenue/spending per capita 	Suffrage (percentage of adult males who could vote)	1820-1913	9 European countries	Fixed effects OLS and 2SLS panel estimates	Franchise extension exhibits a U-shaped association with revenue per capita and a positive association with spending per capita
Ansell (2010)	<ul style="list-style-type: none"> Spending on education (% GDP/% spending) 	Polity index	1960-2000	110 countries	Fixed effects OLS panel estimates	Positive and significant association between democracy and education spending
Boix (2003)	<ul style="list-style-type: none"> Government revenues (% GDP) Government consumption (% GDP) Total expenditure (% GDP) Subsidies and transfers (% GDP) Wages and salaries (%GDP) 	<ul style="list-style-type: none"> Dichotomous measure of democracy Level of turnout 	1950-1999	65 countries	Fixed effects OLS panel estimates	Positive association between democratic regime and the size of the public sector and social spending, though conditional on the level of development
Kaufman and Segura-Ubiergo (2001)	<ul style="list-style-type: none"> Spending on social security (per capita/% GDP/% spending) Spending on health and education (per capita/% GDP/% spending) 	Dichotomous measure of democracy	1973-1997	14 Latin American countries	Fixed effects error-correction panel estimates	Democracy is positively correlated with government expenditure on health and education, but negatively with spending on social security
Lindert (1994)	<ul style="list-style-type: none"> Spending on social transfers (% GNP) 	<ul style="list-style-type: none"> Dichotomous measure of democracy 	1880-1930	21 countries	Tobit panel estimates	Positive association between women vote and voter turnout with social spending

	<ul style="list-style-type: none"> • Spending on welfare and unemployment (% GNP) • spending on health (% GNP) • Spending on pensions (% GNP) 	<ul style="list-style-type: none"> • Female franchise dummy • Voter turnout rate • Executive turnover (the number of times the chief executive post was relinquished to someone not dependent on the incumbent) 				
Mulligan et al. (2004)	<ul style="list-style-type: none"> • Government consumption (% GDP) • Spending on education (% GDP) • Spending on social security (% GDP) • Total revenue (% GDP) • Corporate tax rate • Personal income tax flatness (ratio of the economy-wide average income tax rate to the top marginal income tax rate) • Payroll tax capped 	Democracy index (0-1, a value of 1 identifies the maximum level of democracy)	1960-1990	131 countries	OLS cross-section estimates	Some evidence that democratic countries tend to have “flatter” income taxes
Mulligan et al. (2010)	<ul style="list-style-type: none"> • Spending on social security (% GDP) • Social security benefit formulas • Payroll taxation 	Democracy index (0-1, a value of 1 identifies the maximum level of democracy)	1960-1990	90 countries	OLS cross-section estimates	If anything the democracy index is negatively associated with social security spending. Moreover, it is positively associated with higher payroll taxation (and especially with the payroll tax burdens paid by the employees).
Profeta et al. (2013)	<ul style="list-style-type: none"> • Tax revenue (% GDP) • Personal/Corporate income taxes (% GDP) • Direct/Indirect/Property/Trade taxes (% GDP) • Social security contributions (% GDP) • Government spending (% GDP) • General public services (% GDP) • Defence/Health/Education/Social protection/Public order spending (% GDP) 	<ul style="list-style-type: none"> • Polity index • Civil liberties index 	1990-2005	38 developing countries in three areas (South-East Asia, Latin America and European Union)	Fixed effects OLS panel estimates	The strength of democratic institutions and the protection of civil liberties are not significantly correlated with tax revenues and tax composition. A similar result applies to public spending.

Table 2. 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	1.311*** (0.497)	1.617*** (0.534)	2.538*** (0.867)	1.016*** (0.345)	0.982*** (0.343)	1.079*** (0.351)	1.040*** (0.355)	1.225** (0.487)	1.126** (0.542)
<i>fiscal redistribution_{t-1}</i>				0.477*** (0.067)	0.502*** (0.079)	0.474*** (0.068)	0.500*** (0.082)	0.474*** (0.069)	0.502*** (0.086)
GDP per capita	1.415*** (0.384)	1.517*** (0.384)	1.393*** (0.375)	-0.085 (0.362)	-0.122 (0.376)	-0.039 (0.365)	-0.082 (0.386)	-0.130 (0.369)	-0.171 (0.388)
age dependency	0.108*** (0.024)	0.107*** (0.024)	0.109*** (0.024)	0.070*** (0.016)	0.067*** (0.014)	0.069*** (0.016)	0.066*** (0.014)	0.070*** (0.015)	0.067*** (0.014)
openness	0.011* (0.006)	0.010* (0.006)	0.009 (0.006)	-0.002 (0.006)	-0.002 (0.006)	-0.002 (0.006)	-0.002 (0.006)	-0.002 (0.006)	-0.002 (0.006)
R2	0.094	0.105	0.105	0.367		0.369		0.364	
Observations	743	740	739	652	521	649	519	649	520
Number of countries	138	137	136	131	122	130	121	129	121
Number of instruments					49		49		49
Hansen (p-value)					0.290		0.332		0.291
AR(2) (p-value)					0.300		0.284		0.328

Notes: In all specifications we control for a full set of country and year fixed effects. In the GMM estimates 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 3. Political regime and fiscal redistribution: outliers and income inequality estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Political variable:	FE	FE	FE	GMM	GMM	GMM
	CGV	BMR	POLITY2	CGV	BMR	POLITY2
Panel A: Dropping most noisy inequality data						
dictatorship	1.337*** (0.465)	1.503*** (0.513)	2.519*** (0.813)	0.629** (0.320)	0.743** (0.374)	1.157** (0.517)
R2	0.104	0.113	0.118			
Observations	669	666	666	460	458	458
Number of countries	135	134	134	116	115	115
Number of instruments				49	49	49
Hansen (p-value)				0.329	0.375	0.296
AR(2) (p-value)				0.286	0.272	0.296
Panel B: Dropping Sub-Saharan Africa countries						
dictatorship	1.504** (0.690)	1.884*** (0.687)	3.031** (1.181)	1.177** (0.476)	1.308*** (0.473)	1.574** (0.735)
R2	0.115	0.128	0.129			
Observations	584	581	580	432	430	431
Number of countries	98	97	96	92	91	91
Number of instruments				49	49	49
Hansen (p-value)				0.219	0.275	0.281
AR(2) (p-value)				0.287	0.264	0.321
Panel C: Reducing sample between 1980 and 2010						
dictatorship	1.307** (0.653)	1.620** (0.648)	2.626** (1.087)	0.739** (0.318)	0.879*** (0.320)	1.090* (0.634)
R2	0.073	0.086	0.084			
Observations	609	606	604	438	436	436
Number of countries	138	137	136	122	121	121
Number of instruments				39	39	39
Hansen (p-value)				0.211	0.223	0.235
AR(2) (p-value)				0.341	0.325	0.377
Panel D: Dropping Soviet and Soviet satellite countries						
dictatorship	1.340** (0.530)	1.634*** (0.566)	2.492*** (0.922)	1.000*** (0.366)	1.014*** (0.366)	1.156** (0.483)
R2	0.106	0.114	0.113			
Observations	653	653	652	459	459	460
Number of countries	110	110	109	97	97	97
Number of instruments				49	49	49
Hansen (p-value)				0.389	0.383	0.381
AR(2) (p-value)				0.259	0.247	0.283
Panel E: Testing for outliers						
dictatorship	0.716** (0.313)	0.945** (0.399)	1.048* (0.551)	0.808** (0.333)	0.918** (0.380)	1.069* (0.585)
R2	0.200	0.207	0.198			
Observations	555	558	563	440	438	444
Number of countries	114	114	114	109	108	109
Number of instruments				49	49	49
Hansen (p-value)				0.336	0.338	0.394
AR(2) (p-value)				0.702	0.582	0.711

Notes: In all specifications we control for a full set of country and year fixed effects. In Panel A we repeat the estimates of Table 2 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 2 all Sub-Saharan Africa countries, whereas in Panel C we drop the first two decades of our sample. In Panel D we drop from our sample Soviet and Soviet satellite countries. Finally, in Panel E we remove countries with standardized residuals above 1.96 or below -1.96. All models control for GDP per capita, age dependency and openness, but these coefficients are not reported to save space. In the GMM estimates 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. Political regime and fiscal redistribution: market Gini 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	1.412** (0.585)	1.693*** (0.600)	2.493** (0.985)	1.112*** (0.367)	1.050*** (0.360)	1.315** (0.547)
<i>fiscal redistribution_{t-1}</i>				0.551*** (0.090)	0.550*** (0.095)	0.551*** (0.097)
market Gini	0.084*** (0.024)	0.088*** (0.024)	0.080*** (0.024)	-0.068 (0.043)	-0.064 (0.044)	-0.067 (0.045)
GDP per capita	0.428 (0.405)	0.523 (0.408)	0.380 (0.403)	-0.044 (0.350)	-0.005 (0.358)	-0.079 (0.356)
age dependency	0.113*** (0.029)	0.111*** (0.029)	0.115*** (0.029)	0.074*** (0.016)	0.073*** (0.016)	0.073*** (0.015)
openness	-0.003 (0.007)	-0.004 (0.007)	-0.004 (0.007)	0.001 (0.006)	0.000 (0.006)	-0.000 (0.006)
R2	0.138	0.149	0.144			
Observations	652	649	649	521	519	520
Number of countries	131	130	129	122	121	121
Number of instruments				50	50	50
Hansen (p-value)				0.305	0.337	0.303
AR(2) (p-value)				0.296	0.274	0.326

Notes: see Table 2

Table 5. Political regime and fiscal redistribution: 2SLS results

	(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	2.501** (1.178)	2.673** (1.177)	2.378 (1.659)	3.517*** (1.272)	3.821*** (1.156)	2.729* (1.590)
market Gini				0.064** (0.027)	0.072*** (0.025)	0.073*** (0.027)
GDP per capita	1.346*** (0.432)	1.511*** (0.451)	1.245*** (0.436)	0.341 (0.516)	0.536 (0.554)	0.077 (0.458)
age dependency	0.107*** (0.023)	0.104*** (0.024)	0.110*** (0.024)	0.099*** (0.028)	0.094*** (0.028)	0.106*** (0.028)
openness	0.007 (0.007)	0.006 (0.007)	0.004 (0.007)	-0.002 (0.007)	-0.003 (0.007)	-0.005 (0.007)
	First-Stage Results					
democracy abroad	-0.906** (0.413)	-0.268 (0.376)	-0.825** (0.386)	-0.932** (0.439)	-0.436 (0.464)	-0.966** (0.457)
dictatorship _{t-1}	0.280*** (0.048)	0.307*** (0.053)	0.320*** (0.050)	0.282*** (0.052)	0.301*** (0.056)	0.304*** (0.051)
F-stat	23.556	17.002	24.621	17.791	14.599	21.086
Overidentification test	0.402	0.151	0.462	0.621	0.159	0.765
Observations	697	694	692	617	614	615
No. of Countries	129	128	127	122	121	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 6A. 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:	<i>total revenues (ICTD)</i>			<i>tax revenues (ICTD)</i>			<i>total revenues (GDNGD)</i>			<i>tax revenues (GDNGD)</i>		
dictatorship	-0.214 (0.602)	0.174 (0.517)	-0.537 (0.968)	-0.313 (0.448)	-0.062 (0.431)	-0.155 (0.658)	0.529 (0.952)	0.444 (0.894)	1.094 (1.410)	0.066 (0.836)	-0.494 (0.761)	-0.056 (1.174)
GDP per capita	3.561*** (1.287)	3.546*** (1.289)	3.668*** (1.329)	0.463 (0.561)	0.435 (0.561)	0.452 (0.573)	6.538*** (1.107)	6.538*** (1.117)	6.504*** (1.086)	5.163** (1.976)	5.095** (1.984)	5.155*** (1.959)
age dependency	0.025 (0.056)	0.023 (0.056)	0.026 (0.056)	-0.039 (0.025)	-0.041 (0.025)	-0.040 (0.025)	0.051 (0.044)	0.051 (0.044)	0.050 (0.045)	0.042 (0.043)	0.041 (0.043)	0.042 (0.043)
openness	0.026* (0.016)	0.027* (0.016)	0.030* (0.015)	0.006 (0.010)	0.007 (0.010)	0.009 (0.009)	0.063** (0.026)	0.063** (0.026)	0.063** (0.026)	0.030 (0.026)	0.029 (0.026)	0.030 (0.026)
R2	0.138	0.137	0.144	0.116	0.115	0.116	0.253	0.253	0.254	0.215	0.216	0.215
Observations	658	654	651	705	701	698	451	451	451	452	452	452
Number of countries	133	132	132	133	132	132	103	103	103	103	103	103

Notes: In all specifications we control for a full set of country and year fixed effects. All models are estimated according to equation (6), taking five year averages for all variables except for the variable *dictatorship* that we use its value at time t of each five-year period (t, t+4) of our sample. 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 6B. 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:	<i>social services</i>			<i>subsidies and transfers</i>			<i>cash transfers</i>			<i>health and education</i>		
dictatorship	-0.607 (0.972)	-0.717 (0.929)	-0.949 (1.338)	0.433 (0.564)	0.630 (0.555)	0.649 (1.046)	1.740 (1.215)	1.900* (1.140)	5.354** (2.205)	-0.681*** (0.246)	-0.750*** (0.254)	-1.063** (0.422)
GDP per capita	-0.283 (1.628)	-0.300 (1.624)	-0.180 (1.591)	1.543* (0.837)	1.642** (0.819)	1.574* (0.830)	0.020 (1.818)	0.104 (1.790)	-0.851 (1.586)	-0.479 (0.511)	-0.487 (0.506)	-0.429 (0.525)
age dependency	0.012 (0.031)	0.012 (0.031)	0.016 (0.031)	0.016 (0.029)	0.017 (0.028)	0.017 (0.029)	-0.065 (0.085)	-0.077 (0.083)	-0.108 (0.087)	-0.034* (0.018)	-0.035** (0.017)	-0.033* (0.018)
openness	-0.008 (0.015)	-0.008 (0.015)	-0.008 (0.015)	-0.008 (0.009)	-0.009 (0.009)	-0.009 (0.009)	-0.008 (0.014)	-0.010 (0.014)	-0.007 (0.014)	-0.004 (0.009)	-0.005 (0.010)	-0.004 (0.009)
R2	0.108	0.110	0.109	0.117	0.118	0.116	0.149	0.163	0.238	0.112	0.118	0.116
Observations	411	411	411	769	768	768	215	215	215	390	390	390
Number of countries	101	101	101	127	126	126	80	80	80	99	99	99

Notes: In all specifications we control for a full set of country and year fixed effects. In columns (1)-(3) and (7)-(12) we estimate equation (6), taking five year averages for all variables except for the variable *dictatorship* that we use its value at time 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 2000, we use for the dependent variable only the last observation for each subperiod of our sample. 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 7: Political regime, health-education outcomes and market regulation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	FE	FE	FE	FE	FE	FE	FE	FE	FE
Political variable:	CGV	BMR	POLITY2	CGV	BMR	POLITY2	CGV	BMR	POLITY2

Panel A: Political regime and health and education outcomes

Dependent variable:	<i>infant mortality</i>			<i>life expectancy</i>			<i>primary and secondary enrolment</i>		
dictatorship	8.985*** (2.408)	8.293*** (2.280)	17.295*** (4.122)	-1.328*** (0.427)	-1.178** (0.463)	-2.389*** (0.895)	-1.031* (0.556)	-0.970** (0.480)	-2.332*** (0.816)
R2	0.689	0.688	0.696	0.698	0.697	0.700	0.281	0.281	0.288
Observations	1090	1085	1082	1107	1102	1099	1103	1098	1095
Number of countries	146	145	145	146	145	145	145	144	144

Panel B: Political regime and market regulations

Dependent variable:	credit market regulations			starting a business		
dictatorship	-0.501* (0.262)	-0.260 (0.257)	-1.040** (0.416)	-0.342** (0.146)	-0.322* (0.186)	-0.987*** (0.337)
R2	0.372	0.367	0.375	0.633	0.633	0.637
Observations	892	890	889	402	400	400
Number of countries	132	131	131	132	131	131

Notes: In all specifications we control for a full set of country and year fixed effects. All estimations are based on equation (2). All models control for GDP per capita, age dependency and openness, but these coefficients are not reported to save space. 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.