

The following provides a brief overview of my dissertation, intended as context for the cross-national chapter, which is attached below. The talk will cover material both from the chapter as well as from other parts of the dissertation. This is a work in progress prepared for the Stanford Comparative Politics Workshop. Please do not cite. Comments are very welcome.

Dissertation Project: The Redistributive Consequences of Democratization

“...and where the poor rule, that is a democracy.”

-Aristotle (Politics: Book III, Part VIII)

In my dissertation, *The Redistributive Consequences of Democratization*, I ask whether transitioning to democracy leads to fiscal redistribution from the wealthy to the poor. The redistributive hypothesis traces its intellectual roots through Aristotle and Tocqueville, and is today the dominant view in political science and economics. In its starkest form it implies that electoral democracy is, in essence, a political mechanism designed for redistributing wealth in a credible manner. The result is formalized in Acemoglu and Robinson (2006) and Boix (2003). Contrary to the redistributive view, I argue that democratization is typically the result of intra-elite conflict and that its redistributive effects are therefore endogenous to the institutions within which elites operate. This accords with the recent framework of open- and limited-access orders developed by North, Wallis, and Weingast (2009). Empirically, I find that democratization often exacerbates rather than ameliorates existing inequalities. I test my theory using a time-series approach, analyzing the effects of regime transitions on redistributive outcomes both in large-N statistical analyses and in a detailed study of the canonical case of Argentina for which I conducted ten months of field work in Buenos Aires in 2007.

My argument for the endogeneity of democracy and pre-democratic elite institutions stems from the following observation. The central assumption of the redistributive hypothesis is that democracy constitutes a credible promise of redistribution to the poor – today and in the future. This assumption is empirically unjustified. Looking around the world, we see that even in the most advanced democracies, individual preferences are aggregated via political institutions that stack the deck against strongly redistributive policies. Examples include malapportioned upper chambers, electoral rules that inhibit the entry of new parties, and decentralization of taxes and expenditures as in the case of K-12 public schooling in the United States. Moreover, turnout rates among the poor are exceedingly low, a fact which has led to compulsory voting laws from Argentina to Australia. If the masses wanted a credible commitment to future redistribution, would they really have accepted elections paired with these types of institutions as the mechanism?

A close reading of political histories outside of Western Europe highlights the fact that most transitions to democracy did not occur under the threat of revolution by the poor. Rather, elite out-groups strategically reformed existing institutions to their own benefit by introducing selective elements of popular democracy. Democratic reforms enabled mass political participation but intentionally established electoral, legislative, and party-level barriers to undermine political responsiveness to voter demands. Voters, many of whom had been incorporated into pre-democratic politics through extensive patronage networks remained enmeshed in machine organizations after the transition to full democracy. Thus, formal political institutions, informal party organizations, and voter behavior constituted a durable political equilibrium, the outcomes of which resisted change even after democratization.

I begin by testing the redistributive hypothesis in a cross-national setting using the best available data on government spending and income inequality from sources including the Penn World Table, the U.N. World Income Inequality Database, and the World Bank's World Development Indicators. I estimate the effects of democratization on redistribution in virtually every instance of regime transition around the world from 1950 to the present using the autodistributed lag regression framework with fixed effects. This estimation framework is sensitive to the dynamic, within-country processes implied by the theories. I find that democratization neither increases nor decreases redistribution from the wealthy to the poor in the full sample of countries. To the contrary, democratization tends to reduce redistributive spending in unequal countries – those in which the poor would expect to see the greatest redistributive gains. Moreover, I find that net income inequality tends to rise following democratization in all but the poorest countries.

I then turn to a detailed study of Argentina, 1880-1946, in which I analyze the evolution of redistributive policies across two regime transitions: the Sáenz Peña electoral reform of 1912 and the fall of democracy to a military regime in 1931. If the redistributive hypothesis ought to hold in any context, this is it: Argentina in this period was wealthy, unequal, and its population was among the best educated in Latin America. I spent ten months in Argentina in 2007 in libraries and archives including the *Archivo General de la Nación*, the *Centro de Documentación e Información* in the Ministry of the Economy, the *Biblioteca Nacional de Maestros* at the Ministry of Education and at various public libraries and private collections in Buenos Aires and La Plata to gather evidence. I construct historical time-series measures of the magnitude and composition of government expenditures, the sources of government revenue and progressivity of the tax structure, and the development of public elementary schools in Argentina between 1880 and 1946. The data include national and subnational observations. I find that the period of democratic rule, 1912-1930, did not usher in redistributive taxation or spending. In fact, the oligarchic government spearheaded the most significant investments in public primary schooling starting in the 19th century. Moreover, the military regime that toppled democracy in 1931 introduced Argentina's first income tax and greatly increased the progressivity of the entire revenue structure.

Fourth, I offer a simple formal model of elite-led democratization, the assumptions of which are based on the actual conditions in Argentina in this era. Finally, using a third original data set in which I geo-code every federal expenditure from 1880 to 1946, I show that the adoption of elections shifted the geographic targeting of federal funds towards the newly elected coalition, but that these expenditures did not, in general, shift economic resources to newly empowered voters. These results confirm the redistributive predictions of the empirical model.

Chapter 2: Empirical Tests of the Redistributive Hypothesis: Democratization and Redistribution Around the World Since 1950

Introduction

In this chapter I take a large-N approach to testing the redistributive hypothesis. I estimate the effects of democratization on redistribution in virtually every instance of regime transition around the world from 1950 to the present using a regression framework that is sensitive to the dynamic, within-country processes implied by the theory. In particular, I test whether democratization increases the size of the public sector, whether such growth is especially pronounced following regime transitions in highly unequal countries, and whether democratization reduces net income inequality.

I focus on the post-WWII era, for which relatively high-quality data are readily available across a wide range of countries. Historical coverage of this era allows us to establish baseline levels and subsequent changes in the outcomes of interest during the third wave of democratization (Huntington 1991). We also observe the slide back towards non-democracy in a number of countries throughout the 1950s and 1960s in what Huntington calls the second counter-wave, in which we would expect to see the regressive effects of transitions to non-democratic forms of government.

Contrary to the elegant and intuitive theory, I find that the weight of evidence stands against the redistributive hypothesis. In the full sample of regime transitions, democratization does not lead to redistribution. Where initial inequality is high, in fact, democratization either causes or is accompanied by a *reduction* of redistributive government spending. Moreover, democratization either causes or is accompanied by *growing inequality* as measured by post-tax and transfer income Ginis.

The chapter proceeds as follows: in the first section I make explicit the testable implications of the redistributive hypothesis. Next, I review the empirical findings of existing studies, highlighting both strengths and weaknesses of the estimation strategies employed in each. Third, I explain the empirical estimation strategy I adopt in this chapter and describe the family of autodistributed lag models I employ using dynamic panel data. I test the main empirical implications of the redistributive hypothesis in sections four and five. I conclude with a discussion in section six, in which I offer three hypotheses that are consistent with the findings of this chapter as alternatives to the redistributive hypothesis.

1 Empirical Implications of the Redistributive Hypothesis

What kind of evidence would vindicate the redistributive hypothesis? Would a finding that democratization increases redistributive spending, on average, by one percent constitute sufficient evidence? How about five percent? In order to test the redistributive hypothesis we need a theoretical standard against which to measure the extent of change in taxes and transfers actually observed following democratization. This requires that we specify a model of taxes and transfers under both non-democratic and democratic regimes. In the models of Acemoglu and Robinson (2000) and Boix (2003), taxes are set according to the linear tax model (Meltzer and Richard 1981; Romer 1975) in which the position of the median voter relative to mean income in a labor economy determines the tax rate and thus the share of national income redistributed by the government (Meltzer and Richard 1981, pg 920). Under these assumptions the theoretical expectation is determined by the change in the relative position of the decisive voter due to regime transition. Determining the expected level of redistribution

would require, therefore, that we observe the income distribution and extent of suffrage both before and after democratization.

The real world is obviously not so clean. For one, the redistributive effects of democratization are expected to grow once we relax the assumption of a linear tax and equal lump-sum transfers (Acemoglu and Robinson 2006, pp. 107-109). The degree to which targeting is a factor increases the expected regressivity of taxes and transfers under autocracy and increases the expected level of progressivity under democracy, yet we lack a theory of how targeting is determined.

Second, as Acemoglu and Robinson (2006) have argued, redistribution under autocracy may exceed the equilibrium level preferred by elites due to the threat of revolution from below, which they call the *revolution constraint* (pg 120-123). Even if we could, in principle, identify cases in which the revolution constraint is binding it would be nearly impossible to set an *ex ante* expectation for the rate of redistribution required to prevent a revolt.

Third, contrary to a fundamental assumption in the above-mentioned models, there is wide conceptual space between expanding suffrage rights and democratization. The working assumption in political economy models is that autocracy is an institution in which voting rights are limited to elites who operate in an otherwise democratic government. In this conceptualization the only difference between democracy and non-democracy is the extent of suffrage. It is equally (if not more) plausible, however, to assume that elites do not set impersonal policies through majority rule preceding the transition to full democracy. For example, it may be the case that specific individuals, families, or social groups receive targeted goods, benefits and rights based on personal networks (cf. Haber et al. 2003). The expected rate of redistribution under autocracy is not at all clear on this reading, making it even more difficult to establish an expectation for the degree of change caused by democratization.

In short, the expected change in redistribution due to democratization depends on the extent of suffrage and income distribution under various autocratic regimes, the extent to which taxes and transfers can be targeted under both autocratic and democratic regimes, whether the revolution constraint is binding and the level of additional redistribution it induces, and the mode of redistribution under autocracy. Given a fully specified theory of how each of these factors determines the decision over redistribution we could, in principle, set an exact theoretical expectation. In the absence of such theory and data, we are relegated to testing several qualitative empirical implications of the model, which I proceed to do in this chapter. First, democratization increases redistributive government spending as a share of total economic productivity. Second, there is a positive correlation between the magnitude of this increase and the extent of pre-existing inequality¹. Third, democratization leads to a *ceteris paribus* decrease in net income inequality.

2 Empirical Findings in the Comparative Politics Literature

A number of empirical papers offer *direct* tests of whether democracy generates redistribution. In these papers the independent variable is a measure of democracy and the dependent variable is a

¹ Conditional on a successful transition to democracy. Both Acemoglu and Robinson (2006) and Boix (2003) argue that the likelihood of democratization is negatively correlated with initial levels of inequality, since elites have more of an incentive to repress the poor in unequal societies. For that very reason, however, we should expect to observe more redistribution in such countries in the cases that democratization is indeed successful.

measure of taxes, transfers or services. A related literature offers *indirect* tests of the redistributive hypothesis by studying the factors that explain variation in redistribution *within* democracies. Lindert (1996), for example, asks whether income inequality (pre-tax and transfer) increases social spending. A positive correlation between pre-tax and transfer inequality and social spending levels would lend support to the redistributive hypothesis, whereas a negative (or zero) correlation would undermine the claim that democracies are a political mechanism for wealth redistribution. In this section, I assess the empirical evidence for the redistributive hypothesis, both direct and indirect, noting strengths and limitations of the findings along the way.

Direct Tests

Some of the strongest evidence for the redistributive hypothesis comes from a study of enfranchisement and the size of government in the U.S. states. Husted and Kenny (1997) show that the elimination of poll taxes and literacy tests in Southern states through the 24th Amendment of 1964 and the Voting Rights Act of 1965 increased voter registration among the poor, led to a poorer median voter and increased state-level welfare spending. Interestingly, they find that non-welfare services did not rise, a fact which they attribute to conflicting income and substitution effects. The econometric strategy is exemplary. First, it is plausible that enfranchisement by the federal government is exogenous to variation in state-level spending. Second, by including state and year fixed effects, Husted and Kenny ensure that their estimates are driven by within-state variation due to enfranchisement while controlling for country-wide temporal trends in spending levels. Two caveats are in order, however, in the context of the current study. First, it remains unclear whether the effect generalizes from this particular episode in U.S. history to other countries and eras. Second, voting rights in the U.S. states were enforced by the federal government, typically against the wishes of state-level politicians. The redistributive hypothesis, in contrast, is a theory about regime transitions in sovereign states, in which both democratization and its enforcement are decided from within.

Turning to a comparative context, Boix (2001, 2003) offers the most wide-ranging evidence in defense of the redistributive hypothesis. He shows a conditional positive correlation between democracy and current receipts of general government as a percentage of GDP in a pooled sample of 65 countries, 1950-1990. Boix finds that democracies redistribute more than autocracies beyond a certain threshold of economic development, a gap that increases as per capita GDP rises. The poorest democracies, in contrast, collect less revenue than do the poorest authoritarian states (three percentage points of GDP lower at \$250 GDP per capita, constant 1985 dollars (Boix 2001, pg 9)). Boix argues that the conditional effect is due to the fact limited demand for public services at low levels of development (pg 9), a position consistent with the model in Husted and Kenny (1997).

Yet, in the context of the redistributive hypothesis, in which democracy emerges under the threat of revolution, it should be clear that the choice of welfare transfers versus government services is endogenous to the level of development. Democratization in poor countries should generate redistribution in the form of pure transfers, while democratization at higher levels of development should generate both transfers and government services in accordance with the changing preferences of enfranchised voters. The inconsistency of this conditional result with the logic of redistributive democracy is underscored by the fact that Boix (2003) finds a similar relationship with transfers and subsidies as the dependent variable on a different pooled sample of countries, 1970-99 (pp. 197-198, Table 5.5 and Figure 5.3). A theoretical rationalization for why poor citizens would not want transfers (in contrast to government services) stretches the imagination.

In addition to the lack of fit with Boix's theory, the correlational results are based on pooled cross-national regressions, in which unobserved between-country heterogeneity is almost certainly biasing the estimates. Given the availability of panel data there is simply no reason to shy away from the use of fixed effects and related techniques that bring us closer to a causal estimation framework. Indeed the analysis in Adserà and Boix (2002) moves in this direction. They estimate regressions showing a positive correlation between democracy (interacted with trade openness) and the size of the public sector as measured by current receipts of general government for a panel of 65 countries, 1950-1990. Upon including fixed effects to allow for country-specific intercepts and a lagged dependent variable to correct for serial correlation, however, the statistical significance of the Democracy coefficient disappears, and its magnitude is greatly attenuated².

Addressing the concern that democracies are *less* able than autocracies to extract tax revenue, Cheibub (1998) shows that democratization is not correlated with a reduction of central government revenue in a sample of 108 countries, 1970-90. Cheibub includes country fixed effects and uses a Heckman selection model to account for selective transitions between regime types. Since it is not obvious that the selection process is adequately captured by his econometric model, it is worth noting that in the fixed effects model and assuming an AR1 autocorrelation process, Cheibub finds that democracies tax 1% of GDP more than dictatorships – a result driven by within-country variation. Depending on the extent to which we believe the selection-model results, then, democratization leads to an increase in taxation of between 0% and 1%. Whether these revenues are then transferred to the poor is unknown.

Several cross-sectional studies show a negative correlation between democracy and redistribution. Mulligan et al. (2004) shows that democracy is not correlated with a number of spending variables and is *negatively* correlated with income redistribution as measured by flatness of the income tax in a cross-section of roughly 100 countries, with observations averaged over various periods since 1960. On the other hand, they find that democracy is correlated with higher levels of civil liberties, lower levels of torture, and greater press freedom, implying that the essential characteristic of democracies is to protect rights rather than redistribute social resources. The result is intriguing, but, as already mentioned is based on cross-national evidence.

Reaching further back in history, Lindert (2003, 1994) finds a mixed – but largely negative – correlation between democracy and social spending in a sample of 21 countries (30 in the 1994 article) using decade averages, 1880-1930. At levels of voter turnout under 70%, social spending in democracies is *lower* than in the average non-democracy, but as turnout surpasses 70%, democracies overtake autocracies in social spending. At the sample maximum of 81.7% turnout, social spending is only 0.5% of GNP higher in democracies than in non-democracies. Furthermore, Lindert notes that “elite dominated democracies” like Britain, the Netherlands, Norway, and Sweden spent less than the average non-democracy (pg 179) and that, on average, democracies spend no more than non-democracies (fn 15 on pg 179). Again, the regressions are pooled cross-national estimates, but the data are surprising given Lindert's overall thesis. The estimated coefficients imply that a counterfactual autocratic United States would spend more on social services than it currently does under a democratic government at turnout levels typical of national elections.

² Adserà and Boix note that the joint significance of trade openness, democracy and the interaction term is significant at the 5% level. But given that the trade openness variable is individually significant at the 10% level and neither of the other two terms is individually significant, it is very likely that the joint significance is driven by trade openness rather than by democracy. Unfortunately, they don't run a specification with an uninteracted Democracy variable, because their theory postulates an interactive effect.

Turning to specific government services, Stasavage (2005) finds that the introduction of multiparty competition increases education spending on primary schools in a panel of African countries, 1980-96. Although the pooled OLS estimates show a positive correlation, it disappears completely with the inclusion of country fixed effects³ implying that the cross-sectional results are indeed driven by unobserved heterogeneity across countries. Brown and Hunter (1999) find that democracies feature more social spending than autocracies in a panel of 17 Latin American countries, 1980-1992, as measured by per capita spending on health, sanitation, education, housing, and social security. The importance of these findings for the redistributive hypothesis is dubious. First, many of the components of their social spending variable are public goods and it is not clear that they are redistributive. Second, the pooled sample covers a period with little within-country variation in regime type, so that the estimates are surely driven by cross-sectional variation.

In a related but more rigorous study, Ross (2006) shows that democracy has no effect on infant mortality even though democracies spend more money on health care in a sample of 168 sovereign states, 1970-2000, with data averaged over five-year intervals. Ross runs a pooled model with panel-corrected standard errors, country fixed effects, and a lagged dependent variable. Thus, the estimates are driven by within-country variation in democracy and infant mortality over time, serial correlation has been modeled explicitly, and heteroskedastic panels and contemporaneous correlation are assumed to characterize the data. Ross suggests that the additional spending goes to help the middle and upper classes in democracies but provides no direct evidence for the claim. It is equally plausible, for example, that there are differences in efficiency, or higher overhead costs in developed countries. On the face of it, however, the findings severely undermine the assumption that social spending benefits the poor.

Indirect Tests

In an early article exploring the causes of government growth, Peltzman (1980) offers suggestive evidence that within-country changes in the size of government are positively correlated with equality. Peltzman finds that government grows most under conditions of relative equality both in historical time-series analyses of the United States, Britain, Canada, and Japan, as well as in cross-national regressions and subnational analyses of the U.S. states. One interpretation of this pattern is that the growth of the middle class has been an important factor explaining the growth of government, an explanation consistent with “Director’s Law” (Stigler 1970), which proposes that redistribution flows from the poor and rich to the middle class. It is entirely plausible, of course, that equality is the outcome of government spending rather than its cause, though the historical time series suggest this is not the case.

In a related study, Lindert (1996) finds a negative correlation between pre-tax and transfer inequality and the level of social spending in a sample of 19 OECD countries, 1962-1981, with data averaged over four-year intervals. Lindert finds that expenditures on welfare and unemployment compensation, pensions, and health subsidies *fall* as the gap between poor and middle-class voters grows. Crucially, Lindert’s measure of inequality is based on the gap in pre-tax and transfer incomes, which ameliorates some of the concern about the endogeneity of inequality and social spending. In a similar vein, Perotti (1996) finds little evidence of a correlation between social spending and income inequality in cross-national sample of 67 countries, though it is unclear whether his measure of inequality is based on pre- or post-tax and transfer income.

³ This runs counter to the results of the published article. My reanalysis of the data is available upon request.

Iversen and Soskice (2006) estimate the reduction of inequality as measured by the percent change between pre- and post-tax and transfer income Ginis using data from the Luxembourg Income Study for a sample of 14 countries observed at various points between 1967-1997. Using an error-correction framework, which takes into account the dynamic nature of the data (but does not include country fixed effects) they find a *negative* correlation between pre-tax and transfer inequality and the reduction of inequality. Again this undermines the redistributive model of democracy, although the result is not robust to the inclusion of political variables such as government partisanship and type of electoral system. Nonetheless, in no specification do they find evidence for a positive correlation between initial inequality and the extent of inequality reduction.

Finally, a recent World Bank study of fiscal redistribution and income inequality in Latin America (Goñi et al. 2008) offers striking cross-national evidence that democracy is no guarantee of fiscal redistribution. Using measures of inequality based on pre- and post-tax and transfer income, the authors find that (a) inequality in Latin American countries is, on the whole, barely higher than in European countries when measured pre-tax and transfers; (b) the difference in inequality is greatly exacerbated when measured after taxes and transfers; and (c) the ineffectiveness of Latin American fiscal redistribution is partially due to tax evasion, tax loopholes, and the size of the informal sector (the joint effect of which is to minimize the progressive impact of income taxes; but (d) the biggest factor is that *in Latin America transfers flow to the richest quintiles whereas in Europe transfers benefit citizens across the income distribution*. Evidence of regressive service provision is consistent with Ross (2006), which suggests that democratization increases health care spending but that the benefits accrue to wealthier citizens. The study does not attempt to control for potentially confounding variables and the evidence is purely cross-sectional. Nonetheless, the correlations are striking and the level of detail in the data offer an important glimpse into important variation in the mechanisms of fiscal redistribution.

The cross-national evidence in the Goñi et al. (2008) study raises serious questions about the plausibility of the redistributive hypothesis. First, if the essential function of democratic institutions is to redistribute economic resources from the wealthy to the poor, ***why is there persistent and extreme inequality in democratic countries?*** Latin America is a case in point. From a narrow policy perspective, the article suggests that fiscal systems are to blame. From a broader theoretical perspective, however, this merely begs the question. The redistributive hypothesis assumes there is something inherently credible about redistributive promises under the institution of popular elections, which leads to a second question. If the promise of redistribution is credible under democracy, ***how can we explain the persistence of non-progressive (and even regressive) fiscal systems in democratic countries?*** The fact that fiscal systems in Latin America are so bad at redistributing income undermines the modeling assumption that democracy is a credible vehicle for redistribution.

State of the literature

There is strong evidence that the enfranchisement of poor voters in the United States increased welfare payment at the state level (Husted and Kenny 1997). The research design is exemplary in leveraging what is plausibly an exogenous source of variation in enfranchisement and in analyzing within-unit variation of fine-grained expenditure data over time. Once we expand the scope of analysis beyond the confines of a subnational study within the United States, however, empirical support for the redistributive hypothesis is mixed at best.

The majority of studies rely on blunt measures such as total revenues or government expenditures, which are not sensitive to net redistributive consequences. Fine-grained studies like Goñi et al. (2008) highlight that tax burdens are not uniformly borne and that services are targetable,

implying that aggregate measures are not fully informative and potentially misleading. Second, very few studies test the effects of regime transitions on redistributive outcomes *per se*. By relying on pooled and cross-national samples and by not including fixed effects, their estimates are almost certainly plagued by omitted-variable bias due to the extraordinary cross-national heterogeneity, most of which is almost certainly unobserved. This is true of studies that find positive evidence (e.g. Stasavage 2005; Brown and Hunter 1999), negative evidence (e.g. Lindert 1996; Perotti 1996; Mulligan et al. 2004; Goñi et al. 2008; Iversen and Soskice 2006) and mixed evidence of conditional relationships (e.g. Lindert 2003, 1994; Boix 2003, 2001). Of the studies that do include fixed effects and take the dynamic nature of time-series and panel data seriously, the weight of evidence does not support the redistributive hypothesis (Ross 2006; Peltzman 1980; Adserà and Boix 2002; Cheibub 1998).

A vindication of the redistributive hypothesis would require that two cross-sectional findings in particular be explained away. First, the fact of extreme and persistent inequality in democratic countries. Second, the persistent failure of progressive, fiscal redistribution in a large swath of democracies. Furthermore, we would require that such a vindication employ within-country estimators rather than cross-national correlations.

3 Estimating the Redistributive Hypothesis in a Dynamic Setting

Econometric Challenges

From the above discussion, it should be clear that regression estimates derived from cross-sectional and pooled models are likely to be biased by unobserved between-unit heterogeneity. As such, neither cross-national nor pooled regression models are an appropriate substitute for estimating the within-unit changes over time predicted by theory. An obvious starting point for modeling within-unit changes is to employ a fixed-effects model. By estimating country-specific intercepts, we can control for characteristics that vary across countries, but not within countries over time⁴.

An important concern unaddressed by the inclusion of country fixed-effects is the potential for a spurious correlation driven by secular trends in both democracy and redistribution that are not causally connected. A common solution is to include year fixed-effects, which would allow us to model temporal trends without imposing linearity restrictions. Year fixed-effects are an especially appropriate solution if we have reason to believe that democracy and redistribution are trending together in a similar direction around the world.

The problem with simply including year fixed-effects is that they impose the restriction that both democracy and redistribution trend together in the same direction in all countries. This is not a good assumption, and poses a particular problem if some countries trend towards democracy as the government share of GDP increases but other countries trend away from democracy as they government share of GDP falls. Since time fixed effects are constrained to estimate a year-specific intercept that is common across countries, the model would not pick up the time trends within each country (and thus would not control for the spurious correlation) because – on average – there is no

⁴ For example, some cultures might be inherently more generous than others and also more likely to choose democratic forms of government. Country-specific intercepts capture the higher rate of redistribution in such cases, which is a permanent culture-driven feature, allowing the *Democracy* variable to estimate the difference between democratic and non-democratic regime-years within (culturally) generous and stingy cultures alike.

time trend in the size of government. This would bias the result away from zero. We can relax the assumption that the series trend together in all countries by estimating country-specific time trends, at the cost of imposing linearity on the time trends. But this too is unsatisfying and we lose most of the benefit of pooling the data in the first place.

The General ADL Model

Even the most flexible of the static specifications described above is clunky and inadequate for estimating dynamic panel data. A fixed-effects model, for example, assumes that democratization has an instantaneous impact on redistribution, with no lag and no dampening of the effect over time. This is unrealistic. There are bound to be significant lags as democratization is followed by elections, legislation, and finally implementation. Policy stickiness and the opposition of entrenched conservative groups imply that the greatest gains in redistributive spending may accumulate over time. Alternatively, strategic adjustment of conservative groups may dampen the effects of democratization over time. A static model is likely to underestimate the impact of democratization, with the bias towards zero becoming stronger with slow-moving adjustment processes (Beck and Katz 2009).

Second, serial correlation is almost certainly a feature of the data. It is common for researchers to assume an AR1 process (Boix 2003 but cite others), and “fix” the econometric nuisance using standard tools like a Prais-Winsten or Cochrane-Orcutt regression (cites). We have no *a priori* reason, however, to believe that serial correlation follows an AR1 autoregressive process. It would be better to model serial correlation explicitly by allowing the data to reveal its form, a methodology which has the added benefit of allowing the researcher to model long-term effects of exogenous shocks to the system. In short, the data require techniques developed for analyzing dynamic processes rather than static data (Beck and Katz 2009)

The autoregressive distributed lag (ADL) model offers a general method for estimating dynamic processes and is easily applied to panel data. One benefit of ADL models is that they treat dynamic processes as something to be modeled explicitly rather than “corrected” as an econometric nuisance. More importantly, ADL models allow the researcher to estimate long-run effects correctly by allowing for the effects of an exogenous shock to settle in over multiple periods, as the outcome settles at a new long-run equilibrium level. This corresponds very cleanly to the application at hand, in that the redistributive hypothesis predicts that democratization will lead to a new equilibrium rate of redistribution. Finally, as De Boef and Keele (2005) emphasize, ADL models are especially appropriate if the researcher does not have strong theoretical grounds for imposing restrictions on the dynamic process itself. That is, rather than impose a first difference model or include the first lag of the dependent variable as a regressor, the researcher starts with a general ADL model and tests whether the data justify various restrictions in the model.

Since the functional form of the dynamic relationship between democratization and redistribution remains unknown *a priori*, I start with the general ADL model allowing for country-specific fixed effects and multiple lags of the dependent and independent variables. I then test for restrictions: Should we expect the adoption of democracy to impact redistributive policy immediately? After a one-year lag? After a five-year lag? Is the effect felt all at once or does the rate of redistribution adjust slowly until it reaches a new long-run equilibrium rate? The general model is given in Equation 1:

$$y_{i,t} = \beta_1 x_{i,t} + \beta_2 x_{i,t-1} + \mu y_{i,t-1} + \alpha_i + v_{i,t} \quad (1)$$

where $y_{i,t}$ is the outcome in country i in year t , $x_{i,t}$ is the contemporaneous measure of regime and $x_{i,t-1}$ is its one-year lag, $y_{i,t-1}$ is the first lag of the outcome variable, the α_i are country-specific dummy variables and $v_{i,t}$ is the i.i.d. error term. The model easily generalizes to higher order autoregressive processes, additional lags and additional covariates.

Finally, it is straightforward to calculate the long-run impact of democratization within the ADL framework. For the generic model in Equation 1 above, the long-run multiplier is the non-linear combination $\frac{\beta_1 + \beta_2}{1 - \mu}$, which generalizes easily for the case with additional lags. Hypothesis testing is also straightforward, using the standard errors from the non-linear combination. It is worth pointing out that the long-run effect can be statistically significant even if none of the coefficients are individually significant.

4 Democratization and the Size of the Public Sector

In this section I test the first two empirical implications of the redistributive hypothesis. The primary test is whether democratization generates greater redistribution via government activity. The second test is whether the effect is especially pronounced following democratization in unequal, wealthy, and ethnically fractionalized countries.

The first empirical challenge is to choose a direct measure for redistribution. In the Meltzer and Richard (1981) world, the tax rate is equivalent to the level of redistribution by construction and is perfectly measured by the size of government as a proportion of total income. Unfortunately, the real world is not so clean. Governments impose a wide range of taxes, provide an even greater range of services, and transfer income both indirectly (through subsidies) and directly (through transfers), the net effect of which is nearly impossible to measure directly.

Even seemingly straightforward measures are misleading when observed out of context. For example, one generally assumes that property taxes are an especially progressive form of taxation. In the context of the United States, however, where property taxes are collected locally and largely fund local public schools and local infrastructure improvements, it becomes less obvious that property taxes are a valid measure of progressive redistribution. In a decentralized system, in fact, a reliance on local property taxes rather than a national income tax is conceivable quite regressive.

Despite these and other problems, government revenues and expenditures have been the staple of the literature (e.g. Boix 2003, 2001; Peltzman 1980; Cheibub 1998). The size of the public sector relative to GDP is the most encompassing measure of government spending and retains several advantages over other alternatives. First, redistribution does not always come in the form of direct transfers – government spending on healthcare and education are two important examples – and an encompassing measure of government spending on final goods and services will likely capture this fact. Second, this measure is available for a greater number of countries and years than any other data set, which minimized concerns of sample selection bias (see the data definitions below). Finally, it is one of the standard measures in the literature, which allows for comparability of the current analysis to previous work.

There are two limitations to this measure. The first is that the size of the public sector does not include transfer payments such as social security or unemployment benefits. Although some types of transfers such as corporate subsidies are probably regressive, it is very likely that we are missing a

significant portion of progressive redistribution. The second limitation is that the size of the public sector includes the provision of expensive public goods such as defense, which have little to do with redistribution. The component of government spending due to defense spending is a potentially critical source of bias *against* finding support for the redistributive hypothesis. This would be the case if democracies systematically spend less than non-democracies on defense, a notion supported by a large literature on the democratic peace (cf. Ray (1998a) for a review but Weeks and Cohen (2007) for an illuminating critique). In particular, if democracies are less likely to become embroiled in wars than are non-democracies, then democracies will have systematically lower defense budgets than non-democracies. In that case, any increase in redistributive taxes and transfers due to democratization will be washed out to a great extent by a concomitant decrease in defense spending. Democracies do, in fact, appear to spend much less on defense than non-democracies, both on a per capita basis (\$257 vs. \$326) and as a percentage of GDP (2.6% vs. 5.5%)⁵. The best we can do is include a measure of military expenditures on the right hand side of the estimation equations, which I do throughout the paper⁶.

4.1 Data

Dependent Variable: Size of the Public Sector

The Penn World Table, Version 6.2 (PWT) provides estimates of GDP and its components – consumption, government, and investment – for 188 countries over the period 1950-2004. Particularly useful for our purposes is the government share of GDP, that is, total government expenditures on goods and services. This is very similar to Boix's (2003) measure of final consumption expenditures, except that the government component of GDP includes fixed capital formation while consumption expenditure does not. Importantly, the coverage of the Penn World Table data is far superior to that of data sets used in previous analyses. This is crucial because it minimizes concerns over sample selection bias.

The dependent variable for the analyses is simply the government share of GDP (*kg* in the PWT6.2). This is calculated in the PWT as a percentage of GDP by dividing the government component by real GDP (Laspeyres) plus exports minus imports in 2000 international dollars.

Independent Variable: Democracy

The political economy models that drive the most sophisticated versions of the redistributive hypothesis make abundantly clear that democracy is a distinct regime in which the major innovation is

⁵ These figures are my calculations based on estimates of military expenditures for 175 countries, 1950-2001 (Correlates of War, National Military Capabilities data set version 3.02; dollar amounts converted to international 2000\$ using PPP from the Penn World Table, Version 6.2). It is worth noting that these differences are based on a pooled sample and thus do NOT imply that democratization leads to a decrease in military spending, which would bias against the redistributive hypothesis even in a fixed-effects setting)– but it highlights the potentially serious estimation issue if we fail to adequately account for defense spending.

⁶ I also tried using a measure of the non-military government component of GDP by subtracting military expenditures directly from the original measure from the PWT. This proved to be problematic; military expenditures for a substantial list of countries were not only greater than the government component of GDP, they turned out to be several times the size of *total* GDP. The most likely explanation is that military expenditures are often paid for by outside governments – e.g. Kuwait in the 1990s by the United States, China in the 1970s by the USSR, and so forth. Since I have no principled way of distinguishing the source of military expenditures, I include it as control variable rather than subtracting it from the overall government spending.

an institutional commitment to political equality. “Our definition is ‘Schumpeterian’ (Schumpeter 1942) in the sense that we emphasize that a country is democratic if a certain political process takes place – if certain key institutions, such as free and fair elections and free entry into politics are in place.” (Acemoglu and Robinson 2006, pg 17-18). Acemoglu and Robinson add “The dichotomous distinction we draw between democracy and non-democracy, our desire to bring out the common elements within each regime, and our relentless reliance on Occam’s razor may appear stark, even simplistic. Nevertheless, we believe that this is the correct way to make progress, and our conviction is that this dichotomy is useful for developing intuitive ideas about the forces that lead societies to have different political institutions.” (pg 119)⁷

The regime variable from the *Democracy and Development Extended Data Set* (Alvarez et al. 2002)⁸ fits just this definition: it is a dichotomous measure capturing whether policies are set by an executive and a legislature chosen through free and fair popular elections. I use the Przeworski et al definition for the main specifications in this section. As a robustness check I re-estimate all the regressions using (a) the Boix and Rosato(2001) indicator, which adds the stipulation that at least 50% of the male population have the right to vote; (b) the revised Polity score, a continuous variable scales from -10 for a perfect autocracy to +10 for a perfect democracy, taken from the Polity IV data set; and (c) a dichotomous indicator based on the revised Polity score, which I code 1 for positive values of Polity and zero otherwise. The results for alternative specifications are virtually identical (see Appendix B).

Additional Covariates

Military Expenditures (Correlates of War project; National Military Capabilities Data Set, Version 3.02) Total military expenditures are originally denominated in nominal US dollars. I converted to constant 2000 international dollars using the exchange rate (XRAT) and purchasing power parity (PPP) from the Penn World Table 6.2 (Heston et al. 2006). I then calculated military expenditures on a per capita basis using population counts, also from PWT6.2, and as a percentage of real GDP (2000 international dollars from PWT6.2).

GDP per capita (PWT6.2) Measured in 2000 international dollars (chained series).

Growth in GDP per capita (PWT6.2) Annual growth rate as a percentage of preceding year’s level.

Trade Openness (PWT6.2) Imports plus exports as a percentage of total GDP.

Civil War (Gleditsch and Ward 1999; Gleditsch et al. 2002) Dummy variable indicating the incidence of civil war in each country year.

Interstate War (Gleditsch and Ward 1999; Gleditsch et al. 2002) Dummy variable indicating the incidence of interstate war in each country year.

Inequality (UNU-WIDER 2008) Gini estimates based on initial income inequality for each country. I use the first high quality observation available for each country as explained below.

⁷ It is therefore puzzling that Acemoglu and Robinson explain, on the following page, that they prefer the continuous 7-point scale developed by Freedom House. Especially because the Freedom House index captures all sorts of civil liberties (like freedom of religion – civil or political?). It not immediately obvious that such characteristics are relevant to the redistributive struggle for which democracy is supposedly a political solution.

⁸ The regime variable in particular is attributed to José Cheibub and Jennifer Gandhi.

Ethnic/Linguistic/Cultural Composition I employ five different measures that capture both social heterogeneity and group composition all coded by Fearon (2003). He codes ethnic fractionalization (EF), ethno-linguistic fractionalization (ELF – based on the Atlas Narodov Mira), cultural diversity (CD), the size of the plurality ethnic group (PLU), and the size of the second largest ethnic group (2ND) for all countries with population over 500,000. EF (and ELF) simply measures the expected value that two people drawn at random will be from different ethnic (ethno-linguistic) groups. CD is based on the expected “cultural resemblance” between two people drawn at random, where cultural resemblance is a measure of linguistic similarity between 0 and 1 (see Fearon 2003 for details). Fearon subtract this measure from one to make it analogous to EF, so that a value of 1 indicates maximal cultural diversity.

Descriptive Statistics

Descriptive statistics are presented in Table 1. In addition, I include several time-series graphs of democracy and the size of government in order to illustrate the typical bivariate relationship found in the data (Figure 1). I include similar graphs for all countries in the sample in Appendix A.

Table 1: Descriptive Statistics by country year

Measure	Mean	Standard Deviation	Minimum	Maximum	Non-missing Observations
Size of government (% of GDP)	22.0	11.3	2.1	93.7	6850
Democracy Indicator (Przeworski et al)	0.43	0.49	0	1	6810
Democracy Indicator (Boix and Rosato)	0.40	0.49	0	1	6211
Democracy (Revised Polity score)	0.23	7.59	-10	10	6575
Interstate war	0.04	0.20	0	1	7094
Civil War	0.07	0.25	0	1	7094
Military Expenditures (% of GDP)	4.2	9.6	0	373.4	5772
Ethnic fractionalization	0.48	0.26	0	1	8250
Ethno-linguistic fractionalization	0.42	0.29	0	0.93	6655
Cultural fractionalization	0.31	0.21	0	0.73	8195
Size of plurality group (% of population)	64.6	23.7	12.0	99.9	8195
Size of largest minority group (% of population)	17.2	10.5	1.0	44.0	7755
Gini Index	42.0	13.2	15.5	77.3	7590
Population (thousands)	25,008	94,254	25	1,294,846	9339
Real GDP per capita (2000 I\$)	6,911	7,923	171	84,408	6850
Growth in real GDP per capita (%)	2.0	7.6	-63.3	151.1	6680
Openness (% of GDP)	69.1	53.4	2.0	986.5	6860

NB – Means for dichotomous variables indicate the fraction of country-years coded 1

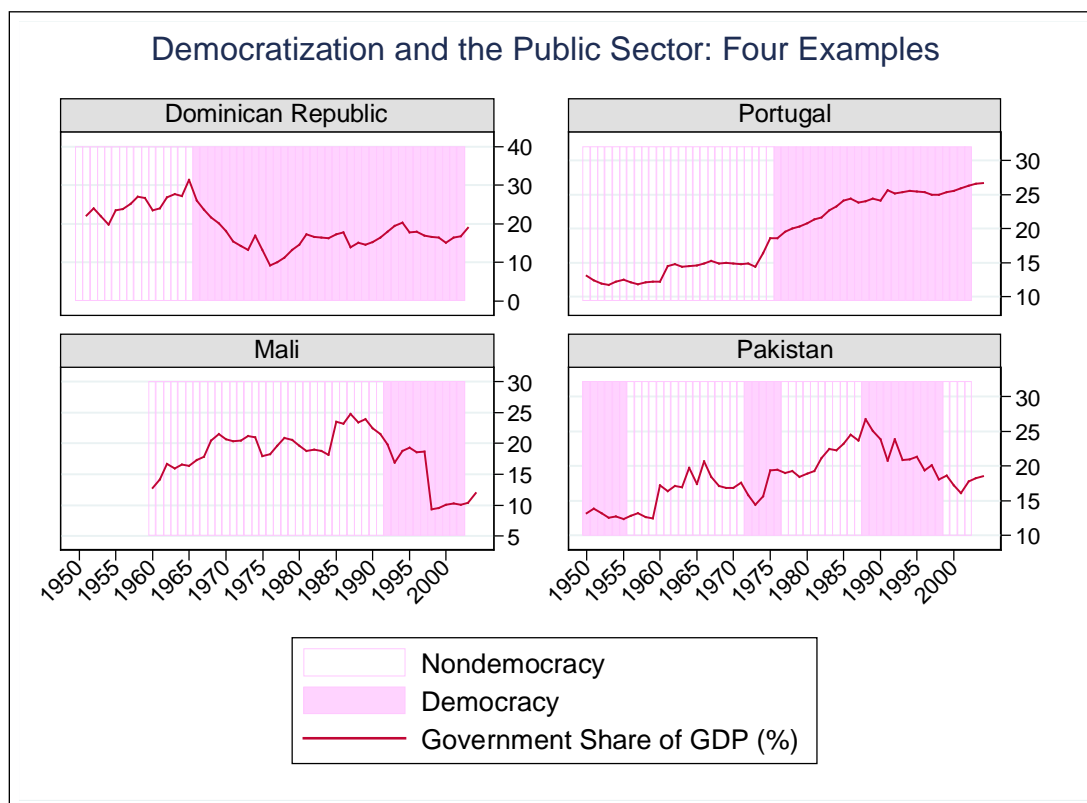


Figure 1. Democracy and Government Size: four examples

4.2 Empirical Model

Static models

We start with a simple bivariate OLS regression in order to get a sense of the basic descriptive relationship between democracy and redistribution. The pooled bivariate regression of the government share of GDP on the 0/1 indicator of democracy shows a *negative* correlation between democracy and government size (Table 2, Model 1). The government share of real GDP is just over 22% in non-democratic country-years and is 0.5% *lower* in democratic country-years, though the difference is not statistically significant.

Including country fixed effects (Table 2, Model 2) we see that the coefficient on *Democracy* turns positive, implying that the government share of GDP is one percentage point higher under democracy than under non-democracy. The positive coefficient, however, does not come close to reaching conventional levels of statistical significance. It is worth noting the sign-flip on *Democracy*, which reflects the difference between running regressions with *between* rather than *within* estimators. I also estimate a model with both country and year fixed-effects (Table 2, Model 3). The result is virtually unchanged, both in terms of the magnitude of the coefficient and its lack of statistical significance. I estimate a final bivariate model with country fixed-effects and country-specific, linear time trends (Table 2, Model 4). The coefficient on democracy has flipped back to being negative, although it remains

statistically insignificant. The findings remain null in static models estimated with the inclusion of the full set of covariates described above (not reported).

Table 2: Democracy and the size of government: bivariate regressions

	(1)	(2)	(3)	(4)
Democracy	-0.58 (1.26)	1.04 (0.77)	1.09 (0.78)	-0.29 (0.55)
Constant	22.16 (0.98)**			
Country Fixed Effects		Yes	Yes	Yes
Year Fixed Effects			Yes	
Time Trend (country-specific)				Yes
Observations	6248	6248	6248	6248
Clusters (# countries)	170	170	170	170
R-squared	0.00	0.79	0.80	0.88

Robust standard errors clustered by country in parentheses

* significant at 5%; ** significant at 1%

The autodistributed lag model (ADL)

Turning to the general ADL model (Equation 1 above) I include the full battery of covariates before testing for restrictions. I first test restrictions on the lagged independent variables, e.g. $\beta_2 = 0$. The second and third lags have miniscule coefficients and do not approach statistical significance, so I comfortably restrict them to zero. The first lag for each of the independent variables is either significant or borderline significant ($p=0.12$ for the lagged measure of democracy), but is often greater in magnitude and statistical significance than the contemporaneous value. I therefore choose to include the first lag of all the independent variables in my main specifications.

In order to model serial correlation I include the various lags of the dependent variable as regressors, and then test for remaining serial correlation. I employ a Lagrange Multiplier test following Beck and Katz (2009) and regress the residuals of each model on the lagged residuals and full set of covariates. A large and statistically significant coefficient on the lagged dependent variable would indicate remaining serial correlation. Following this procedure I find that including two lags of the dependent variable is sufficient for removing serial correlation. I therefore end up with a general ADL model with two lags of the dependent variable and a single lag for each of the independent variables on the right hand side⁹:

⁹ The third lag is statistically significant but substantively tiny so I leave it out, consistent with the recommendations of Beck and Katz (2009). This has no effect on any of the estimated coefficients or standard errors. This raises a more general issue: there are other ways of choosing the appropriate number of lags. Model selection is often based on information criteria (like the AIC or BIC), which in the present context allow for including up to five lags of the dependent variable on the right hand side. That said, although additional lags do reduce the AIC and BIC (slightly), thus implying that the more complicated model is a better fit, there is no reason

$$y_{i,t} = \beta_1 x_{i,t} + \beta_2 x_{i,t-1} + \mu_1 y_{i,t-1} + \mu_2 y_{i,t-2} + \alpha_i + v_{i,t} \quad (2)$$

where $y_{i,t}$ is government component of GDP in country i in year t (in levels, expressed as a percent), $x_{i,t}$ is the contemporaneous measure of regime and $x_{i,t-1}$ is its one-year lag, $y_{i,t-1}$ is the first lag of the government GDP variable, the α_i are country-specific dummy variables and $v_{i,t}$ is the i.i.d. error term.

As argued above, the regression coefficients themselves are not as interesting as the long-run effects they imply. From a theoretical perspective, both democratic and non-democratic regimes will have an equilibrium tax rate and a corresponding equilibrium level of redistribution and it is very likely that it will take several years for the system to adjust fully and settle into a new equilibrium after a major shock to the political system¹⁰. The long-run multiplier given the model estimated in this section (Equation 2 above) is $LRM = (\beta_1 + \beta_2)/(1 - \mu_1 - \mu_2)$, with standard errors calculated simply as the standard error of the non-linear combination.

Main effects

Recall that the dependent variable is the size of government, measured as the government component of GDP over total GDP times 100 and the measure of democracy is a 0/1 indicator. Thus, we can interpret the long-run multiplier as the average change in the equilibrium level of redistribution due to a (within-country) regime transition from non-democracy to democracy. Surprisingly, it appears that in the long run, democratization reduces the size of the public sector on the order of 1% of GDP, although the point estimate is not statistically significant (Table 3, Column 1).

Despite the use of fixed effects, our estimation strategy is not immune to the problem that democratization is not likely to be exogenous. It might be the case, for example, that unobserved processes are driving both within-country regime transitions and changes in the size of government (see Bertrand et al. (2004) for a general analysis of this econometric challenge). In the absence of a convincing instrument, the best we can do is include theoretically relevant covariates that are likely to be a part of such processes. I estimate the model with the inclusion of annual measures of GDP per capita, growth in GDP per capita, trade openness, the incidence of civil war, the incidence of interstate war and military expenditures. The long-run effect of democracy remains negative and statistically insignificant, with point estimates indicating a reduction in the size of the public sector between 0.9% and 1.8% of GDP following democratization (Table 3, Columns 2-8). The fact that the results in the full model (Table 3, Column 8) are virtually identical to the simple bivariate model (Table 3, Column 1) relieves some of the worry of omitted variable bias.

to add them for the purposes of eliminating serial correlation or for estimating the effects of any of the independent variables on the size of government.

¹⁰ It is possible that it takes several years for the system to feel even the initial shock, e.g. a five year delay would imply that we model that 5th and 6th lags rather than the contemporaneous values and 1st lags. I estimated all the models in this paper with various delays of between zero and ten years and found that modeling a delayed reaction produces similar results but tempers the magnitude of the coefficients even further and increases their standard errors. If there's any hope of finding an effect, it is in the model I've chosen with zero delay.

Table 3. Democracy and the Size of Government: ADL with covariates

Government Component GDP (%)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democracy	0.37 (0.27)	0.39 (0.27)	0.38 (0.29)	0.31 (0.27)	0.39 (0.28)	0.35 (0.27)	0.39 (0.30)	0.38 (0.32)
Democracy (lagged)	-0.53 (0.29)	-0.53 (0.29)	-0.59 (0.31)	-0.54 (0.30)	-0.53 (0.30)	-0.56 (0.30)	-0.51 (0.31)	-0.52 (0.33)
Real Per Capita GDP		-0.48 (0.12)**						-0.23 (0.13)
Real Per Capita GDP (lagged)		0.47 (0.12)**						0.21 (0.13)
Real Per Capita GDP Growth			-0.08 (0.02)**					-0.04 (0.02)*
Real Per Capita GDP Growth (lagged)			0.01 (0.01)					0.01 (0.01)
Trade Openness				0.05 (0.01)**				0.04 (0.01)**
Trade Openness (lagged)				-0.04 (0.01)**				-0.04 (0.01)**
Interstate War					1.78 (0.62)**			1.29 (0.43)**
Interstate War (lagged)					-0.21 (0.34)			-0.22 (0.29)
Civil War						0.62 (0.28)*		0.34 (0.30)
Civil War (lagged)						-0.42 (0.19)*		-0.27 (0.23)
Military Expenditures (% GDP)							0.10 (0.01)**	0.08 (0.01)**
Military Expenditures (% GDP) (lagged)							-0.08 (0.01)**	-0.06 (0.01)**
Size of Government (lagged)	0.80 (0.04)**	0.80 (0.04)**	0.82 (0.03)**	0.84 (0.02)**	0.80 (0.05)**	0.81 (0.05)**	0.80 (0.05)**	0.85 (0.03)**
Size of Government (lagged 2)	0.06 (0.04)	0.06 (0.04)	0.04 (0.03)	0.03 (0.02)	0.05 (0.04)	0.05 (0.04)	0.06 (0.04)	0.01 (0.02)
Democracy (Long Run Multiplier)	-1.13 (0.98)	-1.02 (0.99)	-1.55 (0.95)	-1.75 (0.984)	-0.983 (0.98)	-1.46 (0.95)	-0.88 (0.96)	-1.04 (0.92)
Observations	5964	5964	5964	5964	5767	5767	5429	5341
Clusters (# countries)	170	170	170	170	162	162	169	162
R-squared	0.95	0.95	0.95	0.95	0.94	0.94	0.95	0.95

All specifications include country fixed effects. Constant terms not reported.

* significant at 5%; ** significant at 1% (Robust standard errors clustered by country in parentheses).

Conditional Effects: Inequality

A secondary implication of the linear tax model is that redistribution increases as the initial gap between mean and median income grows. Thus, we expect to observe a positive correlation between the level of income inequality under autocracy and the rate of redistribution following a democratic transition.

I estimate the initial level of income inequality in each country using the first high-quality observation available in the UNU-Wider data set starting in 1950. I then classify countries into three equal quantiles according to their level of initial income equality, resulting in a categorical classification of Low, Medium and High inequality countries¹¹. Rather than interact the measure of democracy with the categorical measure of inequality, I run the dynamic regressions separately for each of the categories. Not only does it greatly simplify the interpretation of dynamics, it allows us to test whether countries missing data on income inequality are systematically different from the rest.

The redistributive view predicts that – conditional on a successful regime transition – we should observe that democratization leads to greater redistribution in more unequal countries. The empirical evidence roundly undermines this claim. In countries with low inequality, democratization leads to an increase in government size on the order of 2% of GDP, although the estimate does not come close to reaching statistical significance (Table 4, Column 1). As we move to countries with medium levels of inequality, democratization leads to a *decrease* in the size of government of roughly 3% of GDP. The point estimate just barely fails to reach conventional levels of statistical significance at the 5% level ($p=0.063$ – Table 4, Column 2). Among the most unequal countries we still see that democratization leads to a decrease in the size of the public sector of about 1% of GDP, though in this group too the result is not statistically significant (Table 4, Column 3). The lack of a finding (or the weak negative finding) among the most unequal countries cannot be due to the fact that unequal countries don't democratize in the first place due to repression by elites (Boix 2003). By choosing a fixed-effects strategy, we've guaranteed that the estimates are driven by within-country variation, i.e. by successful regime transitions.

Next, I test the redistributive hypothesis in the set of countries for which data on income inequality are missing (Table 4, Column 4). The results clearly show that data are missing nonrandomly. Among this group of countries, democracy leads to a very large decrease in the size of the public sector of roughly 6% of GDP, which is just shy of reaching statistical significance ($p=0.060$). Although we can't draw any conclusions about the degree to which missing data on inequality induce a bias in previous work, it is important to note the potential problem¹².

¹¹ None of the results that follow depend on this categorization scheme. I re-estimated all the regressions using alternative classifications based on between two and five quantiles, which merely reduced the sample size of each group.

¹² Sample selection bias due to missing inequality data is potentially a serious problem in Boix's (2003) regressions, in which the sample size decreases severely in the main specifications upon the inclusion of inequality data: a decrease of 70% in Table 5.1 (1,998 observations in the full model, 621 with the Gini index), a decrease of over 80% in Table 5.2 (4,627 observations in the full model, 763 with the Gini index), and a decrease of more than 75% in Table 5.3 (2,626 observations in the full model, 598 with the Gini index).

Table 4. Democracy and the size of government: Conditional Effects (Inequality)

	Low Inequality (1)	Medium Inequality (2)	High Inequality (3)	Unknown Inequality (4)
Democracy	0.69 (0.72)	-0.41 (0.25)	0.76 (0.55)	1.63 (1.97)
Democracy (lagged)	-0.15 (0.72)	0.06 (0.29)	-0.88 (0.54)	-2.59 (2.41)
Real Per Capita GDP	-0.70 (0.40)	-1.38 (0.34)**	-0.29 (0.44)	-0.02 (0.14)
Real Per Capita GDP (lagged)	0.71 (0.40)	1.38 (0.35)**	0.22 (0.45)	0.01 (0.12)
Real Per Capita GDP Growth	-0.03 (0.03)	0.02 (0.03)	-0.04 (0.05)	-0.07 (0.02)**
Real Per Capita GDP Growth (lagged)	0.01 (0.02)	0.01 (0.01)	0.00 (0.01)	0.00 (0.01)
Trade Openness	0.01 (0.02)	0.05 (0.00)**	0.02 (0.01)	-0.00 (0.02)
Trade Openness (lagged)	-0.01 (0.01)	-0.05 (0.00)**	-0.01 (0.02)	-0.01 (0.02)
Interstate War	2.96 (1.14)*	0.29 (0.21)	-0.30 (0.20)	3.59 (1.74)
Interstate War (lagged)	-0.46 (0.73)	0.23 (0.33)	0.24 (0.34)	0.27 (0.71)
Civil War	0.94 (0.41)*	0.21 (0.39)	0.56 (0.67)	0.17 (0.36)
Civil War (lagged)	0.03 (0.65)	-0.41 (0.30)	-0.29 (0.45)	-0.49 (1.20)
Military Expenditures (% GDP)	0.04 (0.04)	0.21 (0.07)**	0.17 (0.09)	0.09 (0.01)**
Military Expenditures (% GDP) (lagged)	-0.04 (0.05)	-0.17 (0.06)**	-0.14 (0.07)	-0.07 (0.01)**
Size of Government (lag)	0.78 (0.05)**	0.89 (0.03)**	0.85 (0.06)**	0.82 (0.09)**
Size of Government (lag2)	-0.04 (0.05)	-0.01 (0.03)	0.05 (0.05)	0.03 (0.05)
Democracy (Long Run Multiplier)	2.11 (1.92)	-2.95 (1.55)	-1.16 (1.66)	-6.08 (3.07)
Observations	1054	1913	1797	577
Clusters (# countries)	46	46	46	24
R-squared	0.94	0.97	0.96	0.92

All specifications include country fixed effects. Constant terms not reported.

* significant at 5%; ** significant at 1% (Robust standard errors clustered by country in parentheses)

Table 5. Democracy and the size of government: Conditional Effects (Economic Development)

	Low Income (1)	Middle Income (2)	High Income (3)
Democracy	0.38 (0.53)	0.61 (0.43)	-0.25 (0.41)
Democracy (lagged)	-0.35 (0.54)	-0.91 (0.48)	0.24 (0.27)
Real Per Capita GDP	-2.06 (0.93)*	-1.94 (0.70)**	0.12 (0.15)
Real Per Capita GDP (lagged)	2.02 (0.97)*	1.98 (0.70)**	-0.14 (0.15)
Real Per Capita GDP Growth	0.01 (0.03)	0.01 (0.03)	-0.10 (0.04)*
Real Per Capita GDP Growth (lagged)	0.00 (0.01)	0.02 (0.01)	0.00 (0.01)
Trade Openness	0.05 (0.00)**	0.03 (0.02)	-0.02 (0.01)
Trade Openness (lagged)	-0.05 (0.00)**	-0.02 (0.02)	0.02 (0.01)
Interstate War	0.98 (0.46)*	1.56 (1.33)	1.26 (0.74)
Interstate War (lagged)	-0.32 (0.24)	0.30 (1.27)	-0.25 (0.43)
Civil War	0.14 (0.27)	1.08 (0.67)	0.43 (1.18)
Civil War (lagged)	-0.23 (0.27)	-0.82 (0.39)*	1.37 (0.64)*
Military Expenditures (% GDP)	0.10 (0.05)*	0.08 (0.04)*	0.09 (0.01)**
Military Expenditures (% GDP) (lagged)	-0.06 (0.03)*	-0.08 (0.07)	-0.08 (0.01)**
Size of Government (lag)	0.83 (0.04)**	0.87 (0.04)**	0.87 (0.08)**
Size of Government (lag2)	0.04 (0.04)	-0.01 (0.03)	-0.02 (0.06)
Democracy (Long Run Multiplier)	0.21 (1.43)	-2.12 (1.81)	-0.05 (1.44)
Observations	1889	1795	1657
Clusters (# countries)	55	53	54
R-squared	0.95	0.95	0.96

All specifications include country fixed effects. Constant terms not reported.

* significant at 5%; ** significant at 1% (Robust standard errors clustered by country in parentheses)

Conditional Effects: Economic Development

An additional second-order implication the redistributive hypothesis is based on Wagner's Law, which states that the size of government increases with per capita GDP (Meltzer and Richard 1983; 1981; see also Boix 2003). If Wagner's law holds, we would expect to observe greater growth of the public sector following democratization among wealthier countries. In this section, I test whether the redistributive hypothesis holds, conditional on level of economic development.

I estimate the initial level of development based on the first year in which per capita GDP is available for each country in the Penn World Table (usually 1950)¹³. There is no evidence that democratization increases the size of government conditional on initial GDP per capita, let alone the specific form of the conditional hypothesis implied by Wagner's Law. The estimated effect is zero in Low and High Income countries and implies a decrease of two percentage points in the government share of GDP, which is not statistically significant (Table 5).

Conditional Effects: Ethnic Fractionalization

A third set of conditional effects involves variation in the social structure, which is especially relevant to the Acemoglu and Robinson (2006) analysis of the redistributive hypothesis. In particular, as the mass of citizens under autocracy is better able to solve their organizational collective-action problem, the revolution constraint should increase the equilibrium level of redistribution from the wealthy. Conversely, elites are more likely to set taxes at their preferred rate when the poor cannot organize a credible revolutionary threat. Assuming that coordination is more difficult among ethnically and linguistically fractionalized citizens, we should observe especially low levels of redistribution in heterogeneous autocracies. This implies, however, that conditional on successful democratization, we should observe especially large increases in redistribution in just these cases.

In this section, I test whether the redistributive hypothesis holds conditional on ethnic composition. I categorize countries into three equal quantiles of ethnic fractionalization (EF) and classify them accordingly as Low Fractionalization, Medium Fractionalization, and High Fractionalization¹⁴. I then run the ADL model separately for each of these groups. I find no evidence that the redistributive hypothesis holds conditional on ethnic fractionalization (Table 6, Columns 1-3). The point estimates remain negative and statistically insignificant at all three level of fractionalization. I find a big negative estimate among countries with populations under 500,000 not coded by Fearon (2003), but this is based on a sample of 12 countries and is very imprecisely estimated (Table 6, Column 4). The regression results are virtually identical when I condition on ethno-linguistic fractionalization (ELF) and cultural diversity (CD), as alternative measures of heterogeneity.

¹³ I re-ran all the regression using per capita GDP averaged over all available years and got virtually identical results. I also re-ran the regressions interacting GDP per capita with Democracy and its lag, which also yielded the same results.

¹⁴ I re-ran all the regression models using various classification schemes. The results are not sensitive to finer-grained categorizations (e.g. quartiles, quintiles.)

Table 6. Democracy and the size of government: Conditional Effects (Ethnic Fractionalization and Group Structure)

	Low Fractionalization (1)	Medium Fractionalization (2)	High Fractionalization (3)	Unknown Fractionalization [‡] (4)	Dominant Majority Group (5)	Dominant Minority Group (6)
Democracy	-0.08 (0.25)	0.02 (0.37)	0.60 (0.64)	4.80 (0.98)**	-0.07 (0.22)	0.62 (0.63)
Democracy (lagged)	0.08 (0.22)	-0.25 (0.38)	-0.78 (0.65)	-6.45 (0.55)**	-0.05 (0.21)	-0.84 (0.62)
Real Per Capita GDP	-0.69 (0.34)*	-0.21 (0.52)	-0.45 (0.15)**	0.04 (0.05)	-0.33 (0.38)	-0.69 (0.27)*
Real Per Capita GDP (lagged)	0.67 (0.34)	0.15 (0.53)	0.44 (0.13)**	0.00 (0.06)	0.29 (0.39)	0.63 (0.28)*
Real Per Capita GDP Growth	-0.07 (0.03)*	-0.08 (0.03)*	0.02 (0.03)	-0.06 (0.02)**	-0.07 (0.02)**	-0.01 (0.04)
Real Per Capita GDP Growth (lagged)	0.01 (0.01)	-0.01 (0.01)	0.02 (0.01)	-0.01 (0.01)	-0.00 (0.01)	0.02 (0.01)
Trade Openness	0.04 (0.02)	0.01 (0.02)	0.05 (0.00)**	-0.00 (0.03)	0.02 (0.01)	0.01 (0.01)
Trade Openness (lagged)	-0.03 (0.02)	-0.01 (0.01)	-0.05 (0.01)**	-0.01 (0.03)	-0.02 (0.01)	-0.01 (0.01)
Interstate War	0.24 (0.18)	1.36 (0.83)	2.83 (1.13)*	0.00 (0.00)	0.81 (0.46)	3.34 (1.09)**
Interstate War (lagged)	0.18 (0.19)	0.06 (0.61)	-1.35 (0.82)	0.00 (0.00)	-0.06 (0.34)	-0.92 (1.13)
Civil War	-0.66 (0.55)	0.70 (0.82)	0.81 (0.27)**	0.00 (0.00)	0.25 (0.48)	0.58 (0.32)
Civil War (lagged)	0.57 (0.32)	-0.73 (0.38)	-0.62 (0.34)	0.00 (0.00)	-0.22 (0.28)	-0.57 (0.37)
Military Expenditures (% GDP)	0.00 (0.01)	0.11 (0.02)**	0.08 (0.01)**	-0.16 (0.13)	0.08 (0.02)**	0.09 (0.01)**
Military Expenditures (% GDP) (lagged)	-0.00 (0.01)	-0.09 (0.03)**	-0.05 (0.01)**	0.07 (0.09)	-0.07 (0.03)*	-0.05 (0.01)**
Size of Government (lag)	0.94 (0.04)**	0.86 (0.05)**	0.81 (0.05)**	0.77 (0.13)**	0.89 (0.03)**	0.79 (0.05)**
Size of Government (lag2)	-0.03 (0.04)	-0.02 (0.05)	0.04 (0.03)	0.05 (0.05)	-0.01 (0.03)	0.03 (0.04)
Democracy (Long Run Multiplier)	-0.01 (1.31)	-1.37 (1.41)	-1.19 (1.59)	-9.12 (5.32)	-0.94 (1.12)	-1.20 (1.39)
Observations	1848	1547	1654	292	3478	1647
Clusters (# countries)	50	50	50	12	101	52
R-squared	0.97	0.96	0.94	0.94	0.96	0.95

All specifications include country fixed effects. Constant terms not reported.

* significant at 5%; ** significant at 1% (Robust standard errors clustered by country in parentheses)

[‡] Countries with population under 500,000, which are omitted in Fearon (2003)

As Fearon (2003) notes, measures of fractionalization are unable to distinguish several important features of a country's group structure. In particular, the presence of a dominant majority group (over 50% of the population) or a large minority group (second largest group over 20% of the population in my analyses) has important political implications. For example, a dominant majority group might be able to form a minimum winning coalition all on its own, potentially at the expense of minority groups. A large minority group, on the other hand, might signal that the country is politically divided between two powerful groups¹⁵. I test whether the redistributive hypothesis holds under either of these conditions and cannot reject the null hypothesis of no effect either in the presence of a dominant majority group (Table 6, Column 5) or large minority group (Table 6, Column 6)¹⁶.

Individual country time-series regressions

Thus far, the weight of evidence does not support the thesis that democratization increases the size of the public sector, whether in the full sample of countries or conditional on income inequality, level of development, and ethnic social structure. If anything, the average tendency is for the size of the public sector to *shrink* following democratization. That said, it is worth taking advantage of the fact that we have relatively long time-series measures for each country in order to analyze whether democratization *ever* induces a new redistributive equilibrium.

I ran the basic ADL model (same as in Table 3 Column 1) for each of the 61 countries in the data set that experienced regime change and then calculated the long-run effect of democratization on the size of the public sector. In 50 of the 61 cases, I could not reject the null hypothesis that a change in regime type has no effect on government size. Yet in 10 of the 61, I found a substantively and statistically significant effect¹⁷. Consistent with the average tendency in the full panel of countries, democratization in the Dominican Republic, Mali, Niger, Panama, and Suriname reduced the equilibrium size of government. The point estimates in these cases imply a reduction in government spending between 6.2 and 13.7 percent of GDP (Table 7). These results further erode our confidence in the redistributive model of democratization.

For the first time, however, we also see evidence that democratization can lead to an increase in government spending. In Bangladesh, Greece, Hungary, Portugal, and Spain, democratization *increased* the equilibrium size of government between 1.8 and 10.1 percent of GDP (Table 7). Do these cases vindicate the redistributive hypothesis? Surprisingly, these five countries are characterized by low levels of initial inequality (Gini = 33.3) in comparison to the group of countries with a null finding (Gini = 47.1) and those with a negative finding (Gini = 44.0). This is directionally consistent with the conditional

¹⁵ Fearon (2003) identifies additional configurations of group structure, which I should code and test as well.

¹⁶ One potentially interesting pattern in the data is that the magnitude of the point estimates for Democracy and Democracy (lagged) is quite large (almost 1% of GDP) in High Fractionalization countries, in the small countries missing fractionalization data, and in the presence of a large minority group. Although none are statistically significant and the lagged variable is always negative (and greater in magnitude than the contemporaneous value), the following interpretation might be worth investigating in future work: democratization is (probably) positively correlated with election years, so that the Democracy variable and its lag might be picking up a political business cycle of sorts, which is especially prominent in countries characterized by ethnic pork barrel politics (see Fearon (1999) on why ethnic politics and pork tend to go together). So the year of the election there is a temporary boost in government spending, followed by a decrease that gets the country back to normal levels of government spending.

¹⁷ There is an 11th case with a statistically significant estimate (Albania, -19%), which I did not include because the series has 11 observations, only 2 of which are non-democratic. A brief inspection of the time-series graph for Albania underscores that the regression result is suspect at best.

analysis in which countries with the lowest levels of initial inequality had a positive but statistically insignificant impact on the size of government (Table 4 above). The redistributive hypothesis, however, would lead us to expect the greatest positive effect among highly unequal countries. The individual time-series regressions imply either that democratization has a country-specific effect or that the effect is conditional on an unobserved variable. Either way, however, the findings do not accord with the redistributive hypothesis.

Table 7. Democracy and the size of government: individual country time-series (long-run effects)

Country	Long-Run Effect of Democracy (% GDP)	Standard Error (OLS)	Observations (Years)
Bangladesh	1.8	(0.8)*	29
Greece	2.9	(0.8)**	50
Hungary	4.4	(1.1)**	31
Portugal	10.1	(3.1)**	51
Spain	5.3	(1.8)**	51
Dominican Republic	-11.2	(2.6)**	50
Mali	-9.7	(3.7)*	41
Niger	-11.2	(5.2)*	41
Panama	-6.2	(1.8)**	51
Suriname	-13.7	(5.1)*	27

* significant at 5%; ** significant at 1%

4.3 Discussion

In this section I tested whether democratization increases government spending in a panel of 188 countries from 1950 to the present. In the full sample I consistently find a negative, but statistically insignificant effect of democracy on the size of government. Once we condition on inequality, we find some very surprising results. Conditioning on level of economic development, I find a null effect in low and high income countries, and a negative but statistically insignificant effect in middle-income countries. Conditioning on various dimensions of group structure I find that the point estimates for democratization are consistently negative but statistically insignificant. Finally, in all but the most equal countries, democratization *decreases* the size of government on the order of 2% of GDP, a result that is marginally statistically significant. There is only one subset of countries for which democracy has a positive (but statistically insignificant) effect on the size of government, and that is among the most economically equal countries in the sample. This runs contrary to the logic of the redistributive hypothesis, which predicts that, conditional on a successful regime transition, the level of redistribution should increase rather than decrease as the level of initial inequality grows.

The results of individual country regressions further undermine the plausibility of the redistributive hypothesis. In 50 of the 61 countries experiencing a change in political regime, I could not reject the null of no effect. More surprisingly, I found a large and statistically significant negative effect in five of the 61 countries. It is worth emphasizing that *there are no conditions under which we would expect to see this result if the underlying model of democracy was the one proposed by the redistributive hypothesis*. The individual time-series regressions show a large and statistically significant positive effect in five countries as well. These countries, however, are relatively equal in comparison to the full sample, a pattern that is inconsistent with the predictions of the redistributive hypothesis.

The approach taken in this section improves on previous empirical work in several ways. First, I used a measure of government size from the Penn World Tables, a high-quality data set with coverage far superior to data sets used in previous work both in terms of country coverage and historical coverage. Second, I modeled what clearly is a dynamic process using the ADL framework, which is designed specifically for modeling dynamics. Within this framework, we can more accurately capture long-run dynamics and correctly estimate the effect of regime transitions on equilibrium levels of redistribution. Third, all the estimates in this chapter are driven by within-country variation, i.e. transitions in political regime, rather than by variation in regime type among wildly heterogeneous countries. Fourth, I test for conditional effects in a sensible manner that does not impose stringent functional form restrictions on conditional effects. I also test explicitly whether the selective availability of the conditioning variables poses a problem in terms of sample selection bias (it does). Finally, I drop the pooling assumption of the panel analysis and test for the redistributive hypothesis in each of the 61 countries that experience regime transitions in the sample.

I leave the substantive discussion for the end of the chapter. It is worth noting, however, that the general trend in which democratization reduces government spending is consistent with an alternative view of democracy as a form of limited government (see Weingast (1997) for the seminal game-theoretic analysis). On this interpretation, autocratic governments are able to expropriate wealth from disempowered citizens with relative ease. Democratization implies that heterogeneous groups of citizens have successfully overcome their coordination problems, which induces self-enforcing limits on the regime, thus reducing the size and scope of government. The ability of citizens to coordinate amongst themselves is positively correlated with the strength of democracy.

5 Democratization and Income Inequality

In this section I test whether societies become more equal following the adoption of democracy using the most comprehensive data on inequality currently available. This strategy is related to the indirect tests reviewed above that assess the factors that affect redistribution within democracies. In this section, however, I offer a direct test of the redistributive hypothesis by estimating the effect of regime transition on the degree of inequality and on net inequality reduction.

5.1 Data

Dependent Variable: Income Inequality

I rely on the UNU-Wider estimates of Gini indices as before. In this section, however, I make use of annual country observations since we are now interested in within-country changes in the level of inequality. The Gini data are classified according to the following criteria: **Area Coverage** (e.g., entire country, major cities), **Population Coverage** (e.g., rural, urban), **Age Coverage** (e.g. all ages, over 18),

Statistical Unit (e.g., family, household, individual), and **Quality of Source** (categorical scale, 1-4). In order to maximize coverage I included the entire sample rather than filter it according to any of these classifications.

The Gini estimates also are classified by UNU-Wider according to the **Income Definition** (e.g. disposable income, gross income, consumption) used in the underlying surveys. This is especially useful for the current analysis, because it allows us to restrict the analysis to estimating changes in income inequality net of the various taxes, transfers, subsidies without having to measure each of these components individually. I re-coded the income definition variable as indicating either *gross inequality* or *net inequality*, depending on whether the income data were measured before or after taxes and transfers, respectively. I was able to re-classify 80% of the 5,200 observations as either *gross* or *net*, with 20% of the sample remaining unclassified. See Appendix C for details of the recoding scheme and for descriptive statistics.

Of the 5,200 data points, however, many are due to repeated observations within a country-year. This is a result of the fact that UNU-Wider draw from multiple sources and often report Gini indices for multiple areas, populations, ages, and statistical units. It is possible to restrict the sample only to those observations covering the entire country, all populations, all ages, and so on – but at the cost of severely limiting the estimation sample. I opted to preserve coverage. Following this principle, I simply averaged multiple observations within each country-year and within each of the three Gini measures (Gross, Net, And Unclassified). For example, say that Ghana in 1987 includes four data points: three distinct measures of Gross Inequality, one measure of Net Inequality, and no measures that I could not classify. I would use the simple mean of the three surveys as the Gross Income Gini, the single observation for the Net Income Gini, and the Unclassified Income Gini remains unobserved for that country-year. In order to maintain comparability to the first section I omit the handful of observations in UNU-Wider for countries either not in the Penn World Table or observed before 1950.

Independent Variable: Democracy

I use the Przeworski et al (2000) definition for the main specifications in this section. As a robustness check I re-estimate all the regressions using (a) the Boix and Rosato (2001) indicator, which adds the stipulation that at least 50% of the male population have the right to vote; (b) the revised Polity score, a continuous variable scales from -10 for a perfect autocracy to +10 for a perfect democracy, taken from the Polity IV data set; and (c) a dichotomous indicator based on the revised Polity score, which I code 1 for positive values of Polity and zero otherwise. The results for alternative specifications are virtually identical (see Appendix D).

5.2 Empirical Model

I estimate the effect of democratization on *net inequality* using the ADL framework. A Lagrange Multiplier test indicates that including two lags of the dependent variable is sufficient for removing serial correlation. I could not reject that the coefficients of the lagged independent variables each were zero, meaning that I could not reject the simpler lagged-dependent variable (LDV) model instead of the general ADL model. The difference between the two models is minor; as before the LDV model allows us to estimate the shift in equilibrium levels of inequality due to democratization via the long-run multiplier, but we model the short-term impact as setting in immediately. The bivariate LDV model is given in Equation (3), which easily generalizes to include covariates.

$$y_{i,t} = \beta_1 x_{i,t} + \mu_1 y_{i,t-1} + \mu_2 y_{i,t-2} + \alpha_i + v_{i,t} \quad (3)$$

where $y_{i,t}$ is the Gini index in country i in year t (in levels, expressed as a percent), $x_{i,t}$ is the contemporaneous measure of regime, $y_{i,t-1}$ is the first lag of the Gini variable, the α_i are country-specific dummy variables and $v_{i,t}$ is the i.i.d. error term.

Main Effect

In the simplest specification we see that democratization is associated with an *increase* in net inequality on the order of five points in the Gini index (Table 8, Column 1). Recall that our measure of inequality is measured post taxes and transfers – that is, taking into account all redistributive government activity. To put the magnitude of the effect into perspective, income inequality in the United States has risen six Gini points over the past 60 years – from 38% in 1947 to 44% in 2007¹⁸. The estimate is somewhat attenuated and loses statistical significance when we add GDP per capita as a covariate ($p=0.11$), but in no specification do we find that democratization reduces net inequality. The results are virtually identical when controlling for the incidence of civil war, interstate war, and military expenditures (not reported).

Table 8. Democracy and Net Income Inequality

Gini (post-tax and transfer)	(1)	(2)	(3)	(4)	(5)
Democracy	1.24 (0.52)*	0.98 (0.60)	1.24 (0.50)*	1.00 (0.45)*	0.88 (0.56)
Real Per Capita GDP		0.10 (0.05)*			0.09 (0.06)
Real Per Capita GDP Growth			-0.05 (0.06)		-0.06 (0.06)
Trade Openness				0.02 (0.01)*	0.01 (0.01)
Gini (lagged)	0.40 (0.07)**	0.38 (0.07)**	0.41 (0.07)**	0.39 (0.07)**	0.39 (0.07)**
Gini (lagged 2)	0.34 (0.07)**	0.33 (0.08)**	0.32 (0.08)**	0.33 (0.07)**	0.30 (0.08)**
Democracy (Long Run Multiplier)	4.71 (1.95)*	3.37 (2.05)	4.63 (1.82)*	3.50 (1.52)*	2.83 (1.79)
Observations	612	612	611	612	611
Clusters (# countries)	64	64	64	64	64
R-squared	0.92	0.91	0.92	0.92	0.92

All specifications include country fixed effects. Constant terms not reported.

* significant at 5%; ** significant at 1% (Robust standard errors clustered by country in parentheses).

¹⁸ Based on family income. U.S. Census Bureau (<http://www.census.gov/hhes/www/income/histinc/f04.html>)

Conditional Effects: Economic Development

Testing for conditional effects is a bit more challenging due to the smaller sample size. In particular, I could not estimate the models for the subsets of Low Income countries and Highly Fractionalized countries due to missing data. Also, I do not condition on initial levels of inequality. Nonetheless, the conditional specifications are quite revealing.

I first condition on initial level of economic development, where countries are classified as Low, Middle, and High Income countries as in Section 4 above. I retain the classification of three equal quantiles based on the full sample of countries in the Penn World Table so that the classification faithfully reflects the distribution of countries around the world and to ensure comparability to the analyses above. It should be noted, however, that Low and Middle Income countries are significantly underrepresented in the estimation sample due to the limited coverage of Gini data for these countries.

Among Middle Income countries, democratization is associated with a seven point *increase* in the long-run level of inequality, although the result is not statistically significant, with $p=0.102$ (Table 9, Columns 1 and 2). Among High Income countries, the point estimate is just over four and is significant at the 1% level. The large point estimate for middle income countries suggests that if there is a conditional wealth effect, it is nonlinear. I re-estimated the conditional model for various definitions of high and low income and found that democratization increases inequality by roughly five Gini points for all countries above a threshold of \$1,000 real per capita GDP in 2000 international dollars, significant at the 1% level¹⁹. Below this threshold the point estimate is extremely unstable in the sense that case-wise deletion yields estimates from -40 to +69, none of which are significant due to enormous standard errors.

There are two straightforward interpretations of these results. The first is that there is a conditional effect: democratization increases long-run inequality by an average of five Gini points in countries above a minimal development threshold of \$1,000 real GDP per capita (2000 international dollars) but has no effect below this threshold. The second is that democratization increases long-run inequality by five Gini points in *all* countries (i.e. the effect is absolute with respect to development), but that the data for the poorest countries are plagued with measurement error, which reduces the estimated effect in the full sample, and moreover is lacking in coverage, which results in an unstable estimate for the sample of poor countries. The first interpretation, that of a conditional effect, is simpler and accords with the intuition that there is not much to redistribute in the first place given real GDP per capita under \$3 per day (2000 international dollars). Neither interpretation, however, supports the redistributive hypothesis, which does not predict an increase in inequality under any circumstances.

Conditional Effects: Ethnic Fractionalization

I test whether democracy increases equality conditional on ethnic fractionalization in Low and Medium Fractionalization countries as defined in the previous section. I cannot run the specifications for High Fractionalization countries or those with dominant minority groups due to insufficient data. Within Low Fractionalization countries, democratization is followed by a four point increase in inequality, though the result is not statistically significant (Table 9, Column 4).

¹⁹ The non-poor countries constitute over 80% of the estimation sample: 526 of 628 country-years.

Table 9. Democracy and Net Income Inequality: Conditional Effects

Gini (post-tax and transfer)	Middle Income (1)	High Income (2)	Non-poor [‡] (3)	Low Fractionalization (4)	Medium Fractionalization (5)
Democracy	2.79 (1.00)*	2.00 (0.51)**	2.08 (0.55)**	0.93 (0.69)	-0.60 (0.21)**
Real Per Capita GDP	-0.23 (0.25)	0.17 (0.06)**	0.10 (0.08)	0.12 (0.05)*	-0.01 (0.24)
Real Per Capita GDP Growth	0.13 (0.11)	-0.09 (0.06)	-0.05 (0.07)	0.08 (0.05)	-0.19 (0.06)**
Trade Openness	0.04 (0.06)	0.01 (0.01)	0.01 (0.01)	-0.00 (0.01)	0.05 (0.03)
Gini (lagged)	0.35 (0.11)**	0.31 (0.09)**	0.35 (0.07)**	0.42 (0.09)**	0.25 (0.12)*
Gini (lagged 2)	0.23 (0.07)**	0.24 (0.08)**	0.26 (0.07)**	0.35 (0.11)**	0.17 (0.08)
Democracy (Long Run Multiplier)	6.61 (3.78)	4.48 (0.68)**	5.33 (1.33)**	4.00 (2.92)	-1.02 (0.29)**
Observations	98	411	526	404	156
Clusters (# of countries)	15	36	56	29	23
R-squared	0.95	0.89	0.92	0.92	0.90

All specifications include country fixed effects (not reported).

* significant at 5%; ** significant at 1% (Robust standard errors clustered by country in parentheses).

[‡] Countries with initial levels of real per capita GDP over \$1,000 in constant 2000 international dollars.

Among countries with intermediate levels of ethnic fractionalization, I find that democratization is followed by a decrease in inequality of nearly one Gini point, significant at the 1% level (Table 9, Column 5). The result, however, is not robust to case-wise deletion. In particular, the model cannot be estimated once Pakistan is excluded. In other words, democratization led to a one Gini-point decrease in income inequality in Pakistan, but we simply do not have enough data to test the conditional hypothesis in other countries with medium levels of ethnic fractionalization.

Democratization and the Reduction of Inequality

So far, we've found that democratization increases net inequality, especially in countries that surpass the development threshold of \$1,000 real GDP per capita in 2000 international dollars. What remains unclear, however, is the extent to which net inequality rises *because of* new taxes and transfers as opposed to changes in the pre-tax income distribution. The UNU-Wider data lend themselves to answering this question. Since we have Gini estimates based on both gross and net income we can directly compare the effects of democratization on income inequality as measured before and after taxes and transfers.

Table 10. Gross Inequality, Net Inequality, and the Effective Reduction of Income Inequality

	Full sample			Initial real GDP per capita over \$1,000 (in 2000 I\$)		
	Gross Inequality	Net Inequality	Inequality Reduction [‡]	Gross Inequality	Net Inequality	Inequality Reduction [‡]
	(1)	(2)	(3)	(4)	(5)	(6)
Democracy	1.35 (1.53)	0.88 (0.56)	0.27 (0.62)	3.14 (0.71)**	2.19 (0.54)**	0.93 (0.58)
Real Per Capita GDP	0.12 (0.06)	0.09 (0.06)	0.03 (0.05)	0.14 (0.06)*	0.11 (0.08)	0.07 (0.05)
Real Per Capita GDP Growth	0.00 (0.02)	-0.06 (0.06)	0.13 (0.12)	0.02 (0.02)	-0.07 (0.06)	0.19 (0.12)
Trade Openness	-0.00 (0.01)	0.01 (0.01)	0.02 (0.03)	-0.00 (0.01)	0.01 (0.01)	-0.00 (0.03)
Gini (lagged)	0.42 (0.07)**	0.39 (0.07)**	0.31 (0.13)*	0.36 (0.08)**	0.34 (0.08)**	0.31 (0.15)*
Gini (lagged 2)	0.19 (0.09)*	0.30 (0.08)**		0.15 (0.09)	0.26 (0.07)**	
Democracy (Long Run Multiplier)	3.52 (3.85)	2.83 (1.79)	0.39 (0.89)	6.48 (1.26)**	5.54 (1.27)**	1.35 (0.88)
Observations	383	611	255	356	509	225
Clusters (# countries)	40	64	35	34	51	28
R-squared	0.95	0.92	0.64	0.95	0.92	0.65

All specifications include country fixed effects (not reported).

* significant at 5%; ** significant at 1% (Robust standard errors clustered by country in parentheses).

[‡] The restriction to one lag of the dependent variable could not be rejected with a Lagrange multiplier test.

There are only 29 countries in the data set that (a) experience regime change, (b) have simultaneous measures of both gross and net inequality, and (c) have such measures both under democracy and non-democracy²⁰. For this small, yet diverse group of countries we can simply measure the reduction of income inequality as the difference between the net and gross Ginis. An alternative strategy that retains a larger sample is to estimate the effect of democracy on gross and net inequality separately, and simply compare the two, keeping in mind that each is based on a different estimation sample. The comparison makes sense to the extent that there are no systematic differences between the two samples. Specifically, since we are using a fixed effects estimator, all we require is that there are no systematic differences in the net and gross measures *across regime transitions*. For example, even if it turns out that measures of gross inequality are systematically underestimated in comparison to measures of net inequality, as long as the measurement error in gross inequality is constant before and after democratization the estimated difference between the effect of democratization on gross and net inequality will remain unbiased. The same holds for sample selection bias: as long as unobserved characteristics in countries for which only gross (or only net) measures are available remain constant within countries pre- and post-transition, the omitted variable will not bias the estimate.

Turning to the estimation results, there is ample evidence that democratization increases inequality in the pre-tax income distribution and not through the imposition of regressive tax and transfer schemes (Table 10). The estimates in account for annual measures of real per capita GDP, the growth rate of real per capita GDP, and trade openness. These are especially important covariates because it is plausible to suppose that democratization is accompanied by an opening of markets, and access to global markets would have the effect of increasing gross income inequality²¹. To the extent that including these covariates fails to control statistically for increases in inequality due to globalization, the current estimates of the effect of democratization will be biased upwards.

The left panel reports estimates for the full sample of countries and the right panel reports estimates for all but the poorest countries in the sample, for which we found the strongest and most consistent results in the initial estimation. Focusing on the right panel, we see that democratization increases the equilibrium level of gross inequality by 6.5 Gini points (Table 10, Column 4) and the equilibrium level of net inequality by 5.5 Gini points (Table 10, Column 5). In the sample of countries for which both measures are simultaneously available, democratization reduces gross inequality by one Gini point although this is not statistically significant (Table 10, Column 6). In short, democratization either causes or is accompanied by a large and statistically significant increase in pre-tax and transfer income, which is barely ameliorated by the newly elected democratic regime if at all.

5.3 Discussion

How should we interpret these results? The large, positive point estimates for the effects of net and gross income inequality and the small and statistically insignificant estimate for the reduction of inequality imply that *democratization either causes or is accompanied by a substantial increase in pre-tax and transfer inequality and that new democratic governments do little to attenuate this widening gap*. But what explains the growth of pre-tax and transfer inequality associated with democratization?

²⁰ Mexico, Guatemala, Colombia, Bolivia, Chile, Argentina, Spain, Poland, Hungary, Bulgaria, Moldova, Romania, Mali, Senegal, Ghana, Nigeria, Central African Republic, Uganda, Zambia, South Africa, Lesotho, Madagascar, South Korea, Bangladesh, Sri Lanka, Nepal, Thailand, Philippines and Indonesia.

²¹ I thank Stephen Haber for making this point.

Although a definitive answer is beyond the scope of this chapter, in the concluding section I discuss several plausible explanations as candidates for further testing.

6 Conclusion

The empirical record is inconsistent with the redistributive hypothesis. Democratization neither increases nor decreases redistribution from the wealthy to the poor in the full sample of countries. Conditional on initial levels of inequality, democratization is associated with *growth* in the public sector where initial inequality is low and a *reduction* of government spending where initial inequality is high. Most surprising is the robust evidence that income inequality tends to *rise* following democratization in all but the poorest countries. These findings are based on a sample of virtually every instance of regime transition around the world from 1950 to the present and a regression framework that is sensitive to the dynamic, within-country processes implied by the theory. I offer three interpretations that are consistent with the empirical results of this chapter as alternatives to the redistributive hypothesis.

One possibility is that democratization reduces the size of the public sector and increases inequality because democracy sets limits on government and enables sustained economic growth, the rewards of which are unevenly distributed. In Section 4, I suggested that a reduction in the size of the public sector following democratization is consistent with a view of democracy as a form of limited government. We need to assume that limited government enables greater economic growth (see North and Weingast (1989) for a theoretical explanation of why this might be true. The empirical evidence is inconclusive (e.g. Barro 1991; Ram 1986)) and to suppose that such growth is distributed unevenly.

There are several reasons, however, that this explanation is unsatisfactory. First, there is little evidence that democratization causes growth, at least in the short run (cf. Barro 1997; Levine and Renelt 1992; Przeworski et al. 2000). Second, the regressions predicting net inequality (Section 5) control for levels and changes of per capita GDP. Third, if growth were a foreseeable consequence of democratization, one would expect that, in equilibrium, some of the gains would be captured by the poor at the time of democratization.

A second interpretation is that democratization is part of a process in which the autocratic state apparatus is hollowed out from within by those who run the system, leading to an eventual collapse of the old system. Solnick (1998) argues for such a mechanism to explain the demise of the Soviet Union. Regime transition thus results from elite-level turmoil rather than pressure from below; it represents a crumbling of autocratic structures rather than the creation of new democratic institutions; and greater market inequality reflects the rise of a small group of oligarchs, who emerge from within the system to enormous private gain. This reading, though intriguing, is likely to apply only to cases in which democratization is accompanied by a transition from a planned to a market economy. It also cannot account for the cases in which democratization leads to an increase in the size of the public sector (Table 7).

A third possibility has to do with the inadequacy of the Gini coefficient as a measure of inequality. In particular, an increase in the Gini coefficient indicates greater inequality only if the post-democratization Lorentz curve always lies below the pre-democratization Lorentz curve²². If the before-and-after Lorentz curves cross at any point, a comparison of the Gini coefficients is uninformative about the relative equality of the two distributions. This opens the door for the possibility that democratization induces or is accompanied by a net movement of resources from the wealthy and the

²² I thank Gavin Wright for emphasizing this point.

poor to the middle class, consistent with the redistributive pattern implied by Director's Law (Stigler 1970). One typically associates the rise of a middle class, however, with greater equality, Gini coefficients notwithstanding. This alternative hypothesis is consistent with the finding in this chapter that democratization increases the size of the public sector in countries characterized by initial equality (Table 4). An intriguing possibility for a wide range of countries, this interpretation cannot account for the five cases in which democratization leads to a decrease in the size of the public sector (Table 7).

Only further research will tell which of these three alternatives holds water, whether additional alternatives are required, or the scope conditions under which each of the various explanations might hold. Next steps require the collection of fine-grained, time-series data on the composition of revenues and expenditures and on the net redistributive effect of democratization across the income distribution. Additionally, each of the alternatives implies a distinct set of assumptions about the mechanisms underlying democratization and the mechanisms of redistribution. Careful case studies are therefore required to establish the plausibility of modeling assumptions regarding actors, preferences, and mechanisms in each scenario. In the next several chapters, I turn to these tasks in a detailed case study of Argentina, 1880-1946.

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