CHAPTER 21

Democracy, Redistribution, and Inequality

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Abstract

In this paper we revisit the relationship between democracy, redistribution, and inequality. We first explain the theoretical reasons why democracy is expected to increase redistribution and reduce inequality, and why this expectation may fail to be realized when democracy is captured by the richer segments of the population; when it caters to the preferences of the middle class; or when it opens up disequalizing opportunities to segments of the population previously excluded from such activities, thus exacerbating inequality among a large part of the population. We then survey the existing empirical literature, which is both voluminous and full of contradictory results. We provide new and systematic reduced-form evidence on the dynamic impact of democracy on various outcomes. Our findings indicate that there is a significant and robust effect of democracy on tax revenues as a fraction of GDP, but no robust impact on inequality. We also find that democracy is associated with an increase in secondary schooling and a more rapid structural transformation. Finally, we provide some evidence suggesting that inequality tends to increase after democratization when the economy has already undergone significant structural transformation, when land inequality is high, and when the gap between the middle class and the poor is small. All of these are broadly consistent with a view that is different from the traditional median voter model of democratic redistribution: democracy does not lead to a uniform decline in post-tax inequality, but can result in changes in fiscal redistribution and economic structure that have ambiguous effects on inequality.

Keywords

Democracy, Education, Inequality, Political development, Redistribution, Structural transformation

JEL Classification Codes

P16, O10

21.1. INTRODUCTION

Many factors influence the distribution of assets and income that a market economy generates. These include the distribution of innate abilities and property rights, the nature of technology, and the market structures that determine investment opportunities and the distribution of human and physical capital.

But any market system is embedded in a larger political system. The impact of the political system on distribution depends on the laws, institutions, and policies enacted by that system. What institutions or policies a political system generates depends on the distribution of power in society and how political institutions and mobilized interests aggregate preferences. For example, we expect institutions that concentrate political power within a narrow segment of the population—typical of nondemocratic regimes—to generate greater inequality.[1]

[1] Nondemocracies tend to be dominated by the rich either because the rich wield sufficient power to create such a regime or because those who can wield power for other reasons subsequently use this power to become rich.

As the literature has shown, there are several theoretical mechanisms through which such an impact might operate. One would be the enactment of policies benefiting the politically powerful at the expense of the rest of society, including policies pushing down wages by repression and other means. In Apartheid South Africa prior to 1994, for example, the political system dominated by the minority white population introduced government regulations on the occupation and residential choices of black Africans in order to reduce their wages (e.g., by reducing competition for white labor and by forcing blacks into unskilled occupations, see Lundahl, 1982; Wilse-Samson, 2013). Another mechanism is the one highlighted by Meltzer and Richard's (1981) seminal paper. Building on earlier research by Romer (1975) and Roberts (1977), they developed a model where extensions of the voting franchise, by shifting the median voter toward poorer segments of society, increase redistribution, and reduce inequality.[2]

Despite these strong priors, the empirical literature is very far from a consensus on the relationship between democracy, redistribution, and inequality. Several works have reported a negative relationship between democracy and inequality using specific historical episodes or cross-national studies. Acemoglu and Robinson (2000) argued this was the case based on the economic history of nineteenth-century Europe and some twentieth-century Latin American examples. An important study by Rodrik (1999) presented evidence from a panel of countries that democracy is associated with higher real wages and higher labor share in national income. Lindert (1994, 2004) provided evidence from OECD countries indicating a linkage between democratization and public spending, particularly on education; Persson and Tabellini (2003) presented similar cross-national evidence; and Lapp (2004) pointed to a statistical association between democratization and land reform in Latin America. Other papers point in the opposite direction, however. Sirowy and Inkeles (1990) and Gradstein and Milanovic (2004) have argued that the cross-national empirical evidence on democracy and inequality is ambiguous and not robust. Scheve and Stasavage (2009, 2010, 2012) have claimed that there is little impact of democracy on inequality and policy among OECD countries, and Gil et al. (2004) have forcefully argued that there is no relationship between democracy and any policy outcome in a cross section of countries (Perotti, 1996, was an earlier important paper with similar negative findings).

In this chapter we revisit these issues in a unified theoretical and empirical framework. Theoretically, we review the standard Meltzer-Richard model and point out why the relationship between democracy, redistribution, and inequality may be more complex than the standard model might suggest. First, democracy may be "captured" or "constrained." In particular, even though democracy clearly changes the distribution of de jure power in society (e.g., Acemoglu and Robinson, 2006), policy outcomes

[2] Historically, the fear of expected redistribution has been one of the factors motivating the opposition to democracy (see Guttsman, 1967).

and inequality depend not just on the de jure but also the de facto distribution of power. For example, Acemoglu and Robinson (2008) argue that, under certain circumstances, those who see their de jure power eroded by democratization may sufficiently increase their investments in de facto power (e.g., via control of local law enforcement, mobilization of nonstate armed actors, lobbying, and other means of capturing the party system) in order to continue to control the political process. If so, we would not see an impact of democratization on redistribution and inequality.[3] Similarly, democracy may be constrained by either other de jure institutions such as constitutions, conservative political parties, and judiciaries, or by de facto threats of coups, capital flight, or widespread tax evasion by the elite.

Second, we suggest that democratization can result in "inequality-increasing market opportunities." Nondemocracy may exclude a large fraction of the population from productive occupations (e.g., skilled occupations) and entrepreneurship (including lucrative contracts) as in apartheid South Africa or the former Soviet bloc countries. To the extent that there is significant heterogeneity within this population, the freedom to take part in economic activities on a more level playing field with the previous elite may actually increase inequality within the excluded or repressed group and consequently the entire society.[4]

Finally, consistent with Stigler's (1970) "Director's law", democracy may transfer political power to the middle class rather than to the poor. If so, redistribution may increase and inequality may be curtailed only when the middle class is in favor of such redistribution.

After reviewing the fairly large and heterogeneous prior literature on this topic, the rest of this chapter examines the empirical impact of democracy on tax revenues as a percentage of GDP (as an imperfect measure of redistribution) and on inequality as well as a number of additional macro variables. We evaluate previous empirical claims about the effect of democracy in a consistent empirical framework that controls for a number of confounding variables. Our objective is not to estimate some structural parameters or the "causal" effect of democracy on redistribution, but to uncover whether there is a

[3] Relatedly, there could be reasons for dictators to redistribute and reduce inequality to increase the stability of that regime (e.g., Acemoglu and Robinson, 2001; Albertus and Menaldo, 2012, more generally). Plausible cases of this would be the land reform implemented by the Shah of Iran during his White Revolution of 1963 to help him become more autonomous from elites (McDaniel, 1991), the agrarian reforms made by the Peruvian military regime in the early 1970s (chapter 2 of Seligmann, 1995), or the educational reforms in 19th-century oligarchic Argentina (Elis, 2011).

[4] Our data show that inequality has in fact increased in South Africa between 1990 and 2000 (or 2005) and in ex-Soviet countries between 1989 and 1995 (or 2000), periods that bracket their democratic transitions in 1994 and 1989 respectively. This is probably, at least in part, driven by the increase in inequality among previously disenfranchised blacks and repressed citizens (for details on the post-democracy distributions of income see Whiteford and Van Seventer, 2000, for South Africa and Milanovic, 1998, for ex-Soviet countries).

robust correlation between democracy and redistribution or inequality, and to undertake a preliminary investigation of how this empirical relationship changes depending on the stage of development and various other factors potentially influencing how democracy operates.

The previous literature has used several different approaches (e.g., cross-sectional regressions, time-series and panel data investigations) and several different measures of democracy. We believe that cross-sectional (cross-national) regressions and regressions that do not control for country fixed effects will be heavily confounded with other factors likely to be simultaneously correlated with democracy and inequality. We therefore focus on a consistent panel of countries, and investigate whether countries that become democratic redistributed more and reduced inequality relative to others. We also focus on a consistent definition of democracy based on Freedom House and Polity indices, building on the work by Papaioannou and Siourounis (2008). One of the problems of these indices is the significant measurement error, which creates spurious movements in democracy. To minimize the influence of such measurement error, we create a dichotomous measure of democracy using information from both the Freedom House and Polity datasets as well as other codings of democracy to resolve ambiguous cases. This leads to a measure of democracy covering 184 countries annually from 1960 (or post-1960 year of independence) to 2010. We also pay special attention to modeling the dynamics of our outcomes of interest, taxes as a percentage of GDP, and various measures of structural change and inequality.

Our empirical investigation uncovers a number of interesting patterns (why many of these results differ from some of the existing papers in the literature is discussed after they are presented). First, we find a robust and quantitatively large positive effect of democracy on tax revenue as a percentage of GDP (and also on total government revenues as a percentage of GDP). The long-run effect of democracy in our preferred specification is about a 16% increase in tax revenues as a fraction of GDP. This pattern is robust to various different econometric techniques and to the inclusion of other potential determinants of taxes, such as unrest, war, and education.

Second, we find a positive effect of democracy on secondary school enrollment and the extent of structural transformation (e.g., an impact on the nonagricultural share of employment and the nonagricultural share of output).

Third, however, we find a much more limited effect of democracy on inequality. In particular, even though some measures and some specifications indicate that inequality declines after democratization, there is no robust pattern in the data (certainly nothing comparable to the results on taxes and government revenue). This may reflect the poorer quality of inequality data. But we also suspect it may be related to the more complex, nuanced theoretical relationships between democracy and inequality pointed out above.

Fourth, we investigate whether there are heterogeneous effects of democracy on taxes and inequality consistent with these more nuanced theoretical relationships. The

evidence here points to an inequality-increasing impact of democracy in societies with a high degree of land inequality, which we interpret as evidence of (partial) capture of democratic decision making by landed elites. We also find that inequality increases following a democratization in relatively nonagricultural societies, and also when the extent of disequalizing economic activities is greater in the global economy as measured by U.S. top income shares (though this effect is less robust). These correlations are consistent with the inequality-inducing effects of access to market opportunities created by democracy. We further find that democracy tends to increase inequality and taxation when the middle class is less prosperous relative to the poor. These correlations are consistent with Director's law, which suggests that democracy often empowers the middle class to redistribute from the rest of society to itself. Our results suggest the need for a more systematic investigation of the conditions under which democracy does indeed reduce inequality and increase redistribution.

The chapter proceeds as follows. In the next section we discuss the theoretical connections between democracy, redistribution, and inequality. In Section 21.3 we provide a survey of the existing empirical literature on the impact of democracy on taxes, redistribution, inequality, and some other reduced-form dependent variables potentially associated with inequality (e.g., average calories per person, life expectancy, and infant mortality). Section 21.4 then describes our econometric methodology and data. Section 21.5 presents our new findings, and Section 21.6 concludes.

21.2. THEORETICAL CONSIDERATIONS

In this section, we illustrate some of the linkages between democracy and inequality that have been proposed in the literature. We begin with the seminal Meltzer and Richard (1981) model, but then alter the set of instruments available to the government to show how the logic of the standard model can be altered and even reversed. We will discuss the impact of democracy, modeled as a broader franchise, relative to a nondemocratic regime modeled as a narrower franchise or controlled by a small group. This broadening of access to political power is what our primary cross-country empirical measures of democracy attempt to capture, and is arguably the most important feature of a democratic regime.

21.2.1 The Redistributive and Equalizing Effects of Democracy

We start with the standard "equalizing effect" of democracy, first emphasized formally in Meltzer and Richard's (1981) seminal study (see also Acemoglu and Robinson, 2006). Democratization, by extending political power to poorer segments of society, will increase the tendency for pro-poor policy naturally associated with redistribution, and thus reduce inequality.

Suppose that society consists of agents distinguished only with respect to their endowment of income, denoted by y_i for agent i, with the distribution of income in the society

denoted by the function $F(y)$ and its mean by $\overline{y}$. The only policy instrument is a linear tax τ imposed on all agents, with the proceeds distributed lump-sum again to all agents. We normalize total population to 1 without loss of any generality.

The government budget constraint, which determines this lump-sum transfer T, takes the form

$$T \leq \tau\overline{y} - C(\tau)\overline{y}, \tag{21.1}$$

where the second term captures the distortionary costs of taxation. $C(\tau)$ is assumed to be differentiable, convex and nondecreasing, with $C'(0) = 0$.

Each agent's post-tax income and utility is given by

$$\hat{y}_i = (1-\tau)y_i + \tau\overline{y} - C(\tau)\overline{y}. \tag{21.2}$$

This expression immediately makes it clear that preferences over policy—represented by the linear tax rate τ—satisfy both single crossing and single-peakedness (e.g., Austen-Smith and Banks, 1999). Hence the median voter theorem, and its variants for more limited franchises (see e.g., Acemoglu et al., 2012) hold.[5]

Suppose, to start with, that there is a limited franchise such that all agents with income above y_q, the q^{th} percentile of the income distribution, are enfranchised and the rest are disenfranchised. Consider a "democratization," which takes the form of y_q decreasing, say to some $y_{q'} < y_q$, so that more people are allowed to vote. Let the equilibrium tax rate under these two different political institutions be denoted by τ_q and $\tau_{q'}$, and the resulting post-tax income distribution by F_q and $F_{q'}$. Then from the observation that the median of the distribution truncated at $y_{q'}$ is always less than the median for the one truncated above $y_q > y_{q'}$, the following result is immediate:

Proposition 1
Redistributive Effects of Democracy
Suppose that starting from only those above y_q being enfranchised, there is a further democratization so that now those above $y_{q'} < y_q$ are enfranchised. This democratization leads to higher taxes ($\tau_{q'} \geq \tau_q$), higher redistribution, and a more equal distribution of post-tax income in the sense that $F_{q'}$ is more concentrated around its mean than F_q.

A few comments about this proposition are useful. First, this result is just a restatement of Meltzer and Richard's (1981) main result. Second, the first part of the conclusion is stated as $\tau_{q'} \geq \tau_q$, since if both y_q and $y_{q'}$ are above the mean, with standard arguments, $\tau_{q'} = \tau_q = 0$. Third, the second part of the conclusion does *not* state that F_q is a

[5] Namely, if we assume that policy choices are made by either a direct democracy procedure choosing the Condorcet winner (if one exists) or as a result of competition between two parties choosing (and committing to) their platforms, the equilibrium will coincide with the political bliss point of the median-ranked voter. As Austen-Smith and Banks (1999) discuss in detail, these types of results, though powerful, are rather special and rely, among other things, on the assumption that the policy space is unidimensional.

mean-preserving spread of, or is second-order stochastically dominated by $F_{q'}$, because higher taxes may reduce mean post-tax income due to their distortionary costs of taxation. Instead, the statement is that $F_{q'}$ is more concentrated around its mean than F_q, which implies the following: if we shift $F_{q'}$ so that it has the same mean as F_q, then it second-order stochastically dominates F_q (and thus automatically implies that standard deviation and other measures of inequality are lower under $F_{q'}$ than under F_q).

Finally, the result in the proposition should be carefully distinguished from another often-stated (but not unambiguous) result, which concerns the impact of inequality on redistribution. Persson and Tabellini (1994) and Alesina and Rodrik (1994), among others, show that, under some additional assumptions, greater inequality leads to more redistribution in the median voter setup (which in these papers is also embedded in a growth model). This result, however, is generally not true.[6] It applies under additional assumptions on the distribution of income, such as a log normal distribution, or when the gap between mean and median is used as a measure of inequality (which is rather non-standard). In contrast, the result emphasized here is unambiguously true.

This result of Meltzer and Richard (1981) is the basis for the hypothesis that democracy should increase taxation and income redistribution and reduce inequality. In the model, the only way that redistribution can take place is via a lump-sum transfer. This is obviously restrictive. For example, it could be that individuals prefer the state to provide public goods (Lizzeri and Persico, 2004) or public education. Nevertheless, the result generalizes, under suitable assumptions, to the cases in which the redistribution takes place through public goods or education.

We next discuss another possible impact of democracy and why its influence on redistribution and inequality may be more complex than this result may suggest.

21.2.2 Democracy and the Structural Transformation

The logic of Proposition 1 applies when the main political conflict involves the tax rate but not other policy instruments. One of the most important alternatives, emphasized by Moore (1966) and by Acemoglu and Robinson (2006) in the economics literature, is the combination of policies used to create abundant (and cheap) labor for the rural sector (see also Llavador and Oxoby, 2005). Many nondemocratic agrarian societies use explicit and implicit limits on migration out of the rural sector, together with labor repression, to keep wages low and redistribute income from the population to the politically powerful landed elites. Even industrial sectors in nineteenth century England used the Master and Servant

[6] Consider the following counterexample. In society A, 1/3 of the population has income 2, 1/3 has income 3 and the remaining 1/3 has income 7. If everyone is enfranchised, the Condorcet winner is a tax rate $\tau^A > 0$ with $C'(\tau^A) = 1/4$. In society B, 1/3 of the population has income 0, 1/3 has income 4 and the remaining 1/3 has income 8. If everyone is enfranchised, the Condorcet winner is a tax rate $\tau^B = 0$. Society B has a lower tax rate, and hence less redistribution despite being more unequal (the distribution of income in society A second-order stochastically dominates the distribution of society B).

law to prosecute workers and repress trade unions, and it was only repealed following an expansion of the franchise to workers and decriminalization of workers' organizations (Naidu and Yuchtman, 2013). For example, in rural Africa, land is often controlled by traditional rulers and chiefs and not held as private property. People moving away from particular chieftaincies lose rights over land, which inhibits migration. In Sierra Leone, forced labor controlled by chiefs was common in rural areas prior to the civil war in 1991 (e.g., Acemoglu et al., 2014). We may expect that these policies will be relaxed or lifted when political power shifts either to industrialists, who would benefit from migration out of the rural sector into the industrial one, or to poorer segments of society who are bearing the brunt of lower wages (see Acemoglu, 2006, for a political economy analysis of wage repression and the impact of democracy on it).

To model these issues in the simplest possible way, suppose that there is a single policy instrument denoted by $\eta \in \mathbb{R}_+$ capturing the extent of barriers against mobility out of the rural sector. Suppose now that y_i denotes the land endowment of agent i, so that post-policy income (and utility) of an agent is given by

$$\hat{y}_i = \omega(\eta) + \upsilon(\eta) y_i, \tag{21.3}$$

where $\omega(\eta)$ can be interpreted as the impact of this policy on wage income (thus it applies agents with no land endowment) and naturally we assume that $\omega(\eta)$ is decreasing. On the other hand, $\upsilon(\eta)$ is the impact of its policy on land rents, and is thus increasing. This formulation can also be easily extended to include industrialists who may also be opposed to high values of η, which would reduce the supply of labor to their sector.

Inspection of Equation (21.3) immediately reveals that preferences over η satisfy single crossing, and thus the median voter theorem again applies. This leads to the following result:

Proposition 2
Democracy and Structural Transformation
Consider the model outlined in this subsection. Suppose that starting from only those above y_q being enfranchised, there is a further democratization such that now those above $y_{q'} < y_q$ are enfranchised. This democratization leads to lower mobility barriers out of the rural sector ($\eta_{q'} \leq \eta_q$) and a more equal distribution of income (in the sense that $F_{q'}$ is more concentrated around its means than F_q).

This proposition highlights that the same reasoning that leads to the redistributive and equalizing effects of democracy also weighs in favor of lifting barriers that are against the interest of the middle class and the poor. An important implication of this might be a push toward the structural transformation out of agriculture and into industry and cities that might have been partly arrested artificially by the political process before democratization. An illustrative example of this is the impact of the 1832 Reform Act in Britain, which enfranchised urban manufacturing elites in the newly industrializing cities such as Birmingham and Manchester. This led directly to the

abolition of the Corn Laws in 1846 which was a huge distortionary subsidy to landowners (Schonhardt-Bailey, 2006).

It is also straightforward to apply this reasoning to other policies related to redistribution and structural transformation, such as investment in mass schooling, which we may also expect to be boosted by democratization.

21.2.3 Other Considerations

Obviously, the simple model presented in the previous two subsections leaves out many mechanisms which might influence the extent of redistribution in a democracy and other forces that can shape the political equilibrium (Putterman, 1996, provides an overview of many ideas).[7]

Several papers have investigated how social mobility influences the demand for redistribution even in a democracy (Alesina and La Ferrara, 2005; Bénabou and Ok, 2001; Carter and Morrow, 2012; Wright, 1996). When rates of social mobility are high and tax policy is sticky, people who are poor today may not support high rates of taxation and redistribution because they worry that it will negatively impact them should they become rich in the future. Relatedly, Piketty (1995) suggests that different beliefs about distortionary taxation can be self-fulfilling and lead to multiple equilibria, some with low inequality and a lot of redistribution, and others with high inequality and little redistribution (see also Alesina and Angeletos, 2005; Bénabou, 2001, 2008; Bénabou and Tirole, 2006). Thus, a democratic society could result in an equilibrium with little redistribution.

Alternatively, it could be that social cleavages or identities may be such as to reduce the likelihood that a coalition favoring redistribution would form (De la O and Rodden, 2008; Frank, 2005; Lee, 2003; Roemer, 1998; Roemer et al., 2007; Shayo, 2009). For example, in Roemer's model there is a right-wing political party that does not like taxation and redistribution and a left-wing political party that does. People are ideologically predisposed toward one of the parties, but they also care about religion, as do the parties. If the right-wing party is Catholic, a poor Catholic may vote for it even if it does not offer the tax policy that the voter wishes. Another reason that the above model may fail to characterize the political equilibrium accurately is because ethnic heterogeneity limits the demand for redistribution (Alesina and Glaeser, 2004; Alesina et al., 1999). Daalgard et al. (2005) argue that institutions, particularly ones that influence the

[7] We have also left out a discussion of several other important issues that have been raised in theoretical analysis of redistribution in democracy. In particular, there is a growing and vibrant literature on redistribution in a dynamic context, including Krusell et al. (1997), Krusell and Ríos-Rull (1999), Hassler et al. (2003), Battaglini and Coate (2008), and Acemoglu et al. (2012). Overviews of other aspects of democratic policymaking are provided in Drazen (2000), Persson and Tabellini (2000), Acemoglu and Robinson (2006), and Besley (2007). The political economy literature on the emergence of democracy is also beyond the scope of our chapter, and we refer the reader to the extensive discussions in Acemoglu and Robinson (2006).

efficiency of the state, will influence the demand for redistribution. Finally, recent work has tied the amount of social capital to the extent of redistribution such as in Scandinavia (Algan et al., 2013).

Another idea, due to Moene and Wallerstein (2001), is that most redistribution under democracy does not take the form of transfers from rich to poor but of social insurance. Moene and Wallerstein develop a model to show that the comparative statics of this with respect to inequality may be very different from the Meltzer-Richard model.

In the rest of this section, we will instead focus on what we view as the first-order mechanisms via which democracy may fail to increase redistribution or reduce inequality.

21.2.4 Why Inequality May Not Decline: Captured Democracy and Constraints on Redistribution

In contrast to Propositions 1 and 2, greater democratization may not always reduce inequality. In this and the next two subsections, we discuss several mechanisms for this.

The first possible reason is that even though democracy reallocates de jure power to poorer agents, richer segments of society can take other actions to offset this by increasing their de facto power. This possibility, first raised in Acemoglu and Robinson (2008), can be captured in the following simple way here. Suppose that the distribution of income has mass at two points, the rich elite, who are initially enfranchised, and the rest of the citizens, who make up the majority of the population and are initially disenfranchised. Suppose, in addition, that the rich elite can undertake costly investments to increase their de facto power (meaning the power they control outside those that are strictly institutionally sanctioned, such as their influence on parties' platforms via lobbying or repression through control of local law enforcement or nonstate armed actors; see Acemoglu and Robinson, 2006, 2008; Acemoglu et al., 2013b,c). If they do so, they will "capture the political system," for example, control the political agenda of all parties or change political ideology via the media. Suppose also that this type of capture is costly, with cost denoted by $\Gamma > 0$. Then clearly, when there is a limited franchise, the elite will not need to incur the cost for doing so. Once there is enfranchisement, if this cost is not too large, they will find it beneficial to incur this cost, and may then succeed in setting the tax rate at their bliss point, rather than putting up with the higher redistribution that the majority of citizens would impose.

This reasoning immediately implies the following result:

Proposition 3
Captured Democracy
Suppose that the elite can control the political system after democratization at cost $\Gamma > 0$. Then if Γ is less than some $\overline{\Gamma}$, they will prefer to do so, and democratization will lead to no change in taxes and the distribution of income.

This proposition, in a simple way, captures the main idea of Acemoglu and Robinson (2008), even though the specific mechanism for capture is somewhat different. In Acemoglu and Robinson, each elite agent individually contributes to their collective de facto power, which needs to be greater in democracy to exceed the increased de jure power of poor citizens. Under some conditions, the main result of Acemoglu and Robinson (2008) is that the probability of the elite controlling political power is invariant to democratization—or more generally may not increase as much as it may have been expected to do owing to the direct effect of the change in de jure power.

A related channel to Proposition 3 is that democracy may be highly dysfunctional, or effectively captured, because its institutional architecture is often chosen by previous restricted franchises or dictatorships. Acemoglu et al. (2011) develop a model where the elite can take control of democracy by forming a coalition in favor of the continuation of patronage, keeping the state weak.

Other mechanisms include de jure constitutional provisions that restrict the scope for redistribution (e.g., a cap on τ) after democratization. For instance, Siavelis (2000) and Londregan (2000) argue that the constitution imposed by the Pinochet government in Chile prior to the transition to democracy was a way to constrain future redistribution. Another is the threat of a future coup preventing democracy from pursuing high redistribution. Ellman and Wantchekon (2000) discuss how fear of a military coup induced voters to support the right-wing ARENA party, taking redistribution off the political agenda, and also suggest that similar forces operated in electing Charles Taylor in Liberia in 1997 (see also Acemoglu and Robinson, 2001). An alternative mechanism is the threat of capital flight increasing the cost of redistribution (in the reduced-form model here, this would mean an increase in $C(\tau)$).[8] Moses (1994) argues that this was the case for Sweden in 1992, as well as Campello (2011) and Weyland (2004), among others, who suggest that capital flight restrained redistribution in new Latin American democracies (see also Acemoglu and Robinson, 2006). Mohamed and Finnoff (2003) similarly argue that capital flight constrained redistribution in post-apartheid South Africa (see also Alesina and Tabellini, 1989; Bardhan et al., 2006). All of these constraints would reduce the potential impact of democracy on inequality.

An implication of Proposition 3 and our discussion is that democracy may change neither fiscal policy nor the distribution of income. Nevertheless, it is also useful to note that a variant of this model can lead to an increase in taxes without a major impact on inequality. Suppose, for example, that the elite can use their de facto power to redirect spending toward themselves (e.g., toward some public goods that mostly benefit the elite such as investments in elite universities rather than in primary or secondary education),

[8] A related idea, proposed by Dunning (2008), is that if the main source of tax revenues is from natural resource rents, rather than personal income or wealth taxes, the elite have less incentive to oppose or capture democracy.

but have a more limited ability to control taxes. In that case, a variant of Proposition 3 would apply whereby democracy might be associated with an increase in taxation, but may not have a major impact on inequality. Moreover, in the Acemoglu et al. model mentioned above, democracy may increase taxes in order to use them as payments to state employees, but still not increase redistribution or reduce inequality.

Another variant of this result where elites can block democratization ex-ante, rather than capturing democracies ex-post, shows how selection bias can affect the correlation between democracy and the extent of redistribution observed. If elites can block democratizations that would be highly redistributive, then the only democratizations that are observed would be those that are not particularly redistributive, and we would see no correlation between democracies and increased taxation or redistribution.

A number of studies present empirical evidence consistent with these mechanisms. Larcinese (2011), for example, shows that the democratization of Italy in 1912, though it had a large positive effect on the number of people who voted, had little impact on which parties were represented in the legislature, something he interprets as consistent with the democracy being captured by old elites. Berlinski and Dewan (2011) similarly show that the British Second Reform Act of 1868, though it greatly expanded voting rights, did not have a significant immediate impact on representation.

Anderson et al. (2011) show that in Maharashtra in Western India, areas where the traditional Maratha landlords are powerful as measured by their landholdings, have democratic equilibria that are far more pro-landlord and anti-poor because the Maratha elites control voting behavior via their clientelistic ties to workers. See also Baland and Robinson (2008, 2012) on Chile; McMillan and Zoido (2004) on Peru; Pettersson-Lidbom and Tyrefors (2011) on Sweden; and Albertus and Menaldo (2014) for a cross-country empirical study of how the strength of elites at the time of democratization influences how redistributive democracy is.

There is also qualitative historical evidence on the redistributive constraints faced by democracies. Writers since James Madison have argued that the U.S. constitution is an effective bulwark against redistribution (Beard, 1913; Holton, 2008; McGuire, 2003). Others have noted that the constitution was a large obstacle to slave emancipation (Einhorn, 2006; Waldstreicher, 2009), and Dasgupta (2013) argues that the Indian constitution has been a key component in elites maintaining control of land reform projects.

21.2.5 Why Inequality May Not Decline: Inequality-Increasing Market Opportunities

Our second mechanism for an ambiguous effect of democracy on inequality is inspired by the experiences of South Africa and Eastern Europe. In South Africa, the end of apartheid in 1994 has been associated with an increase in inequality. This is partly because the black majority now takes part in economic activities from which it was previously excluded,

and earnings are more dispersed in these activities than the low-skill, manual occupations to which they were previously confined. Likewise in Eastern Europe after 1989, the collapse of communism created new opportunities for people who were previously trapped in sectors of the economy where they could not use their skills and talents optimally (Atkinson and Micklewright, 1992; Flemming and Micklewright, 2000).

To incorporate this possibility, let us return to the model of structural transformation presented above. Suppose that y_i denotes the "skill" endowment of agent i, and is strictly positive for all agents. Now $\eta \in \{0,1\}$ denotes a policy instrument preventing people from moving into some potentially high-productivity activity, with $\eta = 1$ representing such prevention and $\eta = 0$ as its cessation. Post-policy income of agent i is

$$\hat{y}_i = \upsilon(\eta) y_i \mathbf{I}(y_i > y_q) + (1-\eta) y_i + w_0,$$

where $\upsilon(\eta)$ denotes the return to agents above the $q^{\text{th}} > 0.5$ percentile of the distribution (e.g., the landowners) from preventing the rest of the population's entrance into the high-productivity activities (e.g., banning black workers in South Africa from skilled occupations). The indicator function $\mathbf{I}(y_i > y_q)$ makes sure that this term only applies to agents above the q^{th} percentile. In view of this, it is natural to assume that $\upsilon(\eta = 1) > \upsilon(\eta = 0) + 1$ so that the very rich benefit from this policy. In addition, if $\eta = 1$, then the remaining workers just receive a baseline wage $w_0 > 0$. In contrast, if $\eta = 0$, they are able to take part in economic activities, and in this case, some of them, depending on their type, will be more successful than others.

The median voter theorem still applies in this formulation, and following democratization extending the franchise sufficiently, the political process will lead to a switch to $\eta = 0$. However, this formulation also makes it clear that the increased market opportunities for agents below the q^{th} percentile will create inequality among them. This effect can easily dominate the reduction in inequality resulting from the fact that the very rich no longer benefit from restricting access for the rest of the population. We summarize this result in the next proposition:

Proposition 4
Implications of Inequality-Inducing Market Opportunities
In the model described in this subsection, suppose there is an increase in democracy. If a sufficient number of voters are enfranchised, this will lead to a switch from $\eta = 1$ *to* $\eta = 0$*, but the implications for inequality are ambiguous.*

21.2.6 Why Inequality May Not Decline: The Middle Class Bias

The third possible reason for a limited impact of democracy on inequality is that, with additional tax instruments, greater democratization may empower the middle class (loosely and broadly defined), which can then use its greater power to redistribute to

itself. Suppose society now consists of three groups: the rich elite with income y_r, the middle class with income $y_m < y_r$, and the poor with income $y_p < y_m$. Let the proportions of these three groups be, respectively, δ_r, δ_m, and δ_p. Consider an extension of the baseline model where there are two types of transfers: the lump-sum transfer, T, as before, and a transfer specifically benefiting the middle class, denoted by T_m. The government budget constraint is then

$$T + \delta_m T_m \leq \tau\overline{y} - C(\tau)\overline{y}. \tag{21.4}$$

Now suppose that starting with the rich elite in power there is a democratization, which makes the median voter an agent from the middle class. This will be the case if there is a limited franchise extension only to the middle class and $\delta_r < \delta_m$ (the middle classes are more populous than the rich), or there is a transition to full democracy but the middle class contains the median voter (i.e., $\delta_r + \delta_p < \delta_m$). Clearly, when only the elite are empowered there will be zero taxation (because, given the available fiscal instruments, the elite cannot redistribute to itself). With the middle class in power, there will be positive taxation and redistribution to the middle class using the instrument T_m. The resulting income distribution may be more or less equal (it will be more equal if the middle class is much poorer than the rich, **and less equal if the middle classes are much richer than the poor**).

In this case, the impact of democracy on inequality is generally ambiguous and depends on the specific measure of inequality under consideration, the cost of taxation and the pre-democracy distribution of income. It can be shown that, focusing on the Gini coefficient, when the poor are numerous and not too poor relative to the rich, that is, when

$$\frac{\delta_p}{1-\delta_p} y_p > \frac{\delta_r}{1-\delta_r} y_r, \tag{21.5}$$

inequality increases under democracy.[9] Intuitively, in this case, taxes hurt the poor who also do not benefit from the transfers. When the poor are more numerous and richer, they bear more of the burden of taxation, and this can increase inequality.

Furthermore, whether democratization increases or reduces inequality depends on the shares of income accruing to the rich and the poor before democracy. When either

[9] In particular, the Gini coefficient under autocracy is
$G^A = \delta_p - \delta_r + s_r(\delta_m + \delta_r) - s_p(\delta_p + \delta_m)$,
where the s's denote the income shares of the rich and the poor. The Gini coefficient under democracy can be computed with the same formula but using the post-tax income shares of the rich and the poor, e.g., $\hat{s}_g = s_g(1-\tau^D)/(1-C(\tau^D))$, as
$G^D = \delta_p - \delta_r + s_r \frac{1-\tau^D}{1-C(\tau^D)}(\delta_m + \delta_r) - s_p \frac{1-\tau^D}{1-C(\tau^D)}(\delta_p + \delta_m)$.
The change in the Gini due to democratization is then
$G^D - G^A = s_p\left(\frac{\tau^D - C(\tau^D)}{1-C(\tau^D)}\right)(\delta_p + \delta_m) - s_r\left(\frac{\tau^D - C(\tau^D)}{1-C(\tau^D)}\right)(\delta_m + \delta_r)$.
Noting that $\tau^D > C(\tau^D)$, the result follows.

Equation (21.5) holds or when C is sufficiently convex that the tax choice of the middle class is not very elastic, an increase in the share of income of the rich or a decrease in the share of income of the poor makes it more likely that democracy will reduce inequality.[10] These results are summarized in the next proposition.

Proposition 5
Modified Director's Law

In the model described in this subsection, suppose there is limited enfranchisement to the middle class and $\delta_r < \delta_m$, or there is a transition to full democracy and $\delta_r + \delta_p < \delta_m$. Then there will be an increase in taxes but the effect on inequality—measured by the Gini coefficient—is ambiguous. If Equation (21.5) holds, democracy increases the Gini coefficient. Moreover, if either Equation (21.5) does not hold or C is sufficiently convex, then a larger share of income of the rich (which always increases taxes) makes it more likely that inequality will decline under democracy. If either Equation (21.5) holds or C is sufficiently convex, then a larger share of income of the poor (which also always increases taxes) makes it more likely that inequality will increase under democracy.

We refer to this result as the "Modified Director's law" since it relates to an idea attributed to Aaron Director by Stigler (1970) that redistribution in democracy involves taking from the poor and the rich to the benefit of the middle class (one can derive a similar result in a model of probabilistic voting when the middle class has a larger density for the distribution of its valence term, Persson and Tabellini, 2000, section 7.4).

This result is also related to what Aidt et al. (2009) call the "retrenchment effect" of democratization. They show that local franchise expansion in nineteenth-century Britain to the middle class often reduced expenditure on public good provision since the middle class bore the brunt of property taxes which financed local public good provision. In their model, an expansion of voting rights, by reducing public good provision and taxes on the

[10] First note that higher shares of income of the rich and the poor always increase the preferred tax rate of the middle class $\frac{d\tau^D}{ds_r} > 0$ and $\frac{d\tau^D}{ds_p} > 0$. Next, following on from Footnote 9, the impact of the share of income of the rich on the change in the Gini is
$\frac{d}{ds_r}(G^D - G^A) = -H(\tau^D)(\delta_m + \delta_r) + \left[s_p(\delta_p + \delta_m) - s_r(\delta_m + \delta_r)\right] H'^D) \frac{d\tau^D}{ds_r}$,
where $H(\tau) = (\tau - C(\tau))/(1 - C(\tau))$ is the share of revenue taken by the government in taxes, which is increasing provided that $C'(\tau), C(\tau) < 1$, and $\tau > C(\tau)$, which are automatically satisfied when τ is to the right of the peak of the Laffer curve. The first term, corresponding to the incidence of taxation on the rich, is always negative. The second term is also negative when Equation (21.5) does not hold (otherwise higher taxes, creating more resources to be transferred to the middle class, are dis-equalizing), or dominated by the first term when $\frac{d\tau^D}{ds_r} > 0$ is small, which is the case when C is sufficiently convex (so that taxes do not respond significantly to an increase in s_r).
Similarly, the impact of the share of income of the poor on the changing Gini is given by
$\frac{d}{ds_p}(G^D - G^A) = H(\tau^D)(\delta_p + \delta_m) + \left[s_p(\delta_p + \delta_m) - s_r(\delta_m + \delta_r)\right] H'^D) \frac{d\tau^D}{ds_p}$.
The first term is now positive because inequality increases when the poor bear more of the tax burden. The second effect is also positive when Equation (21.5) holds, or dominated by the first term when C is sufficiently convex.

middle class, can thus increase inequality. Relatedly, Fernandez and Rogerson (1995) show how an equilibrium like this could arise in a political economy model of taxation and educational subsidies.

An important contrast between this result and Proposition 3 is on taxes. In Proposition 3, democracy neither increases taxes nor reduces inequality (but note the contrast with extended versions of the captured democracy mechanism). Here democracy increases taxes, but because the additional revenue is used for the middle class, it may not reduce inequality.[11]

21.2.7 Discussion and Interpretation

The theoretical ideas presented so far suggest that in the most basic framework, we expect democracy to increase redistribution and reduce inequality. We may also expect a boost to structural transformation from democratization. However, several factors militate against this tendency. The elite—the richer segments of society—who stand to lose from increased redistribution can attempt to increase their de facto power to compensate for their reduced de jure power under democracy. As we have seen, this can limit redistribution and/or the potential reduction in inequality. Alternatively, consistent with Director's law, democracy may indeed increase taxes but use the resulting revenues for redistribution to the middle class, thus not necessarily reducing inequality. Finally, democracy may also be associated with the opening up of new economic opportunities to a large segment of society, which can be an additional source of inequality.

After reviewing the existing empirical literature, we will investigate the impact of democracy on redistribution and inequality. We will, in particular, study whether the effect of democracy on redistribution and inequality is heterogeneous and whether it depends on the economic and political forces we have highlighted in this section. In line with the theoretical mechanisms here, we expect the captured democracy effect to be stronger if the elite have more to lose from democracy, for example, if they are more vested in land or other assets that will lose value when wages increase and nondemocratic policies useful for these assets are lifted. Additionally, we expect the position of the middle class in the distribution of income to shape the type and extent of redistribution observed in democracy. Finally, we also expect the inequality-inducing market opportunity effect to be stronger when frontier technologies and global economic activities are more human or physical capital-biased and when society is more urbanized and presents greater opportunities for entrepreneurship and capitalist development. These are some of the ideas we will investigate in greater detail in the empirical analysis.

[11] While we do not explore this in the chapter, this result also suggests that measures of polarization, as discussed in Chapter 5, could be an important source of heterogeneity in the relationship between democracy and redistribution, as the middle class would have more to gain from taxing both the poor and the rich.

21.3. PREVIOUS LITERATURE

In this section, we survey the literature on the effect of democracy on redistribution and inequality. Our emphasis will be on the empirical literature, though we also discuss some of the theoretical ideas that have played an important role in this literature (several theoretical contributions have already been discussed in the previous section).

21.3.1 Democracy, Taxes, and Redistribution

In the basic model of the policy effects of democracy proposed by Meltzer and Richard (1981), an expansion of democracy should lead to greater tax revenues and redistribution. We first consider the tax and spending part of this. While Gil et al. (2004) found no correlation between tax revenues and different components of government spending and democracy in a cross-sectional specification, as we discuss below, there are many studies which do find such results.

This is certainly true of the more historical studies, for example, Lindert (2004), Gradstein and Justman (1999a), and Acemoglu and Robinson (2000). Aidt et al. (2006) and Aidt and Jensen (2009b) examine the impact of democratization measured by the proportion of adults who could vote in a cross-national panel consisting of 12 Western European countries over the period 1830–1938, and in a sample of 10 Western countries over the period 1860–1938, respectively. The latter paper, for example, finds robust positive effects of suffrage on government expenditure as a percentage of GDP and also tax revenues as a percentage of GDP.

One would expect that democracy not only changes the total amount of tax revenues, but also what taxes were used for. For instance, one might expect democracies to move towards more progressive taxation. Aidt and Jensen (2009b) investigated the impact of suffrage on tax incidence. They found, somewhat paradoxically, that suffrage expansion led to lower direct taxes and higher indirect taxes. Aidt and Jensen (2009a) investigated the determinants of the introduction of an income tax. They reported a nonlinear relationship with suffrage, indicating that an expansion of the franchise starting from very restrictive levels reduces the probability that an income tax will be introduced, but also that this probability increases significantly at higher levels of the franchise.

Scheve and Stasavage (2010, 2012) also adopt a long-run approach using data from OECD countries and find no correlation between democracy and either tax progressivity or the rate of capital taxation. Instead, consistent with Tilly (1985) and Besley and Pearson (2011), they emphasize the importance of warfare, a topic to which we return later.

An important study by Lindert (1994) found an impact of democracy on various types of social spending in a panel data consisting of European and North American countries as well as Japan, Australasia, Argentina, Brazil, and Mexico and spanning the period from 1880 to 1930. In his 2004 book, Lindert summarizes his findings as: "Conclusion #1: There was so little social spending of any kind before the twentieth century mainly because political voice was so restricted" (Lindert, 2004, p. 22).

A lot of research is consistent with this. Huber and Stephens (2012) build a panel dataset for Latin America between 1970 and 2007 and measure democracy by the cumulative years a country has been democratic since 1945 and estimate pooled OLS models without fixed effects. They find the history of democracy is significantly positively correlated with education spending, health spending and Social Security, and welfare spending. In a panel data of 14 Latin American countries for 1973–1997, Kaufman and Segura-Ubiergo (2001) show that democracy, as measured by the dichotomous measure introduced by Przeworski et al. (2000), is positively correlated with government expenditure on health and education but not with other components of spending. Brown and Hunter (1999) also focus on Latin America using a panel between 1980 and 1992. They examine the impact of democracy, coded as a dichotomous measure based on Przeworski et al. (2000), on social spending per capita. They also examine various types of interactions between democracy and other variables such as GDP per capita and the growth rate in GDP per capita. Their basic findings suggest that democracies have greater social spending than autocracies.

Using a broader set of countries and a panel between 1960 and 1998, Persson and Tabellini (2003) also find some evidence that democracy, as measured by the Gastil index and the Polity score, has positive effects on government expenditure and government revenues as well as welfare and Social Security spending as percentages of GDP.

Though most studies tend to focus on a broad measure of democracy, an interesting literature has examined female enfranchisement more specifically. The main focus of this research has been on whether enfranchising women has an additional or differential impact on government taxation or spending. Lindert (1994) showed that female enfranchisement had an independent effect on social spending and this finding has held up well (see Aidt and Dallal, 2008, for similar results for a later period). Lott and Kenny (1999) studied the expansion of women's voting rights in the United States between 1870 and 1940 and found that it coincided with increases in per capita state revenues and expenditures. Miller (2008) also examined this process showing that female suffrage increased health spending and led to significant falls in infant mortality.

Of all the research on this topic, only the paper by Aidt and Jensen (2013) provides an identification strategy to tackle the fact that democracy is endogenous. Building on the theoretical ideas in Acemoglu and Robinson (2000, 2006) and their previous work (Aidt and Jensen, 2011), they argue that "revolutionary threat," measured by revolutionary events in other countries, is a viable instrument for democracy in a panel of Western European countries between 1820 and 1913. Using this source of variation, they find that democracy, as measured by the extent of suffrage (proportion of the adult population that is enfranchised), has a robust positive effect on government spending relative to GDP.

In this light, the paper by Gil et al. (2004) appears an outlier in finding no effects of democracy on tax revenues as a percentage of GDP and spending. Nevertheless, there are econometric problems with all of these papers. Specifically, there is little attention to

identification problems and most studies that use panel data do not include country fixed effects, thus confounding the effect of democracy with country-specific factors potentially correlated with democracy and redistribution. Though the important study of Aidt and Jensen (2013) moves the literature a long way forward, their empirical model controls for many endogenous variables on the right side and does not deal with the possibility that revolutionary events in other countries might capture other correlated effects impacting the outcomes of interest (see the discussion of this possibility in Acemoglu et al., 2013a).

21.3.2 Democracy and Inequality

There is an even larger reduced-form empirical literature on the relationship between democracy and inequality, most of it by sociologists and political scientists rather than economists. This has typically delivered ambiguous results. Early work, which consisted mostly of simple cross-national regressions of measures of inequality (usually the income Gini coefficient) on various measures of democracy, was surveyed by Sirowy and Inkeles (1990). They concluded "the existing evidence suggests that the level of political democracy as measured at one point in time tends not to be widely associated with lower levels of income inequality" (p. 151).

Much of this literature, however, also suffers from the econometric problems of the type discussed in the last subsection. Most importantly, there is the possibility that omitted factors are affecting both inequality and democracy, and that reverse causation from inequality to democracy may be present (e.g., Muller, 1988).

Muller (1988), using a larger dataset than the previous literature, found that there was a negative correlation between the number of years a country had been democratic and inequality, which he interpreted as evidence that democracy had to be in place for long enough for inequality to fall. Yet the robustness of his results were challenged by Weede (1989) (see the response by Muller, 1989). Others, such as Simpson (1990), Burkhart (1997), and Gradstein and Justman (1999b) claimed that there was a nonlinear reduced-form relationship between democracy and inequality with inequality being low at both low and high levels of democracy and higher for intermediate levels. The plethora of results is what led Sirowy and Inkeles to be skeptical, though they do suggest that there may be some evidence in favor of the relevance of the history of democracy for inequality (Muller's original finding has been replicated in many subsequent studies, e.g., by Huber et al., 2006; Huber and Stephens, 2012, table 5.10). Nevertheless, there are good reasons for being skeptical about these findings, since the impact of the history of democracy is identified in models that do not include fixed effects, and obviously, it will capture the impact of these omitted fixed effects. More generally, this is just a special case of the difficulty of identifying duration dependence and unobserved heterogeneity—a difficulty that this literature neither tackles nor recognizes.

Three more recent studies used better data and exploited the time as well as the cross-sectional dimensions to investigate the impact of democracy on inequality. Rodrik (1999) showed that either the Freedom House of Polity III measure of democracy was positively correlated with average real wages in manufacturing and the share of wages in national income (in specifications that also control for productivity, GDP per capita and a price index). He illustrated this both in a cross section and in a panel of countries using country fixed effects. He also presented evidence that political competition and participation at large were important parts of the mechanisms via which democracy worked.[12] Scheve and Stasavage (2009) used a long-run panel from 1916 to 2000 for 13 OECD countries with country fixed effects and found that universal suffrage, measured as a dummy, had no impact on the share of national income accruing to the top 1%. Perhaps consistent with a variant of the (upper) middle class bias argument we provided above, they found that there is actually a statistically significant positive correlation between the universal suffrage dummy and what they called the "Top10-1" share, which is the share of income accruing to people between the 90th and 99th percentiles of the income distribution divided by the share accruing to the people above the 99th percentile. Finally, Li et al. (1998) used pooled OLS to show that an index of civil liberties is negatively correlated with inequality (greater civil liberties, lower inequality) though they do not investigate the relationship between inequality and more conventional measures of democracy.

Though this research has been dominated by studies that examine the average effect of democracy, Lee (2005) uses a panel data random effects model to argue that there are heterogeneous effects of democracy on inequality. The panel is unbalanced and covers 64 countries between 1970 and 1994. In particular, he argues that there is a significant interaction between the size of government as measured by tax revenues as a percentage of GDP and democracy. The paper finds that, although there is a significant positive correlation between democracy and inequality, the interaction between democracy and the size of government is significant and negative, suggesting that for large enough levels of government, democracy reduces inequality. Lee interprets this as measuring state strength (similarly to Cheibub, 1998 and Soifer, 2013).

21.3.3 Education and Democracy

The impact of democracy on education has also been examined both historically and using contemporary cross-national data and some of the results were noted in the last section. The work of Lindert (2004, chapter 5) is again central and, as with his work on social spending, Lindert presents evidence that the historical emergence of democracy is connected with educational expansion. A complementary historical study by Engerman and Sokoloff (2005, 2011) points out that within the Americas there is a close

[12] We will return to Rodrik's study below, and particularly in Appendix A, to explain the contrast between his and our results.

connection between the extent of democracy, measured by voting rights, the proportion of adults that voted and an effective secret ballot, and measures of education such as literacy rates.

A great deal of econometric work supports this research using various measures of education. Baum and Lake (2001), for example, found that secondary-school gross enrollment rates also increased with democracy across the developing world, "particularly among regimes that have experienced large changes in democracy" (p. 613) (see also Baum and Lake, 2003). Brown and Hunter (2004), focusing on 17 Latin American countries between 1980 and 1997, find that the Polity index is positively correlated with total educational expenditures per capita and also with the share of expenditures going into primary education. This finding mirrors the earlier one of Brown (1999) who finds that various dichotomous measures of democracy created from the Polity dataset and the measure of Przeworski et al. (2000) were positively correlated with primary school enrollment. Huber and Stephens (2012) also find robust evidence in Latin America for a positive correlation between the history of democracy and educational spending (see also Avelino et al., 2005).

These issues have also been intensively studied in sub-Saharan Africa. Stasavage (2005a) examined the impact of democratization in the 1990s in Africa on education, using a measure of democracy similar to Przeworski et al. (2000), and presented evidence that democracy increases total educational spending as a percentage of GDP. He also found evidence of increases in spending on primary education as a percentage of GDP, though this was not robust to the use of country fixed effects. Stasavage (2005b) provides a case study of democratization and educational expansion in Uganda. More recent research by Harding and Stasavage (2013) reconfirms the impact of democracy on primary education, this time looking at primary enrollment, and shows that the likely channel runs through a greater probability that democratic governments will abolish primary school fees.

Gallego (2010) presents one of the few attempts to develop an identification strategy to examine the impact of democracy on education. There are many reasons why this is important. Most obviously, there is the issue of whether or not there is reverse causation from education to democracy. Though the results of Acemoglu et al. (2005) reduce this concern, the above papers deal with this at best by using lagged democracy as an explanatory variable. Gallego follows Acemoglu et al. (2001, 2002) and uses their data on the historical settler mortality of Europeans and indigenous population density in 1500 as instruments for democracy and finds that democracy in 1900, measured by the Polity score, has a significant causal effect on primary school enrollment in 1900. Gallego recognizes that the exclusion restriction of his instrument may be violated but provides a very careful discussion of the potential biases that this involves and how this works against the findings he focuses on, arguing that he estimates a lower bound on the effect of democracy on education.

Using a broad sample of over 100 countries between 1960 and 2000, Ansell (2010) uses panel data regressions with and without country fixed effects to examine the impact of democracy, measured by the Polity score, on various components of educational spending. He also instruments for democracy using lagged democracy and the levels of democracy in neighboring countries. He finds that democracy has a positive and significant effect on total educational spending as a percentage of GDP, and on educational spending as a percentage of the government budget. Using cross-national regressions he also finds a negative correlation between democracy and private educational spending as a percentage of GDP and also between democracy and primary school expenditure per student by the government. He argues, contrary to Stasavage, that democracy tilts educational spending away from primary and toward secondary and tertiary education.

The likely reconciliation of all these results is that the type of education democracy produces depends on what forces democracy unleashes and who wields power in democracy. In Uganda, when President Museveni allowed democratization, he did so in a society lacking a large middle class who could dominate educational spending decisions. Hence as Stasavage showed, primary school enrollment increased. But in a large cross-national sample, the relationship may be dominated by dictatorships that spend more on primary schooling and democracies that focus on secondary schooling (see also Gradstein et al., 2004; Ansell, 2010, for relevant models).

This may also account for the results in recent work by Aghion et al. (2012), which uses a long but unbalanced panel of 137 countries between 1830 and 2001 and reports a negative correlation between the Polity score and primary school enrollment.

21.3.4 Democracy and Health Outcomes

There is also some other work on the impact of democracy on health outcomes. These are potentially related to inequality, because rapid improvements in health outcomes tend to come at the bottom of the distribution. Many studies, for example, find that democracy is positively correlated with life expectancy (see McGuire, 2010, for an overview and case study and econometric evidence). Besley and Kudamatsu (2006) show this in a panel data model for the post-war period but without using country fixed effects. Wigley and Akkoyunlu-Wigley (2011) in a complementary study have shown that life expectancy is positively correlated with the history of democracy of a country. Kudamatsu (2012) showed in the context of democratic transitions in Africa that health outcomes improved in countries that democratized compared to those that did not.

Blaydes and Kayser (2011) looked at the relationship between democracy and average calories per capita interpreted as a proxy for inequality, because calories consumed decline very quickly with income. Using a trichotomous measure of democracy based on the Polity IV dataset (where greater than 7 is a democracy, less than −7 is an autocracy, and everything in between a "hybrid regime"), they show in a panel

data model with country fixed effects that democracy is positively correlated with average calorie consumption.

Gerring et al. (2012) find using panel data from 1960 to 2000 that, although the current level of democracy, as measured by the Polity score, is not robustly correlated with infant mortality, there is a strong negative correlation between the history of democracy and infant mortality—the more a country has experienced democracy in the past, the lower is infant mortality currently. Contrary to these findings, Ross (2006), using panel data from 1970 to 2000, the Polity score, the Przeworski et al. (2000) dichotomous measure of democracy, and the history of democracy as independent variables, finds no robust correlation between any of them and infant and child mortality. A possible reconciliation of these findings is that, as mentioned above, the history of democracy is nothing but a proxy for the omitted fixed effects, and Ross obtains different results from Gerring et al. because he controlled for fixed effects. Another confounding factor is that this literature in general does not control for the dynamics of democracy and GDP per capita and the endogeneity of democratization (see Acemoglu et al., 2013).

21.3.5 The Intensive Margin

All the papers discussed so far use various national-level measures of democracy, usually based on well-known databases created by political scientists. An important complementary direction is to investigate within-country variation exploiting other measures of "effective" enfranchisement.

In this context, particularly interesting is Fujiwara's (2011) study of changes in the voting technology in Brazil in the 1990s. These, by making it much simpler and easier for illiterate people to vote, massively enfranchised the poor. Fujiwara estimates the effect of this change by exploiting differences in the way the policy was rolled out. He shows that the consequence of the reform was a change in government spending in a pro-poor direction, particularly with respect to health expenditures, and that infant mortality fell as a result. Baland and Robinson (2008, 2012) examine another related reform, the introduction of an effective secret ballot in Chile in 1958. Though they do not directly study any policy outcomes, they do show that the reform led to large increases in the vote share of left-wing parties, which, they argue, is consistent with this democratizing reform moving the political equilibrium towards more pro-poor policies. They also find that land prices fall, which illustrates that the price of land capitalized the value of controlling workers' votes under the open ballot.

Martinez-Bravo et al. (2012) study the effects of elections in China on redistribution and public good provision. They use variation in the introduction of village elections in China, controlling for village and year fixed effects as well as province-level trends. They find that village chairmen experience higher turnover and become more educated and less likely to be Communist Party members following the introduction of elections. They

also find that taxes and public goods increase as a result of the elections. In particular, irrigation increases more in villages with more farmland, and public education increases in villages with more children. They also find that income inequality is reduced, and less land is leased to elite-controlled enterprises.

Naidu (2011) examined the impact of the disenfranchisement of blacks in the US South via poll taxes and literacy tests in the period after the end of Reconstruction. He finds that this reversal of democracy reduced the teacher-student ratio in black schools by 10–23%, with no significant effects on white teacher–student ratios. Also, consistent with Baland and Robinson's results, disenfranchisement increased farm values.

Relatedly, using state-level data Husted and Kenny (1997) examine the impact of the abolition of literacy tests and poll taxes in the United States over the period 1950–1988 and find that this was associated with a significant increase in welfare expenditures but not other types of government expenditures. Using county-level data, Cascio and Washington (2012) find that expansion of voting rights in the South resulted in increased state transfers to previously disenfranchised counties. Besley et al. (2010), on the other hand, show that the abolition of literacy tests and poll taxes was associated with increased political competition in US states. Increased political competition between the Republicans and Democrats reduced government tax revenues relative to state income and increased infrastructure expenditure relative to other components of government expenditure.

21.4. ECONOMETRIC SPECIFICATION AND DATA

Given the conflicting results in the theoretical and empirical literature surveyed above, we now present our econometric framework for investigating the relationship between democracy, redistribution, and inequality. We attempt to evaluate the diverse results within a single empirical strategy and sample, and we provide what we view to be some basic robust facts.

In this section, we describe our econometric specifications and our main data. Our approach is to estimate a canonical panel data model with country fixed effects and time effects while also modeling the dynamics of inequality and redistribution. Both fixed effects and allowing for dynamics (e.g., mean reversion) are important. Without fixed effects, as already noted above, several confounding factors will make the association between democracy and inequality (or redistribution) difficult to interpret. Moreover, we will see that there are potentially important dynamics in the key outcome variables, and failure to control for this would lead to spurious relationships (or make it difficult to establish robust patterns even when such patterns do exist).

Some of the papers we mentioned above have adopted a set-up similar to this, for example Rodrik (1999), Ross (2006), Scheve and Stasavage (2009), Aghion et al. (2012), and Aidt and Jensen (2013), but without modeling the dynamics in inequality

or redistribution. In addition, several of these papers suffer from the "bad control" problem; for example, Scheve and Stasavage (2009) control for both suffrage and education in their investigation of the determinants of the top income shares. If democracy influences inequality via its impact on education, then such an empirical model is bound to find that democracy is not correlated with inequality. Even the pioneering paper by Aidt and Jensen (2013) controls for many endogenous variables on the right side of the regression including the Polity score of the country.[13]

21.4.1 Econometric Specification

Consider the following simple econometric model:

$$z_{it} = \rho z_{it-1} + \gamma d_{it-1} + \mathbf{x}'_{it-1}\beta + \mu_t + \psi_i + u_{it}, \tag{21.6}$$

where z_{it} is the outcome of interest, which will be either (log of) tax revenue as a percentage of GDP or total revenue as a percentage of GDP as alternative measures of taxation, education, structural change, or one of several possible measures of inequality. The dependent variables with significant skewness in their cross-country distribution, in particular, tax to GDP ratio, total government revenues to GDP ratio, agricultural shares of employment, and income and secondary enrollment, will be in logs, which makes interpretation easier and allows the impact of democracy to be proportional to the baseline level. All of the results emphasized in this paper also hold in specifications using levels rather than logs, but these are not reported to conserve space. Lags in this specification will always mean 5-year lags: d_{it-1} is democracy 5 years ago. The lagged value of the dependent variable on the right-hand side is included to capture persistence (and mean reversion) in these outcome measures, which may be a determinant of democracy or correlated with other variables that predict democracy. The main right hand side variable is d_{it}, a dummy for democracy in country i in period t whose construction will be described in detail below. This variable is lagged by one period (generally a 5-year interval) because we expect its impact not to be contemporaneous. All other potential covariates, as well as interaction effects which are included later, are in the vector $\mathbf{x}_{it-1}$, which is lagged to avoid putting endogenous variables on the right-hand side of the regression. In our baseline specification, we include lagged log GDP per capita as a covariate for several reasons.[14] First, as we show in Acemoglu et al. (2013), democracy is much more likely to suffer from endogeneity concerns when the lagged effects of GDP per capita are not controlled for. Second, in Acemoglu et al. (2013), we also show that democracy has a

[13] A more desirable approach would be to develop an instrument for democracy. We believe that the only credible papers on this topic are Gallego (2010), Aidt and Jensen (2013), and our own work, Acemoglu et al. (2013). We do not pursue these directions as this would take us too far from our purpose of surveying and interpreting the literature and presenting what we believe to be the robust correlations in the data.

[14] We will always use GDP to refer to log GDP per capita.

major effect on GDP per capita and changes in GDP per capita may impact inequality independently of the influence of democracy on this variable. In all cases, we also report specifications that do not control for GDP per capita to ensure that the results we report are not driven by the presence of this endogenous control.

Finally, the ψ_i's denote a full set of country dummies and the μ_t's denote a full set of time effects that capture common shocks and trends for all countries. u_{it} is an error term, capturing all other omitted factors, with $E[u_{it}|z_{it-1}, d_{it-1}, \mathbf{x}'_{it-1}, \mu_t, \psi_i] = 0$ for all i and t. We estimate the above equation excluding the Soviet Union and its satellite countries because the dynamics of inequality and taxation following the fall of the Soviet Union are probably different from other democratizations. In some cases, for example, when using the tax to GDP ratio, this restriction is irrelevant because there is no data for these countries. When there is data, as with inequality, we also report results including these countries.

Our estimation framework controls for two key sources of potential bias. First, it controls for country fixed effects, which take into account that democracies are different from nondemocracies in many permanent characteristics that we do not observe and that may also affect inequality and taxation.[15] Second, it allows for mean-reverting dynamics and persistent effects in the dependent variable that may be endogenous to democracy.[16] This focus on *changes* in democracy ignores variation across countries that never change political institutions, for example, the United States, India, and China, but these observations help us in forming the counterfactual outcome conditional on the right-hand side covariates. Put differently, countries that never change political institutions may still be informative about how taxation and inequality change as a function of past taxation and inequality.

The simplest way of estimating Equation (21.6) is by OLS and imposing $\rho = 0$, and this is the most common regression in the prior literature which has used panel data. But, as already pointed out above, if $\rho > 0$, this specification may lead to biased estimates and will not correctly identify the long-run effect of democracy on the outcome of interest. An alternative method is to estimate this equation by OLS (which is just the standard within-group estimator removing the fixed effects by eliminating the mean of country i). This estimator is not consistent when the number of time periods is finite, because the regressor z_{it-1} is mechanically correlated with u_{is} for $s < t$, and this will induce a downward bias in the estimate of ρ (e.g., Wooldridge, 2002, chapter 11). However, the bias

[15] For instance, democracies may have more pluralistic institutions or stronger states, which may independently affect inequality and taxation.

[16] For instance, crisis, turmoil, social unrest, or increases in inequality could trigger a democratization, and also have a persistent effect on the path of our dependent variable. In this case, it becomes important to control for the dynamics of taxes or inequality by adding their lag on the right-hand side.

becomes smaller as the number of periods grows, holding ρ constant, so for large enough T or low enough ρ it becomes negligible (Nickell, 1981).

Our preferred estimation strategy is to deal with this econometric problem using a standard generalized method of moments (GMM) estimator along the lines of Holtz-Eakin et al. (1988) and Arellano and Bond (1991). This involves differencing Equation (21.6) with respect to time

$$\Delta z_{it} = \rho \Delta z_{it-1} + \gamma \Delta d_{it-1} + \Delta \mathbf{x}'_{it-1}\beta + \Delta \mu_t + \Delta u_{it}, \tag{21.7}$$

where the fixed-country effects are removed by time differencing. Although Equation (21.7) cannot be estimated consistently by OLS either, in the absence of serial correlation in the original residual, u_{it} (i.e., no second-order serial correlation in Δu_{it}), z_{it-2} and all further lags, and thus also d_{it-2} and all further lags, are uncorrelated with Δu_{it}, and can be used as instruments for Δz_{it-1}, incorporating them as moment conditions in a GMM procedure.

An alternative procedure removes country fixed effects by taking forward orthogonal differences. In particular, for variable w_{it}, this is given by

$$w_{it}^{\text{fod}} = \sqrt{\frac{T_{it}}{T_{it+1}}}\left(w_{it} - \frac{1}{T}\sum_{s>t} w_{is}\right),$$

where T_{it} is the number of times w_{is} appears in the data for $s > t$. Forward orthogonal differences also remove the fixed effects. In the absence of serial correlation in the original residual, z_{it-1}, d_{it-1}, $\mathbf{x}'_{it-1}$ and all further lags are orthogonal to the transformed error term u_{it}^{fod}, and can be used to form moment conditions in a GMM procedure. Moreover, if the original residuals were i.i.d., then the transformed error term will also be i.i.d.[17]

We will implement this using Arellano and Bond's GMM estimator with different subsets of moments, and after taking first differences or forward orthogonal differences of the data. As Newey and Windmeijer (2009) show, using the full set of moments in two-step GMM may lead to the "too many instruments" bias, since the number of potential moments one could use to estimate the dynamic panel model is quadratic in the time dimension. Thus, we experiment by restricting the number of lags used to form moments in the estimation. In addition to restricting the number of moments, we focus on

[17] Estimates of the model obtained by taking forward orthogonal differences are different from the first difference estimates only in unbalanced panels or when not all Arellano and Bond moments are used, in which case different lags give different moments and these may match dynamics differently.Yet another alternative is Blundell and Bond's (2000) system GMM, which works with the level equation (rather than the difference equation as in Equation 21.7 above) and uses first differences of the dependent variable as instruments for the lagged level. For consistency, this estimator thus requires that the initial value of the dependent variable, in this case democracy, is uncorrelated with the fixed effects. This is unlikely to be a good assumption in our context given the historically determined nature of both democracy and inequality/redistribution.

one-step GMM estimators with a naive weighting matrix that assumes the original residuals are i.i.d.[18] Despite the potential loss in efficiency, these estimators have the advantage of being consistent when T (the time dimension of the panel) and N (the number of countries) are large, even if the number of moments also becomes large (see Alvarez and Arellano, 2003).

As the above description indicates, the source of bias in the estimation of Equation (21.6) with OLS is that the persistence parameter ρ is not estimated consistently when the time dimension does not go to infinity, and this bias translates into a bias in all other coefficient estimates. If we knew the exact value of ρ and could impose it, the rest of the parameters could be estimated consistently by OLS. Motivated by this observation, we also report OLS estimates of Equation (21.6) imposing a range of values of ρ, which shows that our main results are robust to any value of ρ between 0 and 1, increasing our confidence in the GMM estimates.

In all cases, we first focus on results using a 5-year panel, where we take an observation every 5 years from 1960 to 2010. This is preferable to taking averages, which would introduce a complex pattern of serial correlation, making consistent estimation more difficult. The 5-year panel is a useful starting point since we expect many of the results of democracy on the tax to GDP ratio (henceforth, short for tax revenue as a percentage of GDP) and inequality not to appear instantaneously or not even in one or two years. In the case of inequality measures, this is also the highest frequency we can use.[19] For the tax to GDP ratio, the annual data are available, and we also estimate annual panels, which are similar to Equation (21.6) except that in that case we include up to 12 annual lags of both the lagged dependent variable and the democracy measure on the right-hand side.

Finally, it is worth reiterating that in all of our estimates, if democracy is correlated with other changes affecting taxes or inequality, our estimates will be biased. The point of the GMM estimator is to remove the mechanical bias resulting from the presence of fixed effects and lagged dependent variables, not to estimate "causal effects." This would necessitate a credible source of variation in changes in democracy, which we do not use in this paper.

21.4.2 Data and Descriptive Statistics

We construct a yearly and a 5-year panel of 184 countries from independence or 1960, whichever is later, through to 2010, though not all variables are available for all countries

[18] When we take first differences of the data, the weighting matrix has 1 on the main diagonal and −0.5 on the subdiagonals below and above it. When we take forward orthogonal differences, the weighting matrix is the identity matrix.

[19] Our inequality data from SWIID provides yearly observations for the GINI coefficient, but they are 5-year moving averages of observations around that specific year, making them inappropriate for an annual panel.

in all periods. We extend the recent work by Papaioannou and Siourounis (2008) by constructing a new measure of democracy which combines information from Freedom House and Polity IV—two of the more widely used sources of data about political rights and democracy. We create a dichotomous measure of democracy in country c at time t, d_{ct}, as follows. First, we code a country as democratic during a given year if Freedom House codes it as "Free" or "Partially Free," and it receives a positive Polity IV score. If we only have information from one of Polity or Freedom House, we use additional information from Cheibub et al. (2010, henceforth CGV) and Boix et al. (2012, henceforth BMR). In these cases, we code an observation as democratic if either Polity is greater than 0, or Freedom House codes it as "Partially Free" or "Free" *and* at least one of CGV or BMR code it as democratic. We are interested in substantive changes in political power, and so we give priority to the expert codings of Polity and Freedom House, rather than the procedural codings of CGV and BMR.

We omit periods where a country was not independent. Finally, many of the democratic transitions captured by this algorithm are studied in detail by Papaioannou and Siourounis (2008), who code the exact date of the democratization. When we detect a democratization that is also in their sample (in the same country and generally within 4 years of the year obtained by the previous procedure), we modify our democracy dummy to match the date to which they trace back the event using historical sources.

The Papaioannou and Siourounis measure of democracy captures *permanent* changes in political institutions, and they find that this correlates with subsequent economic growth. One limitation of their measure is that they define permanent changes by looking at democratizations that are not reversed in the future, which raises the possibility of endogeneity of the definition of democracy to subsequent growth or other outcomes that stabilize democracy. In addition, it means that they have no variation coming from transitions from democracy to autocracy. Our measure retains the focus on large changes in political regimes while not using any potentially endogenous outcome to classify democratizations.

Our resulting democracy measure is a dichotomous variable capturing large changes in political institutions. Our sample contains countries that are always democratic ($d_{ct}=1$ for all years) like the United States and most OECD countries; countries that are always autocratic ($d_{ct}=0$ for all years) like Afghanistan, Angola, and China; countries that transition once and permanently into democracy like Dominican Republic in 1978, Spain in 1978, and many ex-Soviet countries after 1991. But different from Papaioannou and Siourounis, we also have countries that transition in and out of democracy such as Argentina, which is coded as democratic from 1973 to 1975, falls back to nondemocracy and then democratizes permanently in 1983. For more details on our construction of the democracy measure, see Acemoglu et al. (2013a). In Appendix B, we show robustness of our main results to other measures of democracy constructed by Cheibub et al. (2010) and Boix et al. (2012).

We combine this measure of democratization with national income statistics from the World Bank economic indicators. We use government taxes to GDP and revenues to GDP ratios measures obtained from Cullen Hendrix covering more than 127 countries yearly from 1960 to 2005 (Hendrix, 2010). These data come from a project now updated by Arbetman-Rabinowitz et al. (2011), and puts together in a consistent way information from the World Bank (for 1960–1972), the IMF Government Financial Statistics historical series, the IMF new GFS, and complementary national sources.[20] Other dependent variables we explored include secondary-schooling enrollment, agricultural shares of employment, and GDP from the World Bank; and our inequality data that will be described below.[21]

Our additional covariates include a measure of average intensity of foreign wars over the last 5 years, constructed from Polity IV and ranging from 0 (no episodes) to 10 (most intense episodes); a measure of social unrest from the SPEED project at the University of Illinois averaging the number of events over the last 5 years;[22] and the fraction of the population with at least secondary schooling from the Barro-Lee dataset. In order to explore interactions we use data on the nonagricultural share of employment in 1968 from Vanhanen (2013).[23] We also use the top 10% share of income in the United States from the World Top Incomes Database (Alvaredo et al., 2010).[24] Finally, we construct the average ratio between the share of income held by the top 10% relative to the bottom 50%, and the ratio between the share of income held by the bottom 10 relative to the bottom 50% before 2000 using the World Inequality Indicators Database. From now on we will refer to these measures as the top and bottom shares of income.[25]

There is some debate on the construction and standardization of inequality measures, particularly Gini coefficients, across countries. We use the data in the Standardized World Inequality Indicators Database (SWIID), constructed by Frederick Solt (Solt, 2009). This database uses the Luxembourg Income Study together with the World Inequality Indicators Database in order to construct a comprehensive cross-national panel of Gini coefficients that are standardized across sources and measures. One advantage of this dataset is that it provides both the net Gini, after taxes and transfers, and the gross Gini coefficients. Measuring country-level inequality is very data-demanding, and so no inequality

[20] http://thedata.harvard.edu/dvn/dv/rpc/faces/study/StudyPage.xhtml?globalId=hdl:1902.1/16845.

[21] In the Appendix A we consider manufacturing wages, compiled by Martin Rama from UNIDO statistics and averaged over 5-year intervals.

[22] http://www.clinecenter.illinois.edu/research/speed-data.html.

[23] http://www.fsd.uta.fi/en/data/catalogue/FSD1216/meF1216e.html.

[24] http://topincomes.g-mond.parisschoolofeconomics.eu/.

[25] The World Inequality Indicators Database reports income shares created using different proxies for income, including consumption, monetary income, disposable income, and others. We standardized these ratios by regressing them on a full set of dummies for each income concept and using the residuals. The raw ratios are presented only in the summary statistics.

database is completely satisfactory, but we believe the SWIID provides the most comprehensive and consistent measure for the panel regressions we are estimating. We have experimented with a number of other measures of Gini coefficients, but none have the standardized sample coverage of the SWIID. In particular, we also created a panel with data every 5 years using observations for the Gini coefficient from the World Income Inequality Database (WIID) and CEDLAS (for Latin American countries), and obtained very similar results.

Descriptive statistics for all variables used in the main sample are presented in Table 21.1, separately by our measure of nondemocracy and democracy (observations in a country that was nondemocratic at the time or democratic). In each case, we report means, standard deviations, and also the total number of observations (note that our

Table 21.1 Summary statistics

	Nondemocracies			Democracies		
Variable	**Mean**	**Std. Dev.**	***N***	**Mean**	**Std. Dev.**	***N***
Tax revenue as a percentage of GDP	15.82	9.50	660	20.94	9.73	569
Total government revenue as a percentage of GDP	20.74	12.85	660	25.42	11.01	569
Gini coefficient, net income	38.91	10.76	338	36.81	10.19	497
Gini coefficient, gross income	43.92	11.72	338	45.11	7.71	497
Foreign wars (polity)	0.15	0.70	740	0.07	0.39	623
Social unrest (SPEED)	5.35	24.99	927	9.16	35.40	705
Share with secondary enrollmenty (Barro-Lee)	17.59	16.00	745	32.07	19.23	652
Nonagricultural share of population	64.54	28.51	138	81.39	19.55	301
Nonagricultural share of GDP	74.05	16.65	627	86.32	13.47	649
Secondary enrollment	45.95	31.50	492	76.01	29.90	545
Land Gini	59.96	15.21	214	62.96	16.23	399
Nonagricultural share of population in 1968	35.60	20.94	803	56.55	25.30	598
United States top 10% income share	36.03	5.07	1050	39.43	5.47	822
Top share	1.77	1.32	81	1.34	1.06	237
Bottom share	0.10	0.03	81	0.10	0.03	237
GDP per capita in 2000 dollars	2061.78	3838.08	718	8160.03	9415.89	770

Note: Summary statistics broken by observations during nondemocracy (left panel) and democracy (right panel). See the text for a full description of the data.

sample is not balanced). The summary statistics show that democracies tend to be significantly more economically developed than nondemocracies, with much higher GDP per capita, more education, and smaller agricultural shares of employment (both on average in the sample and in 1968) and GDP. These patterns are relatively well known and are sometimes interpreted as support for modernization theory (but see Acemoglu et al., 2008, 2009 on why this cross-sectional comparison is misleading).

The differences in tax to GDP ratios and revenue to GDP ratios are much smaller; both variables are roughly 4 percentage points higher in democracies than nondemocracies, although not significantly so.[26] Consistent with this tax difference reflecting increased redistribution, after-tax inequality, measured by the net Gini, is almost three points lower in democracies, whereas pretax inequality is one point higher (the Gini is measured on a 0- to 100-scale). Figure 21.1 shows the evolution of average democracy in our sample between 1960 and 2010.[27]

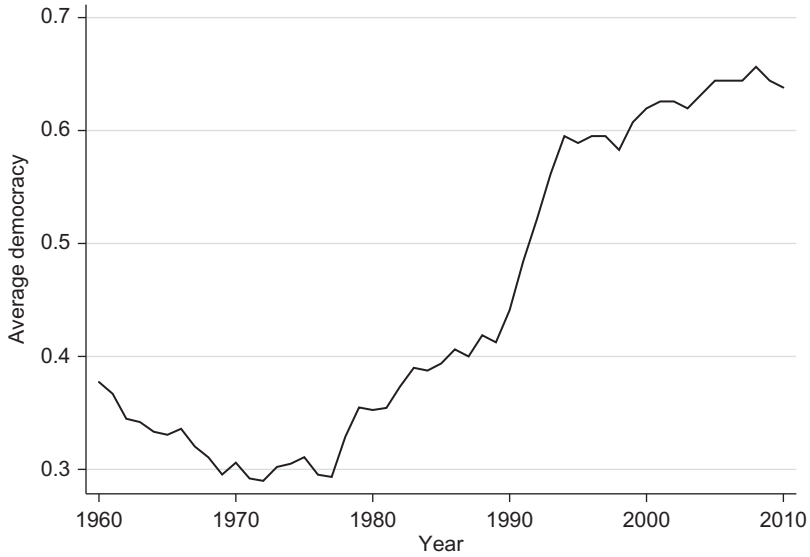

Figure 21.1 Worldwide average democracy since 1960.

[26] This comparison is broadly consistent with the cross-national regressions of Gil et al. (2004), though it is interesting that even in this cross section we do see some differences between democracies and nondemocracies.

[27] Note that democracies appear to be associated with a higher income share of the top 10% in the United States. This is because of the trend shown in Figure 21.1, making democracies more common in the recent past when this variable has also been higher.

21.5. MAIN RESULTS

21.5.1 The Effect of Democracy on Taxes

Our first results are contained in Table 21.2, which reports estimates of Equation (21.6) with the log of tax revenue to GDP ratio (tax to GDP ratio for short) as the dependent variable.

Column 1 is estimated by OLS imposing $\rho = 0$ in Equation (21.6). Though biased when $\rho > 0$, this is a natural benchmark, particularly since it corresponds to a specification often used in the literature. In all columns, we report standard errors corrected for arbitrary heteroskedasticity and serial correlation at the country level. We multiply the coefficient on democracy by 100 to ease interpretation. Throughout, we always report the number of observations, number of countries in the sample, and the number of switches in democracy from 0 to 1 or vice versa in the estimation sample (which is 92 in this case). All models include a lag of GDP per capita as a control, but the coefficients are not reported to save space. The coefficient on the estimated effect of democracy in this column, 15.00 (to two decimal places), implies a 15% increase in the tax to GDP ratio with a standard error of 4.33, and is thus statistically significant at less than the 1% confidence level. This estimate is also economically significant. It indicates that democratization—that is, a change in our democracy dummy—is associated with a 2.4 percentage points increase in the tax to GDP ratio.

Column 2 includes the lag of tax to GDP ratio on the right-hand side, thus relaxing the assumption that $\rho = 0$. The effect of democracy, γ, is now estimated to be 11.7 (approximately 11.7%, with standard error = 3.38) and is again statistically significant at less than the 1% level. In the presence of the lagged dependent variable on the right-hand side in this specification, γ is now merely the short-run impact of democracy on the tax to GDP ratio, not the long-run effect. The estimate of ρ is 0.27, and is significant, suggesting that there is indeed some persistence in the dependent variable. To obtain the long-run effect, we set $z_{it} = z_{it-1}$ so that the dynamics in the outcome variable converge to the new "steady state." This gives the long-run effects of a switch to democracy as

$$\frac{\gamma}{1-\rho},$$

and is reported at the bottom, together with the p-value for the hypothesis that it is equal to 0. In Column 2, this long-run effect implies a 16% increase in the tax to GDP ratio from a permanent switch to democracy.

Figure 21.2 shows the effect of democracy on the tax to GDP ratio visually. Here, similar to an event study analysis, we place all transitions to democracy at $t = 0$, and those observations before then (with $t < 0$) show the trends in tax to GDP ratio before democratization, and those with $t > 0$ correspond to changes in the tax to GDP ratio after

Table 21.2 Effects of democratization on the log of tax revenue as a percentage of GDP

			GMM			Assuming AR(1) coefficient				
						$\rho = 0$	$\rho = 0.25$	$\rho = 0.5$	$\rho = 0.75$	$\rho = 1$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Democracy lagged	15.00★★★	11.71★★★	11.27	18.68★★	14.63★★	15.00★★★	11.92★★★	8.84★★★	5.77★★	2.69
	(4.33)	(3.38)	(7.23)	(8.78)	(5.98)	(4.33)	(3.27)	(2.55)	(2.48)	(3.11)
Dep. Var. lagged		0.27★★★	0.27★★★	0.29★★★	0.33★★★					
		(0.06)	(0.10)	(0.07)	(0.08)					
Observations	944	944	816	816	816	944	944	944	944	944
Countries	128	128	125	125	125	128	128	128	128	128
Number of moments			81	61	61					
Hansen *p*-value			0.12	0.05	0.06					
AR2 *p*-value			0.92	0.83	0.78					
Democracy changes in the sample	92	92	82	82	82	92	92	92	92	92
Long-run effect of democracy	15.00	15.97	15.49	26.35	21.97	15.00	15.89	17.68	23.06	.
p-Value for the long-run effect	0.00	0.00	0.11	0.03	0.01	0.00	0.00	0.00	0.02	.

Note: OLS estimates (Columns 1–2) include a full set of country and year fixed effects. Arellano and Bond's GMM estimators of the dynamic panel model (Columns 3–4) remove country fixed effects by taking first differences of the data, or by taking forward orthogonal differences (Column 5) and then constructing moment conditions using predetermined lags of the dependent variable and democracy. Columns 4 and 5 use up to the fifth lag of predetermined variables to create moments, restricting the number of moments used. Columns 6–10 impose different values for the autocorrelation coefficient in the tax revenue as a percentage of GDP series, and estimates the effect of democracy including a full set of country and year fixed effects. All models control for lagged GDP per capita but this coefficient is not reported to save space. Robust standard errors, adjusted for clustering at the country level, are in parentheses. ★★★: significant at 1%; ★★: significant at 5%; ★: significant at 10%. We do not report long-run effects and their *p*-values in Column 10 because they are not defined for $\rho = 1$.

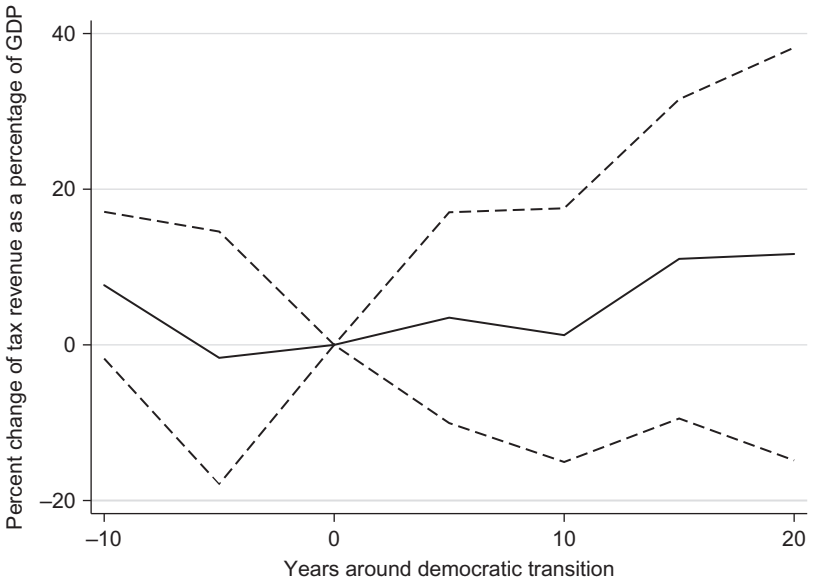

Figure 21.2 Tax revenue as a percentage of GDP around a democratization. Constructed using the 5-year panel.

democratization. The figure shows that there is no discernible change in the tax to GDP ratio before democratization, increasing our confidence in the results concerning the effect of democracy on taxes. It also confirms that the effect of democracy on the tax to GDP ratio evolves only slowly, reaching a maximum 15 years after the democratization takes place. This underscores the role of the lagged dependent variable in our econometric specifications.

As a second diagnostic for our estimates, Figure 21.3 shows a scatterplot of the residuals of the tax to GDP ratio (in logs) on the vertical axis against the residuals of the lag of our democracy measure on the horizontal axis. All covariates, including year and country fixed effects, and the lagged dependent variable, are partialed out. Each point corresponds to a particular country/year observation. The slope of the regression line coincides with our estimated coefficient of 11.7. The figure shows that the estimated relationship does not seem to be driven by any particular outlier. To explore this more formally we removed 49 observations whose Cook distance was above the rule of thumb $4/N$, with N the sample size and reestimated our model. The coefficient of democracy falls to 8.28 with standard error 2.46, and is still significant at the 1% level. The bottom panel of Figure 21.3 shows the scatterplot excluding these outliers. We have experimented with a number of other methods for dealing with outliers, such as Huber M-regressions and excluding outliers with estimated standardized errors > 1.96, and our results on tax to GDP ratios remain generally unchanged.

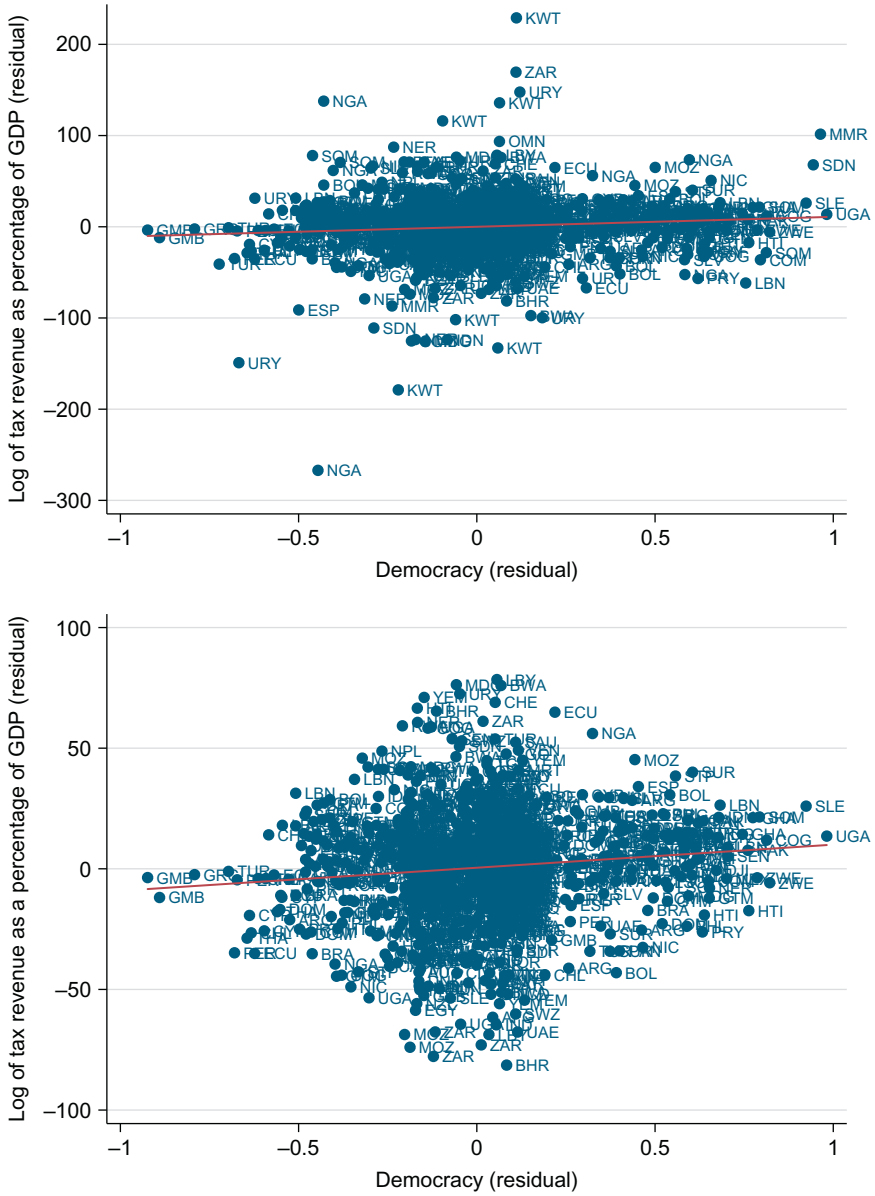

Figure 21.3 Residual of tax revenue as a percentage of GDP (vertical axis) against the residual of our democracy indicator. Each dot is a country/year observation, and there are a total of 975 observations. Bottom figure excludes outliers.

As noted in the previous subsection, the OLS estimator of Column 2 is inconsistent because of the (downward) bias in the estimation of ρ. Column 3 reports the GMM estimator described earlier with the full set of moments (in this case, this corresponds to 82 moments as noted in the table). Notably, the estimate for ρ is identical up to two decimal places, indicating in fact that if there was a downward bias in the estimation of Column 2, it was negligible, suggesting that the large-T assumption (given the low persistence ρ) is a good approximation. The estimate for γ also decreases marginally, but the standard error increases substantially, making the resulting estimate insignificant at conventional levels. However, the long-run impact is very similar to the OLS estimate of approximately 15 (15%), with a p-value of 0.11. It should also be noted that the tests for second-order autocorrelation in the error term and the Hansen's J test for over identification pass comfortably, thus further increasing our confidence in this specification.

Columns 4 and 5 present alternative GMM estimators with fewer moments and with forward-differencing, respectively. Both estimates only use up to the fifth valid lags of democracy and the dependent variable to form moment conditions. The point estimates on both γ and ρ are larger than Columns 2 and 3, and significant at the 5% level, and hence imply the significantly larger long-run effects, 26% and 21%, respectively, reported at the bottom.

Columns 6–10 estimate Equation (21.6), imposing different values for ρ spanning the entire interval from 0 to 1. We use the same sample as in Column 2, which is also the same one as in Column 1 and thus implies that in this case Column 6, which sets $\rho = 0$, is identical to Column 1 (this will not be the case in some of our later tables). As noted above, the problem with the OLS estimation (with fixed effects) stems from the bias in the estimate of ρ, so conditional on the correct value for this variable, the OLS estimate of the impact of democracy is consistent. In almost all cases, with the exception of the last column, there is a statistically and economically significant impact of democracy on the tax to GDP ratio. The long-run impact is smaller when ρ is assumed to take a small value, and comparable to that in Column 2 when we impose $\rho = 0.25$. The coefficient gets smaller and less significant the farther the imposed value of ρ is from the estimated values in Columns 2–5.[28] In sum, the median estimated long-run effect of democracy on the tax to GDP ratio from this table is almost 16%, with estimates that range from 15% to 26%.

Table 21.3 has the same structure as Table 21.2, but uses total government revenue to GDP ratio as the dependent variable. Though the impact of democracy is a little smaller, the pattern is qualitatively very similar, with slightly larger long-run effects in the GMM estimators relative to the OLS estimators. The estimates in Column 2 show that the coefficient of lagged democracy is 7.55 (standard error = 2.35), which is significant at the

[28] In Column 10 where we impose $\rho = 1$, we do not compute the long-run impact, since this is undefined in this unit-root specification. The coefficient in this specification is small and insignificant, suggesting that there is not much variation in growth rates of tax to GDP to be explained by democratization.

Table 21.3 Effects of democratization on the log of total government revenue as a percentage of GDP

			GMM			Assuming AR(1) coefficient				
						$\rho = 0$	$\rho = 0.25$	$\rho = 0.5$	$\rho = 0.75$	$\rho = 1$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Democracy lagged	9.31***	7.55***	9.37*	11.13**	10.04**	9.31***	8.06***	6.81***	5.56***	4.31
	(3.44)	(2.35)	(5.01)	(5.58)	(4.37)	(3.44)	(2.60)	(2.08)	(2.15)	(2.76)
Dep. Var. lagged		0.35***	0.47***	0.52***	0.53***					
		(0.03)	(0.06)	(0.06)	(0.06)					
Observations	944	944	816	816	816	944	944	944	944	944
Countries	128	128	125	125	125	128	128	128	128	128
Number of moments			81	61	61					
Hansen *p*-value			0.05	0.04	0.05					
AR2 *p*-value			0.36	0.39	0.40					
Democracy changes in the sample	92	92	82	82	82	92	92	92	92	92
Long-run effect of democracy	9.31	11.64	17.77	22.96	21.47	9.31	10.74	13.61	22.23	.
p-Value for the long-run effect	0.01	0.00	0.07	0.05	0.03	0.01	0.00	0.00	0.01	.

Note: OLS estimates (Columns 1–2) include a full set of country and year fixed effects. Arellano and Bond's GMM estimators of the dynamic panel model (Columns 3–4) remove country fixed effects by taking first differences of the data, or by taking forward orthogonal differences (Column 5) and then constructing moment conditions using predetermined lags of the dependent variable and democracy. Columns 4 and 5 use up to the fifth lag of predetermined variables to create moments, restricting the number of moments used. Columns 6–10 impose different values for the autocorrelation coefficient in the total government revenue as a percentage of GDP series, and estimates the effect of democracy including a full set of country and year fixed effects. All models control for lagged GDP per capita but this coefficient is not reported to save space. Robust standard errors, adjusted for clustering at the country level, are in parentheses. ***: significant at 1%; **: significant at 5%; *: significant at 10%. We do not report long-run effects and their *p*-values in Column 10 because they are not defined for $\rho = 1$.

1% level. The long-run effect of democracy is to increase total revenue as a percentage of GDP by 11.64 and is significant at the 1% level. The baseline GMM estimator leads to larger values of ρ and γ, resulting in a larger long-run effect of 17.8%. Figure 21.4 is the analogue of Figure 21.2, but using the total revenue to GDP ratio measure instead, and shows a similar pattern, although there is a slight downward trend prior to democracy in this variable. In sum, the evidence again suggests that democracy results in larger government revenues as a share of GDP.

Table 21.4 estimates Equation (21.6) for the annual panel. Column 1 includes just four (annual) lags of the dependent variable and democracy on the right-hand side, and is estimated by OLS. Even though individual lags of democracy are not significant, they are jointly significant as witnessed by the long-run effect reported at the bottom, which is similar to the OLS long-run effect in Table 21.2. Column 2 adds four more lags and Column 3 adds four further lags, for a total of 12 lags of democracy and the dependent variable on the right-hand side (to economize on space, we only report the p-values for F-tests for the joint significance of these additional lags). The overall pattern and the long-run effects are very similar to Column 1. Columns 4–6 estimate the same models using the Arellano and Bond GMM estimator. The long-run effects are substantially higher and comparable to the one estimated in Columns 3–5 in Table 21.2 using the 5-year panel.

Table 21.5 probes the robustness of the tax to GDP ratio results, focusing on the 5-year panel. Odd-numbered columns report OLS estimates of Equation (21.6), whereas even-numbered columns are for the GMM estimator (equivalent to Column 3 of

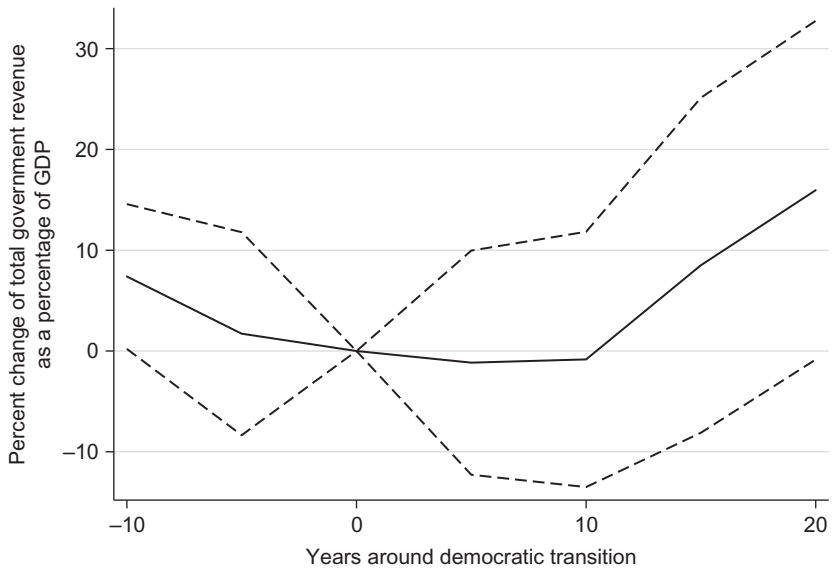

Figure 21.4 Total government revenue as a percentage of GDP around a democratization. Constructed using the 5-year panel.

Table 21.4 Effects of democratization on the log of tax revenue as a percentage of GDP, yearly panel

	OLS			GMM		
	(1)	(2)	(3)	(4)	(5)	(6)
D_{t-1}	3.43	3.45	4.06	5.49	9.49	8.11★
	(2.82)	(2.91)	(3.13)	(3.83)	(5.82)	(4.86)
D_{t-2}	−2.31	−2.01	−2.08	−1.66	−0.67	−1.04
	(2.83)	(2.86)	(3.00)	(2.67)	(2.56)	(2.86)
D_{t-3}	0.66	−0.03	−1.66	1.25	0.53	−0.88
	(2.21)	(2.36)	(2.58)	(2.24)	(2.21)	(2.46)
D_{t-4}	1.65	2.83	3.88★	6.14★★★	3.45★	4.29★★
	(1.63)	(2.03)	(2.14)	(2.32)	(1.81)	(1.93)
p-Value, first four democracy lags	0.02	0.10	0.20	0.06	0.07	0.06
p-Value, second four democracy lags	.	0.61	0.21	.	0.13	0.09
p-Value, third four democracy lags	.	.	0.80	.	.	0.82
y_{t-1}	0.65★★★	0.64★★★	0.62★★★	0.58★★★	0.53★★★	0.52★★★
	(0.04)	(0.04)	(0.05)	(0.05)	(0.04)	(0.06)
y_{t-2}	0.06	0.08	0.09★	0.03	0.04	0.06
	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)
y_{t-3}	0.09	0.10	0.12	0.07	0.07	0.11
	(0.09)	(0.09)	(0.10)	(0.08)	(0.09)	(0.09)
y_{t-4}	−0.00	−0.03	−0.06	−0.03	−0.03	−0.06
	(0.06)	(0.06)	(0.06)	(0.07)	(0.05)	(0.05)
p-Value, first four tax to GDP lags	0.00	0.00	0.00	0.00	0.00	0.00
p-Value, second four tax to GDP lags	.	0.61	0.55	.	0.05	0.19
p-Value, third four tax to GDP lags	.	.	0.51	.	.	0.11
Observations	4434	3925	3425	4306	3799	3301
Countries	128	126	124	128	125	123
Number of moments				373	637	837
Hansen *p*-value				1.00	1.00	1.00
AR2 *p*-value				0.30	0.39	0.96
Democracy changes in sample	75	73	69	75	73	68
Long-run effect of democracy	16.49	19.11	12.49	32.38	38.85	25.40
p-Value for long-run effect	0.00	0.00	0.06	0.03	0.01	0.02

Note: OLS estimates (Columns 1–3) include a full set of country and year fixed effects. Arellano and Bond's GMM estimators of the dynamic panel model (Columns 4–6) remove country fixed effects by taking first differences of the data and then constructing moment conditions using as many predetermined lags of the dependent variable and democracy as included in the model. To save space we only report the *p*-value of a joint test of significance for lags 5–8 (second four lags) and lags 9–12 (third four lags). All models control for as many lags of GDP per capita as lags of democracy in the equation, but these coefficients are not reported to save space. ★★★: significant at 1%; ★★: significant at 5%; ★: significant at 10%. We do not report long-run effects and their *p*-values in Column 10 because they are not defined for $\rho = 1$.

Table 21.5 Effects of democratization on the log of tax revenue as a percentage of GDP with different set of controls

	Ex. GDP per capita		Adding other controls							
	OLS	GMM	OLS	GMM	OLS	GMM	OLS	GMM	OLS	GMM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Democracy lagged	10.91***	12.59*	12.22***	12.42*	11.70***	10.73	11.59**	15.01**	11.68***	15.34**
	(3.69)	(6.67)	(3.52)	(6.73)	(3.38)	(7.00)	(3.46)	(7.59)	(3.48)	(6.90)
Dep. Var. lagged	0.28***	0.31***	0.27***	0.28***	0.27***	0.28***	0.31***	0.32***	0.31***	0.34***
	(0.07)	(0.11)	(0.06)	(0.10)	(0.06)	(0.10)	(0.07)	(0.12)	(0.07)	(0.12)
War lagged			−1.60	−2.38					−2.30	−4.26
			(2.56)	(3.91)					(3.03)	(4.06)
Unrest lagged					0.01	−0.06			0.01	−0.09
					(0.02)	(0.07)			(0.02)	(0.08)
Education lagged							−0.16	0.02	−0.15	−0.30
							(0.19)	(0.63)	(0.20)	(0.69)
Observations	1090	957	889	771	927	802	844	734	803	700
Countries	133	133	118	115	125	122	110	107	103	100
Number of moments		80		82		82		82		84
Hansen *p*-value		0.22		0.07		0.17		0.15		0.21
AR2 *p*-value		0.24		0.88		0.91		0.76		0.77
Democracy changes in the sample	101	90	89	80	92	82	77	68	77	68
Long-run effect of democracy	15.22	18.26	16.64	17.27	15.97	14.84	16.76	22.15	16.97	23.17
p-Value for the long-run effect	0.00	0.07	0.00	0.06	0.00	0.12	0.00	0.04	0.00	0.02

Note: OLS estimates (odd columns) include a full set of country and year fixed effects. Columns 3–10 include lagged GDP per capita as a control. Arellano and Bond's GMM estimators of the dynamic panel model (even columns) remove country fixed effects by taking first differences of the data and then constructing moment conditions using predetermined lags of the dependent variable and democracy. Robust standard errors, adjusted for clustering at the country level, are in parentheses. ****: significant at 1%; **: significant at 5%; *: significant at 10%.

Table 21.2). The first two columns exclude GDP per capita as a control. Reassuringly, however, our coefficients remain positive and significant, implying a 10–15% increase in the tax to GDP ratio following a democratization. Columns 3 and 4 include the lagged index of foreign wars. This is useful since several authors have claimed that either in history or in the recent past, war has been a major determinant of taxation and redistribution policies. For example, the famous Tilly (1985) hypothesis explains the growth of the state with war and preparation for war (see also Besley and Persson, 2011). More recently, Atkinson et al. (2011) have pointed to large wars and the concomitant economic changes as some of the most significant events correlated with declines of 1% income shares in combatant countries (see also Scheve and Stasavage, 2010, 2012). In contrast to these hypotheses, we do not find any effect of war on the tax to GDP ratio in our post-war panel. The effect of democracy on the tax to GDP ratio remains essentially unchanged when the external war index is included.

Columns 5 and 6 include the lagged measure of social unrest from the SPEED data. This variable is insignificant and has no effect on the coefficient of democracy. Columns 7 and 8 include the stock of education, measured as the fraction of the population with at least secondary schooling from the Barro-Lee dataset, which could be an important determinant of fiscal policy and inequality. Once again, this variable has no major effect on the estimate of the impact of democracy on the tax to GDP ratio and is itself insignificant. Columns 9 and 10 include all three of these variables together, again with a very limited impact on our estimates and no evidence of an effect on war, unrest or the stock of education. The long-run effects at the bottom are very similar to those in Table 21.2 and highly significant.[29]

Overall, the evidence in Tables 21.2–21.5 shows a strong and robust impact of democracy on taxes as measured by the tax to GDP ratio or the government revenue to GDP ratio. This evidence suggests that democracy does lead to more taxes. This evidence is consistent with several of the works discussed above, though it is in stark contrast with Gil et al. (2004). The main difference is the cross-national focus of Gil, Mulligan, and Sala-i-Martin, which contrasts with our econometric approach exploiting the within-country variation (with country fixed effects and also controlling for the dynamics of the tax to GDP ratio). For reasons explained above, we believe that the cross-sectional relationship is heavily confounded by other factors and is unlikely to reveal much about the impact of democracy on redistribution and taxes.

We next investigate whether there is an impact of democracy on inequality.

[29] Another relevant robustness check is to include ex-Soviet countries in the sample. However, fiscal data are only available for Hungary, Poland, and Romania, and then only for the 1990–1995 period, which results in the observations being absorbed by the fixed effects. We thus do not report this robustness check for these specifications (but will report it for our inequality results).

21.5.2 The Effect of Democracy on Inequality

Tables 21.6 and 21.7 turn to the effect of democracy on inequality. Each panel of Table 21.6 mirrors Table 21.2, with the top panel using the net Gini coefficient (after tax and transfers) and the bottom panel using the gross Gini coefficient (before tax and redistribution) as dependent variables.

Though the sample is smaller and data quality may be lower, the most important message from these tables is that there is no consistent evidence for a significant effect of democracy on inequality. Some of our specifications show negative effects of democracy on inequality, particularly on the gross Gini coefficient, but these tend to have large standard errors and are not stable across specifications.

For example, in Table 21.6, most of our estimates suggest there is a negative effect of democracy on the net Gini coefficient, but none of these estimates is statistically significant at the standard levels. For instance, the estimates in Column 3 imply that democracy reduces the Gini coefficient (measured on a 0- to 100-scale) by 2.01 points (standard error = 1.59) in the short run, and by 3.1 points in the long run. Given the standard deviation of the net Gini of 10.76 (see Table 21.1), these effects are quantitatively sizable (though they are also smaller in other columns) but also statistically insignificant. The magnitudes for the gross Gini are similar, but a few specifications contain significant results (those with imposed values of $\rho > 0.5$). This may be because there is less measurement error in this measure relative to the net Gini, which does depend on potentially misreported taxes and transfers.

The AR2 test for the GMM estimator for the net Gini suggests there is higher order autocorrelation in the transformed errors, which invalidates the use of second lags as instruments. However, when we only use deeper lags to form valid moment conditions we get very similar results, with smaller effects of democracy on inequality, consistent with the fact that the Hansen overidentification test passes comfortably. The specification tests (AR2 and Hansen J test) for our models using the gross Gini as dependent variable also pass comfortably.

Figure 21.5, which is similar to Figures 21.2 and 21.4, visually shows that there is no substantial fall in inequality following a democratization. There is no pre-trend in inequality. But there is a temporary increase in inequality prior to democratization, which could have persistent effects biasing our estimates unless we control for the dynamics of inequality, further motivating our specifications controlling for such dynamics.

As a second diagnostic of our estimates, Figure 21.6 again shows a scatterplot of the residuals of the net Gini on the vertical axis against the residuals of the lag of our democracy measure on the horizontal axis. All covariates, including year and country fixed effects and the lagged dependent variable, are partialed out. Each point corresponds to a particular country/year observation. The slope of the regression line coincides with our estimated coefficient of −0.744 in Column 2 of the top panel in Table 21.6. The

Table 21.6 Effects of democratization on inequality

			GMM			Assuming AR(1) coefficient				
						$\rho = 0$	$\rho = 0.25$	$\rho = 0.5$	$\rho = 0.75$	$\rho = 1$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable: Gini coefficient, net income										
Democracy lagged	0.62	−0.74	−2.01	−2.60	−1.60	−0.42	−0.67	−0.92	−1.17	−1.42
	(0.78)	(0.88)	(1.59)	(1.63)	(1.51)	(0.93)	(0.89)	(0.89)	(0.93)	(1.00)
Dep. Var. lagged		0.32***	0.35***	0.39***	0.32***					
		(0.07)	(0.10)	(0.12)	(0.12)					
Observations	657	537	420	420	424	537	537	537	537	537
Countries	127	113	100	100	100	113	113	113	113	113
Number of moments			81	61	61					
Hansen *p*-value			0.60	0.69	0.30					
AR2 *p*-value			0.02	0.03	0.01					
Democracy changes	65	47	31	31	31	47	47	47	47	47
Long-run effect	0.62	−1.10	−3.12	−4.28	−2.36	−0.42	−0.90	−1.84	−4.67	.
p-Value	0.43	0.40	0.21	0.12	0.30	0.65	0.45	0.31	0.21	.
Dependent variable: Gini coefficient, gross income										
Democracy lagged	−1.22	−1.50	−1.45	−1.88	−1.22	−1.51	−1.50	−1.50*	−1.49*	−1.49
	(0.99)	(0.90)	(1.44)	(1.59)	(1.27)	(1.15)	(1.00)	(0.90)	(0.87)	(0.92)
Dep. Var. lagged		0.50***	0.64***	0.64***	0.76***					
		(0.06)	(0.11)	(0.11)	(0.11)					
Observations	657	537	420	420	424	537	537	537	537	537
Countries	127	113	100	100	100	113	113	113	113	113
Number of moments			81	61	61					
Hansen *p*-value			0.54	0.29	0.37					
AR2 *p*-value			0.59	0.57	0.48					
Democracy changes	65	47	31	31	31	47	47	47	47	47
Long-run effect	−1.22	−2.98	−3.99	−5.26	−5.15	−1.51	−2.00	−3.00	−5.97	.
p-Value	0.22	0.11	0.36	0.30	0.42	0.19	0.14	0.10	0.09	.

Note: OLS estimates (Columns 1–2) include a full set of country and year fixed effects. Arellano and Bond's GMM estimators of the dynamic panel model (Columns 3–4) remove country fixed effects by taking first differences of the data, or by taking forward orthogonal differences (Column 5) and then constructing moment conditions using predetermined lags of the dependent variable and democracy. Columns 4 and 5 use up to the fifth lag of predetermined variables to create moments, restricting the number of moments used. Columns 6–10 impose different values for the autocorrelation coefficient of the dependent variable, and estimates the effect of democracy including a full set of country and year fixed effects. All models control for lagged GDP per capita but this coefficient is not reported to save space. Robust standard errors, adjusted for clustering at the country level, are in parentheses. ***: significant at 1%; **: significant at 5%; *: significant at 10%. We do not report long-run effects and their *p*-values in Column 10 because they are not defined for $\rho = 1$.

Table 21.7 Effects of democratization on inequality adding controls

	Ex GDP per capita		Baseline sample								Inc. Ex-Soviets	
	OLS	GMM	OLS	GMM	OLS	GMM	OLS	GMM	OLS	GMM		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dependent variable: Gini coefficient, net income												
Democracy lagged	−0.87	−2.81**	−0.71	−1.87	−0.75	−2.16	−0.72	−1.46	−0.72	−1.69	−0.26	−1.51
	(0.82)	(1.31)	(0.93)	(1.68)	(0.88)	(1.58)	(1.03)	(1.87)	(1.06)	(1.86)	(0.77)	(1.32)
Dep. Var. lagged	0.33***	0.49***	0.32***	0.34***	0.32***	0.36***	0.32***	0.34***	0.32***	0.33***	0.31***	0.53***
	(0.07)	(0.10)	(0.07)	(0.11)	(0.07)	(0.10)	(0.08)	(0.12)	(0.08)	(0.12)	(0.06)	(0.10)
War lagged			0.12						0.33	0.27		
			(0.28)						(0.28)	(0.49)		
Unrest lagged					−0.01	0.00			−0.00	0.00		
					(0.00)	(0.01)			(0.00)	(0.01)		
Education lagged							−0.02	0.06	−0.02	0.01		
							(0.04)	(0.13)	(0.05)	(0.16)		
Observations	556	435	512	402	523	409	502	399	480	382	611	473
Countries	115	103	106	95	110	97	100	91	95	87	134	121
Number of moments		80		82		82		82		84		81
Hansen *p*-value		0.82		0.76		0.59		0.67		0.77		0.42
AR2 *p*-value		0.03		0.04		0.02		0.04		0.04		0.03
Democracy changes	49	34	44	30	47	31	38	28	37	27	61	39
Long-run effect	−1.30	−5.55	−1.06	−2.85	−1.10	−3.39	−1.05	−2.19	−1.06	−2.51	−0.37	−3.23
p-Value	0.29	0.03	0.44	0.26	0.39	0.18	0.48	0.44	0.49	0.37	0.74	0.27
Dependent variable: Gini coefficient, gross income												
Democracy lagged	−1.51*	−2.18*	−1.57*	−1.90	−1.39	−1.39	−1.80*	−1.29	−1.70*	−1.28	−0.97	−0.79
	(0.89)	(1.24)	(0.95)	(1.52)	(0.89)	(1.40)	(1.00)	(1.65)	(1.02)	(1.65)	(0.84)	(1.41)
Dep. Var. lagged	0.53***	0.75***	0.50***	0.60***	0.49***	0.65***	0.49***	0.62***	0.49***	0.62***	0.49***	0.72***
	(0.06)	(0.09)	(0.06)	(0.12)	(0.06)	(0.11)	(0.06)	(0.12)	(0.07)	(0.12)	(0.06)	(0.08)
War lagged			0.06	−0.03					0.21	−0.03		
			(0.26)	(0.44)					(0.27)	(0.46)		

Unrest lagged					−0.01**	−0.00			−0.01**	−0.00		
					(0.00)	(0.01)			(0.00)	(0.01)		
Education lagged							0.02	−0.02	0.02	−0.02		
							(0.06)	(0.12)	(0.06)	(0.16)		
Observations	556	435	512	402	523	409	502	399	480	382	611	473
Countries	115	103	106	95	110	97	100	91	95	87	134	121
Number of moments		80		82		82		82		84		81
Hansen *p*-value		0.51		0.70		0.45		0.79		0.84		0.28
AR2 *p*-value		0.50		0.52		0.45		0.66		0.50		0.50
Democracy changes	49	34	44	30	47	31	38	28	37	27	61	39
Long-run effect	−3.19	−8.69	−3.15	−4.72	−2.75	−3.95	−3.56	−3.38	−3.34	−3.43	−1.91	−2.84
p-Value	0.11	0.13	0.11	0.25	0.13	0.37	0.08	0.46	0.10	0.45	0.26	0.59

Note: OLS estimates (odd columns) include a full set of country and year fixed effects. Columns 3–12 control for lagged GDP per capita. Arellano and Bond's GMM estimators of the dynamic panel model (even columns) remove country fixed effects by taking first differences of the data and then constructing moment conditions using predetermined lags of the dependent variable and democracy. Robust standard errors, adjusted for clustering at the country level, are in parentheses. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

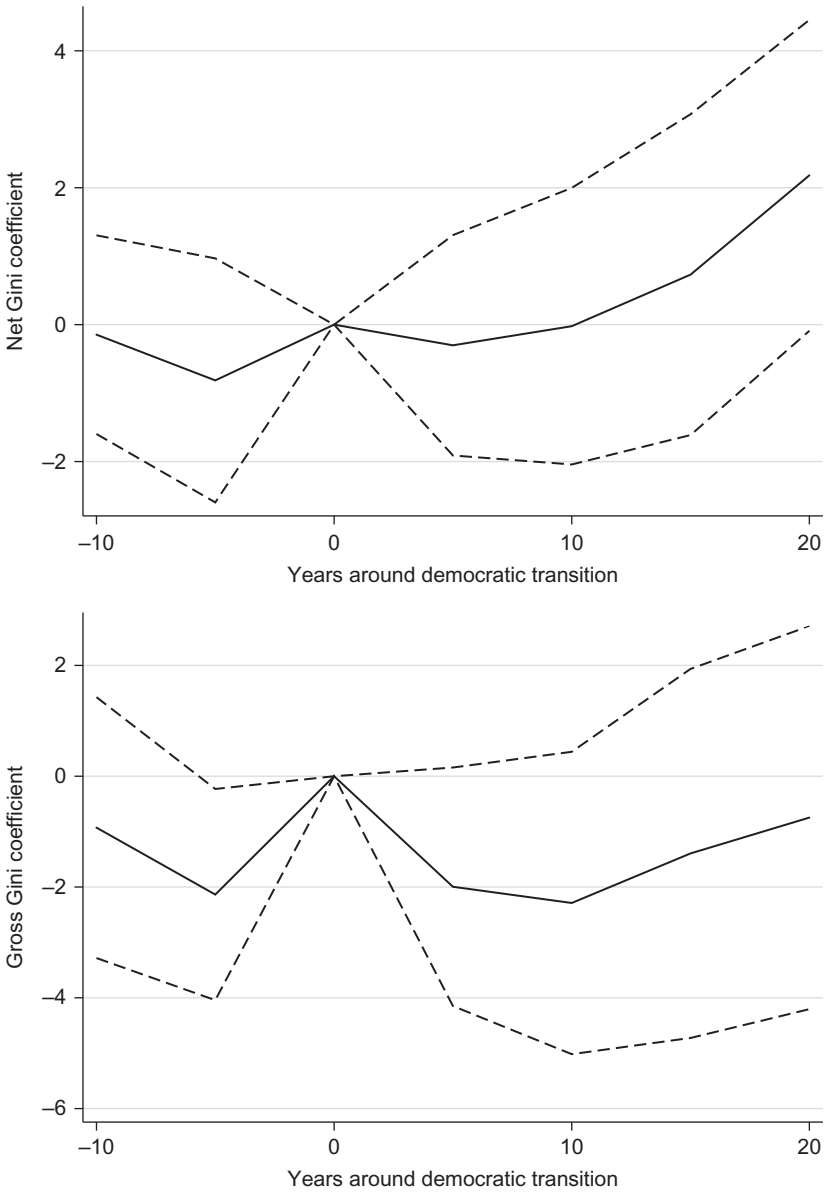

Figure 21.5 Gini coefficient around a democratization. Constructed using the 5-year panel.

figure shows that the estimated relationship does not seem to be driven by any particular outlier. Figure 21.7 shows the same scatterplot, except with gross Gini on the y-axis, and again suggests a negative, if imprecise, relationship. We explored the impact of outliers further, using a procedure similar to the one we used before. We therefore removed observations whose Cook distance was above the rule of thumb $4/N$, with N the sample

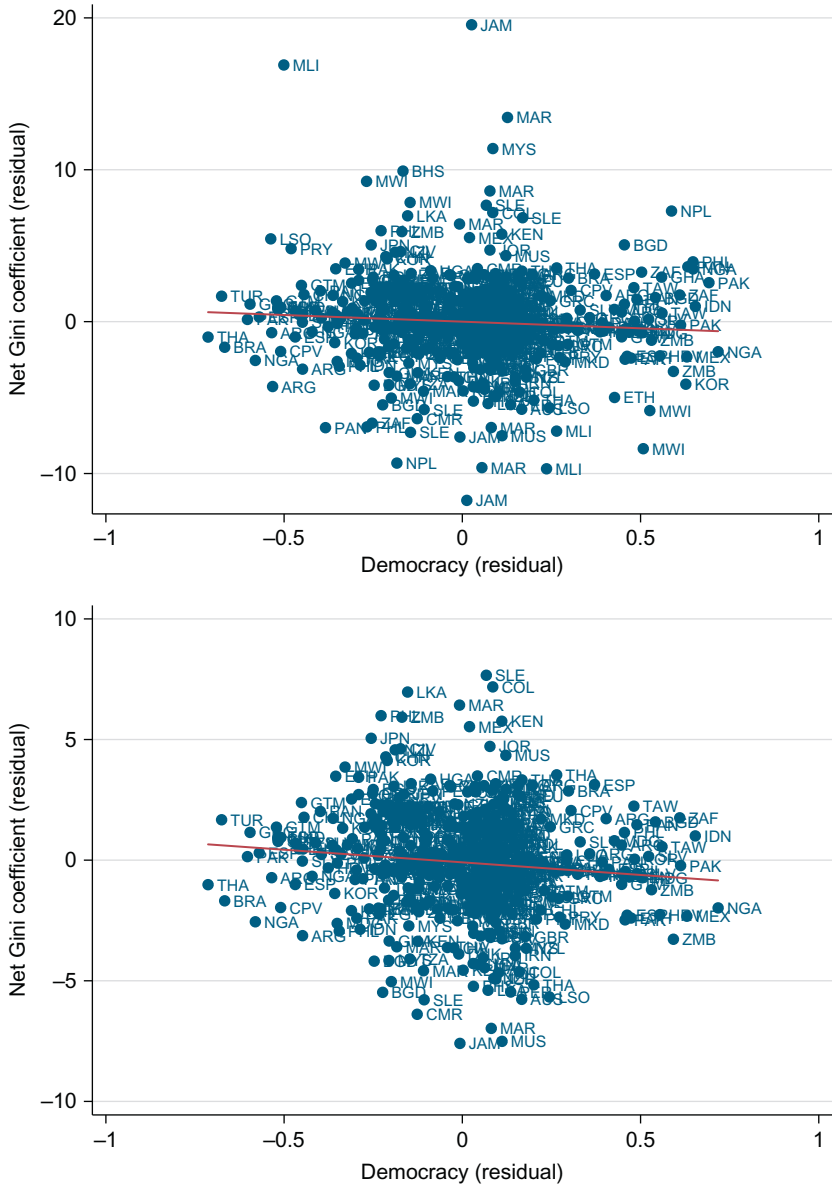

Figure 21.6 Residual of net Gini (vertical axis) against the residual of our democracy indicator. Each dot is a country/year observation, and there are a total of 538 observations. The bottom figure excludes outliers.

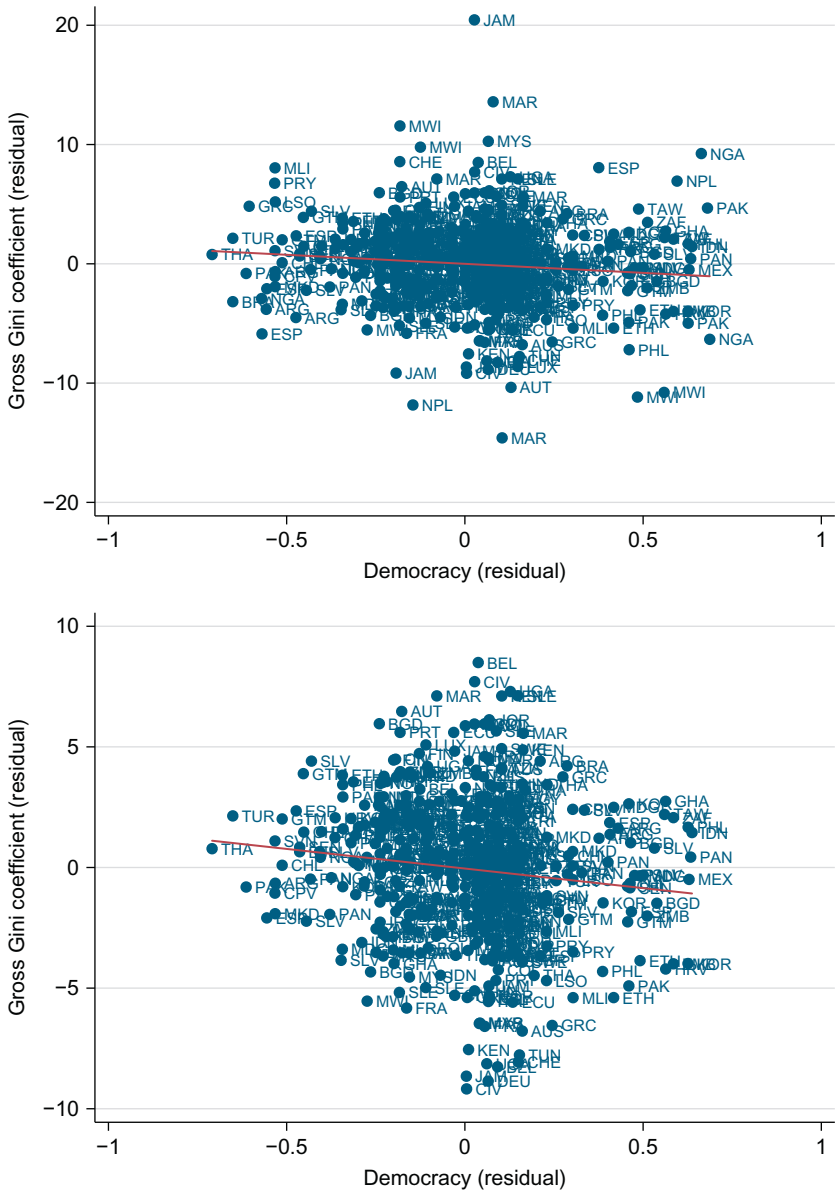

Figure 21.7 Residual of gross Gini (vertical axis) against the residual of our democracy indicator. Each dot is a country/year observation, and there are a total of 538 observations. The bottom figure excludes outliers.

size and reestimated our model. Democracy has no significant effect in this sample without the outliers for the net Gini, but there is a moderately significant effect on the gross Gini in some specifications. In addition, we found a marginally significant effect on both the net and the gross Gini when we used Huber's M estimator. When excluding observations with standardized residuals >1.96, we again found a significant negative effect on the gross Gini but not on the net Gini.

Table 21.7 adds covariates, as in Table 21.5, for the tax variables, and comprises two panels, one for each Gini measure. The only difference is that it adds two columns including ex-Soviet countries in the estimation sample. The addition of controls does not change the patterns shown in Table 21.6, although omitting income as a control does lead to moderately significant negative effects in the GMM estimate on net Gini, and in both the OLS and GMM estimates for gross Gini. This suggests that there may be other forces correlated with GDP and democracy that influence inequality, such as some of the structural transformation variables we examine below. Social unrest is the only variable that has an effect on inequality that is significant in the gross Gini specifications, and our point estimates on democracy are roughly unchanged. The addition of ex-Soviet countries to our estimation sample results in smaller magnitudes of the effect of democracy on inequality, consistent with the idea that inequality went up in these countries following democratization.

We also found (but are not reporting to save space) that democracy does not have any significant effect on other measures of inequality. In particular, in Appendix A we show that, with updated data and our sample, democracy appears to have no effect on the log of industrial wages and explain why this result is different from those of Rodrik (1999).

We have also experimented with other estimates of the Gini using a panel with data every 5 years constructed from the World Income and Inequality Dataset. Controlling for indicators of type of concept used to calculate the Gini (i.e., disposable income, consumption and so on) as well as indicators for data quality, we found broadly similar results, though generally for smaller samples.

Overall, although some specifications do show a negative impact of democracy on inequality, particularly the gross Gini, there is no consistent and robust impact. This contrasts with our results on tax to GDP ratio (or the total government revenue to GDP ratio). Though this could be because of the lower quality of inequality data, it might also reflect some of the theoretical forces we have suggested in the previous section. We will turn to an investigation of some of these channels after looking at the relationship between democracy and structural transformation next.

21.5.3 Democracy and Structural Transformation

While our results above suggest that democracy has little net impact on inequality despite increasing taxation, some of the theoretical models we examined above suggest

mechanisms by which democracy could affect inequality independently of government redistribution. (The lowering of barriers to entry, provision of public goods, and the expansion of market opportunities under democracy could be **offsetting** any redistribution accomplished by the fiscal system.) Therefore we examine the effect of democracy on economic structure and education.

Tables 21.8–21.10 look at the impact of democracy on various measures of structural transformation and public goods provision. We focus on the nonagricultural share of employment, nonagricultural share of value added, and secondary enrollment (which is a flow measure, thus better reflecting the effect of democracy on educational investments). Each table has two panels: the top one has the same structure as Table 21.2, whereas the bottom one is similar to Table 21.5 and shows the robustness of the results. Overall, we find significant effects of democracy on these measures of structural transformation.

For example, Tables 21.8 and 21.9 show some significant effects of democratization on the size of the nonagricultural sector.[30] Table 21.8 shows that democratization increases the (log of) nonagricultural share of employment, but this effect is generally only significant at the 10% level in the top panel, and is not completely robust to all exogenously imposed values of ρ in Columns 6–10. The bottom panel shows more consistent and significant estimates, but the coefficients differ substantially between the OLS and GMM estimators. Table 21.9, on the other hand shows that democratization increases the nonagricultural share of GDP. We find significant effects across OLS and most GMM specifications, imposing lower values for ρ, and with various sets of controls. The estimated magnitudes are plausible, with democracy increasing the nonagricultural employment by 4–11% and nonagricultural share of GDP by between 6% and 10% in the long run.

Table 21.10 shows a generally robust long-run effect of democratization on log secondary school enrollment. Although the coefficient magnitudes differ substantially between the GMM and OLS estimators, the long-run effect is uniformly positive and generally significant. Together with the taxation results, this suggests that one important economic change that democracies implement is to tax and provide public goods such as schooling. Our GMM specification in Column 3 of the top panel shows that democracy increases secondary enrollment by 67.6% in the long run, with an associated p-value of 0.07.[31]

[30] Bates and Block (2013) find that democratization significantly increased agricultural productivity in Africa, which may also be part of the process of structural change.

[31] The contrast of these results with Aghion et al. (2012), who find that democracy, as measured by the polity score, reduces primary school enrollment, is partly owing to their different sample, dependent variable, and econometric specification. Indeed, Aghion et al. (2012) estimate models without the lagged dependent variable and also include several additional variables on the right-hand side, most notably, military expenditure per capita (which is problematic since it is correlated with democracy, making it a potential "bad control"). They also focus on primary schooling, and according to our discussion above, democracy may have different effects on primary and secondary enrollment depending on the current level of education of the median voter.

Table 21.8 Effects of democratization on the log of the nonagricultural share of population

			GMM			Assuming AR(1) coefficient				
						$\rho = 0$	$\rho = 0.25$	$\rho = 0.5$	$\rho = 0.75$	$\rho = 1$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Democracy lagged	0.81	0.61*	1.86*	1.71*	1.66*	1.48	1.22	0.96	0.69*	0.43
	(1.74)	(0.33)	(0.95)	(0.92)	(0.86)	(1.63)	(1.18)	(0.75)	(0.39)	(0.38)
Dep. Var. lagged		0.83***	0.83***	0.83***	0.84***					
		(0.05)	(0.06)	(0.06)	(0.06)					
Observations	350	313	252	252	252	313	313	313	313	313
Countries	62	61	60	60	60	61	61	61	61	61
Number of moments			56	40	40					
Hansen *p*-value			0.33	0.12	0.07					
AR2 *p*-value			0.10	0.08	0.10					
Democracy changes	23	21	18	18	18	21	21	21	21	21
Long-run effect	0.81	3.59	10.79	9.91	10.09	1.48	1.62	1.91	2.77	.
p-Value	0.64	0.06	0.01	0.02	0.02	0.37	0.31	0.21	0.08	.

	Ex. GDP per capita		Adding other controls							
	OLS	GMM	OLS	GMM	OLS	GMM	OLS	GMM		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Democracy lagged	0.96*	1.34	0.70*	2.24*	0.68*	2.00**	0.90*	1.60***	0.85*	1.38***
	(0.51)	(1.19)	(0.40)	(1.19)	(0.35)	(0.96)	(0.48)	(0.62)	(0.50)	(0.52)
Dep. Var. lagged	0.83***	0.84***	0.82***	0.81***	0.82***	0.81***	0.79***	0.79***	0.77***	0.77***
	(0.04)	(0.05)	(0.05)	(0.06)	(0.05)	(0.06)	(0.03)	(0.04)	(0.04)	(0.04)
War lagged			0.09	0.13					0.12	0.50*
			(0.08)	(0.17)					(0.12)	(0.28)
Unrest lagged					−0.00	0.01			−0.00	0.03**
					(0.01)	(0.02)			(0.01)	(0.01)
Education lagged							−0.04	−0.06*	−0.03	−0.04
							(0.03)	(0.03)	(0.03)	(0.03)
Observations	341	279	229	184	294	237	227	183	189	153

Continued

Table 21.8 Effects of democratization on the log of the nonagricultural share of population—cont'd

	Ex. GDP per capita		Adding other controls							
	OLS	GMM	OLS	GMM	OLS	GMM	OLS	GMM		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Countries	62	61	45	44	57	56	44	43	36	35
Number of moments		55		57		57		52		54
Hansen p-value		0.29		0.81		0.31		0.92		1.00
AR2 p-value		0.82		0.57		0.21		0.23		0.29
Democracy changes	22	19	18	16	21	18	8	6	8	6
Long-run effect	5.72	8.42	3.79	11.87	3.84	10.63	4.20	7.52	3.77	5.90
p-Value	0.04	0.16	0.08	0.03	0.04	0.01	0.07	0.01	0.10	0.01

Notes for top panel: OLS estimates (Columns 1–2) include a full set of country and year fixed effects. Arellano and Bond's GMM estimators of the dynamic panel model (Columns 3–4) remove country fixed effects by taking first differences of the data, or by taking forward orthogonal differences (Column 5) and then constructing moment conditions using predetermined lags of the dependent variable and democracy. Columns 4 and 5 use up to the fifth lag of predetermined variables to create moments, restricting the number of moments used. Columns 6–10 impose different values for the autocorrelation coefficient in the percentage of nonagricultural population series, and estimate the effect of democracy including a full set of country and year fixed effects. Robust standard errors, adjusted for clustering at the country level, are in parentheses. *Notes for bottom panel*: OLS estimates (odd columns) include a full set of country and year fixed effects. Columns 3–10 include lagged GDP per capita as a control. Arellano and Bond's GMM estimators of the dynamic panel model (even columns) remove country fixed effects by taking first differences of the data and then constructing moment conditions using predetermined lags of the dependent variable and democracy. Robust standard errors, adjusted for clustering at the country level, are in parentheses. ***: significant at 1%; **: significant at 5%; *: significant at 10%. We do not report long-run effects and their p-values in Column 10 because they are not defined for $\rho = 1$.

Table 21.9 Effects of democratization on the log of nonagricultural share of GDP

			GMM			Assuming AR(1) coefficient				
						$\rho = 0$	$\rho = 0.25$	$\rho = 0.5$	$\rho = 0.75$	$\rho = 1$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Democracy lagged	3.96***	2.49***	2.66*	1.58	2.62**	4.00***	3.34***	2.68***	2.02**	1.36
	(1.38)	(0.95)	(1.56)	(2.15)	(1.31)	(1.34)	(1.11)	(0.95)	(0.92)	(1.01)
Dep. Var. lagged		0.57***	0.73***	0.74***	0.73***					
		(0.05)	(0.08)	(0.09)	(0.07)					
Observations	1033	978	833	833	834	978	978	978	978	978
Countries	147	144	140	140	140	144	144	144	144	144
Number of moments			100	70	70					
Hansen *p*-value			0.21	0.18	0.08					
AR2 *p*-value			0.72	0.71	0.40					
Democracy changes	90	88	78	78	78	88	88	88	88	88
Long-run effect	3.96	5.81	9.86	6.14	9.76	4.00	4.46	5.36	8.09	.
p-Value	0.00	0.01	0.10	0.49	0.08	0.00	0.00	0.01	0.03	.

	Ex. GDP per capita		Adding other controls							
	OLS	GMM	OLS	GMM	OLS	GMM	OLS	GMM		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Democracy lagged	2.63***	3.61**	2.63**	3.05*	2.43**	2.67*	2.78***	3.70**	2.82***	3.89**
	(0.91)	(1.57)	(1.01)	(1.68)	(0.94)	(1.54)	(1.05)	(1.66)	(1.05)	(1.67)
Dep. Var. lagged	0.61***	0.78***	0.58***	0.73***	0.58***	0.75***	0.60***	0.76***	0.60***	0.74***
	(0.04)	(0.07)	(0.05)	(0.08)	(0.05)	(0.08)	(0.04)	(0.07)	(0.04)	(0.07)
War lagged			−0.45	−1.82**					−0.29	−1.79*
			(0.44)	(0.74)					(0.43)	(0.96)
Unrest lagged					0.00	−0.01			0.00	−0.01
					(0.01)	(0.01)			(0.01)	(0.01)
Education lagged							−0.01	−0.15	0.01	−0.10
							(0.04)	(0.13)	(0.05)	(0.13)

Continued

Table 21.9 Effects of democratization on the log of nonagricultural share of GDP—cont'd

	Ex. GDP per capita		Adding other controls							
	OLS	GMM	OLS	GMM	OLS	GMM	OLS	GMM		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Observations	1010	861	852	730	924	789	823	709	762	658
Countries	148	143	121	117	134	130	113	109	103	99
Number of moments		99		101		101		101		103
Hansen *p*-value		0.28		0.27		0.30		0.21		0.31
AR2 *p*-value		0.28		0.52		0.61		0.74		0.64
Democracy changes	91	81	81	74	88	78	70	63	69	62
Long-run effect	6.69	16.61	6.24	11.23	5.78	10.47	7.00	15.39	7.05	14.87
p-Value	0.01	0.08	0.01	0.09	0.01	0.10	0.01	0.02	0.01	0.02

Notes for top panel: OLS estimates (Columns 1–2) include a full set of country and year fixed effects. Arellano and Bond's GMM estimators of the dynamic panel model (Columns 3–4) remove country fixed effects by taking first differences of the data, or by taking forward orthogonal differences (Column 5) and then constructing moment conditions using predetermined lags of the dependent variable and democracy. Columns 4 and 5 use up to the fifth lag of predetermined variables to create moments, restricting the number of moments used. Columns 6–10 impose different values for the autocorrelation coefficient in the nonagricultural share of GDP series, and estimate the effect of democracy including a full set of country and year fixed effects. Robust standard errors, adjusted for clustering at the country level, are in parentheses. *Notes for bottom panel*: OLS estimates (odd columns) include a full set of country and year fixed effects. Columns 3–10 include lagged GDP per capita as a control. Arellano and Bond's GMM estimators of the dynamic panel model (even columns) remove country fixed effects by taking first differences of the data and then constructing moment conditions using predetermined lags of the dependent variable and democracy. Robust standard errors, adjusted for clustering at the country level, are in parentheses. ***: significant at 1%; **: significant at 5%; *: significant at 10%. We do not report long-run effects and their *p*-values in Column 10 because they are not defined for $\rho = 1$.

Table 21.10 Effects of democratization on the log of secondary enrollment

			GMM			Assuming AR(1) coefficient				
						$\rho = 0$	$\rho = 0.25$	$\rho = 0.5$	$\rho = 0.75$	$\rho = 1$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Democracy lagged	12.31**	12.30***	17.41**	20.35**	13.39	19.28***	16.25***	13.22***	10.19**	7.17
	(5.17)	(4.67)	(8.21)	(9.28)	(8.41)	(5.64)	(5.00)	(4.56)	(4.40)	(4.54)
Dep. Var. lagged		0.58***	0.74***	0.75***	0.82***					
		(0.06)	(0.12)	(0.12)	(0.12)					
Observations	825	630	453	453	489	630	630	630	630	630
Countries	150	141	127	127	129	141	141	141	141	141
Number of moments			77	56	57					
Hansen *p*-value			0.04	0.04	0.12					
AR2 *p*-value			0.83	0.91	0.79					
Democracy changes	71	51	29	29	29	51	51	51	51	51
Long-run effect	12.31	29.03	67.56	82.43	76.17	19.28	21.67	26.44	40.77	.
p-Value	0.02	0.01	0.07	0.09	0.16	0.00	0.00	0.00	0.02	.
	Ex. GDP per capita		Adding other controls							
	OLS	GMM	OLS	GMM	OLS	GMM	OLS	GMM		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Democracy lagged	11.19**	16.77**	12.85**	21.47**	12.81***	19.55**	10.92**	18.39**	11.08**	17.36**
	(4.45)	(8.50)	(4.98)	(8.49)	(4.70)	(7.79)	(5.11)	(8.67)	(5.25)	(8.13)
Dep. Var. lagged	0.58***	0.80***	0.57***	0.72***	0.57***	0.73***	0.61***	0.82***	0.61***	0.81***
	(0.06)	(0.11)	(0.06)	(0.12)	(0.06)	(0.12)	(0.06)	(0.09)	(0.07)	(0.09)
War lagged			0.73	−0.07					0.31	−1.21
			(0.98)	(1.87)					(1.01)	(1.92)
Unrest lagged					0.04	0.04			0.06	0.12
					(0.04)	(0.06)			(0.05)	(0.07)
Education lagged							−0.29*	−0.78*	−0.32*	−0.74*
							(0.17)	(0.42)	(0.18)	(0.39)

Continued

Table 21.10 Effects of democratization on the log of secondary enrollment—cont'd

	Ex. GDP per capita		Adding other controls							
	OLS	GMM	OLS	GMM	OLS	GMM	OLS	GMM		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Observations	686	495	563	411	610	442	553	407	519	385
Countries	151	134	121	111	133	121	116	106	106	99
Number of moments		76		78		78		78		80
Hansen *p*-value		0.08		0.13		0.04		0.19		0.18
AR2 *p*-value		0.66		0.67		0.61		0.51		0.59
Democracy changes	54	33	48	29	51	29	43	26	42	26
Long-run effect	26.50	84.72	30.11	76.17	30.04	71.71	28.20	103.65	28.32	93.58
p-Value	0.01	0.11	0.01	0.04	0.00	0.04	0.02	0.04	0.02	0.03

Notes for top panel: OLS estimates (Columns 1–2) include a full set of country and year fixed effects. Arellano and Bond's GMM estimators of the dynamic panel model (Columns 3–4) remove country fixed effects by taking first differences of the data, or by taking forward orthogonal differences (Column 5) and then constructing moment conditions using predetermined lags of the dependent variable and democracy. Columns 4 and 5 use up to the fifth lag of predetermined variables to create moments, restricting the number of moments used. Columns 6–10 impose different values for the autocorrelation coefficient in the secondary enrollment series, and estimate the effect of democracy including a full set of country and year fixed effects. Robust standard errors, adjusted for clustering at the country level, are in parentheses. *Notes for bottom panel*: OLS estimates (odd columns) include a full set of country and year fixed effects. Columns 3–10 include lagged GDP per capita as a control. Arellano and Bond's GMM estimators of the dynamic panel model (even columns) remove country fixed effects by taking first differences of the data and then constructing moment conditions using predetermined lags of of the dependent variable and democracy. Robust standard errors, adjusted for clustering at the country level, are in parentheses. ⋆⋆⋆: significant at 1%; ⋆⋆: significant at 5%; ⋆: significant at 10%. We do not report long-run effects and their *p*-values in Column 10 because they are not defined for $\rho = 1$.

In sum, there is strong evidence that democratization does not just redistribute income, but also results in a degree of structural change of the economy and investment in public goods.[32] As our theoretical discussion implied, this could explain why democratization has a statistically weak effect on inequality. Democracy may be bringing new opportunities and economic change, which may increase inequality, while simultaneously lowering barriers to entry and investing in public goods, which may reduce inequality, and the net result could be either an increase or decrease in inequality, despite the increased taxation documented in Tables 21.2 and 21.3. This reasoning, as well as the theoretical ideas discussed in Section 21.2, underscores the importance of investigating the heterogeneous effects of democracy on inequality, a topic we turn to next.

21.5.4 Investigating the Mechanisms: Heterogeneity

We now turn to heterogeneity in the effect of democracy on inequality. We first consider the effect of democracy interacted with the land Gini, which we take to be a measure of landed elite power, to test the "capture" channel discussed above. We show only effects on net and gross Gini for most of the interactions to save space, and then discuss the heterogeneous effects on tax to GDP and government revenue to GDP ratios in the text.

Table 21.11 shows a positive and generally significant interaction of democracy with land inequality, suggesting that the power of landed elites to capture the state or thwart any redistributive tendencies of democratization results in higher inequality. The magnitudes are sizable, suggesting that a democratization in, say, Myanmar, with the highest land Gini (=77 in a 0- to 100-scale) among nondemocracies in our sample, would increase the after-tax Gini by approximately 0.72–2.42 points and the pretax Gini by 0.2–1.6 points. Our results suggest that democracy may increase inequality in societies with strong landed elites. This could be the case if democracy creates inequality increasing market opportunities while the elite manages to reduce taxation through de facto channels. An alternative explanation is given in Acemoglu and Robinson (2008), where a transition to democracy can lead to more pro-elite policies. The intuition for this somewhat paradoxical result is that the elite invests more in de facto power under democracy because, besides the benefits of being able to impose their favorite economic institutions, investments in de facto power increase the likelihood of a transition to autocracy.

The difference between the net and gross measures may reflect the importance of nonfiscal channels. Consistent with this, we see only moderate attenuation of the effect of democracy on the tax to GDP ratio, and no significant heterogeneity on the government revenue to GDP ratio (omitted to save space). For example, the equalizing effects of lowering barriers to mobility out of the agricultural sector may only be seen in societies

[32] Event study figures analogous to Figures 21.2, 21.4, and 21.5 reveal no pre-trends for these variables and an increase after the democratization, but are not included to save space.

Table 21.11 Effects of democratization on inequality (Includes interaction of democracy with Land Gini (averaged over all years with available data))

			GMM			Assuming AR(1) coefficient				
						$\rho = 0$	$\rho = 0.25$	$\rho = 0.5$	$\rho = 0.75$	$\rho = 1$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable: Gini coefficient, net income										
Democracy lagged	0.29	−0.91	−1.01	−2.44	−1.56	−0.56	−0.81	−1.06	−1.31	−1.57
	(0.90)	(1.02)	(1.31)	(1.86)	(1.46)	(1.04)	(1.02)	(1.05)	(1.12)	(1.22)
Lagged democracy × Land Gini	0.18***	0.11**	0.23**	0.33***	0.27***	0.15***	0.12**	0.09	0.06	0.03
	(0.04)	(0.05)	(0.10)	(0.10)	(0.10)	(0.05)	(0.05)	(0.06)	(0.07)	(0.08)
Dep. Var. lagged		0.35***	0.36***	0.33***	0.34***					
		(0.07)	(0.08)	(0.08)	(0.10)					
Observations	485	407	326	326	329	407	407	407	407	407
Countries	86	78	72	72	72	78	78	78	78	78
Democracy changes in the sample	32	23	16	16	16	23	23	23	23	23
Dependent variable: Gini coefficient, gross income										
Democracy lagged	−2.89**	−2.51**	−2.47*	−4.38**	−2.57**	−2.88*	−2.69**	−2.50**	−2.30**	−2.11**
	(1.32)	(1.04)	(1.28)	(2.07)	(1.29)	(1.48)	(1.21)	(1.03)	(0.96)	(1.05)
Lagged democracy × Land Gini	0.22***	0.18***	0.27***	0.46***	0.29***	0.24***	0.21***	0.18**	0.15**	0.12
	(0.08)	(0.07)	(0.08)	(0.12)	(0.07)	(0.09)	(0.08)	(0.07)	(0.07)	(0.07)
Dep. Var. lagged		0.48***	0.56***	0.49***	0.66***					
		(0.07)	(0.09)	(0.11)	(0.09)					
Observations	485	407	326	326	329	407	407	407	407	407
Countries	86	78	72	72	72	78	78	78	78	78
Democracy changes in the sample	32	23	16	16	16	23	23	23	23	23

Note: OLS estimates (Columns 1–2) include a full set of country and year fixed effects. Arellano and Bond's GMM estimators of the dynamic panel model (Columns 3–4) remove country fixed effects by taking first differences of the data, or by taking forward orthogonal differences (Column 5) and then constructing moment conditions using predetermined lags of the dependent variable, the interaction term and democracy. Columns 4 and 5 use up to the fifth lag of predetermined variables to create moments, restricting the number of moments used. Columns 6–10 impose different values for the autocorrelation coefficient in the tax to GDP series, and estimate the effect of democracy and the interaction term including a full set of country and year fixed effects. All models control for lagged GDP per capita but this coefficient is not reported to save space. Robust standard errors, adjusted for clustering at the country level, are in parentheses. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

with politically weak agricultural elites. Although land inequality is potentially correlated with many other economic and social factors that may also mediate the effect of democracy on inequality, we view this as some evidence of the "capture" channel modeled above.

We next consider the effect of democracy depending on the extent of structural transformation, motivated by our hypothesis that democracy induces structural change and may increase inequality by expanding opportunities, such as skilled occupations and entrepreneurship, for previously excluded groups.

Table 21.12 shows the effect of democratization interacted with the share of nonagricultural employment in 1968 as a measure of the extent of structural transformation (results are similar with the 1978 share). We find that democratization *increases* inequality more (or fails to reduce inequality) in places that have smaller agricultural employment shares. This is consistent with democracy expanding access to inequality-increasing market opportunities especially in more urban societies where skilled occupations and entrepreneurship are potentially more important. The magnitudes suggest that democratization in a country that was 10% points less agricultural than the mean in 1968 (measured by the percentage of nonagricultural employment), will bring an increase between 1 and 1.6 net Gini points (1.3 and 2.3 gross Gini points) relative to the average effect in the short run, and between 1.6 and 2.2 net Gini points (2.5 and 5.6 gross Gini points) in the long run. We have also estimated these specifications using our other proxies for structural transformation and obtained uniformly positive, although often imprecise, coefficients on the interaction variables. The results using the gross Gini coefficient show a similar pattern and similar, though slightly larger, estimates.

While we do not show these results for space reasons, there is no significant heterogeneity by nonagricultural employment in the effect of democracy on taxation, and this result is robust to all proxies for the extent of structural transformation we have tried, including the 1970 values of urbanization, education, and nonagricultural share of GDP. This suggests that the mechanisms via which democracy increases inequality in relatively more economically modernized countries has less to do with lowering government redistribution or public good provision, and more to do with other mechanisms emphasized in our discussion of disequalizing market opportunities opened up by democracy for entrepreneurs, educated workers, and capitalists.

Table 21.13 looks further at heterogeneity by the level of potential inequality created by market opportunities. We interact democratization in year t with the top 10% share of income in the United States in the same year. This is a proxy (albeit a highly imprecise and imperfect one) for the extent of inequality increasing market opportunities available at the time and their potential to create inequality, shaped by world-level forces such as globalization, technological and organizational changes that either originate or find widespread adoption in the United States (Panitch and Gindin, 2012). We did not find significant interaction effects of this sort on the tax to GDP ratio or the government revenue

Table 21.12 Effects of democratization on inequality (Includes interaction of democracy with the percentage of nonagricultural population in 1968)

			GMM			Assuming AR(1) coefficient				
						$\rho = 0$	$\rho = 0.25$	$\rho = 0.5$	$\rho = 0.75$	$\rho = 1$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable: Gini coefficient, net income										
Democracy lagged	0.91	−0.32	−0.45	−1.81	−0.80	−0.05	−0.27	−0.49	−0.71	−0.92
	(0.74)	(0.78)	(1.35)	(1.54)	(1.28)	(0.82)	(0.79)	(0.81)	(0.88)	(0.98)
Lagged democracy × nonagricultural	0.12***	0.11**	0.16*	0.16**	0.13*	0.13***	0.12**	0.10**	0.09	0.08
pop. in 1968	(0.03)	(0.05)	(0.08)	(0.07)	(0.07)	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)
Dep. Var. lagged		0.31***	0.29***	0.29***	0.36***					
		(0.07)	(0.09)	(0.10)	(0.10)					
Observations	614	506	402	402	406	506	506	506	506	506
Countries	112	100	91	91	91	100	100	100	100	100
Democracy changes in the sample	55	41	29	29	29	41	41	41	41	41
Dependent variable: Gini coefficient, gross income										
Democracy lagged	−0.81	−0.85	−0.40	−1.15	−0.71	−0.72	−0.79	−0.86	−0.92	−0.99
	(0.98)	(0.76)	(1.22)	(1.43)	(1.18)	(0.97)	(0.83)	(0.76)	(0.79)	(0.90)
Lagged democracy × nonagricultural	0.15***	0.13**	0.21***	0.23***	0.19***	0.17***	0.15***	0.13**	0.11**	0.08*
pop. in 1968	(0.05)	(0.05)	(0.08)	(0.08)	(0.07)	(0.07)	(0.06)	(0.05)	(0.05)	(0.04)
Dep. Var. lagged		0.48***	0.54***	0.55***	0.66***					
		(0.06)	(0.10)	(0.10)	(0.10)					
Observations	614	506	402	402	406	506	506	506	506	506
Countries	112	100	91	91	91	100	100	100	100	100
Democracy changes in the sample	55	41	29	29	29	41	41	41	41	41

Note: OLS estimates (Columns 1–2) include a full set of country and year fixed effects. Arellano and Bond's GMM estimators of the dynamic panel model (Columns 3–4) remove country fixed effects by taking first differences of the data, or by taking forward orthogonal differences (Column 5) and then constructing moment conditions using predetermined lags of the dependent variable, the interaction term and democracy. Columns 4 and 5 use up to the fifth lag of predetermined variables to create moments, restricting the number of moments used. Columns 6–10 impose different values for the autocorrelation coefficient in the tax to GDP series, and estimate the effect of democracy and the interaction term including a full set of country and year fixed effects. All models control for lagged GDP per capita but this coefficient is not reported to save space. Robust standard errors, adjusted for clustering at the country level, are in parentheses. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

Table 21.13 Effects of democratization on inequality (Includes interaction of democracy with share of income held by the top 10 decile in the United States at the time of democratization)

			GMM			Assuming AR(1) coefficient				
						$\rho = 0$	$\rho = 0.25$	$\rho = 0.5$	$\rho = 0.75$	$\rho = 1$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable: Gini coefficient, net income										
Democracy lagged	0.68	−0.76	−2.35	−3.06★	−0.88	−0.46	−0.70	−0.94	−1.18	−1.42
	(0.79)	(0.89)	(1.57)	(1.64)	(1.52)	(0.94)	(0.90)	(0.90)	(0.93)	(1.00)
Lagged democracy × Top	0.22★	0.19★	−0.10	−0.12	0.22	0.27★	0.21★	0.14	0.08	0.01
10 share in the US	(0.13)	(0.11)	(0.19)	(0.19)	(0.17)	(0.15)	(0.12)	(0.10)	(0.09)	(0.10)
Dep. Var. lagged		0.31★★★	0.35★★★	0.36★★★	0.47★★★					
		(0.07)	(0.10)	(0.11)	(0.08)					
Observations	657	537	420	420	424	537	537	537	537	537
Countries	127	113	100	100	100	113	113	113	113	113
Democracy changes in the sample	65	47	31	31	31	47	47	47	47	47
Dependent variable: Gini coefficient, gross income										
Democracy lagged	−1.04	−1.55	−0.68	−0.71	0.44	−1.61	−1.58	−1.54	−1.51★	−1.48
	(0.98)	(0.95)	(1.46)	(1.76)	(1.69)	(1.18)	(1.04)	(0.94)	(0.89)	(0.91)
Lagged democracy × Top	0.72★★★	0.36★★★	0.25	0.28	0.37★★	0.72★★★	0.52★★★	0.33★★★	0.13	−0.06
10 share in the US	(0.17)	(0.11)	(0.18)	(0.21)	(0.16)	(0.19)	(0.15)	(0.11)	(0.09)	(0.10)
Dep. Var. lagged		0.46★★★	0.61★★★	0.60★★★	0.71★★★					
		(0.06)	(0.11)	(0.11)	(0.11)					
Observations	657	537	420	420	424	537	537	537	537	537
Countries	127	113	100	100	100	113	113	113	113	113
Democracy changes in the sample	65	47	31	31	31	47	47	47	47	47

Note: OLS estimates (Columns 1–2) include a full set of country and year fixed effects. Arellano and Bond's GMM estimators of the dynamic panel model (Columns 3–4) remove country fixed effects by taking first differences of the data, or by taking forward orthogonal differences (Column 5) and then constructing moment conditions using predetermined lags of the dependent variable, the interaction term and democracy. Columns 4 and 5 use up to the fifth lag of predetermined variables to create moments, restricting the number of moments used. Columns 6–10 impose different values for the autocorrelation coefficient in the tax to GDP series, and estimate the effect of democracy and the interaction term including a full set of country and year fixed effects. All models control for lagged GDP per capita but this coefficient is not reported to save space. Robust standard errors, adjusted for clustering at the country level, are in parentheses. ★★★: significant at 1%; ★★: significant at 5%; ★: significant at 10%.

to GDP ratio. However, we do see generally significant impact of this interaction on the gross Gini, which appears to be further increased by democracy when there is greater inequality in the United States. There is also a similar effect on the net Gini but is much weaker and not present when using the GMM estimators. Though on the whole this evidence is on the weak side, it is broadly consistent with a story in which democratization increases inequality at times when the expanded market opportunities available are more disequalizing.

Finally, Tables 21.14–21.17 provide some preliminary evidence on Director's law. Recall from our discussion in Section 21.2, in particular Proposition 5, that our (modified) Director's law implies that the negative effect of democracy on inequality should be visible or greater in places where the rich have a large share of income (Meltzer-Richards also predicts this) and, more uniquely, should be positive where the poor have a higher share of income (which is the opposite of the Meltzer-Richards prediction). Thus, we investigate the heterogeneous effect of democracy depending on the shares of the top and bottom of the income distribution (in each case relative to the share of the middle, i.e., using the top and bottom income shares described above). Recall also that the effect of the income share of the rich on inequality in democracy is related to whether there is capture of democracy by the elite, which provides a reason why this prediction of Proposition 5 may not hold even when a greater share of income of the poor may increase inequality as posited in Proposition 5.

Indeed, Table 21.14 shows that when the top decile is richer relative to the middle, there is no significantly heterogeneous effect on inequality, although coefficients are generally negative. This might be because this estimate is picking up both an elite capture effect (as in the land Gini interaction specifications) as well as additional demand for redistribution by the median voter as in our (modified) Director's law, with higher incidence on the rich. Table 21.15 provides support for the possibility that top tail inequality, as measured by the top share, could be picking up elite capture effects. It shows that the effect of democracy on the tax to GDP ratio is significantly attenuated by income inequality as measured by the top share (but there is no effect on government revenue as a fraction of GDP), contrary to what Meltzer and Richards model or our (modified) Director's law would predict. Our conclusion from this exercise is that our research design does not allow us to separate the effects of democracy through the demand for redistribution and the incidence of taxation emphasized in our modified Director's law from the possibility that democracies with large upper tail inequality are more likely to be captured by the wealthier elite.

Tables 21.16 and 21.17, on the other hand, provide support for the more unique prediction of the (modified) Director's law, that democracy should increase inequality more when the poor are closer to the middle class in nondemocracy. Table 21.16 looks at the interaction of the bottom income share with democracy, and finds that the net Gini does in fact increase with democratization, while there is no effect on the gross Gini. This

Table 21.14 Effects of democratization on inequality (Includes interaction of democracy with the average share of income held by the top decile relative to share of mid 50th earners before 2000)

			GMM			Assuming AR(1) coefficient				
						$\rho = 0$	$\rho = 0.25$	$\rho = 0.5$	$\rho = 0.75$	$\rho = 1$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable: Gini coefficient, net income										
Democracy lagged	0.79	−0.54	−1.39	−1.73	−1.24	−0.20	−0.48	−0.76	−1.04	−1.32
	(0.80)	(0.88)	(1.47)	(1.49)	(1.33)	(0.95)	(0.91)	(0.91)	(0.95)	(1.03)
Lagged democracy × Top share	−0.01	−0.01	−0.02	−0.02	−0.01	−0.01	−0.01	−0.01	−0.01	−0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Dep. Var. lagged		0.30***	0.29***	0.30***	0.32***					
		(0.07)	(0.08)	(0.08)	(0.08)					
Observations	606	503	397	397	401	503	503	503	503	503
Countries	110	102	93	93	93	102	102	102	102	102
Democracy changes in the sample	55	41	29	29	29	41	41	41	41	41
Dependent variable: Gini coefficient, gross income										
Democracy lagged	−0.76	−1.29	−1.73	−2.30	−1.55	−1.02	−1.16	−1.30	−1.45	−1.59
	(0.93)	(0.85)	(1.31)	(1.41)	(1.30)	(0.98)	(0.89)	(0.85)	(0.88)	(0.97)
Lagged democracy × Top share	−0.00	−0.00	0.00	−0.00	0.00	−0.00	−0.00	−0.00	−0.01	−0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Dep. Var. lagged		0.48***	0.52***	0.54***	0.60***					
		(0.06)	(0.08)	(0.08)	(0.08)					
Observations	606	503	397	397	401	503	503	503	503	503
Countries	110	102	93	93	93	102	102	102	102	102
Democracy changes in the sample	55	41	29	29	29	41	41	41	41	41

Note: OLS estimates (Columns 1–2) include a full set of country and year fixed effects. Arellano and Bond's GMM estimators of the dynamic panel model (Columns 3–4) remove country fixed effects by taking first differences of the data, or by taking forward orthogonal differences (Column 5) and then constructing moment conditions using predetermined lags of the dependent variable, the interaction term and democracy. Columns 4 and 5 use up to the fifth lag of predetermined variables to create moments, restricting the number of moments used. Columns 6–10 impose different values for the autocorrelation coefficient in the tax to GDP series, and estimate the effect of democracy and the interaction term including a full set of country and year fixed effects. All models control for lagged GDP per capita but this coefficient is not reported to save space. Robust standard errors, adjusted for clustering at the country level, are in parentheses. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

Table 21.15 Effects of democratization on the log of tax and total government revenue as a percentage of GDP (Includes interaction of democracy with the average share of income held by the top decile relative to share of mid 50th earners before 2000)

			GMM			Assuming AR(1) coefficient				
						$\rho = 0$	$\rho = 0.25$	$\rho = 0.5$	$\rho = 0.75$	$\rho = 1$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable: Tax revenues as a percentage of GDP										
Democracy lagged	18.75***	14.54***	20.93***	21.97**	19.86**	18.75***	14.50***	10.24***	5.99**	1.74
	(4.88)	(3.72)	(8.02)	(9.86)	(8.55)	(4.88)	(3.46)	(2.48)	(2.52)	(3.54)
Lagged democracy × Top share	−0.10***	−0.08***	−0.22**	−0.19**	−0.20**	−0.10***	−0.08***	−0.06***	−0.03*	−0.01
	(0.04)	(0.03)	(0.09)	(0.08)	(0.10)	(0.04)	(0.03)	(0.02)	(0.02)	(0.02)
Dep. Var. lagged		0.25***	0.23***	0.25***	0.30***					
		(0.06)	(0.09)	(0.08)	(0.08)					
Observations	843	843	730	730	730	843	843	843	843	843
Countries	113	113	110	110	110	113	113	113	113	113
Democracy changes in the sample	72	72	67	67	67	72	72	72	72	72
Dependent variable: Total government revenues as a percentage of GDP										
Democracy lagged	10.56**	8.46***	14.27**	15.50**	13.97**	10.56**	9.09***	7.61***	6.13***	4.66
	(4.03)	(2.43)	(6.11)	(7.07)	(6.86)	(4.03)	(2.81)	(2.02)	(2.19)	(3.17)
Lagged democracy × Top share	−0.03	−0.02	−0.10	−0.11	−0.12	−0.03	−0.02	−0.02	−0.01	−0.00
	(0.03)	(0.02)	(0.06)	(0.07)	(0.08)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
Dep. Var. lagged		0.36***	0.43***	0.48***	0.50***					
		(0.04)	(0.06)	(0.06)	(0.06)					
Observations	843	843	730	730	730	843	843	843	843	843
Countries	113	113	110	110	110	113	113	113	113	113
Democracy changes in the sample	72	72	67	67	67	72	72	72	72	72

Note: OLS estimates (Columns 1–2) include a full set of country and year fixed effects. Arellano and Bond's GMM estimators of the dynamic panel model (Columns 3–4) remove country fixed effects by taking first differences of the data, or by taking forward orthogonal differences (Column 5) and then constructing moment conditions using predetermined lags of the dependent variable, the interaction term and democracy. Columns 4 and 5 use up to the fifth lag of predetermined variables to create moments, restricting the number of moments used. Columns 6–10 impose different values for the autocorrelation coefficient in the tax to GDP series, and estimate the effect of democracy and the interaction term including a full set of country and year fixed effects. All models control for lagged GDP per capita but this coefficient is not reported to save space. Robust standard errors, adjusted for clustering at the country level, are in parentheses. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

Table 21.16 Effects of democratization on inequality (Includes interaction of democracy with the average share of income held by the bottom decile relative to share of mid 50th earners before 2000)

			GMM			Assuming AR(1) coefficient				
						$\rho = 0$	$\rho = 0.25$	$\rho = 0.5$	$\rho = 0.75$	$\rho = 1$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable: Gini coefficient, net income										
Democracy lagged	0.92	−0.41	−2.11⋆	−2.64⋆	−1.93⋆	−0.07	−0.35	−0.64	−0.92	−1.21
	(0.78)	(0.85)	(1.28)	(1.35)	(1.14)	(0.91)	(0.88)	(0.88)	(0.93)	(1.01)
Lagged democracy × Bottom	0.52⋆	0.67⋆⋆	0.94⋆	0.71	0.58	0.71⋆⋆	0.68⋆⋆	0.65⋆⋆	0.62⋆⋆	0.59⋆⋆
share	(0.29)	(0.28)	(0.56)	(0.51)	(0.44)	(0.30)	(0.28)	(0.27)	(0.28)	(0.30)
Dep. Var. lagged		0.30⋆⋆⋆	0.33⋆⋆⋆	0.35⋆⋆⋆	0.43⋆⋆⋆					
		(0.07)	(0.07)	(0.08)	(0.07)					
Observations	606	503	397	397	401	503	503	503	503	503
Countries	110	102	93	93	93	102	102	102	102	102
Democracy changes in the sample	55	41	29	29	29	41	41	41	41	41
Dependent variable: Gini coefficient, gross income										
Democracy lagged	−0.68	−1.24	−1.57	−2.35⋆	−1.58	−0.95	−1.10	−1.25	−1.40	−1.55
	(0.95)	(0.86)	(1.26)	(1.42)	(1.24)	(1.00)	(0.91)	(0.87)	(0.89)	(0.97)
Lagged democracy × Bottom	0.29	0.28	−0.08	−0.24	0.09	0.31	0.29	0.28	0.26	0.25
share	(0.37)	(0.31)	(0.52)	(0.56)	(0.49)	(0.37)	(0.33)	(0.31)	(0.32)	(0.35)
Dep. Var. lagged		0.48⋆⋆⋆	0.58⋆⋆⋆	0.60⋆⋆⋆	0.66⋆⋆⋆					
		(0.06)	(0.08)	(0.08)	(0.07)					
Observations	606	503	397	397	401	503	503	503	503	503
Countries	110	102	93	93	93	102	102	102	102	102
Democracy changes in the sample	55	41	29	29	29	41	41	41	41	41

Note: OLS estimates (Columns 1–2) include a full set of country and year fixed effects. Arellano and Bond's GMM estimators of the dynamic panel model (Columns 3–4) remove country fixed effects by taking first differences of the data, or by taking forward orthogonal differences (Column 5) and then constructing moment conditions using predetermined lags of the dependent variable, the interaction term and democracy. Columns 4 and 5 use up to the fifth lag of predetermined variables to create moments, restricting the number of moments used. Columns 6–10 impose different values for the autocorrelation coefficient in the tax to GDP series, and estimate the effect of democracy and the interaction term including a full set of country and year fixed effects. All models control for lagged GDP per capita but this coefficient is not reported to save space. Robust standard errors, adjusted for clustering at the country level, are in parentheses. ⋆⋆⋆: significant at 1%; ⋆⋆: significant at 5%; ⋆: significant at 10%.

Table 21.17 Effects of democratization on the log of tax and total government revenue as a percentage of GDP (Includes interaction of democracy with the average share of income held by the bottom decile relative to share of mid 50th earners before 2000)

			GMM			Assuming AR(1) coefficient				
						$\rho = 0$	$\rho = 0.25$	$\rho = 0.5$	$\rho = 0.75$	$\rho = 1$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable: Tax revenues as a percentage of GDP										
Democracy lagged	18.72***	14.44***	18.47*	22.49**	17.16**	18.72***	14.42***	10.11***	5.81**	1.50
	(5.18)	(3.98)	(9.43)	(10.35)	(8.34)	(5.18)	(3.70)	(2.61)	(2.49)	(3.45)
Lagged democracy × Bottom share	5.04***	3.88***	7.34	9.31*	6.36	5.04***	3.87***	2.70**	1.54	0.37
	(1.88)	(1.46)	(4.84)	(5.58)	(5.54)	(1.88)	(1.44)	(1.14)	(1.12)	(1.38)
Dep. Var. lagged		0.25***	0.24***	0.24***	0.30***					
		(0.06)	(0.09)	(0.08)	(0.07)					
Observations	843	843	730	730	730	843	843	843	843	843
Countries	113	113	110	110	110	113	113	113	113	113
Democracy changes in the sample	72	72	67	67	67	72	72	72	72	72
Dependent variable: Total government revenues as a percentage of GDP										
Democracy lagged	10.78***	8.30***	13.23*	14.20**	11.74*	10.78***	9.03***	7.29***	5.54**	3.80
	(4.07)	(2.46)	(6.91)	(7.05)	(7.10)	(4.07)	(2.84)	(2.01)	(2.12)	(3.08)
Lagged democracy × Bottom share	1.55	0.74	2.92	4.14	3.37	1.55	0.98	0.40	−0.17	−0.74
	(1.63)	(1.21)	(3.09)	(3.86)	(3.77)	(1.63)	(1.29)	(1.10)	(1.14)	(1.39)
Dep. Var. lagged		0.35***	0.43***	0.47***	0.50***					
		(0.04)	(0.06)	(0.06)	(0.05)					
Observations	843	843	730	730	730	843	843	843	843	843
Countries	113	113	110	110	110	113	113	113	113	113
Democracy changes in the sample	72	72	67	67	67	72	72	72	72	72

Note: OLS estimates (Columns 1–2) include a full set of country and year fixed effects. Arellano and Bond's GMM estimators of the dynamic panel model (Columns 3–4) remove country fixed effects by taking first differences of the data, or by taking forward orthogonal differences (Column 5) and then constructing moment conditions using predetermined lags of the dependent variable, the interaction term and democracy. Columns 4 and 5 use up to the fifth lag of predetermined variables to create moments, restricting the number of moments used. Columns 6–10 impose different values for the autocorrelation coefficient in the tax to GDP series, and estimate the effect of democracy and the interaction term including a full set of country and year fixed effects. All models control for lagged GDP per capita but this coefficient is not reported to save space. Robust standard errors, adjusted for clustering at the country level, are in parentheses. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

relative difference between the pre-fiscal and post-fiscal effects suggests that government redistribution may be (part of) the mechanism. Table 21.17 confirms this by showing that the tax to GDP ratio does go up following a democratization in a society where the poor are initially relatively well-off compared to the middle class.

Subject to the major caveats about omitted variables and measurement error, this evidence thus provides some support to our (modified) Director's law: middle classes empowered by democracy appear to be able to use the government to transfer resources from the poor to themselves, increasing post-fiscal inequality. As far as we know, this is the first evidence of this kind on how democracy might redistribute in a way that *increases* inequality.

We have investigated a number of other sources of heterogeneity, including various measures of ethnolinguistic fragmentation, wheat-sugar land suitability ratio (as a measure of the type of agriculture), constitutional provisions against redistribution, and average level of social unrest, and found no robust results.

Overall, the important concerns about endogeneity and measurement error notwithstanding, the results presented in this section paint a picture in which democracy does indeed create greater pressures for redistribution, but the pathways via which these affect inequality are more nuanced than the standard Meltzer-Richard mechanism presumes. In particular, the correlation between democracy and inequality appears to be more limited than one might have at first expected (and more limited than the effect on taxes). On the other hand, the evidence on heterogeneity of effects, even if not as robustly estimated as the impact on taxes, indicates that interactions with elite capture, structural transformation, middle-class bias in redistribution, and the disequalizing market opportunities opened up by democracy might be playing some role in modulating the influence of democracy on inequality.

21.6. CONCLUSION

The effect of democracy on redistribution and inequality is important for understanding how democracies function and use the available policy instruments. Nevertheless, our survey of the relevant literature shows that the social science literature on this topic is far from a consensus or a near-consensus on this topic.

We explained why the baseline expectation in the literature has been that democracy should increase redistribution and reduce inequality (for example, based on Meltzer and Richard's, 1981 seminal paper), and why this expectation may not be borne out in the data because democracy may be captured or constrained; because democracy may cater to the wishes of the middle class; or because democracy may simultaneously open up new economic opportunities to the previously excluded, contributing to economic inequality. This ambiguity may be one of the reasons why the large empirical literature on this topic comes to such inconclusive findings, though the use of datasets with different

qualities and different methodologies and econometric practices, many of which are far from satisfactory, are also contributing factors. It may also be that because different researchers have looked at different sets of countries in different periods, the differing results are to some extent picking up situations where one or another of the mechanisms we have identified is more dominant.

The bulk of the chapter empirically investigated the (dynamic) relationship between democracy and various economic outcomes related to redistribution and inequality. Our results, which come from panel data models controlling for the dynamics and persistence in our outcome variables, indicate that democratization does indeed increase government taxation and revenue as fractions of GDP. This confirms the basic prediction of the standard Meltzer-Richard model. In contrast, we have found no robust evidence that democracy reduces inequality, although our estimated coefficients are quite imprecise in this case. Our results also suggest that democracy increases the share of GDP and population not in agriculture, as well as secondary school enrollment. This is consistent with democracy triggering a more rapid structural transformation, for example, because this structural transformation may have been arrested or slowed down by the nondemocratic political system. The relationship between democratic institutions and structural change is worth further investigation.

These patterns suggest that the effect of democracy on redistribution and inequality may be more nuanced than often presumed and highly heterogeneous across societies. We tried to make some tentative progress on this issue by providing additional correlations pertaining to these heterogeneous effects and mechanisms on which they might be based. We found some results suggesting that democratization in the presence of powerful landed elites may increase inequality, and that structural transformation may induce an expansion of opportunities that counteract any additional redistribution, and either of these could explain the absence of an effect on inequality. This interpretation is confirmed by our finding that democracy increases inequality in places that have a lower share of population in agriculture, and at times when the global technological and organizational frontier is more inequality inducing. A natural next step for research is isolating exogenous variation in these heterogeneous effects across democracies and nondemocracies.

In addition, we also found some evidence consistent with a (modified) Director's law, which suggests that democracy redistributes from the rich and the poor to the middle class, and therefore its effect on inequality may depend on the relative position of the middle class vis-a-vis the poor and the rich. Further research on whether and how democracies transfer from the poor to the middle class would be an important contribution.

(Overall, the evidence suggests that to the impact of democracy on inequality is limited, and these limited effects work by altering pre-redistribution market outcomes, while the fiscal mechanisms stressed by the literature play at most a small role in explaining any

effect of democracy on inequality, and may in fact be inequality-increasing. We hope that further research on these issues, tackling the first-order endogeneity concerns and exploiting within-country as well as cross-national variation, will more systematically uncover the mechanisms at work.)

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APPENDIX A. COMPARISON TO RODRIK (1999)

This appendix replicates and extends the analysis in Rodrik (1999). At a first glance, the fact that we find no robust effect on net or gross income inequality seems at odds with Rodrik's findings that democracies pay higher real wages in manufacturing. These opposite findings could be explained by a logic similar to the one outlined in Proposition 4. In particular, democracies may increase wages by allowing workers to reallocate to new sectors, but this may also increase inequality if there is sufficient heterogeneity in labor productivity and wages were previously compressed and reduced by labor market institutions. Besides this conceptual difference we also explore the differences between our empirical setting and Rodrik's. We show that while the results are robust to our democracy measure, they are fragile in a number of other directions.

Rodrik's data generating model is given by

$$\log w_{it} = \beta D_{it} + X_{it}\gamma + \delta_i + \delta_t + \Sigma_{it},$$

with w_{it} manufacturing wages from the UNIDO dataset compiled by Martin Rama. However, this model cannot be estimated because wage data comes grouped on averages for the years $t, t+1, t+2, t+3, t+4$ for every 5 years from 1960 onward. Thus, only the average wages between 1960 and 1964, 1965 and 1969, and so on are observed. Thus, Rodrik estimates

$$\log w_{it,t+4} = \beta D_{it,t+4} + X_{it,t+4}\gamma + \delta_i + \delta_t + \epsilon_{it,t+4}. \tag{21.A1}$$

with all variables averaged over 5 year periods (from t to $t+4$), and the model is estimated in a panel covering $1960, 1965, \ldots, 1990$. Though Rodrik presents cross-sectional and panel estimates, we focus on the latter which are the more convincing ones and are also closer to the empirical strategy adopted in this chapter.

In the top panel of Table 21.A1 we present different estimates of Equation (21.A1) using a normalized polity score between 0 and 1, a normalized Freedom House index between 0 and 1 and our democracy measure separately as proxies for democracy. We always control for the log of GDP per capita, the log of worker value added in

Table 21.A1 Replication of Rodrik's results on the log of manufacturing wages

	Original wage data			Updated wage data		
	(1)	(2)	(3)	(4)	(5)	(6)
Averaging democracy measure over *t*, *t*+4						
Polity index at *t*, *t*+4	19.25***			14.48**		
	(5.72)			(6.00)		
Freedom house index at *t*, *t*+4		15.78**			7.60	
		(7.55)			(8.68)	
Our democracy index at *t*, *t*+4			8.48**			6.51
			(3.66)			(4.20)
Observations	442	365	468	451	364	467
Countries	93	98	99	90	92	92
Using democracy measure at *t*						
Polity index at *t*	8.40			9.01		
	(6.15)			(5.89)		
Freedom house index at *t*		11.03			11.52	
		(10.55)			(9.77)	
Our democracy index at *t*			1.98			2.89
			(3.54)			(3.39)
Observations	429	285	455	437	294	456
Countries	91	96	97	85	87	90

Dependent variable is log of average wages between *t* and *t*+4.
Note: OLS estimates include a full set of country and year fixed effects. All models control for the log of GDP per capita, log of worker value added and log of the price level, but these coefficients are not reported to save space. Robust standard errors, adjusted for clustering at the country level, are in parentheses. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

manufacturing and the log of the price index (from the Penn World Tables) following Rodrik's original setup. The estimates of β are multiplied by 100 to ease their interpretation. The left panel uses Rodrik's original wage data and the right panel uses an updated version. In all models we present robust standard errors adjusting for clustering at the country level, which are reflected in slightly higher standard errors than the ones found by Rodrik.

Our estimates show that democracy, measured by any of the indices, is associated with higher wages using the original wage data, which replicates Rodrik's findings. There are still some small differences caused by updates to Polity and Freedom House, but qualitatively his conclusions hold. In particular, an increase in the polity score from 0 to 1 increases wages by 19.72% (s.e. = 5.98); an increase in the Freedom House index from 0 to 1 increases wages by 20.57% (s.e. = 8.13), and a switch from nondemocracy to democracy in our measure increases wages by 8.54% (s.e. = 3.88). The results using the new wage data are less clear, smaller, and not significant for Freedom House and

our democracy measure. The results suggest that the association between democracy and wages is not robust if one uses the updated wage data and the same empirical strategy as Rodrik.

There are two more issues that are important to consider in weighing the importance of Rodrik's evidence. The wage data are in the form of 5-year averages. First, this will tend to induce nontrivial serial correlation in the dependent variable, inducing error in the presence of lagged dependent variables on the right-hand side (which our estimates suggest are present). Second, by averaging the democracy index, Rodrik's specification induces the correlation between wages at t and democracy at $t+1, t+2, t+3$ and $t+4$, which of course does not reflect the effect of democracy on wages, to influence the estimate for β.

To address the second issue and get closer to the empirical strategy we used in this chapter, we can estimate the model

$$\log w_{it,t+4} = \beta D_{it} + X_{it}\gamma + \delta_i + \delta_t + \epsilon_{it}.$$

This model still averages the dependent variable, which cannot be undone given the wage data, but uses the baseline value of the democracy index and the controls for the years $1960, 1965, \ldots, 1990$. The bottom panel in Table 21.A1 presents our results using the original wage data (left panel) and updated wage data (right panel). The estimates for β are significantly smaller and never significant. The comparison between the top panel—which uses Rodrik's original specification—and our preferred specification in the bottom suggests that Rodrik's results are, at least in part, driven by a correlation between wages at t and democracy at $t+1, t+2, t+3$ and $t+4$.

Finally, we present estimates of the model

$$\log w_{it,t+4} = \rho \log w_{it-5,t-1} + \beta D_{it} + X_{it}\gamma + \delta_i + \delta_t + \epsilon_{it}, \tag{21.A2}$$

which comes closest to the empirical specification we used throughout the paper. Table 21.A2 has the same structure as Table 21.2 in the paper and presents several estimates of the dynamic panel model in Equation (21.A1). In this case, the lagged dependent variable also controls for the nontrivial autocorrelation patterns induced by averaging the dependent variable. The results confirm that there is no effect of democracy at time t on average wages between t and $t+4$. Only the GMM estimates show large effects that are almost significant at conventional levels. But these estimates are unreliable because they are significantly above the fixed effect models with different imposed values of ρ (and these estimates should bracket them). Moreover, the estimated ρ is too small compared to the fixed effects estimates (it should typically be larger). We believe that this pattern may be caused by the averaging of the dependent variable, which invalidates the moment conditions of GMM estimation.

Table 21.A2 Effects of democratization on the log of manufacturing wages controlling for worker value added, prices and GDP per capita

			GMM			Assuming AR(1) coefficient				
						$\rho = 0$	$\rho = 0.25$	$\rho = 0.5$	$\rho = 0.75$	$\rho = 1$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Democracy at t	2.89	2.65	15.42	15.91	13.22	3.84	2.65	1.45	0.25	−0.95
	(3.39)	(4.01)	(9.64)	(10.35)	(10.22)	(3.85)	(3.89)	(4.38)	(5.19)	(6.20)
Dep. Var. lagged		0.25★★★	0.20★	0.21★	0.17					
		(0.09)	(0.12)	(0.12)	(0.11)					
Observations	456	384	297	297	298	384	384	384	384	384
Countries	90	86	79	79	79	86	86	86	86	86
Number of moments			40	38	38					
Hansen p-value			0.52	0.44	0.49					
AR2 p-value			0.21	0.21	0.29					
Democracy changes in the sample	47	45	35	35	35	45	45	45	45	45
Long-run effect of democracy	2.89	3.53	19.31	20.01	15.93	3.84	3.53	2.90	1.00	.
p-Value for the long-run effect	0.40	0.50	0.10	0.11	0.19	0.32	0.50	0.74	0.96	.

Dependent variable is log of average wages between t and $t+4$.

Note: OLS estimates (Columns 1–2) include a full set of country and year fixed effects. Arellano and Bond's GMM estimators of the dynamic panel model (Columns 3–4) remove country fixed effects by taking first differences of the data, or by taking forward orthogonal differences (Column 5) and then constructing moment conditions using predetermined lags of the dependent variable and democracy. Columns 4 and 5 use up to the fifth lag of predetermined variables to create moments, restricting the number of moments used. Columns 6–10 impose different values for the autocorrelation coefficient in the tax revenue as a percentage of GDP series, and estimate the effect of democracy including a full set of country and year fixed effects. All models control for the log of GDP per capita, log of worker value added and log of the price level, but these coefficients are not reported to save space. Robust standard errors, adjusted for clustering at the country level, are in parentheses. ★★★: significant at 1%; ★★: significant at 5%; ★: significant at 10%. We do not report long-run effects and their p-values in Column 10 because they are not defined for $\rho = 1$.

Rodrik also estimates models using wage data compiled by the Bureau of Labor Statistics for a smaller set of countries. The very small number of democratizations in this sample (only Portugal, South Korea, and Spain) makes these results less reliable. In any case, using Rodrik's original specification, we find that our democracy measure is associated with a 37% increase in wages (standard error = 14.23), but when we estimate the specification in Equation (21.6), including the lagged dependent variable, the effect becomes smaller and no longer significant.

APPENDIX B. RESULTS USING OTHER MEASURES OF DEMOCRACY

In this section we study whether our results are driven by our new measure of democracy. In particular we use Cheibub et al. (2010) Democracy-Dictatorship data (CGV) and Boix-Miller-Rosato's Complete Dataset of Political Regimes, 1800–2007 (BMR). Both datasets are different updates and revisions of the Przeworski et al. (2000) measure. We estimate our basic dynamic panel model using the log of tax revenue as a percentage of GDP, and the Gini coefficient for net and gross income as dependent variable. We only report fixed effects estimates and the Arellano and Bond GMM estimates for each of these variables.

The top panel in Table 21.A3 presents the results using Cheibub et al. (2010) democracy measure; while the bottom panel presents the results using Boix et al. (2012) democracy measure. We find a similar pattern and similar magnitudes, though our GMM estimates on the tax to GDP ratio are less precise and not significant. Again, there is

Table 21.A3 Effects of democratization on the log of tax revenue as a percentage of GDP per capita, and Gini coefficient of net and gross income

	Tax ratio		Net Gini		Gross gini	
	OLS	GMM	OLS	GMM	OLS	GMM
	(1)	(2)	(3)	(4)	(5)	(6)
Using Cheibub et al. (2010) democracy measure						
Democracy lagged	9.48**	11.44	−0.55	−1.45	−1.02	−1.56
	(3.80)	(7.58)	(0.89)	(1.77)	(0.81)	(1.26)
Dep. Var. lagged	0.27***	0.28***	0.32***	0.35***	0.49***	0.77***
	(0.06)	(0.10)	(0.07)	(0.10)	(0.06)	(0.09)
Observations	942	814	537	420	537	420
Countries	128	125	113	100	113	100
Number of moments		81		81		81
Hansen *p*-value		0.17		0.59		0.34
AR2 *p*-value		0.89		0.02		0.45
Democracy changes in the sample	92	82	47	31	47	31
Long-run effect of democracy	12.98	15.82	−0.80	−2.22	−2.01	−6.87

Continued

Table 21.A3 Effects of democratization on the log of tax revenue as a percentage of GDP per capita, and Gini coefficient of net and gross income—cont'd

	Tax ratio		Net Gini		Gross gini	
	OLS	GMM	OLS	GMM	OLS	GMM
	(1)	(2)	(3)	(4)	(5)	(6)
p-Value for the long-run effect	0.01	0.12	0.53	0.41	0.21	0.24
Using Boix et al. (2012) democracy measure						
Democracy lagged	9.94***	10.57	−0.43	−1.99	−1.23	−2.16
	(3.10)	(9.06)	(0.88)	(1.65)	(0.86)	(1.46)
Dep. Var. lagged	0.27***	0.28***	0.32***	0.35***	0.49***	0.63***
	(0.06)	(0.10)	(0.07)	(0.10)	(0.06)	(0.11)
Observations	944	816	537	420	537	420
Countries	128	125	113	100	113	100
Number of moments		81		81		81
Hansen *p*-value		0.16		0.61		0.42
AR2 *p*-value		0.91		0.02		0.64
Democracy changes in the sample	92	82	47	31	47	31
Long-run effect of democracy	13.61	14.66	−0.63	−3.08	−2.42	−5.81
p-Value for the long-run effect	0.00	0.24	0.62	0.22	0.17	0.17

Note: Odd columns present OLS estimates with a full set of country and year fixed effects. Even columns present Arellano and Bond's GMM estimators of the dynamic panel model which remove country fixed effects by taking first differences of the data and then constructing moment conditions using predetermined lags of the dependent variable and democracy. All models control for the lag of GDP per capita but these coefficients are not reported to save space. Robust standard errors, adjusted for clustering at the country level, are in parentheses. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

an effect on tax revenue as a percentage of GDP, which holds in a more robust way when we focus on specifications in levels that are not reported here to save space. We also continue to find no robust effect on inequality.

Overall, the results are broadly similar using other measures of democracy, though they are more precise and consistent with our preferred measure—as would be expected if our measure removes some of the measurement error present in other indices. This was one of the main goals for its construction.

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