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Democratization and the Conditional Dynamics of Income Distribution

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DEMOCRATIZATION AND THE CONDITIONAL DYNAMICS OF INCOME DISTRIBUTION*

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Abstract

Most theoretical accounts imply that democratization will reduce income inequality as representative governments become accountable to citizens who would benefit from increased redistribution from the elite. Yet, available empirical evidence does not support the notion that democratization, on average, leads to more equal income distributions. This paper starts from the simple observation that autocracies are quite heterogeneous and govern extreme distributional outcomes (also egalitarian). From extreme initial conditions, democratization may lead income distributions to a “middle ground”. We thus examine the extent to which initial inequality levels determine the path of distributional dynamics following democratization. Using fixed effects and instrumental variable estimates we demonstrate that egalitarian autocracies become more unequal following democratization, whereas democratization has an equalizing effect in highly unequal autocracies.

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1 Introduction

This paper reconsiders the effect of democracy on the level of income inequality in society. We start from the simple observation that autocratic regimes are highly heterogeneous entities. From monarchistic, to business-friendly militaristic, to populist, to communistic, autocratic regimes of the late 20th and early 21st centuries are almost defined by their competing ideologies concerning how spoils should be divided within the economies they govern. Indeed, the differences are not only ideological, but are reflected in the historical income inequality data – in our sample, autocratic countries have had Gini coefficients as low as 20 and as high as 75.¹ It naturally follows that income inequality dynamics following transitions from autocracy to democracy may also be quite heterogeneous. This simple observation is our starting point, from which we provide thorough empirical documentation of a non-linearity that has not been examined in the literature. Namely, we demonstrate how income inequality dynamics following a switch to democracy depend on the initial (pre-democracy) level of income inequality. Intuitively, our results suggest that democracy provides a kind of “middle ground” – autocrats who governed extreme distributional outcomes are replaced by political processes that gravitate towards more centrist outcomes. More importantly, we provide evidence that democratization strongly affects the degree of income inequality despite the fact that the unconditional mean effect is null.

The most common narrative in the economics and political science literatures is that democratization should reduce inequality levels. Autocracies are often elite-dominated societies that have implemented political and economic institutions designed to protect the elite’s wealth. Shifting to a democratic political institution allows for a broader set of economic interests to be served. In their canonical rational choice model of political transitions, Acemoglu and Robinson (2001) show how following the political enfranchisement of the poor, the decisive voter (or, decisive political preference) becomes relatively more poor and, all else equal, should call for inequality-reducing redistributions, following the classic rational theories of income taxation and redistribution (Meltzer and Richard 1981; Roberts 1977; Romer 1975). Moreover, the greater the initial degree of inequality before democracy, the greater should be the decline in inequality following a shift to democracy.²

Yet, the empirical literature concerning the effect of democracy on economic inequalities has not reached a consensus supporting this straightforward empirical prediction.

¹Moreover, we later show that the heterogeneity in terms of income distribution is not driven by the economic development level. We show that the dispersion of Gini coefficients among autocratic countries is quite similar within income groups.

²See also Ansell and Samuels (2014) and Boix (2003) for alternative narratives from political science.

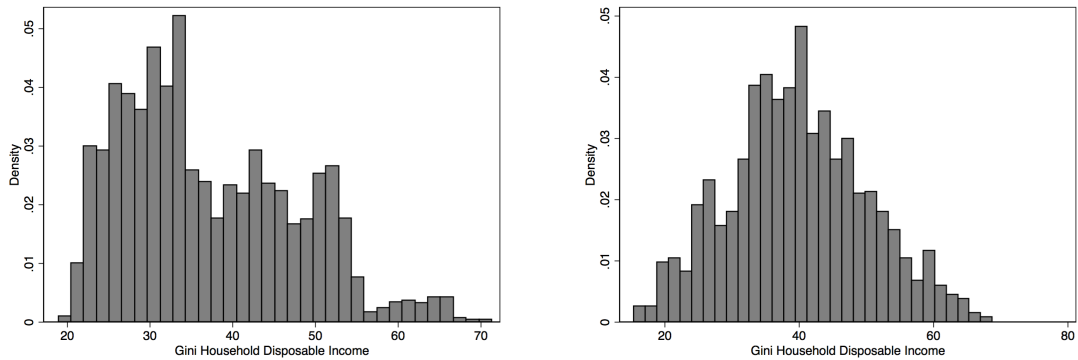


Figure 1: The distribution of Net Gini coefficients among autocracies (left) and democracies (right).

Acemoglu, Naidu, Restrepo, and Robinson (2015) carefully review this empirical literature, where results vary as widely as the methods employed and conclude that there is no clear evidence that inequality decreases following democratization.³ Employing fixed effects dynamic panel regression models, Acemoglu et al. (2015) go on to show that there is no robust statistically significant relation between switches to democracy and inequality levels. Such null results have led researchers to re-consider the extent to which the conflicts that drive democratization are distributive in nature (see, for example, Bidner et al. 2014 or Dorsch and Maarek 2015.)

However, the Acemoglu et al. (2015) study does not fully address the fact that autocracies are heterogeneous, a point made forcefully by Jones and Olken (2005), who demonstrate that economic performances of autocratic countries are highly leader-specific (see also De Long and Shleifer 1993 and Reynolds 1985). Just as not all autocracies have histories of sclerotic growth, not all autocracies feature extreme income inequality. Figure 1 provides histograms of the net (after tax and transfer) Gini coefficient for autocracies and for democracies. Note that the tails of the distribution among autocracies are thicker, supporting the notion that autocratic countries govern relatively extreme income distributions. Table 1 provides some statistics concerning the distribution of net Gini coefficients across different per capita income ranges for autocratic and democratic countries. Note that the diversity among autocratic countries does not depend on the overall level of economic development.

³From case studies on 19th century Europe and 20th century Latin America (Acemoglu and Robinson, 2001), to cross sectional regressions (Gradstein and Milanovic, 2004; Mulligan et al., 2004; Perotti, 1996; Sirowy and Inkeles, 1990), to event histories (Aidt and Jensen, 2009), to sophisticated dynamic panel regressions (Acemoglu et al., 2015), the empirical literature has not established a convincing link between democratization and income inequality. For papers that investigate other proxies for inequality and/or redistribution, see also Rodrik (1999) Lindert (1994, 2004), Scheve and Stasavage (2009). See Lee (2005) for a study that highlights the importance of state capacity for the ability of new democracies to redistribute income.

Autocratic countries are heterogeneous according to their income distributions for a variety of reasons. Historical differences in settler identities, institutional foundations, and types of agricultural cultivation shape differential inequality trajectories across autocracies, where structural inequalities may have been inherited from the past. Some autocratic countries are competently managed and have established good institutions that allow for equitable development, whereas others have not. Despite not having to stand for elections, autocratic regimes must cultivate political support from segments of the population in order to survive. Whether acting as a representative of the elite or of the downtrodden, autocratic rulers take ideological stances and redistributive policies to please their political “coalition” members that vary widely. As a result, also for political reasons, autocratic countries are heterogeneous according to income distributions.

We follow the intuition established by Larsson-Seim and Parente (2013), who describe democracy as a middle ground on which formerly autocratic countries converge in terms of institutions and economic performances. We apply this intuition to modeling the dynamics of income inequality following a democratic switch. Extreme distributional outcomes that were politically sustainable under autocracy are unlikely to last once a switch to democracy occurs. Highly unequal autocracies are likely to see inequality reduced after democratization, when political institutions become more inclusive to the poorer segment of the population, which should pressure for more redistribution and pro-poor policies. On the contrary, highly equal autocracies are not likely to see inequality decrease after democratization since inequality was not a concern in those countries. In autocratic regimes that rely on a poor segment of the population for political support, unwinding a legacy of populist policies upon democratic liberalization creates opportunities for wealth creation that increases inequality levels.

Our basic point is that without taking into account how the effect is conditional on initial (pre-democracy) income levels, the contrasting experiences of switches to democracy in high and low inequality autocratic countries will cancel each other out, yielding the familiar null result, as in Acemoglu et al. (2015), for example.

We follow an empirical strategy that is broadly similar to Acemoglu et al. (2015): we employ fixed effects dynamic panel regression models to estimate the effect of switches to democracy as measured by an indicator that is constructed from three leading quantitative measures of democracy (following the example of Papaioannou and Siourounis 2008a). Our contribution beyond their study is two-fold. First, using the simple observation that autocratic countries are quite heterogeneous, we demonstrate that the

impact of democratic switches *conditional on initial levels of inequality* is a robustly statistically significant determinant of income inequality dynamics.⁴ We demonstrate that, on average, relatively egalitarian autocracies become more unequal following democratization, whereas democratization has an equalizing effect in the relatively unequal autocracies. Our finding that the effect of democracy on inequality is conditional on initial inequality levels rationalizes the mixed results in the literature, where the relationship has typically been estimated unconditionally. As a result, contrary to prior views, democracy actually strongly affects the degree of inequalities. Second, we pursue an instrumental variable strategy for democratic switches that allows us to argue that the effect can be interpreted causally. Acemoglu, Naidu, Restrepo, and Robinson (2014) calculate, roughly speaking, the dynamic regional share of countries that are democratic as an instrument for democracy in their study that estimates how democratization affects growth. We construct a similar “democratic wave” instrument for our Two Stage Least Squares [2SLS] analysis, which confirms the importance of investigating the impact of democratization conditional on initial levels of inequality. Interacting the regional share democracy instrument with pre-democracy inequality levels gives us a strong and arguably exogenous set of instruments and we show that the instrumented conditional effect of a democratic switch is quite similar in magnitude to that from the simple OLS estimations. Ours is the first study to investigate the effect of democracy on inequality using a valid instrument for democratic change. We pursue a wide range of alternative specifications to demonstrate the robustness of our results. Among those exercises, we present results from 2SLS estimations that also instrument for initial (pre-democracy) inequality levels.

We then reflect on the possible mechanisms. Democratic switches occur for a multitude of reasons. When highly unequal, elite-dominated autocracies become democratic and political power is shifted to the middle, inequality gets reduced through redistribution and pro-poor policies (in line with Meltzer and Richard 1981; Roberts 1977; Romer 1975). However, more recent theoretical work in economics has suggested that the democratization process may not be about redistribution at all (Bidner et al., 2014; Dorsch and Maarek, 2015), and there is some empirical evidence to that effect (Aidt and Jensen, 2009; Haggard and Kaufman, 2012; Knutsen and Wegmann, 2016). Indeed, for formerly communist or collectivist autocracies, democratization was accompanied by market liberalizations and greater economic competition that may have increased inequalities from low initial levels. Autocratic societies are highly heterogeneous and

⁴Similar to the empirical literature on macro-economic convergence, we demonstrate how the dynamics of inequality following a switch to democracy depends on the initial (pre-democracy) level of income inequality.

regression analyses that do not take this into account are ignoring important non-linearities in the effect of democracy on income inequality.

The paper proceeds as follows. In the next section we describe the variables of interest and the data used for the analysis. The third section provides the details of our empirical strategy and results. In the fourth section, we discuss some mechanisms that may be behind our findings, while the final section concludes briefly.

2 Data

To investigate the extent to which democratization decreases (or increases) inequality levels, we gathered data from a variety of sources and constructed a country-level panel from 1960 – 2010. We begin by analyzing the yearly panels, but later in the paper we also analyze five-year panels.

Democratic political institution indicator. We construct binary indicators for the political system that follow Papaioannou and Siourounis (2008a) and later Acemoglu et al. (2015, 2014), in that we use several governance indicators to code our binary democracy variable. We combine the composite Polity2 index of the Polity IV dataset (Marshall et al., 2010) with the political freedom and civil liberties indexes of Freedom House (2013).⁵ Specifically, we consider a state as democratic when Freedom House codes it as “Free” or “Partially Free” *and* the Polity 2 index is positive. When one of those two criteria is not satisfied, the state is considered as autocratic. When one of the two criteria is satisfied but the other one is missing, we verify if the country is also coded as democratic by the binary indicator developed by Cheibub et al. (2010).⁶ Combining these three leading indicators allows us to address the issue of measurement error that the democracy indices may suffer from individually. The democracy indicator $[D(0, 1)_{i,t}]$ takes value zero if country i is determined to be autocratic in period t and

⁵The Polity index codes the quality of democratic institutions by observation of, among other things, the competitiveness of political participation, the openness and competitiveness of choosing executives, and the constraints on the chief executive. The composite Polity index ranges from -10 to 10, where -10 represents a fully autocratic political system and 10 represents a fully competitive democratic political institution. The Freedom House data measures political rights and civil liberties, both measured on a scale of 1 (most free) to 7 (least free). Political rights include free participation in the political process, including the right to vote for distinct alternatives in political elections, complete for public office, join parties or other political organizations, and elect representatives who actually have an impact on policy choices. Civil liberties include freedom of expression and belief, the right to join associations or organizations, protection under the rule of law, and personal autonomy from the state.

⁶We have directly followed the methodology developed by Papaioannou and Siourounis (2008a) and later applied by Acemoglu et al. (2014). See those papers for a more detailed description of the methodology.

it takes value one if country i is determined to be democratic in period t .⁷ Our results are robust to different thresholds for the indices we use and to more simple criteria for considering a country as democratic.

Both the political science and the economics literatures point to the possibility that democratization may be endogenously determined in this relationship, however. The multitude of papers that use variation in lagged income inequality to explain democratic transitions (though without consistent results), alerts us to the possibility that trends in inequality may be sufficiently persistent that even future inequality dynamics are influencing contemporaneous transitions to democracy.⁸ As such, we also pursue an instrumental variable strategy that isolates variation in our democracy indicator that is arguably exogenous to the dynamics of national income distributions. We follow the strategy of Acemoglu et al. (2014) and employ an instrument that relies on the observation that political transitions have historically occurred in regional “waves”⁹ by calculating the evolution of the fraction of countries with democratic institutions in a region among countries that shared the same political institutions at the beginning of the period.

Beyond addressing the possible reverse causality bias caused by any simultaneous determination, employing an instrument for democratization seems prudent for the following reasons. First, it allows us to deal with any time-varying omitted variables for which our baseline fixed-effects dynamic panel cannot fully control. Second, despite the fact that our democracy indicator is composed of several indicators, measurement error on marginal country-year cases remains a serious concern. To the extent that it is a strong first-stage predictor of democratization events, our instrument based on dynamic regional share of democracy smooths out the estimated impact of erroneously coded transitions.

⁷Note that we code both permanent and transitory transitions to democracy, and reversals to non-democracy. Nothing indicates that the dynamics of inequality should be different in a democracy that eventually reverses to autocracy and democracy fully consolidated. Our measure of democracy captures a bundle of institutions that characterize electoral democracies. The indexes we use to construct our democracy variable include free and competitive elections, checks on executive power, and an inclusive political process that permits various groups of society to be represented politically. Our measure of democracy also incorporates the expansion of civil rights through the Freedom House’s index. Acemoglu et al. (2014) show that these institutional components are quite strongly correlated.

⁸See, for example, Acemoglu and Robinson (2001, 2006); Acemoglu et al. (2015); Ansell and Samuels (2014); Boix (2003); Dorsch and Maarek (2014a,b); Freeman and Quinn (2012); Gassebner et al. (2013); Gradstein and Milanovic (2004); Haggard and Kaufman (2012); Houle (2009); Papaioannou and Siourounis (2008b).

⁹See Huntington (1993) for the classic exposition. In the modern economics literature, see, for example, Ellis and Fender (2011) and Dorsch and Maarek (2015) for theory and Aidt and Jensen (2014) or Persson and Tabellini (2009) for evidence. Though democracies have not been consolidated following the Arab Spring, the successive political transitions from autocracy also provides credence to the notion.

More formally, we construct the following instrument for democratization events in country i of region r in period t , which we denote by $Z_{i,t}^r$:

$$Z_{i,t}^r = \frac{1}{N_{i,0}^r - 1} \sum_{j \in r, D_{j,0}=D_{i,0}, j \neq i} D_{j,t}$$

where $N_{i,0}^r$ corresponds to the number of countries in the region of country i with the same institution as country i at the beginning of the panel ($D_{j,0} = D_{i,0}$). For a country i we sum the number of countries sharing i 's initial type of political institution ($j \neq i$, $j \in N_{i,0}^r$) in the region r that are democratic at time t ($D_{j,t}$) excluding country i . The idea is to observe the evolution of democratic institutions in the countries in the same region as country i which share the same institution initially. For instance, in a region in which initially 10 countries were autocratic, when considering one of them (country i), we look at the evolution of our democracy indicator in the 9 others in order to explain changes in country i . Intuitively, we expect what happens in the regional countries is not related to the degree of inequality in the domestic country i , except through its influence on domestic political institutions.¹⁰ When a “wave” of democratization reaches a region that was initially autocratic, this increases the probability that country i democratizes. Figure 5 plots the country-specific instrument for six example countries from three different regions.

We have strong theoretical priors that such an instrument would be highly relevant and indeed, we later report some first-stage F-statistics well over 100. Logically, the instrument also seems quite likely to satisfy the exclusion restriction as national income distributions should not necessarily be affected by variation in regional political institutions other than through its effect on domestic political institutions. One limit of our instrument may be the fact that transitions in neighbor countries may affect growth there, which could affect growth in country i if the regional economies are somewhat integrated and affect both inequality and the probability to observe a transition in country i . Growth may, for instance, affect the probability of democratization through the opportunity cost channel à la Acemoglu and Robinson (2001). There exists some empirical evidence for such an effect (see, for instance, Brückner and Ciccone 2011 or Burke and Leigh 2010). Growth may also affect inequality through the hypothesized “Kuznets curve” relation (Kuznets, 1955), though empirical evidence of such a relation is mixed. We thus control for the log of real GDP per capita in every specification of our paper. For the OLS specifications, it is a routine and obvious control. For the

¹⁰We classify countries into the following ten regions: (1.) Eastern Europe and post Soviet Union, (2.) Latin America, (3.) North Africa and Middle East, (4.) Sub-Saharan Africa, (5.) Western Europe and North America, (6.) East Asia, (7.) South-East Asia, (8.) South Asia, (9.) The Pacific, and (10.) The Caribbean.

IV specifications, controlling for economic growth should help to satisfy the exclusion restrictions due to the indirect effect of democratization in neighboring countries on economic growth.

Regional countries may also share some common structural characteristics that may simultaneously affect political institutions and inequality, but all of our regressions include country fixed effects to capture those common features. Once conditioning the effect of the democracy instrument on within-country lags of inequality and economic growth, as well as period and country fixed effects, our set of excluded instruments should not have a direct effect on future period inequality in country i . Indeed, the results presented in the next section consistently fail to reject the null hypothesis that the set of instruments excluded from the second stage regressions are exogenous. Though not strictly accurate, we refer to the instrument for democracy as the “dynamic regional share of democracies” for simplicity.

Income inequality. For the inequality data, our benchmark specifications report results using the most standard measure of income inequality, the Gini coefficient. The Gini coefficient is a normalized measure between 0 and 100, where higher levels indicate a more unequal income distribution. We employ the Standardized World Inequality Indicators Database [SWIID], introduced by Solt (2009). The SWIID combines the Luxembourg Income Study with the World Inequality Indicators Database and standardizes the measurements across the two databases yielding a cross-national panel that is significantly enlarged from the individual databases. The Solt database also reports Gini coefficients for both the net income distribution (after taxes and transfers) and the gross income distribution. Throughout, we report results using both the net and the gross Gini coefficients, as democratization could both affect inequality due to direct redistribution or, more generally, transformation of institutions that may redistribute economic power in the population. As inequality levels may be path dependent and change rather slowly over time, in most specifications we also include lagged dependent variables to take into account the dynamics of inequality that may be independent of democratization events.

We are interested in observing how democratization events affect future inequality levels. We have hypothesized that the level of inequality before democratization will shape the direction of the relationship. In order to capture this conditional effect of democracy on inequality, we add an interaction between our democratization variable and the degree of inequality in the country prior to democratization. We define a fixed pre-democracy inequality variable for these interactions. Note that the level of inequality in the year of the democratic switch may not accurately reflect the level of inequality prevailing in autocracy since, for example, the regime may have made concessions

of inequality using the instrument proposed by Easterly (2007). He finds (and we corroborate) that the abundance of land suitable for growing wheat relative to that suitable for growing sugarcane is strongly negatively correlated with the pre-democracy level of inequality in countries that have transitioned from autocracy to democracy over the period of our sample. The basic idea is that the land endowments suitable for growing commodities featuring economies of scale and the use of slave labor (sugar cane) is historically associated with high inequality. In contrast, commodities grown on family farms (typically wheat) promoted the growth of a large middle class and lower inequality levels.

Income per capita. Finally, in all regressions we have controlled for the lag of logged real GDP per capita, as measured by the Penn World Table (Heston et al., 2012). It is important to control for per capita income levels for two principle reasons. First, we take Lipset’s Modernization Theory (Lipset, 1959) and the Kuznetz curve (Kuznets, 1955) seriously, so omitting per capita income would bias estimates of the effect of democracy, since both the likelihood of democracy and the evolution of income inequality may depend on economic development levels. Second, as mentioned above, controlling for per capita income makes us more confident that the democracy instrument satisfies the exclusion restriction. Summary statistics of all the variables used in the analysis are presented in table 2.

3 Panel regression results

This section presents the results of a series of panel regression models that highlight how the effect of democratization on inequality depends on initial (pre-democratization) levels of inequality. In our table of baseline results, we first present results from regressions where democratization and initial inequality are not interacted and then present a series of regressions that highlight how the effect of democratization significantly interacts with initial inequality levels. The tables then go on to present analogous results using our external instruments for democratization. We have run all of the regressions on annual panels as well as on five-year panels. First, using the annual panels, we present our baseline tables that use as dependent variable the net Gini coefficient (table 3) and the gross Gini coefficient (table 4). Table 5 presents results that use simplified methods for calculating the initial (pre-democracy) inequality levels, table 6 considers several intuitive alternative samples, table 7 provides a series of 2SLS estimations that also instrument for the initial inequality level, and table 8 considers alternative democracy indicators. We then present the analogues of tables 3 – 7 using five year panels in

tables 9 – 13. Finally, table 14 provides results using GMM estimators for both the annual and the five-year panels. Though it is common in the empirical literature on political institutions to consider five-year panels, we prefer to focus attention on the annual panels due to the fact that our preferred specifications include lagged dependent variables and the so-called “Nickel bias” in dynamic panel regressions with fixed effects is less of a concern when there are many time periods in the panel (Nickel, 1981). All specifications control for the lag of logged per capita real national income, country fixed effects, and period fixed effects. In all tables, we report standard errors that have been clustered at the country level.

3.1 Baseline regression analysis

The first column of table 3 tests the extent to which democratization can explain within-country variation in inequality levels. Using ordinary least squares [OLS], we estimate:

$$Gini_{i,t} = \rho Gini_{i,t-1} + \alpha D(0,1)_{i,t-1} + \beta GDP_{i,t-1} + \gamma_i + \delta_t + u_{i,t}, \quad (1)$$

where $D(0,1)_{i,t} = 1$ is the indicator for democracy that was described above, the γ_i ’s denote a full set of country dummies that capture any time-invariant country characteristics that affect inequality levels, and the δ_t ’s denote a full set of period dummies that capture common shocks to inequality levels. The error term $u_{i,t}$ captures all other factors not correlated with our controls which may also explain democratic improvements, with $E(u_{i,t}) = 0$ for all i and t . In general, in our estimations, the autoregressive effect is quite strong, suggesting that democratization takes time in order to produce sizable impacts on inequality. Thus, it is important that a dynamic estimator is employed. The second column allows for a stronger auto-regressive component to the estimated inequality dynamics by including four lagged dependent variables. The first two columns of table 3 demonstrate that the unconditional effect of lagged democratizations does not explain inequality levels with statistical significance. We also calculate the long-run effect on inequality levels of a switch to democracy as

$$\frac{\hat{\alpha}}{1 - \sum_{j=1}^L \hat{\rho}_{t-j}}, \quad (2)$$

where L represents the number of lags on the dependent variable included in the specification.

The third and fourth columns of table 3 test the extent to which the effect of democratization is conditional on initial inequality levels using an interaction term

between the democracy indicator and initial inequality levels. Formally, we estimate:

$$\begin{aligned} Gini_{i,t} = & \rho Gini_{i,t-1} + \alpha_1 D(0,1)_{i,t-1} + \alpha_2 D(0,1)_{i,t-1} \times \overline{Gini}_i \\ & + \beta GDP_{i,t-1} + \gamma_i + \delta_t + u_{i,t}. \end{aligned} \quad (3)$$

Allowing for a conditional effect yields statistically significant estimates for the effect of democratization on inequality levels. For low initial levels of inequality a switch to democracy increases inequality, whereas for high initial levels of inequality democratization decreases inequality. When presenting estimation results that include the interaction term, we also report the p-value from an F-test of joint significance on the coefficients α_1 and α_2 . Here as well, we calculate the long-run effect of a switch to democracy on inequality. But, note that the marginal effect of democratization when we include the interaction term is given by $\alpha_1 + \alpha_2 \times \overline{Gini}_i$. For concreteness, we calculate the long-run effect at the 10th and 90th percentile inequality level (among autocratic countries, $\overline{Gini}^{10} = 27.5$ and $\overline{Gini}^{90} = 57$) as

$$\frac{\hat{\alpha}_1 + \hat{\alpha}_2 \overline{Gini}^{pc}}{1 - \sum_{j=1}^L \hat{\rho}_{t-j}}, \quad (4)$$

where again L indicates the number of lagged dependent variables we include in the specification. The regression estimates from column 3 imply that the long-run impact of a switch to democracy for a country in the 10th percentile of inequality is for the net Gini coefficient to *increase* by nearly 4 points. By contrast, the long-run impact for a country in the 90th percentile of inequality is for the Gini coefficient to *decrease* by more than 6 points. This simple estimation demonstrates how transitions to democracy, on average, bring extreme income distributions to some “middle ground”.

Figure 3 provides a visualization of the conditional marginal effect estimated in column 3. The plotted line shows the marginal effect of a switch from $D_{i,t-2} = 0$ to $D_{i,t-1} = 1$ on inequality levels in period t as a function of pre-democracy inequality levels. The plot is super-imposed over a histogram of the distribution of net Gini coefficients to provide a sense of the empirical relevance of the range of initial inequality levels for which the effect of a switch to democracy is statistically significant.

The next four columns of table 3 present results from a two-stage instrumental variable procedure (2SLS). We consider both the democracy indicator and its interaction term as potentially endogenous and instrument for both of them. Thus, the first stage equations we estimate are:

$$\begin{aligned} D(0,1)_{i,t} = & \alpha_1 Z_{i,t} + \alpha_2 Z_{i,t} \times \overline{Gini}_i + \beta GDP_{i,t-1} + \gamma_i + \delta_t + e_{i,t} \text{ and} \\ D(0,1)_{i,t} \times \overline{Gini}_i = & \alpha_1 Z_{i,t} + \alpha_2 Z_{i,t} \times \overline{Gini}_i + \beta GDP_{i,t-1} + \gamma_i + \delta_t + e_{i,t}. \end{aligned} \quad (5)$$

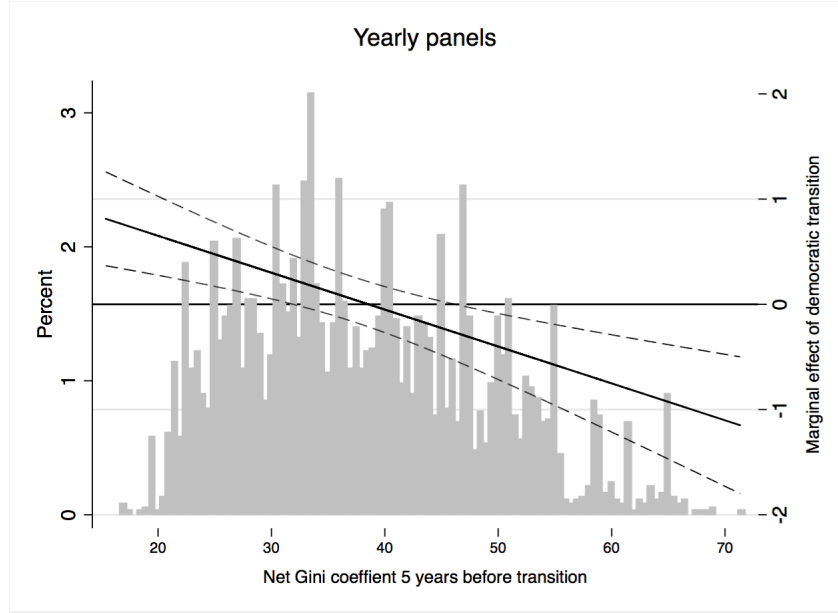


Figure 3: The marginal effect of a democratic transition on net Gini coefficients, conditional on the initial (pre-democracy) level of inequality. The figure is based on regression estimates from column (3) of table 3. Dashed lines represent 90% confidence intervals.

We use the fitted values from equations (5) in the second stage:

$$Gini_{i,t} = \rho Gini_{i,t-1} + \alpha_1^{2S} \widehat{D(0,1)_{i,t-1}} + \alpha_2^{2S} \widehat{D(0,1)_{i,t-1}} \times \overline{Gini_i} + \beta GDP_{i,t-1} + \gamma_i + \delta_t + u_{i,t}. \quad (6)$$

Columns 5 and 6 include a single lagged dependent variable, while columns 7 and 8 include four. To save space, we present only the second stage results (though we report first-stage F-statistics as justification for the strength of the instruments). Columns 5 and 7 are exactly identified (the number of excluded instruments is the same as the number of endogenous variables). The specifications in columns 6 and 8 are over-identified, allowing us to report the Hansen p-values that test whether the set of excluded instruments can be considered exogenous. As an extra excluded instrument we also use the second lag of the share of a country's region that is democratically governed. In the next sub-section where we investigate the robustness of the baseline results, we also instrument for the pre-democracy Gini coefficient in the interaction term.

To conserve space, we do not report the unconditional effect of a switch to democracy (as in columns 1 and 2), but note that it is also insignificant when we use an instrument for democracy. However, conditional on initial levels of inequality, the effect is highly statistically significant (columns 5 – 8). First-stage F-statistics indicate that the set of instruments is strong (well above rule of thumb 10). Recalling that the

null hypothesis of the Hansen J-test is that the set of excluded instruments are exogenous, the p-values from the over-identified regressions (in column 6 and 8) confirm the validity of the set of instruments along this dimension. We also calculated the implied long-run impact of a switch to democracy and report similarly that democratization, on average, brings extreme income distributions towards a “middle ground.” The estimates from column 5, for example, imply that a switch to democracy for an autocracy with an initial inequality level at the 10th (90th) percentile leads to a long-run increase by more than 6 points (decrease by nearly 7 points) of the Gini coefficient. Such movements correspond to a greater than 50% reduction in the gap between the 90th and 10th percentile inequality levels for countries that have switched to democracy.

The 2SLS estimates are quite close to the simple OLS estimates. The 2SLS estimates imply a larger increase in inequality for perviously egalitarian autocracies (when $\overline{Gini} = 0$) that decreases more rapidly as \overline{Gini} increases. In other words, for both low and high initial levels of inequality, OLS slightly underestimates the impact of a switch to democracy. Such an underestimate would be consistent with endogeneity concerns centered around the notion that autocrats might adjust their policies to try to prevent a democratization – redistribute in elite-dominated autocracies or liberalize some markets in collectivist autocracies.

3.2 Robustness analysis

This subsection briefly presents the various robustness checks that we have conducted. The results are generally robust to estimation with the alternative specifications that we describe below.

Market income inequality. In table 4, we use the gross Gini coefficient, rather than the net Gini coefficient, as reported in the previous table. The coefficient estimates are similar, though the calculated long-run effects have some interesting differences in the specifications with multiple lagged dependent variables. At the 90th percentile Gini, the calculated long-run decrease is larger for market inequality than for net inequality. At the 10th percentile Gini, the calculated long-run increase is larger for the net inequality than for the market inequality. This may indicate that the effect of democratization occurs through different channels. Evolving market opportunities following democratization, for example, may affect income distributions beyond the effect of the change in redistributive policies that the literature typically focuses upon.

Alternative pre-democracy inequality coding. In table 5, for transparency, we employ simpler constructions of the interaction term. In columns 1 – 4, the pre-democracy inequality variable is simply the level of inequality during the year of democratization, which we keep fixed for periods following the democratization. In columns

5 – 8, we simply interact the democracy indicator with the raw Gini data, allowing it to change during the period of the democratic switch. Results are robust to these simplified coding schemes.

Restricted sample. Table 6 considers several intuitive sub-samples. First, columns 1 – 3 drop countries that were officially part of the former Soviet Union. Columns 4 – 6 additionally drops the Central and Eastern European countries that were signatories of the Warsaw Pact.¹¹ That the results are generally quite similar after dropping these groups of countries is quite encouraging. The non-linearity is not being driven by a particular group of countries, but the pattern appears to be more general. Finally, columns 7 – 9 of table 6 drops countries that have never been autocratic over the length of the panel.

Instrumenting for pre-democracy inequality as well. In table 7 we also consider the possibility that the initial (pre-democracy) levels of inequality are endogenously determined. For example, even when using the most stringent coding for the initial level of inequality (five years before the switch to democracy), it’s possible that autocrats use redistribution in attempt to prevent a democratic transition. Such an endogenous relationship would mean that our baseline results have over-estimated the conditional effect of democratic switches (even if we should partly prevent this using lagged values of inequalities much before transition occurs as our initial degree of inequality variable). Using the historical agricultural instrument proposed by Easterly (2007) for inequality, we can quite confidently eliminate the possible bias caused by such a strategic redistribution. Recalling that the instrument is the ratio of land suitable for growing wheat relative to sugarcane, Ag_ratio_i is strongly negatively correlated with \overline{Gini}_i (initial pre-democracy inequality) in our sample. We interact the dynamic share democracy instrument with the Ag_ratio to instrument the interaction term of interest. The first stage regressions are the following:

$$\begin{aligned} D(0,1)_{i,t} &= \alpha_1 Z_{i,t} + \alpha_2 Z_{i,t} \times Ag_ratio_i + \beta GDP_{i,t-1} + \gamma_i + \delta_t + e_{i,t} \text{ and} \\ D(0,1)_{i,t} \times \overline{Gini}_i &= \alpha_1 Z_{i,t} + \alpha_2 Z_{i,t} \times Ag_ratio_i + \beta GDP_{i,t-1} + \gamma_i + \delta_t + e_{i,t}. \end{aligned} \quad (7)$$

We use the fitted values from equations (8) in the second stage as before.

While the historical nature of the inequality instrument ensures that it satisfies the exclusion restriction, it comes at the cost of it’s relevance for explaining modern

¹¹While we do not have data for all of these countries, modern countries that were formerly part of the Soviet Union include Russia, Ukraine, Uzbekistan, Kazakhstan, Belarus, Azerbaijan, Georgia, Tajikistan, Moldova, Kyrgyzstan, Lithuania, Turkmenistan, Armenia, Latvia, and Estonia. The original signatories to the Warsaw Treaty Organization were the Soviet Union, Albania, Poland, Czechoslovakia, Hungary, Bulgaria, Romania, and the German Democratic Republic.

income inequality (especially net). Nevertheless, the results in table 7 are encouraging. In columns 1 and 2 *Ag_ratio* is used to instrument for the interaction term with our baseline coding for the initial inequality level, whereas in columns 3 and 4 it's used to instrument for the interaction with the simple coding. Unsurprisingly, the second stage estimate for the interaction effect is less precisely estimated using *Ag_ratio* as an instrument for \overline{Gini} , but we note that the joint effect remains statistically significant. In columns 5 – 8, we pursue a more “reduced form” specification in which we interact the *Ag_ratio* instrument directly with the democratic transition variable (as the potentially endogenous variable) and then instrument for it with the interaction of the regional share democracy with the *Ag_ratio*. More formally, the first stage regressions that we estimate for the second stage results shown in columns 5 – 8 are the following:

$$\begin{aligned} D(0,1)_{i,t} &= \alpha_1 Z_{i,t} + \alpha_2 Z_{i,t} \times Ag_ratio_i + \beta GDP_{i,t-1} + \gamma_i + \delta_t + e_{i,t} \\ D(0,1)_{i,t} \times Ag_ratio_i &= \alpha_1 Z_{i,t} + \alpha_2 Z_{i,t} \times Ag_ratio_i + \beta GDP_{i,t-1} + \gamma_i + \delta_t + e_{i,t}, \end{aligned} \tag{8}$$

Columns 5 and 6 show the second stage results when the net Gini is the dependent variable and columns 7 and 8 are the analogues for when we use the gross Gini coefficient as the dependent variable.

Alternative democracy indicator coding. In table 8, we consider several alternative coding specifications for the democracy indicator. In columns 1 – 4, we continue to utilize the method of Papaioannou and Siourounis (2008a) and combine three different sources of information concerning the quality of democratic political institutions, but we employ more stringent criteria for coding a country-year observation as democratic. In the more stringent version used here, a country-year observation must achieve a Polity IV score of greater than 4 to be considered democratic. In the event that the Polity IV data is not available, the more stringent criteria does not come into effect. In columns 5 – 8, we employ a democracy indicator that uses only the Polity IV data. As is common, the indicator based only on the Polity IV defines a country-year observation as a democracy for positive values and as a non-democracy for non-positive values of the Polity IV index. Results are robust to both of these alternative codings for the democracy indicator.

3.3 Regression analysis using five-year panels

Tables 9 – 13 are the analogues to tables 3 – 7 using five-year panels. Starting from 1960, we take the variables' values in the first year of each five-year panel. Variables that are lagged one period are thus lagged five years. Results are qualitatively consistent with those from the estimations using annual data, but estimates are somewhat

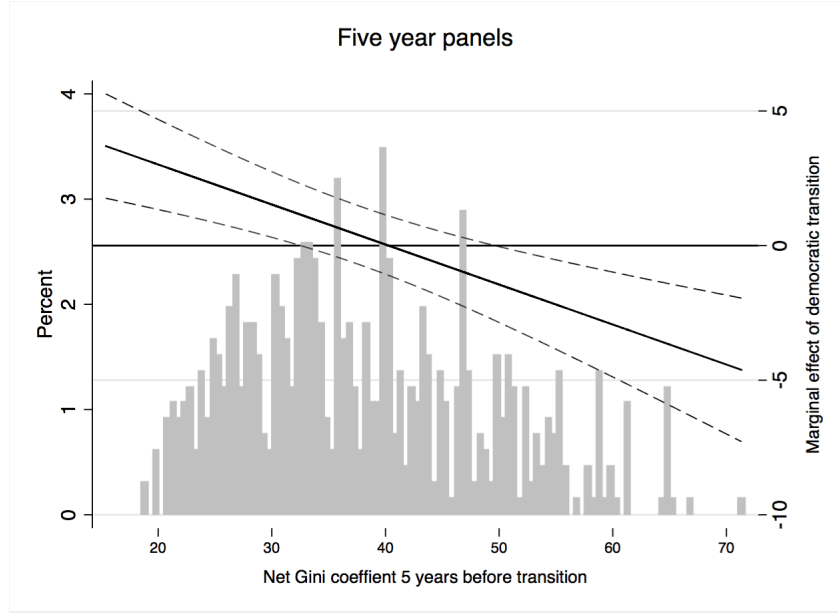


Figure 4: The marginal effect of a democratic transition on net Gini coefficients, conditional on the initial (pre-democracy) level of inequality. The figure is based on regression estimates from column (4) of table 9. Dashed lines represent 90% confidence intervals.

more volatile across the various specifications. Obviously, we are left with far fewer observations when using the five-year panels and the panel length for each country is shortened significantly. Nevertheless, the main themes from the annual panels hold up: (i) there is no statistically significant unconditional effect of democratic switches, (ii) the effect of democratic switches conditional on initial inequality levels is highly statistically significant (see figure 4), (iii) after a democratic switch inequality increases in countries that were initially egalitarian and inequality decreases in countries that were initially unequal, (iv) our proposed set of instruments is both relevant and exogenous and 2SLS regressions corroborate the OLS findings (though the OLS bias is larger when using the five-year panels), (v) the results are robust to using market data, to simpler constructions of pre-democracy inequality levels, and to intuitive sample restrictions, and (vi) results from a 2SLS procedure that also instruments for the initial level of inequality using the Easterly instrument are consistent with those from the baseline specification.

3.4 GMM regressions

Finally, since fixed effects regression estimates can be biased by the inclusion of lagged dependent variables (Nickel, 1981), in table 14 we consider estimation of our baseline specification with a standard generalized method of moments (GMM) estimator along

the lines of Arellano and Bond (1991), rather than with fixed effects OLS regressions. The table presents our baseline specification using both annual and five year panels, using both net and gross income inequality measures, estimated using the Arellano-Bond GMM estimator. Following Acemoglu et al. (2015), we include the lagged dependent variable as a control, remove country fixed effects by taking forward orthogonal differences, collapse the number of lags used as instruments, and report the AR(2) p-value. Our main result is robust to this alternative estimation technique.

4 Discussion

4.1 Possible mechanisms

In this section, we briefly discuss several possible mechanisms that could be driving the conditional income inequality dynamics that we have empirically documented. The first relates to the economic institutional change that accompanies political transitions to democracy, which could plausibly contribute to increases or decreases in inequality levels. The second relates to the shift in political representation towards the preferences of the “median voter”. Third, we note that democratization is often associated with structural economic transitions out of agricultural production that may increase inequality levels. Finally, we observe that inequality may have very historically deep roots that autocratic governments may not attempt to reverse. On net, the mechanisms through which democratization may affect the distribution of income do not operate uniformly, and indicate why unconditional tests of the effect have usually turned up null results. Empirically teasing out which mechanism is the most relevant and has the strongest impact on our middle ground result is beyond the scope of the present paper, however.

First of all, transitions to democratic political institutions involve institutional changes that transcend the political, including those that govern economic, educational, and public health systems. Generally speaking, reforms to these areas of public life could have either equalizing or polarizing effects on income distributions. First, liberalizing political institutions often leads to liberalizing economic reforms that make economies more competitive through, for example, the removal of barriers to entry (De Haan and Sturm, 2003; Djankov et al., 2002; Fidrmuc, 2003; Méon and Sekkat, 2016; Rode and Gwartney, 2012). Increased entrepreneurial opportunities may allow for some new high incomes to be created, increasing inequality. Indeed, Dorsch and Maarek

(2015) provide a theory of political transitions in which removal of such opportunity-suppressing barriers to entry may be the institutional grievance that prompts a democratic concession by the elite.¹² Additionally, democracies tend to invest more in the productive capacity of their populations through greater provision of public goods as well as increased and more broadly spread education and health care (Acemoglu et al., 2005, 2015). The net effect of greater public investment on inequality levels could go either way.

Second, upon democratization, leaders must establish broader coalitions of political support than those necessary to maintain power in an autocracy (Bueno de Mesquita et al., 2003). Many autocrats establish small coalitions of intense support in the extremes of the income distribution, with populist regimes courting the poor and elitist regimes courting the rich (Acemoglu et al., 2004). Which subgroups a leader forms coalitions with may depend on many factors, such as the cost of revolt and/or suppression, geography and the quality of infrastructure, the country’s military tradition, etc. Autocratic leaders cultivate political coalitions according to the relative power of the subgroups in society and form extreme policies to benefit their coalition of support. Extreme policy positions yield (or protect) extreme distributional outcomes, from highly equal communistic regimes to highly unequal elite-dominated regimes. Democratic governance is more inclusive and political leaders must establish broader coalitions which naturally expand to the middle. Thus, in practice, the critical member of the minimum winning coalition in a democracy is likely to be more central in the distribution of policy preferences than previously. If policy preferences are monotonic in income level, the center of the policy preference distribution corresponds to the center of the income distribution. As such, for both highly unequal and egalitarian autocracies, the distributional policy is likely to move to a middle ground following democratization, reducing inequalities in the elite-dominated societies and increasing inequality in the communistic societies.

Third, democratization may be associated with structural economic changes. For agricultural societies, democratization may induce industrialization and urbanization. As worker productivity and wages increase as the economy industrializes, many former peasants are brought out of poverty. As industrial production has more unequal incomes than agricultural production, a structural shift towards industrial production should lead to higher income inequality. This is the main argument Kuznets (1955) uses to explain why development affects economic inequality. In autocracies, independent of the level of economic development (which induces structural transformation and for

¹²Though they do not explicitly derive the impact of democratization on income inequality, in their model, democratization that removes barriers to entry would increase the level of inequality in society.

which we control in the regressions), the elite may block the introduction of new modern technologies of production (which would induce structural change if implemented) in order to keep rents in sectors they operate. In line with such theories, Acemoglu et al. (2014) show that democratization fosters structural change through the development of industry. This effect (and the subsequent impact on income inequalities) could be strong in countries in which the elite dominated some sectors.

Finally, politics in autocracies may have had nothing to do with the level of income inequality and don't pursue policies to modify them. Inequality may simply have deep historical roots (Easterly, 2007) and following a transition to democracy, politicians will engage in redistribution and specific policies for reducing inequalities only in countries in which inequalities are high and there exists some margin to redistribute.

4.2 Comparison with related literature

The paper is most closely related to Acemoglu et al. (2015), who also investigate the effect of democracy on levels of inequality. The literature review found there convincingly documents that there is no empirical consensus concerning the effect of democracy on inequality levels. Using fixed effects panel regression techniques, Acemoglu et al. (2015) find mainly null results in tests of the unconditional correlation between democracy and inequality.

Acemoglu et al. (2015), however, include some specifications which allow for democracy to have heterogeneous effects according to land inequality, share of agriculture in the economy, as well as top and bottom decile income shares. They find evidence that countries where land is more unequally distributed see income inequality increase upon democratization, which they describe as providing evidence for elite capture of new democracies. If land inequality and (pre-democracy) income inequality are inversely related, then this result is consistent with our results.

They also find evidence that highly agricultural economies become more unequal after democracy, which they describe in terms of the increased market opportunities that are available outside of agriculture following a switch to democracy. Again, to the extent that highly agricultural economies are those with low levels of (pre-democracy) inequality, then this is consistent with the results that we have presented.

However, they find no consistent evidence that bottom or top decile income shares shape post-democratization income inequality dynamics. While Acemoglu et al. (2015) do consider several interesting heterogeneous responses to democratic switches, our paper complements their results substantially. The conditional effect that we have investigated is more general and rests on the intuition that democracy provides a

middle ground on which societies with relatively extreme income distributions can converge. Our results clearly indicate that the effect is conditional on initial levels of inequality, and not a more general process of economic development following democratization. Furthermore, we have pursued an instrumental variables strategy and demonstrated that the conditional effect of democracy on income inequality can be interpreted causally.

Even though our paper has considered the effect of democratization on inequality levels, it is also relevant for the literature on the causes of democratization. The canonical rational choice model of democratization (Acemoglu and Robinson, 2001, 2006) supposes that democratically-determined fiscal redistribution follows the logic established by Meltzer and Richard (1981), Roberts (1977), and Romer (1975), where democracies with greater inequality redistribute more. In the democratization model, higher inequalities are associated with a higher probability of a transition to democracy, as the greater potential for democratic fiscal redistribution increases the revolutionary threat.¹³ The empirical literature that examines this relationship has also found largely null results (see, for example, Houle 2009 and Teorell 2010 and the references therein), but the tests are generally on the unconditional effect of inequality. Some scholars have questioned whether inequality and redistribution are actually important for democracy and transitions towards it (Knutsen and Wegmann 2016, for example). Our results show that critiques of the logic established by the canonical rational choice model of democratization based on the available empirical evidence are perhaps not convincing. We have demonstrated that inequality *does* fall following democratization in countries where inequality was very high. Therefore, high inequality levels can be a source of tension that drives democratization, but it's not a general pattern because some autocracies are quite egalitarian with little to redistribute.

5 Concluding remarks

There is no consensus in the empirical literature about whether or not autocracies that democratize become more egalitarian. We propose that the reason for this is that autocracies are highly heterogeneous, especially with respect to how incomes are distributed. Intuitively, autocracies allow for extreme policy outcomes that might not be possible in democratically governed societies, where policy choices should follow more closely the preferences in the middle of the distribution of preferences. Allowing for the effect of democracy to be conditional on pre-democracy inequality levels, we

¹³Technically, the theory predicts an inverted U relationship, as if the redistribution under democratic fiscal policy is anticipated to be too great, then the elite will fight to prevent it.

demonstrate a robustly statistically significant conditional effect of democratization on inequality levels. Highly unequal autocracies become more equal following switches to democracy, whereas egalitarian autocracies become less equal following switches to democracy. In sum, democratization has a strong impact on inequality levels, but the effect pushes in opposite directions depending on prevailing levels of inequality prior to the switch to democracy, which rationalizes the typical null result found in the literature. An instrumental variable analysis suggests that the effect of democracy on income inequality can be interpreted causally.

The objective of our paper was mainly to explain the counter-intuitive null result found in the literature and demonstrate that democratic transitions are not at all neutral events in terms of the dynamics of income inequality. The basic observation with which we began the paper, that autocracies are very heterogeneous according to inequality levels, provides an interesting starting point for further future research. We have briefly discussed some intuition for these heterogeneities and for the converging dynamics that democratization sets off, but a full characterization of those mechanisms is an important research program for the future.

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6 Tables

Table 1: Distribution of Gini coefficients by political institutions, yearly data

	Non-democracies			Democracies		
	Income range	10th p. Gini	90th p. Gini	Income range	10th p. Gini	90th p. Gini
0 - 25th p. income p.c.	min – 1067.55	30.30	59.03	min – 4365.62	33.13	54.02
25th - 50th p. income p.c.	1067.55 – 2046.39	33.45	54.35	4365.62 – 10321.44	32.55	54.85
50th - 75th p. income p.c.	2046.39 – 4890.67	31.07	51.38	10321.44 – 21952.59	24.13	37.83
75th - 100th p. income p.c.	4890.671 – max	30.47	50.55	21952.59 – max	22.90	34.01

Table 2: Summary for baseline sample, yearly data

	Non-democracies			Democracies		
	Mean	Std.Dev.	Obs.	Mean	Std.Dev.	Obs.
Gini coefficient, net income	41.23	9.40	1327	37.20	10.48	2525
Gini coefficient, gross income	44.85	10.20	1276	45.39	7.77	2521
Real GDP per capita, chain series	3890.25	5278.98	1327	14031.77	11814.53	2525
Share of region democracy	0.25	0.25	1327	0.73	0.30	2467
Inequality instrument	0.05	0.14	1327	0.12	0.21	2525

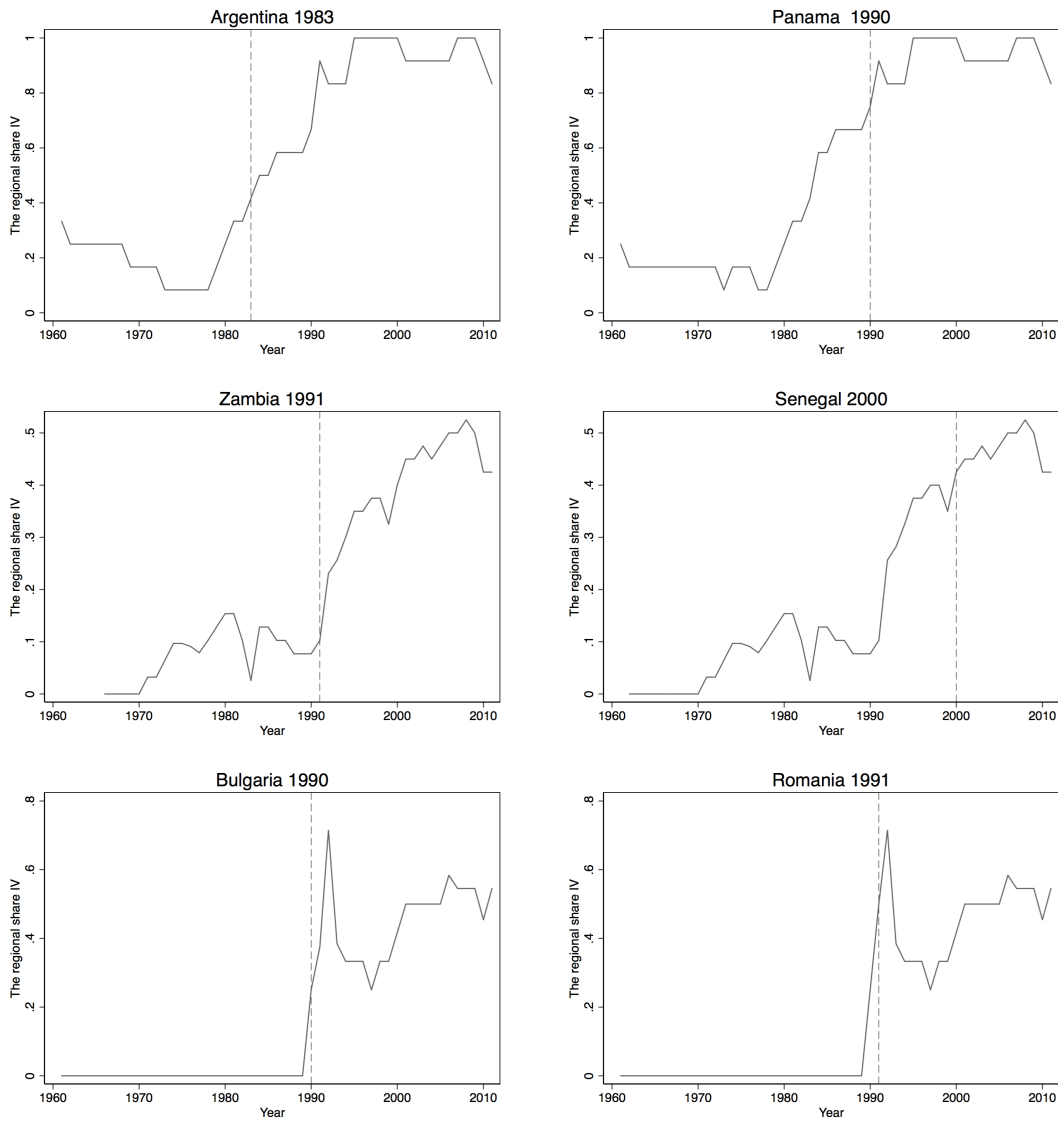


Figure 5: Democratic switches and the regional share instrument.

Table 3: Effects of democracy on the net Gini coefficient, yearly panels

	OLS				Two-Stage Least Squares			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
democracy _{t-1}	-0.0920 (0.153)	-0.0902 (0.112)	1.3525*** (0.425)	1.2665*** (0.287)	1.9331*** (0.572)	1.8223*** (0.552)	1.5974*** (0.478)	1.5334*** (0.458)
democracy _{t-1} × $\overline{\text{gini}}$			-0.0351*** (0.011)	-0.0335*** (0.007)	-0.0462*** (0.014)	-0.0456*** (0.014)	-0.0380*** (0.010)	-0.0380*** (0.010)
gini _{t-1}	0.8905*** (0.009)	1.2536*** (0.044)	0.8947*** (0.009)	1.2547*** (0.043)	0.8956*** (0.008)	0.8956*** (0.008)	1.2570*** (0.043)	1.2568*** (0.043)
gini _{t-2}		-0.1966** (0.079)		-0.1947** (0.078)			-0.1951** (0.078)	-0.1951** (0.078)
gini _{t-3}		-0.2669*** (0.074)		-0.2683*** (0.074)			-0.2696*** (0.074)	-0.2697*** (0.074)
gini _{t-4}		0.0928*** (0.034)		0.0947*** (0.034)			0.0944*** (0.034)	0.0947*** (0.034)
Country & year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
within-R ²	0.8535	0.8851	0.8550	0.8865	—	—	—	—
Joint F-test p-value	—	—	0.0062	0.0000	0.0023	0.0025	0.0010	0.0009
Excluded instruments	—	—	—	—	2	3	2	3
C-D F-stat on excluded instruments	—	—	—	—	214.329	156.814	174.682	126.946
K-P rk F-stat on excluded instruments	—	—	—	—	16.006	11.942	11.449	8.291
Hansen J-test p-value	—	—	—	—	—	0.6049	—	0.3865
Weak-instrument-robust p-value	—	—	—	—	0.0071	0.0233	0.0023	0.0064
N	3788	3253	3788	3253	3726	3708	3203	3203
Countries	154	143	154	143	147	147	141	141
Number of democracy changes	104	76	104	76	104	103	76	76
Long-run effect at 10th percentile Gini	-0.84	-0.77	3.69	3.03	6.34	5.44	4.86	4.31
Long-run effect at 90th percentile Gini	-0.84	-0.77	-6.13	-5.66	-6.73	-7.45	-5.03	-5.57
Years	1962 – 2010	1965 – 2010	1962 – 2010	1965 – 2010	1961 – 2010	1961 – 2010	1964 – 2010	1964 – 2010

Notes: Robust standard errors clustered by country are in parentheses. All specifications control for the log of lagged GDP per capita.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 4: Effects of democracy on the gross Gini coefficient, yearly panels

	OLS				Two-Stage Least Squares			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
democracy _{t-1}	-0.1413 (0.167)	-0.1291 (0.147)	1.6664*** (0.563)	1.5348*** (0.461)	2.1408*** (0.675)	2.0454*** (0.654)	1.4162** (0.590)	1.3418* (0.585)
democracy _{t-1} × $\overline{\text{gini}}$			-0.0402*** (0.013)	-0.0379*** (0.010)	-0.0391** (0.016)	-0.0401** (0.016)	-0.0315** (0.015)	-0.0326* (0.015)
gini _{t-1}	0.9084*** (0.011)	1.2326*** (0.044)	0.9154*** (0.010)	1.2392*** (0.044)	0.9169*** (0.010)	0.9168*** (0.010)	1.2390*** (0.044)	1.2390*** (0.044)
gini _{t-2}		-0.1512** (0.070)		-0.1506** (0.070)			-0.1514** (0.070)	-0.1516** (0.070)
gini _{t-3}		-0.3260*** (0.071)		-0.3279*** (0.071)			-0.3258*** (0.070)	-0.3260*** (0.070)
gini _{t-4}		0.1513*** (0.034)		0.1514*** (0.034)			0.1500*** (0.034)	0.1502*** (0.033)
Country & year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
within-R ²	0.8643	0.8923	0.8655	0.8934	—	—	—	—
Joint F-test p-value	—	—	0.0081	0.0003	0.0064	0.0075	0.0538	0.0625
Excluded instruments	—	—	—	—	2	3	2	3
C-D F-stat on excluded instruments	—	—	—	—	183.570	137.964	136.604	101.817
K-P rk F-stat on excluded instruments	—	—	—	—	14.169	10.851	9.041	7.119
Hansen J-stat p-value	—	—	—	—	—	0.5219	—	0.4007
Weak-instrument-robust p-value	—	—	—	—	0.0071	0.0218	0.0732	0.1312
N	3713	3173	3713	3173	3651	3635	3123	3123
Countries	154	143	154	143	147	147	141	141
Number of democracy changes	102	75	102	75	102	101	75	75
Long-run effect at 10th percentile Gini	-1.54	-1.38	3.61	2.88	9.84	8.29	3.97	2.71
Long-run effect at 90th percentile Gini	-1.54	-1.38	-8.82	-8.38	-2.46	-4.29	-5.38	-6.92
Years	1961 – 2010	1961 – 2010	1961 – 2010	1961 – 2010	1961 – 2010	1961 – 2010	1964 – 2010	1964 – 2010

Notes: Robust standard errors clustered by country are in parentheses. All specifications control for the log of lagged GDP per capita.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 5: Effects of democracy on the net Gini coefficient with simple interactions, yearly panels

	Fixed initial inequality interaction – simple				Once lagged initial inequality interaction			
	OLS		2SLS		OLS		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
democracy _{t-1}	0.8589* (0.440)	1.0012*** (0.348)	1.5858** (0.622)	1.3847** (0.538)	1.7975*** (0.458)	0.9346*** (0.348)	3.5848*** (0.893)	1.1700** (0.581)
democracy _{t-1} × $\overline{\text{gini}}$	-0.0230** (0.011)	-0.0269*** (0.008)	-0.0382** (0.015)	-0.0328*** (0.013)				
democracy _{t-1} × gini _{t-1}					-0.0460*** (0.011)	-0.0252*** (0.008)	-0.0922*** (0.022)	-0.0271* (0.014)
gini _{t-1}	0.8990*** (0.010)	1.2642*** (0.044)	0.9045*** (0.010)	1.2688*** (0.044)	0.9127*** (0.010)	1.2493*** (0.043)	0.9277*** (0.012)	1.2515*** (0.043)
gini _{t-2}		-0.1964** (0.078)		-0.1970** (0.079)		-0.1815** (0.078)		-0.1812** (0.077)
gini _{t-3}		-0.2676*** (0.074)		-0.2689*** (0.074)		-0.2688*** (0.074)		-0.2700*** (0.074)
gini _{t-4}		0.0931*** (0.034)		0.0927*** (0.034)		0.0932*** (0.034)		0.0927*** (0.034)
Country & year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Joint F-test p-value	0.1188	0.0014	0.0339	0.0279	0.0003	0.0035	0.0001	0.1233
within-R ²	0.0398	0.8858	–	–	0.8534	0.8534	–	–
Excluded instruments	–	–	3	3	–	–	3	3
C-D F-stat on excluded instruments	–	–	154.946	125.194	–	–	151.803	127.346
K-P rk F-stat on excluded instruments	–	–	12.558	8.302	–	–	10.896	8.567
Hansen J-test p-value	–	–	0.6337	0.3986	–	–	0.5983	0.4094
N	3788	3253	3708	3203	3602	3253	3545	3203
Countries	154	143	147	141	149	143	145	141
Number of democracy changes	104	76	103	76	94	76	94	76
Long-run effect at 10th percentile Gini	2.23	2.46	5.62	4.61	6.11	2.25	14.50	3.96
Long-run effect at 90th percentile Gini	-4.50	-4.96	-6.16	-4.66	-9.43	-4.64	-23.10	-3.502
Years	1965 – 2010	1965 – 2010	1965 – 2010	1965 – 2010	1970 – 2010	1970 – 2010	1970 – 2010	1970 – 2010

Notes: Robust standard errors clustered by country are in parentheses. All specifications control for the log of lagged GDP per capita.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 6: Effects of democracy on the net Gini, alternative samples, yearly panels

	Excluding former USSR			Excluding former USSR & Warsaw Pact			Excluding countries never autocratic		
	OLS	2SLS		OLS	2SLS		OLS	2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
democracy _{t-1}	1.3745*** (0.431)	1.9469*** (0.570)	1.8626*** (0.555)	1.2818* (0.669)	1.9428** (0.879)	1.8069** (0.842)	1.7054*** (0.466)	2.7210*** (0.632)	2.6614*** (0.612)
democracy _{t-1} × $\overline{\text{gini}}$	-0.0356*** (0.011)	-0.0490*** (0.014)	-0.0488*** (0.014)	-0.0337** (0.016)	-0.0501** (0.019)	-0.0486** (0.019)	-0.0415*** (0.012)	-0.0574*** (0.015)	-0.0578*** (0.015)
gini _{t-1}	0.8963*** (0.009)	0.8973*** (0.009)	0.8967*** (0.009)	0.8954*** (0.010)	0.8984*** (0.010)	0.8972*** (0.010)	0.8880*** (0.009)	0.8865*** (0.009)	0.8860*** (0.009)
Country & year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
within-R ²	0.8593	—	—	0.8553	—	—	0.8636	—	—
Joint F-test p-value from 2nd stage	0.0065	0.0016	0.0016	0.0749	0.0316	0.0303	0.0018	0.0001	0.0001
Excluded instruments	—	2	3	—	2	3	—	2	3
C-D F-stat on excluded instruments	—	209.787	153.323	—	188.342	142.049	—	73.242	54.041
K-P rk F-stat on excluded instruments	—	15.616	11.562	—	12.981	10.067	—	7.703	5.831
Hansen J-stat p-value	—	—	0.4377	—	—	0.5857	—	—	0.4683
Weak-instrument-robust p-value	—	0.0046	0.0122	—	0.0554	0.1186	—	0.0018	0.0053
N	3569	3507	3493	3397	3335	3322	2530	2519	2509
Countries	139	132	132	133	126	126	111	106	106
Number of democracy changes	99	99	98	94	94	93	104	104	103
Long-run effect at 10th percentile Gini	3.81	5.84	5.04	3.38	5.56	4.58	5.04	10.08	9.40
Long-run effect at 90th percentile Gini	-6.30	-8.21	-8.90	-6.12	-8.97	-9.37	-5.87	-4.83	-5.56
Years	1961 – 2010	1961 – 2010	1961 – 2010	1961 – 2010	1961 – 2010	1961 – 2010	1961 – 2010	1961 – 2010	1961 – 2010

Notes: Robust standard errors clustered by country are in parentheses. All specifications control for the log of lagged GDP per capita.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 7: Effects of democracy on the Gini coefficient, instrumenting for initial inequality levels as well, yearly panels

	Two Stage Least Squares							
	Net Gini				Gross Gini			
	Baseline interaction		Simple interaction		Reduced form		Reduced form	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
democracy _{t-1}	11.4462** (4.726)	11.6551** (4.774)	13.2240* (6.844)	13.3585* (6.884)	7.8304** (3.465)	7.9296** (3.463)	23.6874*** (8.304)	23.7034*** (8.188)
democracy _{t-1} × $\overline{\text{gini}}$	-0.2396* (0.139)	-0.2406* (0.140)	-0.2764 (0.187)	-0.2744 (0.187)				
democracy _{t-1} × gini IV					-0.1304 (0.085)	-0.1274 (0.084)	-0.5830*** (0.211)	-0.5719*** (0.207)
Country & year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Joint F-test p-value from 2nd stage	0.0176	0.0138	0.0578	0.0474	0.0508	0.0410	0.0170	0.0151
Excluded instruments	2	3	2	3	2	3	2	3
C-D F-stat on excluded instruments	65.615	44.345	63.404	42.971	166.991	122.458	144.962	107.320
K-P rk F-stat on excluded instruments	4.450	2.903	3.826	2.554	9.619	6.641	8.170	5.689
Hansen J-test p-value	—	0.3747	—	0.3134	—	0.2285	—	0.0830
Weak-instrument-robust p-value	0.0776	0.0547	0.0852	0.0562	0.0852	0.0562	0.0166	0.0282
N	3736	3718	3726	3708	3726	3708	3651	3635
Countries	147	147	147	147	147	147	147	147
Number of democracy changes	106	105	104	103	104	103	102	101
Long-run effect at 10th percentile Gini	4.86	5.04	5.62	5.81	4.24	4.43	7.66	7.98
Long-run effect at 90th percentile Gini	-2.21	-2.06	-2.52	-2.28	0.40	0.67	-9.53	-8.88
Years	1961 – 2010	1961 – 2010	1961 – 2010	1961 – 2010	1961 – 2010	1961 – 2010	1961 – 2010	1961 – 2010

Notes: Robust standard errors clustered by country are in parentheses. All specifications control for the log of lagged GDP per capita.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 8: Effects of democracy on the net Gini coefficient with alternative democracy indicators, yearly panels

	ANNR – more stringent				Polity demo			
	OLS		2SLS		OLS		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
democracy _{t-1}	0.9437** (0.402)	0.9462*** (0.290)	1.8079*** (0.580)	1.4900*** (0.474)	1.7458*** (0.396)	1.3989*** (0.268)	2.0043*** (0.579)	1.6379*** (0.474)
democracy _{t-1} × $\overline{\text{gini}}$	-0.0243** (0.010)	-0.0246*** (0.006)	-0.0449*** (0.014)	-0.0367*** (0.011)	-0.0433*** (0.010)	-0.0353*** (0.006)	-0.0498*** (0.015)	-0.0401*** (0.011)
gini _{t-1}	0.8932*** (0.009)	1.2556*** (0.043)	0.8953*** (0.009)	1.2584*** (0.043)	0.8973*** (0.009)	1.2449*** (0.043)	0.8978*** (0.008)	1.2464*** (0.042)
gini _{t-2}		-0.1957** (0.078)		-0.1963** (0.079)		-0.1903** (0.079)		-0.1904** (0.079)
gini _{t-3}		-0.2685*** (0.074)		-0.2700*** (0.074)		-0.2582*** (0.074)		-0.2597*** (0.075)
gini _{t-4}		0.0939*** (0.034)		0.0942*** (0.033)		0.0932*** (0.034)		0.0937*** (0.034)
Country & year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Joint F-test p-value	0.0398	0.0005	0.0051	0.0025	0.0001	0.0000	0.0018	0.0009
within-R ²	0.8543	0.8859	–	–	0.8578	0.8870	–	–
Excluded instruments	–	–	3	3	–	–	3	3
C-D F-stat on excluded instruments	–	–	149.168	118.386	–	–	130.340	107.440
K-P rk F-stat on excluded instruments	–	–	10.492	7.766	–	–	10.570	7.127
Hansen J-test p-value	–	–	0.5528	0.3765	–	–	0.6315	0.4118
N	3791	3256	3708	3203	3634	3130	3556	3080
Countries	154	143	147	141	145	134	139	132
Number of democracy changes	104	76	103	75	103	75	102	75
Long-run effect at 10th percentile Gini	2.58	2.35	5.48	4.23	5.39	3.88	6.23	4.87
Long-run effect at 90th percentile Gini	-4.14	-3.98	-7.16	-5.28	-7.05	-5.55	-8.13	-5.86
Years	1965 – 2010	1965 – 2010	1965 – 2010	1965 – 2010	1970 – 2010	1970 – 2010	1970 – 2010	1970 – 2010

Notes: Robust standard errors clustered by country are in parentheses. All specifications control for the log of lagged GDP per capita.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 9: Effects of democracy on the net Gini coefficient, 5-year panels

	OLS			Two-Stage Least Squares				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
democracy _{t-1}	0.6091 (0.735)	0.0283 (0.724)	4.5702** (2.199)	5.9845*** (1.773)	2.3348 (1.709)	8.5507*** (3.281)	1.6781 (1.600)	8.5776*** (3.019)
democracy _{t-1} × $\overline{\text{gini}}$			-0.1016* (0.053)	-0.1486*** (0.044)		-0.1654*** (0.062)		-0.1928*** (0.061)
gini _{t-1}		0.3610*** (0.067)		0.3819*** (0.064)	0.3473*** (0.066)	0.3723*** (0.063)	0.3593*** (0.071)	0.3801*** (0.067)
Country & year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Joint F-test p-value	—	—	0.1188	0.0033	—	0.0229	—	0.0061
within-R ²	0.0427	0.1758	0.0515	0.2042	—	—	—	—
Excluded instruments	—	—	—	—	1	2	2	3
C-D F-stat on excluded instruments	—	—	—	—	70.233	33.797	35.117	20.560
K-P rk F-stat on excluded instruments	—	—	—	—	23.102	11.080	10.683	6.422
Hansen J-test p-value	—	—	—	—	—	—	0.7100	0.5888
Weak-instrument-robust p-value	—	—	—	—	0.1524	0.0313	0.5505	0.0228
N	799	660	666	660	641	641	590	590
Countries	154	141	141	141	130	130	115	115
Number of democracy changes	91	69	73	69	69	69	64	64
Long-run effect at 10th percentile Gini	0.61	0.04	1.78	3.07	3.6	6.37	2.62	5.28
Long-run effect at 90th percentile Gini	0.61	0.04	-1.22	-4.02	3.6	-1.40	2.62	-3.88
Years	1960 – 2010	1970 – 2010	1970 – 2010	1970 – 2010	1965 – 2010	1965 – 2010	1965 – 2010	1965 – 2010

Notes: Robust standard errors clustered by country are in parentheses. All specifications control for the log of lagged GDP per capita.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 10: Effects of democracy on the gross Gini coefficient, 5-year panels

	OLS				Two-Stage Least Squares			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
democracy _{<i>t-1</i>}	-0.3182 (1.086)	0.0063 (0.847)	3.0439 (4.629)	8.2006*** (2.491)	3.2686 (2.169)	7.7957** (3.175)	1.9050 (1.920)	8.3816*** (2.816)
democracy _{<i>t-1</i>} × $\overline{\text{gini}}$			-0.0644 (0.099)	-0.1862*** (0.055)		-0.1251* (0.069)		-0.1876*** (0.066)
gini _{<i>t-1</i>}		0.5267*** (0.055)		0.5508*** (0.051)	0.5248*** (0.055)	0.5414*** (0.053)	0.5163*** (0.054)	0.5429*** (0.050)
Country & year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Joint F-test p-value	—	—	0.8047	0.0038	—	0.0491	—	0.0082
within-R ²	0.0946	0.3184	0.0672	0.3446	—	—	—	—
Excluded instruments	—	—	—	—	1	2	2	3
C-D F-stat on excluded instruments	—	—	—	—	66.186	28.133	34.144	16.825
K-P rk F-stat on excluded instruments	—	—	—	—	21.738	9.876	9.878	5.263
Hansen J-test p-value	—	—	—	—	—	—	0.7337	0.7753
Weak-instrument-robust p-value	—	—	—	—	0.1283	0.0475	0.5869	0.0430
N	791	651	656	651	631	631	585	585
Countries	154	141	141	141	129	129	115	115
Number of democracy changes	89	68	71	68	68	68	63	63
Long-run effect at 10th percentile Gini	-0.32	0.01	0.86	4.22	6.88	7.77	3.94	7.05
Long-run effect at 90th percentile Gini	-0.32	0.01	-0.82	-6.61	6.88	0.64	3.94	-5.05
Years	1960 – 2010	1970 – 2010	1970 – 2010	1970 – 2010	1965 – 2010	1965 – 2010	1965 – 2010	1965 – 2010

Notes: Robust standard errors clustered by country are in parentheses. All specifications control for the log of lagged GDP per capita.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 11: Effects of democracy on the net Gini coefficient with simple interactions, 5-year panels

	Fixed initial inequality interaction – simple				Once lagged initial inequality interaction			
	OLS		2SLS		OLS		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
democracy _{<i>t-1</i>}	2.1068 (2.379)	7.3131*** (1.793)	9.2385** (3.732)	9.2889*** (3.303)	3.9756 (2.459)	7.8343*** (1.737)	10.6912*** (3.525)	9.8057*** (3.257)
democracy _{<i>t-1</i>} × $\overline{\text{gini}}$	-0.0411 (0.054)	-0.1789*** (0.043)	-0.1774*** (0.065)	-0.2030*** (0.061)				
democracy _{<i>t-1</i>} × gini _{<i>t-1</i>}					-0.0936 (0.058)	-0.1957*** (0.045)	-0.2258*** (0.064)	-0.2205*** (0.064)
gini _{<i>t-1</i>}		0.4225*** (0.063)	0.4096*** (0.063)	0.4246*** (0.065)		0.4058*** (0.058)	0.4041*** (0.058)	0.4016*** (0.062)
Country & year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Joint F-test p-value	0.6653	0.0003	0.0247	0.0038	0.2656	0.0001	0.0021	0.0025
within-R ²	0.0398	0.2076	–	–	0.0548	0.1890	–	–
Excluded instruments	–	–	2	3	–	–	2	3
C-D F-stat on excluded instruments	–	–	35.103	22.052	–	–	39.920	24.968
K-P rk F-stat on excluded instruments	–	–	11.703	7.247	–	–	11.897	7.763
Hansen J-test p-value	–	–	–	0.6486	–	–	–	0.9903
Weak-instrument-robust p-value	–	–	0.0499	0.0257	–	–	0.0079	0.0296
N	660	660	641	590	630	617	591	549
Countries	141	141	130	115	141	141	123	109
Number of democracy changes	69	69	69	64	64	64	64	60
Long-run effect at 10th percentile Gini	2.11	2.39	7.38	6.44	1.40	4.13	7.52	6.25
Long-run effect at 90th percentile Gini	2.11	-2.88	-1.48	-3.95	-1.36	-5.59	-3.65	-4.61
Years	1965 – 2010	1965 – 2010	1965 – 2010	1965 – 2010	1970 – 2010	1970 – 2010	1970 – 2010	1970 – 2010

Notes: Robust standard errors clustered by country are in parentheses. All specifications control for the log of lagged GDP per capita.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 12: Effects of democracy on the net Gini, alternative samples, 5-year panels

	Excluding former USSR			Excluding former USSR & Warsaw Pact			Excluding countries never autocratic		
	OLS	2SLS		OLS	2SLS		OLS	2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
democracy _{t-1}	5.9925*** (1.779)	8.6633*** (3.235)	8.5415*** (3.021)	5.7362** (2.784)	9.0813* (5.397)	9.2706* (4.852)	7.9126*** (1.832)	14.9298*** (4.406)	14.5982*** (4.029)
democracy _{t-1} × $\overline{\text{gini}}$	-0.1487*** (0.044)	-0.1749*** (0.060)	-0.1923*** (0.061)	-0.1438** (0.063)	-0.1911* (0.100)	-0.2126** (0.095)	-0.1794*** (0.045)	-0.2207*** (0.083)	-0.2289** (0.091)
gini _{t-1}	0.3746*** (0.066)	0.3673*** (0.065)	0.3784*** (0.068)	0.3784*** (0.071)	0.3841*** (0.071)	0.3955*** (0.072)	0.3554*** (0.074)	0.3172*** (0.069)	0.3125*** (0.073)
Country & year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
within-R ²	0.2025	—	—	0.1834	—	—	0.2421	—	—
Joint F-test p-value from 2nd stage	0.0035	0.0138	0.0065	0.0685	0.1380	0.0663	0.0002	0.0029	0.0013
Excluded instruments	—	2	3	—	2	3	—	2	3
C-D F-stat on excluded instruments	—	32.817	20.385	—	29.006	19.862	—	11.415	6.256
K-P rk F-stat on excluded instruments	—	11.042	6.416	—	10.357	6.326	—	4.963	2.814
Hansen J-stat p-value	—	—	0.6078	—	—	0.9917	—	—	0.8582
Weak-instrument-robust p-value	—	0.0208	0.0245	—	0.1643	0.2057	—	0.0024	0.0034
N	627	609	582	595	578	552	436	423	390
Countries	126	116	111	120	111	103	110	90	78
Number of democracy changes	66	66	62	62	62	58	69	69	64
Long-run effect at 10th percentile Gini	3.05	6.09	5.24	2.87	6.21	5.67	4.62	12.98	12.07
Long-run effect at 90th percentile Gini	-3.96	-2.06	-3.88	-3.95	-2.94	-4.70	-3.58	3.45	2.26
Years	1965 – 2010	1965 – 2010	1965 – 2010	1965 – 2010	1965 – 2010	1965 – 2010	1965 – 2010	1965 – 2010	1965 – 2010

Notes: Robust standard errors clustered by country are in parentheses. All specifications control for the log of lagged GDP per capita.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 13: Effects of democracy on the Gini coefficient, instrumenting for initial inequality levels as well, 5-year panels

	Two Stage Least Squares							
	Net Gini				Gross Gini			
	Baseline interaction	Simple interaction	Reduced form	Reduced form	Baseline interaction	Simple interaction	Reduced form	Reduced form
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
democracy _{t-1}	13.2406** (6.465)	14.8166** (7.232)	12.4255* (7.542)	13.6282* (8.183)	8.6127** (4.124)	9.0616** (4.154)	14.9709** (7.213)	15.0699** (7.509)
democracy _{t-1} × $\overline{\text{gini}}$	-0.2361 (0.175)	-0.2987 (0.208)	-0.2130 (0.194)	-0.2560 (0.218)				
democracy _{t-1} × gini IV					-0.1120 (0.099)	-0.1294 (0.102)	-0.2900 (0.179)	-0.3207* (0.185)
Country & year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Joint F-test p-value from 2nd stage	0.0385	0.0391	0.0851	0.0913	0.0521	0.0438	0.0995	0.1277
Excluded instruments	2	3	2	3	2	3	2	3
C-D F-stat on excluded instruments	10.996	5.694	13.059	7.245	27.980	17.772	24.576	16.006
K-P rk F-stat on excluded instruments	3.477	1.913	3.462	1.772	7.592	4.244	6.788	3.832
Hansen p-value	—	0.7794	—	0.6134	—	0.6274	—	0.5303
Weak-inst. robust p-value	0.0427	0.0504	0.0469	0.0652	0.0469	0.0652	0.1155	0.1835
N	647	597	641	590	641	590	631	585
Countries	130	116	130	115	129	115	129	115
Number of democracy changes	73	68	69	64	69	64	68	63
Long-run effect at 10th percentile Gini	6.75	6.60	6.57	6.59	5.53	5.50	5.15	4.21
Long-run effect at 90th percentile Gini	-0.21	-2.20	0.29	-0.96	2.23	1.69	-2.42	-4.17
Years	1965 – 2010	1965 – 2010	1965 – 2010	1965 – 2010	1965 – 2010	1965 – 2010	1965 – 2010	1965 – 2010

Notes: Robust standard errors clustered by country are in parentheses. All specifications control for the log of lagged GDP per capita.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 14: Effects of democracy on the the Gini coefficient, GMM regressions

	Yearly panels		5-year panels	
	Net Gini (1)	Gross Gini (2)	Net Gini (3)	Gross Gini (4)
$democracy_{t-1}$	3.0915*** (0.840)	2.0718* (1.074)	7.3395** (3.395)	6.1339* (3.344)
$democracy_{t-1} \times \overline{gini}$	-0.0731*** (0.020)	-0.0403* (0.022)	-0.1751** (0.084)	-0.1291* (0.076)
$gini_{t-1}$	0.9662*** (0.018)	0.9738*** (0.015)	0.5896*** (0.075)	0.7136*** (0.056)
Year fixed effects	yes	yes	yes	yes
Joint F-test p-value	0.0013	0.1529	0.0977	0.1893
Number of instruments	272	272	220	219
AR(2) p-value	0.264	0.066	0.749	0.898
N	3788	3713	660	651
Countries	154	154	141	141
Number of democracy changes	104	102	69	68
L-R effect at 10th percentile Gini	32.01	26.98	6.15	6.16
L-R effect at 90th percentile Gini	-31.84	-13.26	-6.43	-5.61

Notes: Robust standard errors clustered by country are in parentheses. All specifications control for the log of lagged GDP per capita. Data covers the period 1961 – 2010.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 15: Democratic Switches in our Baseline Sample

Year	Country	Initial net Gini	Country	Year	Initial net Gini
1997	Albania	28.09	1991	Madagascar	46.87
1983	Argentina	40.03	1994	Malawi	58.69
1991	Bangladesh	28.32	1972	Malaysia	50.56
2009	Bangladesh	37.68	1983	Malaysia	43.86
1998	Armenia	41.19	1992	Mali	36.02
1983	Botswana	53.53	1994	Mexico	46.18
1985	Brazil	55.18	1990	Nepal	28.06
1990	Bulgaria	22.68	1999	Niger	42.14
2003	Burundi	38.38	1999	Nigeria	52.27
1991	Cape Verde	40.11	1988	Pakistan	33.61
1993	Central African Rep.	58.69	1990	Panama	47.26
1983	Sri Lanka	45.02	1993	Peru	55.14
1989	Chile	51.17	1986	Philippines	45.16
1992	Taiwan	26.90	1989	Poland	24.91
2002	Comoros	50.78	1994	Guinea-Bissau	51.56
1999	Croatia	31.12	1999	Guinea-Bissau	48.27
1983	El Salvador	46.95	2005	Guinea-Bissau	38.97
1983	Fiji	39.13	1991	Romania	19.69
1990	Fiji	41.36	2000	Senegal	40.35
1999	Djibouti	38.12	1996	Sierra Leone	61.41
1996	Ghana	38.56	2001	Sierra Leone	54.52
1974	Greece	33.67	1973	South Africa	65.07
1986	Guatemala	43.60	1983	South Africa	64.91
1992	Guyana	42.12	1978	Zimbabwe	55.30
1990	Haiti	53.77	1976	Spain	30.36
1994	Haiti	53.80	1974	Thailand	49.07
1989	Hungary	21.54	1978	Thailand	47.08
1999	Indonesia	36.73	1992	Thailand	49.66
2000	Cote d'Ivoire	37.01	1973	Turkey	52.96
2002	Kenya	47.40	1983	Turkey	50.09
1987	Korea, South	35.92	1985	Uruguay	40.50
2005	Kyrgyzstan	35.65	1991	Zambia	61.29
2005	Lebanon	43.48			
1993	Lesotho	59.06			
1999	Lesotho	59.01			

Notes: Democratic switches are coded as in the baseline specification.