Democratization and the conditional dynamics of income distribution*

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Abstract

Despite strong theoretical reasons to believe that democratization equalizes income distributions, existing empirical studies do not find a statistically significant effect of democratization on income inequality. This paper starts from the simple observation that autocracies are heterogeneous and govern quite extreme distributional outcomes (also egalitarian). Democratization may drive extreme income distributions to a "mid-dle ground". We examine the extent to which initial inequality levels determine the path of distributional dynamics following democratization. Using fixed effects and instrumental variable regressions we demonstrate that egalitarian autocracies become more unequal following democratization, whereas democratization has an equalizing effect in highly unequal autocracies. The effect is driven by changes in the gross (market) inequality, suggesting that democratization leads to redistribution of market opportunities, rather than to direct fiscal redistribution. We then investigate which kind of reforms are at work following democratizations that may rationalize our findings.

Keywords: Democracy, inequality, non-linearity, middle ground

JEL Codes: D30, O15, P48

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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 populistic, to communistic, since the second world war, autocratic regimes have varied dramatically in their 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. However the mean level of inequality is quite similar to the democratic countries. 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 empirically investigate a non-linearity that has not been examined in the literature. 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" – autocratic regimes which governed extreme distributional outcomes are replaced by political processes that gravitate towards more centrist outcomes. We provide evidence of a highly statistically significant conditional effect of democratization on 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 impact inequality levels, as democratization is commonly theorized to be driven by distributional issues. Autocracies are often elite-dominated societies that have implemented institutions designed to protect the elite's economic power. Shifting to democracy allows for a broader set of economic interests to be served. In major theories of political transitions, grievances against the ruling autocratic class is often seen to be distributional. For instance, some rational choice models of political transitions, (Acemoglu and Robinson, 2001, 2006; Boix, 2003) show how following the political enfranchisement of the poor, the decisive voter 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). Ansell and Samuels (2014) provide a divergent perspective in their analysis of the inequality issue in an intra-elite game, where an emerging middle class seeks protection against predation by the ruling elite. To the extent that there is a theoretical expectation that distributional issues are driving the democratization process, then we should expect the income distribution to be significantly affected once a democratically elected government comes to power.



Figure 1: The distribution of net Gini coefficients (income after taxes and transfers) among autocracies (left) and democracies (right). Data comes from Solt (2009).

Yet, the empirical literature concerning the effect of democracy on economic inequalities has not reached a consensus. 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.¹ Employing fixed effects dynamic panel regression models, Acemoglu et al. (2015) go on to show that there is no robust statistically significant relation between democratization and inequality. Such null results have led researchers to re-consider the extent to which drivers of democratization are distributive in nature (Aidt and Jensen, 2009; Haggard and Kaufman, 2012; Knutsen and Wegmann, 2016).

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). 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 the distribution of the net Gini coefficient 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 (proxied by GDP per capita).

¹From 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), 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.

	Non	democracies		Der	nocracies	
	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	1:	Distrib	ution o	of Gini	coefficients	by	political	institutions

Notes: Calculations by the author. Gini coefficients are calculated from net income (after taxes and transfers) by Solt (2009) and GDP per capita data comes from the Penn World Table. For the non-democracies, there are 332 observations in each income quartile, while for the the democracies there are 632 observations in each income guartile.

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 (De Long and Shleifer, 1993; Olson, 1993; Wintrobe, 1998).

We combine the observation that autocracies are more heterogeneous than democracies with some theories of autocratic rule, namely the selectorate theory of Bueno de Mesquita et al. (2003) or the divide and rule model of Acemoglu et al. (2004). In this class of models, an autocrat relies on maintaining a minimum winning coalition of supporters that he can buy off the most efficiently and which gives a disproportionate political weight to a smaller fraction of the population than the majority. But there is no clear systematic insight about which fraction of the population is easier to mobilize and buy for the survival of an autocratic regime. This may depend on many things, from ideology of the ruler, the distribution of power in the civil society, the cost of mobilizing for different groups in society, the distribution of preferences among groups, or the division of society along non-economic dimensions. 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 rich-elite-dominated regimes. Democratization would necessarily expand the size of the "winning coalition", giving more power to the fraction of the population that was previously excluded from the autocrat's coalition and pushing political competition towards the preferences of the newly enfranchised, which would necessarily be more central in the space of policy of preferences.

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. Applied to income inequality, extreme distributional outcomes that were politically sustainable with a narrow coalition 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 that relied on a poor segment of the population for political support, are likely to see inequality rise, as democratic liberalization unwinds a legacy of restrictive economic policies, opening up new entrepreneurial opportunities and wealth creation.

Our basic point is that without taking into account how the effect is conditional on initial income inequality 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, demonstrated by Acemoglu et al. (2015). Autocratic societies are highly heterogeneous and regression analyses that do not take this into account are ignoring important non-linearities in the effect of democracy on income inequality. To establish the non-linearity, 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 Papaioannou and Siourounis 2008a). Our contribution to the literature on democracy and inequality is multi-faceted.

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. 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 findings in the literature, we demonstrate that democratization actually strongly affects inequality levels.

Second, we pursue an instrumental variable strategy for democratic switches that allows a causal interpretation of the result. Accemoglu et al. (2017) 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 economic growth. We construct a similar "democratic wave" instrument for our Two Stage Least Squares [2SLS] analysis. 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.

We pursue a wide range of alternative specifications to demonstrate the robustness of our results and the validity of our instrumental variable strategy. First, we investigate the impact on gross (market) income inequality rather than on the net (after taxes and transfers) income inequality levels. Coefficient estimates are very similar compared to the those when using the net Gini coefficient as the dependent variable, suggesting that the impact of democratization on inequality occurs mostly through market mechanisms and redistributions of economic opportunities among citizens, rather than direct fiscal redistributions. Second, we check that our results are not being driven by a single group of countries, namely the formerly communist and Warsaw Pact countries. When excluding those countries, the conditional results hold with the same magnitude, suggesting that the relationship is a quite general pattern among democratization episodes. Further checks include: use alternative democratization indicators to be sure that the result is not driven by the composite indicator we employ in the baseline specification; we run some placebo regressions in order to be sure we are not simply identifying a more global mean reversion process; and we investigate several channels through which the exclusion restriction on our set of instruments could be violated, including the mean degree of inequality in the neighboring countries for which we construct the democratic wave instrument. Further robustness tests are collected in an online appendix.

Finally, we also provide an investigation into the potential channels through which democratization may affect inequality. 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 may get reduced through fiscal redistribution and pro-poor policies (in line with Meltzer and Richard 1981). Many structural reforms follow democratization, such as market reforms or great public provision of education (see Acemoglu et al. 2017, 2015, among others), that have a strong impact on the distribution of market opportunities in the population. Those reforms may be quite heterogeneous depending on the initial degree of inequality. However, 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. We empirically investigate the plausibility of the various channels through which democratization may affect inequality levels.

We first confirm our intuition that democratization does not lead to dramatic changes in pure fiscal redistribution (which the first robustness check teases). We then focus on establishing which policy shifts can explain the heterogeneous dynamics of income inequality following switches to democracy. We then show that democratization leads to different kinds of structural reforms according to the initial degree of inequality. For instance, we demonstrate that following democratization low inequality countries increase market liberalization, while high income inequality countries increase investments in state capacity and public services that may disproportionately benefit the poor. Thus, the channels through which democratization leads income distributions to a middle ground also depend on the initial degree of inequality.

The paper proceeds as follows. In the next section we describe the variables of interest, the data used for the analysis, and give some preliminary results. The third section provides the details of our empirical strategy, our baseline results, and a series of robustness checks. In the fourth section, we discuss some mechanisms that may be behind our finding of the heterogeneous effects of democratization and test which are the most empirically plausible, while the final section offers our brief concluding remarks.

2 Data and preliminary results

To investigate the extent to which democratization affects inequality levels, we employ a country-level panel from 1960 - 2010. In the paper, we present results from estimations on yearly panels. In an online appendix, we present results from the analogous specifications estimated on five-year panels.

Democratic political institution indicator. We construct a binary indicator for the political system that follows Papaioannou and Siourounis (2008a) and later Acemoglu et al. (2017, 2015). 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).² Specifically, we consider a state as democratic when Freedom House codes it as "Free" or "Partially Free" and the Polity2 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).³ 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

²The 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).

³See Papaioannou and Siourounis (2008a) for a more detailed description of the methodology.

politically. Our measure of democracy also incorporates the expansion of civil rights through the Freedom House's index.⁴ Furthermore, combining these three leading indicators allows us to address the issue of measurement error that the democracy indices may suffer from individually.

Of course, the method of aggregating a binary measure of democracy also has its weakness: (i) the thresholds are arbitrarily chosen, (ii) it comes from several indicators which do not necessarily focus on the same dimensions of democratization, and (iii) it does not take into account that in many cases there are no clear jumps from autocracy to democracy but some progressive improvements. To demonstrate that our results are robust to these issues, we (i) modify the thresholds we use for the aggregate measure, (ii) construct a binary variable based solely on the Polity2 index, and (iii) use the Polity2 index in its raw form as a continuous variable (values from -10 to 10). The democracy indicator $[D(0,1)_{i,t}]$ takes value zero if country *i* is determined to be autocratic in period *t* and it takes value one if country *i* is determined to be democratic in period t.⁵

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.⁶ 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. (2017) and employ an instrument that relies on the observation that political transitions have historically occurred in regional "waves"⁷ 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 panel.⁸

 $^{^{4}}$ While measuring different characteristics of democracy, Acemoglu et al. (2017) show that these institutional components are quite strongly correlated.

⁵Note that we code both permanent and transitory transitions to democracy, and reversals to nondemocracy. Nothing indicates that the initial dynamics of inequality should be different in a democracy that eventually reverses to autocracy and democracy that eventually consolidates.

⁶See, for example, Ansell and Samuels (2014); Freeman and Quinn (2012); Gradstein and Milanovic (2004); Haggard and Kaufman (2012); Houle (2009); Papaioannou and Siourounis (2008b).

⁷See Huntington (1993) for the classic exposition. More recently, see, for example, Dorsch and Maarek (2015) for theory and Aidt and Jensen (2014) or Persson and Tabellini (2009) for evidence.

⁸Beyond 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

More formally, we construct the following instrument for democratization events in country *i* of region *r* in period *t*, which we denote by Z_{it}^{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*. 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.⁹ We refer to the instrument for democracy as the "dynamic regional share of democracies". Figure 2 plots the country-specific instrument for examples of the IV for countries from four 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 integrated and affect both inequality and the probability to observe a transition in country *i*. Growth rates may, for instance, affect the probability of democratization through the opportunity cost channel à la Acemoglu and Robinson (2001) or through a process of modernization (Lipset, 1959). 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. To further demonstrate that our set of instruments satisfies the exclusion restriction, a robustness check also includes a battery of time-varying variables to shut down other channels through which the exclusion restriction could be possibly violated.

predictor of democratization events, our instrument based on dynamic regional share of democracy smooths out the estimated impact of erroneously coded transitions.

⁹We classify countries into the following ten regions: Eastern Europe and post Soviet Union, Latin America, North Africa and Middle East, Sub-Saharan Africa, Western Europe and North America, East Asia, South-East Asia, South Asia, The Pacific, and The Caribbean.



Figure 2: Democratic switches and the regional share instrument. The dashed vertical lines represent the year in which the switch to democracy occurred in the example cases.

Income inequality. For the inequality data, we use the most standard measure of income inequality, the Gini coefficient, which 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 and gross income distributions. As inequality levels may be path dependent and change rather slowly over time, we also include lagged dependent variables in all specifications 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



Figure 3: For the left-hand side, Δ Net Gini = $15.06^{***} - 0.38^{***} \times \overline{\text{Gini}}$. $R^2 = 0.342$. For the right-hand side, Δ Gross Gini = $21.39^{***} - 0.48^{***} \times \overline{\text{Gini}}$. $R^2 = 0.366$.

of inequality in the country prior to democratization. We define a fixed pre-democracy inequality variable for these interaction terms. 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 through redistribution before being forced to democratize. Therefore, whenever possible, we take as the pre-democracy level of inequality the level of inequality prevailing five years before democratization occurs. When not available, we take the closest observation available for inequality to the five year window (for instance, four years before democratization occurs if the observation five years before is not available). We label this transition-specific variable as $\overline{Gini_i}$.¹⁰

To provide further intuition for the battery of regression results that follow, we first consider several descriptive figures. We calculate the difference in the Gini coefficient ten years after a transition from its pre-democracy initial level. The left-hand side of Figure 3 scatters this difference against the pre-democracy level for the net Gini coefficients (\overline{Gini}). The negative relationship is strongly statistically significant and the R² is quite high for such a simple regression. The right-hand side of Figure 3 is the analogue for the gross Gini coefficient, for which the correlation is even stronger. The figures show that 10 years after a switch to democracy, inequality increases in countries that were egalitarian autocracies and inequality decreases in countries that were unequal autocracies. The democratic switches and the raw data for the left-hand side of Figure 3 are presented in Table 2.

In the 2SLS estimations that instrument for democratization using the dynamic regional share of democracies, we also instrument for the interaction term by simply interacting the

¹⁰In online appendix Table A6, we also consider some simpler codings of the pre-democracy inequality variable for use in the interaction term, such as the contemporaneous degree of inequality and the degree of inequality the year of the democratization.

Voor	Country	Initial Gini	A Gini	Voor	Country	Initial Gini	A Girci
rear	Country	initiai Gini		rear	Country	initiai Gini	
1997	Albania	28.09	3.35	1999	Lesotho	59.01	-10.29
1983	Argentina	40.03	2.63	1990	Madagascar	46.87	-2.41
1998	Armenia	41.19	-2.76	1994	Malawi	58.69	-18.43
1991	Bangladesh	28.32	7.20	1972	Malaysia	50.56	0.35
2009	Bangladesh	37.68	-5.81	1983	Malaysia	43.86	-0.78
1983	Botswana	53.53	1.49	1992	Mali	36.02	2.67
1985	Brazil	55.12	-3.77	1994	Mexico	46.18	0.37
1990	Bulgaria	22.68	2.44	1990	Nepal	28.06	15.90
2003	Burundi	38.37	-5.14	1999	Niger	42.14	0.76
1991	Cape Verde	40.11	14.35	1999	Nigeria	52.27	-9.52
1993	Central African Republic	58.69	-14.90	1972	Pakistan	25.07	8.54
1983	Sri Lanka	45.02	-8.13	1988	Pakistan	33.61	-1.70
1989	Chile	51.17	1.05	1990	Panama	47.26	4.27
1992	Taiwan	26.90	3.37	1993	Peru	55.13	-1.84
1999	Croatia	31.12	-3.28	1986	Philippines	45.16	1.12
1983	El Salvador	46.95	0.38	1989	Poland	24.91	4.64
1995	Ethiopia	43.21	-14.29	1994	Guinea-Bissau	51.56	-15.80
1983	Fiji	39.13	4.35	1999	Guinea-Bissau	48.27	-10.61
1990	Fiji	41.36	2.18	2005	Guinea-Bissau	38.97	-1.32
1999	Djibouti	38.12	0.81	1991	Romania	19.69	7.95
1996	Ghana	38.56	2.81	2000	Senegal	40.35	-3.88
1974	Greece	33.67	-1.44	1996	Sierra Leone	61.41	-17.65
1986	Guatemala	43.60	9.83	2001	Sierra Leone	54.52	-10.76
1990	Haiti	53.77	0.38	1973	South Africa	65.07	-1.52
1994	Haiti	53.80	0.78	1983	South Africa	64.91	-5.32
1989	Hungary	21.54	8.65	1978	Zimbabwe	55.30	-3.15
1999	Indonesia	36.73	-0.54	1976	Spain	30.36	-1.87
2000	Cote d'Ivoire	37.01	8.55	1978	Thailand	47.08	3.03
2002	Kenya	47.40	-1.29	1992	Thailand	49.66	-3.69
1987	Korea, South	35.92	-6.23	1973	Turkey	52.96	-2.42
2005	Kyrgyzstan	35.65	0.84	1983	Turkey	50.09	-6.19
2005	Lebanon	43.48	-0.55	1985	Uruguay	40.50	0.32
1993	Lesotho	59.06	-8.70	1991	Zambia	61.29	-7.74

Table 2: Democratic switches in our baseline sample, using composite method

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

pre-democracy level of inequality (\overline{Gini}) with the dynamic regional share of democracies, as recommended by Wooldridge (2010).

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 (Feenstra et al., 2015). For the OLS specifications, it is a routine and obvious control since both the likelihood of democracy and the evolution of income inequality may depend on economic development levels. For the 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. Summary statistics of all the variables used in the benchmark analysis are presented in Table 3.

	١	lon-democra	acies		Democraci	es
	Obs.	Mean	Std.Dev.	Obs.	Mean	Std.Dev.
Gini coefficient, net income Gini coefficient, gross income Real CDP per capita, chain series	1290 1245 1290	40.77 44.24 3901 56	9.49 10.19 5309 79	2485 2478 2485	37.13 45.20	10.50 7.68 11845 30
Share of region democracy	1290	0.25	0.25	2485 2429	0.72	0.29

Table 3: Summary for baseline sample

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 levels of inequality. In our tables 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. First, we present our baseline tables that use as dependent variable the net Gini coefficient (Table 4) and the gross Gini coefficient (Table 6). Table 7 considers several intuitive alternative samples, while Table 8 considers alternative democracy indicators. Table 9 presents several placebo tests and Table 10 investigates the possible violations to the exclusion restriction. An online appendix presents some additional results and further robustness checks.

3.1 Baseline regression analysis

The first column of Table 4 tests the extent to which democratization can explain withincountry variation in inequality levels. Using ordinary least squares [OLS], we first estimate:

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

where $D(0,1)_{i,t}$ 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 switches, with $E(u_{i,t}) = 0$ for all *i* and *t*. In general, we estimate the autoregressive effect to be 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 4 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

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

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

The third and fourth columns of Table 4 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:

$$Gini_{i,t} = \rho Gini_{i,t-1} + \alpha_1 D(0,1)_{i,t-1} + \alpha_2 D(0,1)_{i,t-1} \times \overline{Gini_i} \\
+ \beta GDP_{i,t-1} + \gamma_i + \delta_t + u_{i,t}.$$
(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, 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, $\overline{Gini}^{10} = 26.9$ and $\overline{Gini}^{90} = 59.2$) as

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

where again L indicates the number of lagged dependent variables we include in the specification.¹¹ 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 nearly 7 points. This simple estimation demonstrates how transitions to democracy, on average, bring extreme income distributions to some "middle ground".¹²

 $^{^{11}\}mathrm{See}$ Acemoglu et al. (2017) for the derivation of this equation.

 $^{^{12}}$ Table A6 in the online appendix reproduces our main results with simplified interaction terms that do not lag the "initial" inequality level as in our baseline specification.

		0	S			Two-Staged L	east Squares	
1	(1)	(2)	(3)	(4)	(5)	(9)	(2)	(8)
democracy $_{t-1}$	-0.0912 (0.153)	-0.0906 (0.112)	1.3535*** (0.425)	1.2653*** (0.287)	0.2171 (0.326)	0.1877 (0.280)	1.8782*** (0.580)	1.7150*** (0.472)
democracy $_{t-1} imes \overline{gini}$			-0.0351*** (0.011)	-0.0335*** (0.007)			-0.0450*** (0.015)	-0.0427*** (0.010)
log GDP per capita $_{t-1}$ gini $_{t-2}$ gini $_{t-3}$ gini $_{t-4}$	0.6804*** (0.182) 0.8905*** (0.009)	0.3193* (0.179) 1.2534*** (0.044) -0.1960** (0.079) -0.2674*** (0.074) 0.0928*** (0.034)	0.5689*** (0.172) 0.8947*** (0.009)	0.1997 (0.187) 1.2545*** (0.043) -0.1941** (0.078) -0.2688*** (0.074) 0.0947***	0.7041*** (0.205) 0.8908*** (0.009)	0.3454* (0.201) 1.2565*** (0.044) -0.1972** (0.079) -0.2679*** (0.075) 0.0917***	0.5589*** (0.194) 0.8957*** (0.009)	0.1883 (0.206) 1.2569*** (0.043) -0.1947** (0.079) -0.2700*** (0.074) 0.0950***
Country & year fixed effects Excluded instruments within-R ² Joint F-test p-value C-D F-stat on excluded instruments Hansen J-test p-value	yes 	yes 	yes - 0.8550 	yes - 0.0000	yes 2 - 211.542 0.6523	yes 2 2 170.964 0.4997	yes 3 - 0.0043 129.138 0.6184	yes 3 0.0001 0.5308
N Countries Number of democracy changes Long-run effect at 10th percentile Gini Long-run effect at 90th percentile Gini Years	3781 154 66 -0.83 -0.83 1961 - 2010	3251 143 52 -0.77 -0.77 1964 - 2010	3781 154 66 3.89 -6.85 1961 – 2010	3251 143 52 3.20 -6.30 1964 – 2010	3696 147 65 1.99 1.99 1.99	3198 141 52 1.61 1.61 1.61	3696 147 65 6.38 -7.55 1961 – 2010	3198 141 52 5.01 -7.17 1964 - 2010
<i>Notes</i> : Robust standard errors clustered by co 5.45 for 2SLS specifications with three excluded hypothesis is that the set of instruments is wea rejected. *** / ** / ** represent significance at the	untry are in parent d instrument and a ak. The Hansen J- e 0.01 / 0.05 / 0.10	theses. Stock-Yogo re 19.93 / 7.25 for 2 statistic tests for ex levels, respectively	weak identification 2SLS specifications cogeneity of the set	test for the set of with two excluded of instruments an	instruments has cri instruments. Referr d has null hypothes	tical values for 10% ing to the Cragg-Dc is that the set of in	s / 25% maximal IV onald (C-D) F-statis struments is exoge	size are 13.43 / tic, the test's null nous, cannot be

Table 4: Effects of democracy on the net Gini coefficient

			0	LS LS					Two-Staged I	-east Squares		
	(1a)	(1b)	(2)	(3a)	(3b)	(4)	(5a)	(2b)	(9)	(7a)	(4Z)	(8)
democracy $_{t-1}$	-0.0912 (0.153)	-0.1358 (0.158)	-0.0906 (0.112)	1.3535*** (0.425)	1.3857*** (0.436)	1.2653*** (0.287)	0.2171 (0.326)	0.0405 (0.385)	0.1877 (0.280)	1.8782*** (0.580)	1.3374** (0.611)	1.7150*** (0.472)
democracy $_{t-1} imes \overline{gini}$				-0.0351*** (0.011)	-0.0376*** (0.011)	-0.0335*** (0.007)				-0.0450*** (0.015)	-0.0361** (0.015)	-0.0427*** (0.010)
log GDP per capita $_{t-1}$	0.6804*** (0.182)	0.4996** (0.226)	0.3193* (0.179)	0.5689*** (0.172)	0.3638 (0.223)	0.1997 (0.187)	0.7041*** (0.205)	0.5189** (0.242)	0.3454* (0.201)	0.5589*** (0.194)	0.3840 (0.243)	0.1883 (0.206)
$gini_{t-1}$	0.8905***	0.9130***	1.2534*** 0.044)	0.8947***	(0.009) (0.009)	1.2545*** (0.043)	(0.000) (0.000)	0.9136*** 0.0136***	1.2565*** (0.044)	0.8957***	0.9162***	1.2569*** (0.043)
$gini_{t-2}$	(2020)	(00000)	-0.1960**	(20010)	(2000)	-0.1941**	(00000)	(2.2.2)	-0.1972**	(20010)	(0.000)	-0.1947**
gini _{t-3}			(0.079) -0.2674***			(0.078) -0.2688***			(0.079) -0.2679***			(0.079) -0.2700***
$gini_{t-4}$			(0.074) 0.0928*** (0.034)			(0.074) 0.0947*** (0.034)			(0.075) 0.0917*** (0.034)			(0.074) 0.0950*** (0.034)
Country & year FE's	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
excluded IIIstruments within-R ²	0 8535	- 0 8535	0 8851	0 8550	0 8553	_ 0 8895	N I	N I	N I	οI	ו מ	ו מ
Joint F-test p-value		1		0.0062	0.0027	0.0000	I	I	I	0.0043	0.0545	0.0001
C-D F-stat on excl. IV's	I	I	I	I	I	I	211.542	172.596	170.964	129.138	103.097	102.061
Hansen J-test p-value	I	I	I	I	I	I	0.6523	0.9874	0.4997	0.6184	0.9721	0.5308
Z	3781	3251	3251	3781	3251	3251	3696	3198	3198	3696	3198	3198
Countries	154	143	143	154	143	143	147	141	141	147	141	141
N. democracy changes	66	52	52	66	52	52	65	52	52	65	52	52
L-R effect at 10th p. Gini	-0.83	-1.56	-0.77	3.89	4.47	3.20	1.99	0.47	1.61	6.38	4.35	5.01
L-R effect at 90th p. Gini	-0.83	-1.56	-0.77	-6.85	-10.01	-6.30	1.99	0.47	1.61	-7.55	9.54	-7.17
Notes: Robust standard error: specifications with three exclu- instruments is weak. The Hans / 0.05/0.10 levels reservively.	s clustered by ded instrument is clustered by ded instrument is the sample with the sample wit	country are in p and are 19.93 sts for exogenei	7.25 for 2SLS 7.7.25 for 2SLS ity of the set of ii form 1961 – 2010	ock-Yogo weak specifications v nstruments and With 4 lacs th	identification tesi vith two excluded has null hypothes e sample runs fro	t for the set of i l instruments. B sis that the set of m 1964 - 2010	nstruments has eferring to the (instruments is e	critical values fr Dragg-Donald (C exogenous, canr	or 10% / 25% n C-D) F-statistic, not be rejected.	naximal IV size a the test's null hyr *** / ** / * represe	are 13.43 / 5.45 pothesis is that ent significance a	for 2SLS the set of tt the 0.01
	· · · · · · · · · · · · · · ·											

Table 5: Effects of democracy on the net Gini coefficient



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 3 of Table 4. Dashed lines represent 90% confidence intervals.

Figure 4 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 4 present results from a 2SLS procedure. 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:

$$D(0,1)_{i,t} = \zeta Gini_{t-1} + \eta_1 Z_{i,t} + \eta_2 Z_{i,t} \times \overline{Gini}_i + \theta GDP_{i,t-1} + \gamma_i + \delta_t + e_{i,t} D(0,1)_{i,t} \times \overline{Gini}_i = \zeta Gini_{t-1} + \eta_1 Z_{i,t} + \eta_2 Z_{i,t} \times \overline{Gini}_i + \theta GDP_{i,t-1} + \gamma_i + \delta_t + e_{i,t},$$

$$(5)$$

where $Z_{i,t}$ is a vector of excluded instruments. We use the fitted values from equations (5) in the second stage:

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

Columns 5 – 8 are the 2SLS analogues of columns 1 – 4. In the main text, we present only the second stage results (though we report first-stage F-statistics as justification for the strength of the instruments). First stage results are available in online appendix Table A4. We report results from specifications that are over-identified, allowing us to report the Hanson J-statistic p-values that test whether the set of excluded instruments can be considered exogenous. As a third excluded instrument we also use the second lag of the share of a country's region that is democratically governed. As in the OLS regressions, the unconditional effect of a switch to democracy is insignificant when we instrument for democracy. However, conditional on initial levels of inequality, the effect is highly statistically significant (columns 7 – 8). First-stage F-statistics indicate that the set of instruments is strong (well above the rule of thumb 10). Moreover the tables report the Stock-Yogo critical values to which the Cragg-Donald F-statistics refer and the null hypothesis that the set of instruments is weak is soundly rejected. The Hansen J-test has a null hypothesis that the set of excluded instruments is exogenous and the large p-values also comfortably confirm the validity of the set of instruments along this dimension as well.

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 7, 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 more than 7 points) of the Gini coefficient. Such movements correspond to a greater than one-third reduction in the gap between the 90th and 10th percentile inequality levels for autocratic countries. The 2SLS estimates are quite close to the simple OLS estimates. The 2SLS estimates imply a larger increase in inequality for previously 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.

To conserve space, in the rest of the analysis, we only provide results with the more complete specification that includes four lags for inequality. First, results proved to be more stable in specifications that include four lags. Second, if the past level of inequality is correlated with the probability of having a democratization episode (as many theories suggest) or with the regional wave of democracy, it is preferable to include more lags in our regression specifications in order to deal with endogeneity issues and better satisfy exclusion restrictions. In practice, including four lags rather than one only marginally affects our results (see Table 4).

3.2 Further results and robustness analysis

This subsection briefly presents several further results and some of the various robustness checks that we have conducted. We believe that two of the robustness checks stand out. First, we show that results using the market Gini as dependent variable look very similar in magnitude to the results using the net Gini. This suggests that change in market opportunities may be driving our result rather than pure redistribution mechanisms. Second, we show that our results hold when excluding former communist countries. This suggests our results are not driven by a specific group of countries and that the conditional pattern is quite general.

Market income inequality. In Table 6, we use the gross Gini coefficient, rather than the net Gini coefficient. When using the gross Gini, the coefficient estimates on the effect of democratization are very similar to the impact on the market Gini – the estimated effects lie within their respective confidence intervals. This may indicate that the impact of democratization on the net Gini mostly occurs through changes in the market Gini and that pure fiscal redistribution is not the driving force behind the changes in the net Gini that we observe following democratization. We confirm this indication more formally in the following section that investigates the heterogeneous mechanisms through which democratization affects income inequality levels, by looking directly at the difference between net and market Gini coefficients as a proxy for direct fiscal redistribution. That the impact seems to work through the market Gini coefficient indicates that the effect of democratization occurs through an uneven evolution of market opportunities among citizens following democratization, rather than a shift of redistributive policies that the literature typically emphasizes.

Restricted sample: dropping the Eastern bloc. Table 7 considers several intuitive sub-samples. First, columns 1 - 2 drop countries that were part of the former Soviet Union. Columns 3 - 4 further drops the Central and Eastern European countries that were signatories of the Warsaw Pact.¹³ That the results are generally quite similar after dropping these groups of countries is reassuring. Coefficient estimates and predicted long-run changes in inequality levels remain stable across the various samples. The non-linearity is not being

¹³While we do not have data for all of these countries, modern states 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. Henderson et al. (2005) interestingly notes that inequalities in these socialist autocratic regimes were much higher than official data suggests.

	C	DLS	2	SLS
	(1)	(2)	(3)	(4)
democracy _{t-1}	-0.1292	1.5354***	0.2180	1.5526***
	(0.148)	(0.461)	(0.370)	(0.573)
$democracy_{t-1}\times \overline{gini}$		-0.0379*** (0.010)		-0.0367*** (0.014)
log GDP per capita $_{t-1}$	0.2975	0.1699	0.2919	0.1581
	(0.266)	(0.256)	(0.286)	(0.265)
Country & year fixed effects	yes	yes	yes	yes
Lagged DV's	4	4	4	4
Excluded instruments within-R ²	_ 0.8923	_ 0.8934	2 -	3
Joint F-test p-value	_	0.0003	_	0.0181
C-D F-stat on excluded IV's		—	148.554	81.141
Hansen J-stat p-value	–	_	0.4260	0.4803
N	3171	3171	3118	3118
Countries	143	143	141	141
Number of democracy changes	51	51	51	51
Long-run effect at 10th p. Gini	-1.38	3.56	2.37	4.21
Long-run effect at 90th p. Gini	-1.38	-8.96	2.37	-7.94

Table 6: Effects of democracy on the gross Gini coefficient

Notes: Robust standard errors clustered by country are in parentheses. The panel runs from 1964 – 2010 for all specifications. *** / ** / * represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.

driven by a particular group of countries, but the pattern appears to be more general. Finally, columns 5 - 6 of Table 7 drops countries that have never been autocratic over the length of the panel. Results are robust to estimation over these alternative samples.

Alternative democracy indicator coding. In Table 8, we consider several alternative coding specifications for the democracy indicator. In columns 1 - 2, we continue to utilize the method of Papaioannou and Siourounis (2008a), combining 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 have a Polity2 score of at least 5 to be considered democratic. In the event that the Polity2 data is not available, the more stringent criteria does not come into effect. In columns 3 - 4, we employ a democracy indicator that uses only the Polity2 data. As is common, the indicator based only on the Polity2 defines a country-year observation as a democracy for positive values and as a non-democracy for non-positive values of the Polity2 index. Finally, in columns 5 - 6 we simply use raw Polity2 index as a continuous variable. As there is no precise transition date, we simply use the once lagged Gini coefficient for the interaction term. Results are robust to these alternative codings for the democracy indicator and suggest that the institutional characteristics measured by the

	Excluding for	ormer USSR	Excl. USSR	& Warsaw Pact	Excluding ne	ever autocratic
	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
democracy $_{t-1}$	1.2940***	1.5841***	1.2298***	1.7158**	1.6350***	2.5409***
	(0.277)	(0.447)	(0.421)	(0.720)	(0.268)	(0.534)
$democracy_{t-1}\times \overline{gini}$	-0.0341***	-0.0401***	-0.0328***	-0.0431***	-0.0410***	-0.0562***
	(0.006)	(0.010)	(0.009)	(0.014)	(0.006)	(0.014)
log GDP per capita $_{t-1}$	0.1447	0.1397	0.1214	0.0969	0.1409	0.0813
	(0.186)	(0.201)	(0.188)	(0.200)	(0.203)	(0.242)
Country & year FE's	yes	yes	yes	yes	yes	yes
Lagged DV's	4	4	4	4	4	4
Excluded instruments within-R ²	_ 0.8908	3 - 0.0001	- 0.8876 0.0002	3 -	_ 0.8945	3
C-D F-stat on excl. IV's	-	101.014	-	90.571	-	40.455
Hansen J-stat p-value	-	0.4825	-	0.6177	-	0.5087
N	3065	3012	2908	2855	2140	2136
Countries	128	126	122	120	101	100
Democracy changes L-R effect at 10th p. Gini	50 3.54	50 4.76 7.48	47 3.26	47 5.36 8.07	52 4.29	52 8.12
Country & year FE's Lagged DV's Excluded instruments within-R ² Joint F-test p-value C-D F-stat on excl. IV's Hansen J-stat p-value N Countries Democracy changes L-R effect at 10th p. Gini L-R effect at 90th p. Gini	yes 4 - 0.8908 0.0000 - 3065 128 50 3.54 -6.82	yes 4 3 - 0.0001 101.014 0.4825 3012 126 50 4.76 -7.48	yes 4 - 0.8876 0.0003 - 2908 122 47 3.26 -6.68	yes 4 3 - 0.0028 90.571 0.6177 2855 120 47 5.36 -8.07	yes 4 - 0.8945 0.0000 - 2140 101 52 4.29 -6.39	yes 4 3 - 0.0000 40.455 0.5087 2136 100 52 8.12 -6.19

Table 7: Effects of democracy on the net Gini, alternative samples

Notes: Robust standard errors clustered by country are in parentheses. The panel runs from 1964 – 2010 for all specifications. *** / ** / ** represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.

Polity2 index may be more important for income inequality dynamics than those measured by the Freedom House index. Table A7 in the online appendix considers three additional alternative measures of democracy, including alternative codings with the Polity2 index and the machine-learning index developed by Gründler and Krieger (2016).

Placebo tests. In Table 9, we have conducted several placebo tests to demonstrate that it is the process of democratization that leads to the "middle ground" convergence of inequality levels, rather than some more general mean reversion process. First, as placebo treatments, we lead the democratization variable by both ten and fifteen years. We interact the placebo treatment with the initial level of inequality (five years before the placebo treatment). Columns 1 - 2 (3 - 4) of Table 9 report the results using the 10 (15) year lead placebo treatments. We also generated some purely random placebo treatments that do not rely on actual democratization events. For each country we allowed for false democratization episodes generated randomly from a uniform distribution over the support (1960 - 2010). In column 5 - 6 we generate a randomly dated transition only for countries which have experienced at least one transition during the period. In column 7 - 8 we also generate randomly which country has experienced a transition and, following the previous process, we draw a random transition date for those countries only. We consider in our sample that 20 percent

	Composite –	more stringent	Polity IV	/ binary	Polity IV	continuous
	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
democracy $_{t-1}$	0.9450***	1.6641***	1.3989***	1.8338***	0.0668***	0.1006***
	(0.290)	(0.494)	(0.268)	(0.479)	(0.023)	(0.037)
$democracy_{t-1}\times \overline{gini}$	-0.0246***	-0.0411***	-0.0353***	-0.0448***	-0.0018***	-0.0024**
	(0.006)	(0.011)	(0.006)	(0.011)	(0.001)	(0.001)
log GDP per capita $_{t-1}$	0.2598	0.2295	0.2056	0.2037	0.2428	0.2506
	(0.183)	(0.196)	(0.195)	(0.221)	(0.191)	(0.216)
Country & year FE's	yes	yes	yes	yes	yes	yes
Lagged DV's	4	4	4	4	4	4
Excluded instruments within-R ²	- 0.8859 0.0005	3 - 0.0007	- 0.8870 0.0000	3 - 0.0001	- 0.8860 0.0077	3 - 0.0181
C-D F-stat on excl. IV's Hansen J-stat p-value		96.080 0.5112		91.693 0.5294		77.206 0.5480
N	3254	3198	3130	3080	3130	3080
Countries	143	141	134	132	134	132
Democracy changes	47	47	50	50	-	-
L-R effect at 10th p. Gini	2.46	4.93	4.06	5.73	1.08	2.06
L-R effect at 90th p. Gini	-4.45	-6.76	-6.25	-7.46	-2.25	-2.37

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

Notes: Robust standard errors clustered by country are in parentheses. For the continuous measures of democracy, the long-run effects are calculated for a within-sample one-standard deviation of the Polity IV index. The panel runs from 1964 – 2010 for all specifications. *** / ** / * represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.

of countries have never been democratic and that 20 percent have always been. Reassuringly, neither the placebo treatments nor their interactions with the initial inequality level have a statistically significant effect on the net income inequality levels.

Investigating the exclusion restriction. In Table 10, we include a battery of timevarying co-variates into the 2SLS regressions in order to block off channels through which the exclusion restriction might be violated. The table presents results from the second stage of the 2SLS procedure.¹⁴ In all cases, the set of excluded instruments remains strong and exogenous when we control for these time varying variables, coefficients of interest remain highly statistically significant, and the calculated long-run effects are quite close to those from the baseline specification.

First, regional waves of political liberalization may be associated with regional waves of economic liberalization, increasing trade flows within regions and leading to increased inequalities in the home country as factors specific to tradeable production should see incomes grow. If regional democratic waves lead to increases in inequality through this effect on regional trade, this would imply a violation of the exclusion restriction. Accordingly, columns 1 and 2 include controls for export's share of GDP and total trade's share of GDP,

¹⁴Table A5 in the online appendix presents the first-stage results from these specifications.

	10 yea	ar lead	15 yea	ar lead	Rando	m date	Rand. co	untry/date
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
placebo _{t-1}	-0.0050	-0.0052	0.0251	0.0290	0.0143	0.3474	0.0266	0.4626
	(0.099)	(0.098)	(0.089)	(0.091)	(0.165)	(0.466)	(0.122)	(0.323)
$placebo_{t-1} imes \overline{gini}$		-0.0019 (0.011)		0.0098 (0.019)		-0.0084 (0.012)		-0.0124 (0.009)
log GDP per capita $_{t-1}$	0.2927	0.2931	0.2860	0.2848	0.3257*	0.3107*	0.3218*	0.3141*
	(0.204)	(0.204)	(0.203)	(0.201)	(0.185)	(0.185)	(0.182)	(0.180)
Country & year FE's	yes	yes	yes	yes	yes	yes	yes	yes
Lagged DV's	4	4	4	4	4	4	4	4
within-R ²	0.8890	0.8890	0.8851	0.8851	0.8851	0.8852	0.9010	0.9010
Joint F-test p-value	-	0.9802	-	0.7572	-	0.3535	-	0.8477
N	2999	2999	2798	2798	3256	3256	3256	3256
Countries	143	143	143	143	143	143	129	129

Table 9: Placebo tests

Notes: Robust standard errors clustered by country are in parentheses. The panel runs from 1964 – 2010 for all specifications. *** / ** / * represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.

respectively, to shut off the possibility that the instrument set's relevance is driven by this channel.

Next, migration patterns may be affected as countries in a region democratize. Theoretically, the effect on inequality in the home country could go either way. High-skill labor may leave in favor of the labor markets in the democratizing neighboring countries. Political instabilities in the region may also lead to refugee flows, which would affect the supply of low-skill labor. As migration data is only spottily available over the panel that we are interested in, column 3 thus controls for the change in the home country's population as a proxy for changes in migration patterns.

Relatedly, regional democratization may be associated with civil conflicts, which may destroy capital assets held abroad in the region by the wealthy, reducing inequalities in the home country. Also, civil conflict in the region may catalyze refugee flows, and increase inequalities in the home country. We calculate the regional share of the countries where there was an armed conflict (of any size) between the government and one or more internal opposition groups, using the data provided by UCDP/PRIO (Themnér and Wallensteen, 2013). Column 4 controls for this civil conflict variable.

Finally, we construct a time-varying regional average Gini coefficient measure that picks up regional trends in income inequalities that could be associated with regional trends in political liberalization. Column 5 includes this powerful "reduced form" control for the vector of potential violations to the exclusion restriction from regional spillover effects.

The final column adds all of the channels at the same time. The set of excluded instruments remains very strong and exogenous throughout these specifications that include

		Two-s	stage least squ	ares – Seconc	l stage	
	(1)	(2)	(3)	(4)	(5)	(6)
democracy _{t-1}	1.8683*** (0.602)	1.8887*** (0.606)	1.7125*** (0.468)	1.7055*** (0.487)	1.5336*** (0.536)	1.7947*** (0.661)
$democracy_{t-1}\times \overline{gini}$	-0.0479*** (0.012)	-0.0482*** (0.012)	-0.0427*** (0.010)	-0.0403*** (0.011)	-0.0398*** (0.011)	-0.0447*** (0.013)
log GDP per capita $_{t-1}$	0.2238 (0.228)	0.2296 (0.232)	0.1834 (0.205)	0.1522 (0.218)	0.1781 (0.202)	0.1861 (0.238)
exports/GDP $_{t-1}$	-0.0022 (0.003)					0.0046 (0.011)
total trade/GDP $_{t-1}$		-0.0020 (0.002)				-0.0049 (0.007)
$population_{t-1}$			0.0000 (0.000)			-0.0000 (0.000)
regional share civil $conflict_{t-1}$				0.5396 (0.468)		0.5269 (0.521)
regional mean gini $_{t-1}$					0.0214 (0.017)	0.0148 (0.018)
Country & year FE's	yes	yes	yes	yes	yes	yes
Lagged DV's	4	4	4	4	4	4
Excluded instruments	3	3	3	3	3	3
Joint F-test p-value	0.0002	0.0002	0.0001	0.0007	0.0007	0.0026
C-D F-stat on excl. IV's	88.375	88.379	104.056	91.177	96.968	79.950
Hansen J-stat p-value	0.5046	0.5087	0.5296	0.4842	0.4709	0.4139
Ν	3086	3086	3198	2901	3198	2789
Countries	140	140	141	141	141	140
Democracy changes	49	49	52	51	52	48
L-R effect at 10th p. Gini	5.14	5.24	4.98	5.42	3.93	5.07
L-R effect at 90th p. Gini	-8.56	-8.56	-7.22	-5.96	-6.97	-7.27

Table 10: Effect of democracy on the Gini coefficient, with other time-varying controls

Notes: Robust standard errors clustered by country are in parentheses. The panel runs from 1964 – 2010 for all specifications. *** / ** / * represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.

additional time-varying controls, which but tresses our confidence in the validity of our set of excluded instruments.¹⁵

Further results and robustness checks. In addition to the robustness checks already presented in the main text, we have some further results and more robustness checks that appear in the online appendix. We simply give an overview of these additional specifications in this sub-section, with details relegated to the online appendix.

Further results include an investigation of the effect of democratic breakdowns in Table A1 (it is the opposite, though not as precisely estimated),¹⁶ whether the type of autocratic regime matters in Table A2 (it does not), and instrumenting for the initial level of inequality

 $^{^{15}}$ We thank anonymous referees for suggesting that we investigate additional time-varying controls such as the ones that we have considered in this sub-section.

¹⁶Relatively equal societies become more equal and relatively unequal societies become more unequal following democratic breakdowns, as policy outcomes tend back towards their extremes.

as well using the instrument proposed by Easterly (2007) in Table A3.

Tables A4and A5 report the first-stage results from our 2SLS estimations for the key specifications. Further robustness checks involve simpler methods for generating the interaction terms in Table A6, alternative constructions of the democracy indicator in Table A7,¹⁷ alternative methods for calculating the long-run effect in Table A8 by estimating the effect of democratization on 10 year panels. Additionally, each table from the main text is reproduced in the online appendix estimated over five-year panels in Tables A9 – A13. Finally Table A14 in the online appendix presents estimations using a GMM-type estimator on both yearly and five-year panels.

4 Discussion

4.1 Heterogeneous outcomes, heterogeneous mechanisms

In this section, we briefly discuss several possible mechanisms that could be driving the conditional income inequality dynamics that we have empirically documented. From the theoretical perspective that we have already discussed in the introduction of our paper, we begin with the characterization of autocratic power established by selectorate theory and models of divide and rule (Acemoglu et al., 2004; Bueno de Mesquita et al., 2003). We then investigate the impact of democratization on several policy areas that should affect the income distribution, for which the impact of democratization. Here, we consider pure fiscal redistribution, pro-poor public goods provision, education, regulatory economic policy, and measures of economic freedom.

As previously mentioned, autocrats typically target relatively small segments of the population will be courted by autocrats to form "winning coalitions" of support (Acemoglu et al., 2004; Bueno de Mesquita et al., 2003). However, the characteristics of the coalition members in autocracies are not the same across autocratic regimes, which cultivate political support according to the relative power of the sub-groups in society and form extreme policies to benefit their coalition members. The policies targeted to the winning coalition can take the form of pure redistribution and public goods provision (in the egalitarian autocracies) or protection of monopoly rights (in the elite-dominated autocracies) that are likely to affect the degree of inequality. For instance if the winning coalition includes the relatively poor, it is likely that the government provides some targeted public goods, such as basic

¹⁷First, we consider a binary variable that takes value one if the Polity index jumped at least two points. We also consider the composite index constructed with a machine learning algorithm developed by Gründler and Krieger (2016) in its continuous form and also in a binary coding.

health services or primary education, that benefit disproportionately the poor segment of the population.

Upon democratization, leaders must establish broader coalitions of political support than those necessary to maintain power in an autocracy. As democratic governance is more inclusive and autocratic coalitions tend to court the extremes, the broader democratic coalitions naturally expand to the middle if it was not previously the case under autocracy. Thus, in practice, the critical member of the (more inclusive) winning coalition in a democracy is likely to be more central in the distribution of policy preferences than previously in autocracies that were governing extreme outcomes. Since excluded members of the coalition are very different across autocracies (high and low inequalities), one should expect very different policy modifications to satisfy the center following democratization given the fact that the center is being approached from polar starting points. Thus, if the degree of inequality is a good proxy for the nature of the autocratic coalition, one should expect very different policies to emerge following democratization in countries whose initial level of inequality was different. Put differently, those policies that are pursued in new democracies should be very different depending on whether the autocratic coalition was pro-poor (low inequality) or pro-elite (high inequality).

Therefore, this sub-section provides an empirical investigation into the extent to which policy outcomes react to democratization heterogeneously according to the pre-democracy level of income inequality. For expositional clarity, we provide a series of sub-sample regression analyses where we split the sample into low and high inequality countries. We split the sample with respect to initial, pre-democracy inequality levels at the calculated (from column 3 of Table 4) pre-democracy inequality level at which the estimated impact of democratization on the Gini coefficient switches from positive to negative (about 38.5). The dependent variables that we investigate are the policy areas that may be (heterogeneously) affected by democratization.

Redistribution and public goods. Contrary to prevailing wisdom, we do not expect democratization to affect inequalities through a pure fiscal redistribution mechanism given the fact that we have previously observed that the net and market Gini coefficients seem to respond to democratization by very similar magnitudes. To formalize the investigation, we estimate the extent to which fiscal redistribution is affected by democratization. As a measure of fiscal redistribution, we follow the recommendation of Solt (2009) and calculate the difference between the market Gini coefficient and the net Gini coefficient (after taxes and transfers). The first two columns of Table 11 demonstrate that the level of direct fiscal redistribution is not affected by democratization, for neither the low nor the high inequality

	Redist	ribution	State c	capacity	Infant ı	mortality
	Low Gini	High Gini	Low Gini	High Gini	Low Gini	High Gini
	(1)	(2)	(3)	(4)	(5)	(6)
democracy _{t-1}	0.2787	-0.0922	0.0051	0.0310**	0.1185	-0.7060***
	(0.326)	(0.462)	(0.018)	(0.013)	(0.096)	(0.192)
log GDP per capita $_{t-1}$	-0.0220	-1.1545	-0.0068	0.0081	0.9838***	0.5676**
	(0.514)	(0.765)	(0.019)	(0.029)	(0.267)	(0.285)
Country & year FE's	yes	yes	yes	yes	yes	yes
Lagged DV's	1	1	1	1	1	1
Within-R ²	0.8298	0.8099	0.6799	0.5452	0.9979	0.9973
N	1185	713	1998	2080	2124	2093
Countries	52	43	92	98	99	102
Democracy changes	9	13	23	49	26	49

Table 11: Effects of democracy on redistribution and public goods

Notes: Robust standard errors clustered by country are in parentheses. Redistribution data is available over the period 1976 - 2010. The state capacity data is available over the period 1962 - 2012. The infant mortality rate data is available over the period 1963 - 2012. *** / ** / * represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.

sub-samples.¹⁸

As direct fiscal redistribution does not seem to be driving the middle ground effect, we proceed to investigate other potential channels that are likely to affect inequality and that may have been affected by democratization differentially depending on the degree of inequality prior democratization. First, and perhaps most generally, democracies tend to invest in state capacity, in the sense that states with greater capacity are more able to make citizens pay taxes. We use the measure of Arbetman-Rabinowitz et al. (2014) as our measure for state capacity (as in Acemoglu et al. 2015). It corresponds to the tax revenue to GDP ratio compared to what would be predicted by development level and other characteristics of a country. Columns 3 and 4 of Table 11 indicate that democratization increases state capacity in the high inequality sub-sample, but there is no effect in the low inequality subsample. We interpret this as support for the notion that governments are providing some pro-poor public goods that reduce inequalities in the high inequality sub-sample, where the poor may have been previously excluded from the winning coalition under autocracy. Our next two results, on public health and education, confirm this interpretation.

There is existing evidence of an unconditional impact of democratization on public health, proxied by infant mortality (Acemoglu et al., 2017; Besley and Kudamatsu, 2006), and educational outcomes (Acemoglu et al., 2015; Lindert, 1994, 2004). We also investigate infant mortality rates, using data from the WDI, but find a heterogeneous effect of democratization. Columns 5 and 6 of Table 11 show that democratization improves this measure

 $^{^{18}}$ We are grateful to an anonymous referee for suggesting this exercise.

	Primary	education	Secondary	education	Tertiary	education
	Low Gini	High Gini	Low Gini	High Gini	Low Gini	High Gini
	(1)	(2)	(3)	(4)	(5)	(6)
democracy $_{t-1}$	0.7484	1.4539***	-0.0258	0.2672	0.3839	0.2989
	(0.466)	(0.477)	(0.449)	(0.351)	(0.239)	(0.203)
log GDP per capita $_{t-1}$	-0.2785	-0.9578	-0.4550	0.8329	-0.3119	0.5351
	(0.823)	(0.832)	(0.687)	(0.574)	(0.684)	(0.371)
Country & year FE's	yes	yes	yes	yes	yes	yes
Lagged DV's	1	1	1	1	1	1
Within-R ²	0.8701	0.9023	0.9258	0.9755	0.9839	0.9713
N	1710	1478	1575	1207	1522	996
Countries	93	91	93	87	84	85
Democracy changes	21	42	22	30	20	30

Table 12: Effects of democracy on education

Notes: Robust standard errors clustered by country are in parentheses. Educational enrollment rates are avalablee over the period 1972 – 2012. *** / ** / * represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.

of public health only for the high inequality sub-sample, which supports the notion that democratization improves pro-poor policies in the autocracies that previously excluded the poor.

Concerning education, we focus on school enrollment rates in primary, secondary, and tertiary education from UNESCO. In Table 12, we find that primary school enrollment rates increase following democratization only for the high inequality sub-sample and that there is no effect on secondary school enrollment rates nor on the tertiary school enrollment rates for either sub-sample. Our interpretation of the primary education result is similar to that of the result on public health, that democratization in high inequality countries broadens that winning coalition to include the poor and pro-poor policies emerge. Improvements in public health and education should enhance market opportunities for the poorest, and the fact that the effect of democratization is more concentrated in the high inequality sub-sample explains reductions in inequality in that sub-sample.

Economic liberalization. We have also examined the extent to which liberalizing political institutions are associated with economic liberalizations 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, especially in economies where opportunities for income growth and the resulting income inequality were suppressed by egalitarian populist policies.We expect that this mechanism could explain the increase in inequality observed in low inequality countries.

Regulatory quality		Property rights		Freedon	n to trade
Low Gini	High Gini	Low Gini	High Gini	Low Gini	High Gini
(1)	(2)	(3)	(4)	(5)	(6)
0.5041***	0.1550	-0.0161	0.2472	0.7237*	0.3564
(0.163)	(0.122)	(0.296)	(0.213)	(0.407)	(0.283)
-0.3533	-0.6245***	-0.4978	-0.3006	-0.3129	-0.7070*
(0.293)	(0.191)	(0.419)	(0.374)	(0.438)	(0.380)
yes	yes	yes	yes	yes	yes
1	1	1	1	1	1
0.6992	0.5189	0.5042	0.3189	0.5605	0.5865
329	294	312	288	326	302
66	65	64	64	65	64
21	32	21	32	23	29
	Regulato Low Gini (1) 0.5041*** (0.163) -0.3533 (0.293) yes 1 0.6992 329 66 21	Regulatory quality Low Gini (1) High Gini (2) 0.5041*** 0.1550 (0.163) (0.122) -0.3533 -0.6245*** (0.293) (0.191) yes yes 1 1 0.6992 0.5189 329 294 66 65 21 32	Regulatory quality Proper Low Gini High Gini Low Gini (1) (2) (3) 0.5041*** 0.1550 -0.0161 (0.163) (0.122) (0.296) -0.3533 -0.6245*** -0.4978 (0.293) (0.191) (0.419) yes yes yes 1 1 1 0.6992 0.5189 0.5042 329 294 312 66 65 64 21 32 21	$\begin{tabular}{ c c c c c c c } \hline Regulatory quality & Property rights \\ \hline Low Gini & High Gini \\ (1) & (2) & Low Gini & High Gini \\ (3) & (4) & (4) & (3) & (4) & (4) & (6$	$\begin{tabular}{ c c c c c c c c c c c c c c c c c c c$

Table 13: Effects of democracy on economic freedoms

Notes: Robust standard errors clustered by country are in parentheses. Regulatory quality indexes are available in five year panels over the period 1975 - 2012. The democracy variable switches to one if a country switches in any of the years during the five year panel. *** / ** / * represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.

We will use data from the Fraser Institute on the degree of regulation on credit, labor, and good markets (proxies for barriers to entry); the strength of property rights protection; and the extent to which domestic actors are "free to trade" internationally. For all three indicators, higher values indicate more free market policy environments, i.e., greater economic freedom. For regulatory quality and freedom to trade, the results of Table 13 confirm our intuition that increases in inequalities in the egalitarian autocracies upon political liberalization may have been driven by an accompanying economic liberalization. For both regulatory quality and freedom to trade internationally, democratization was associated with greater freedom of economic opportunity for the low inequality sub-sample, but there was no effect in the high inequality sub-sample. There was, however, no impact on the protection of property rights for either of the sub-samples.

On the whole, our results concerning the channels through which democratization impacts income inequality reveal that just as the impact on inequality levels is conditional upon the initial degree of inequality, so too are the policy channels through which the effect operates. Autocracies that were dominated by the elite see democratization expand the winning political coalitions to include the poor and a shift towards more pro-poor policies, which increases the incomes of the poor and reduces inequality levels. Our results indicate that this is accomplished through a redistribution of market opportunities, as opposed to a direct fiscal redistribution as is often supposed. On the other hand, pro-poor egalitarian autocracies see democratization expand the winning political coalitions to include more higher class interests and a shift towards more free market policies, which allows for income growth and an increase in inequality levels. On the whole, we interpret the impact of democratization to be an expansion of economic opportunities towards the previously excluded segment of society, which increases the market potential of those segments and drives the middle ground effect on income inequality levels that we have demonstrated in Section 3.

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 income inequality, which are confirmed by Gründler and Krieger (2016) and Knutsen (2015).

Acemoglu et al. (2015) do, 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 some evidence of a heterogeneous effect with regard to the distribution of land and with regard to the agricultural share.¹⁹ 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. Furthermore, we have pursued an instrumental variables strategy and demonstrated that the conditional effect of democracy on income inequality can be interpreted causally.

In our investigation of the channels through which democratization may affect inequality, the result that direct fiscal transfers do not increase upon a transition to democracy is consistent with existing evidence for England and European countries provided by Aidt et al. (2006) and Aidt et al. (2010). As in our study, those authors find that democracy increases the amount of public expenditure on education and health. We push the analysis further by investigating a conditional effect and focus on a broader range of countries.

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

 $^{^{19}}$ See also Ansell and Samuels (2014) for additional evidence that differential relationships between democracy and land versus income inequality.

Richard (1981), where democracies with greater inequality redistribute more. In the democratization model, higher inequalities affect the probability of a transition and are associated with greater redistribution if the country democratizes.

We have demonstrated that inequality *does* fall following democratization, but on average, only in countries where inequality was initially 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. Furthermore, the mechanism that drives reductions in inequality in the high inequality countries is not direct fiscal redistribution, as supposed by redistributionist models of transition, such as Acemoglu and Robinson (2001) and Boix (2003). The result that democratization does not affect inequality through a purely redistributive channel is consistent with an emerging literature that suggests that the primary motive for democratization is *not* purely redistributionist (Aidt and Jensen, 2009; Ansell and Samuels, 2014; Haggard and Kaufman, 2012; Knutsen and Wegmann, 2016).²⁰ However, the fact that highly unequal countries become more equal through other fiscal mechanisms or due to changes in market opportunities does not invalidate the mechanism highlighted by previous models in which inequality is the grievance. As a result we see our contribution as a confirmation that this grievance may describe well the pressures on autocrats to democratize. We show, however, that this grievance does not concern all countries but only a subgroup whose initial inequality levels were relatively high.

In the theory of democratization presented by Ansell and Samuels (2014), high inequality autocracies democratize as an upper middle class emerges in the early stages of urbanized economic development, reducing Gini coefficients. In their story, property rights protections are the issue of contention. While, we do not find that property rights are affected in a statistically significant way, our general finding that democratization affects the income distribution through enhanced access to market opportunities seems most consistent with the Ansell and Samuels (2014) account of democratization.

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

 $^{^{20}}$ Relatedly, Mulligan et al. (2004) also finds that democratic countries do not pursue more redistributive policies than non-democratic countries.

middle of the distribution of preferences. Allowing for the effect of democracy to be conditional on pre-democracy inequality levels, we 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.

Moreover, we have provided an initial empirical venture into the mechanisms that are driving the result and demonstrated a heterogeneity there too. In autocratic countries that were initially egalitarian, democratization leads to economic liberalization, both domestically and internationally, which may be leading to increased inequality following a switch to democracy in those countries. On the other hand, in autocratic countries that were initially unequal societies, democratization seems to improve the state's capacity, in general, and it's provision of pro-poor public goods, in particular. Both of these mechanisms would be consistent with our broader theoretical observation, that policy decisions shift towards a middle ground following a democratization and that income distributions tend to follow. In general, it seems that democratization's impact on income distribution works through a redistribution of economic opportunities, rather than through direct fiscal redistributions that have emphasized in previous theoretical work.

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A For online publication

In this online appendix, we provide some further results, more details on some of our benchmark specifications, and many robustness tests.

A.1 Further results

Democratic breakdowns. We have also considered the effect of democratic breakdowns, which we code as when the country switches from a democratic classification (according to our baseline coding) to an autocratic classification. An instrument is similarly created for regional share of countries that are autocratic. The results in Table A1 are mixed in terms of the statistical significance of the effect of a democratic breakdown. When the conditional effect of an autocratic breakdown is statistically significant, the impact conditional on initial inequality levels is the opposite as for a democratization. Interestingly, and in line with our middle-ground intuition, income distributions become more extreme following a democratic breakdown: relatively unequal societies become more unequal, while relatively equal societies become more equal.

Autocratic regime type. Additionally, we have checked that our results are not being driven by the transition experiences of certain types of autocratic regimes. Using the database of Geddes et al. (2014) on autocratic breakdown and transitions, we have coded autocratic regimes into four broad binary variables: military rule, monarchical, party rule, and personal rule. We then interact the democracy indicator with the lagged binary regime indicator to investigate the extent to which the regime type matters for inequality dynamics. Table A2 demonstrates that regime type does not matter and the coefficients on the interaction term with initial inequality levels remain stable and statistically significant.

Instrumenting for initial level of inequality. 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. Table A3 pursues this analysis and results are broadly consistent with the results presented in the core of the paper, though must be interpreted

with caution due to the historical nature of the inequality instrument. Details of this analysis are presented in a later subsection of the appendix.

A.2 Further robustness tests

First stage results. First, we present the first-stage estimates from our key specifications (Tables 4 and 9) in Tables A4 and A5.

Simplified Interactions. In Table A6, for transparency, we employ simpler constructions of the interaction term. In columns 1 - 2, 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 3 - 4, 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.

Democracy indicators. We have also conducted our baseline analysis with other alternative democracy indicators. In Table A7, we consider (i) a binary indicator that takes value one when the Polity2 index increases by at least two points and zero otherwise, (ii) a binary indicator that takes value one when the machine learning indicator developed by Gründler and Krieger (2016) takes value greater than the midpoint of 0.5 and takes value zero otherwise, and (iii) simply taking Grunder and Kreiger's indicator as a continuous variable.²¹ Results are shown to be robust to estimation with these alternative democracy indicators.

The long-run effect. We investigated an alternative method for calculating the long-run effect. Table A8 looks at 10 year panels to more directly estimate the long-run effect, rather than relying on the linear extrapolation of the immediate impact of democratization that is implicit in our baseline analysis. As can be seen in the estimation, the long-run effect is not being driven by the extrapolation of the immediate effect.

Five-year panels. We re-estimate most of the specifications we have presented in the paper 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 more volatile across the various specifications. Nevertheless, the main themes from the annual panels hold up. In online appendix Tables A9 – A13, we show that in the five-year panels: (i) there is no statistically significant unconditional effect of democratic switches; (ii) the effect of democratic switches conditional on initial inequality

²¹For constructing the interaction terms, we follow the method developed above for the binary variables that rely on the discreet jump and threshold crossing criteria, respectively. For the continuous measures, we simply interact the indicators with the lagged value of the net Gini coefficient.

levels is highly statistically significant; (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; and (v) the results are robust to using market data, to intuitive sample restrictions, to estimation with alternative democracy indicators, and to the inclusion of further time-varying controls. Figure 5 provides a visual presentation of the conditional effect of democratization for the baseline OLS specification.

GMM-type regressions. Finally, since fixed effects regression estimates can be biased by the inclusion of lagged dependent variables (Nickel, 1981), in the online appendix we also estimate 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. Using the Arellano-Bond GMM estimator, we estimate our baseline specification with both annual and five year panels, using both net and gross income inequality measures, in Table A14. 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 qualitatively robust to this alternative estimation technique, though we note that our concern of a lagged dependent variable bias is only minor, as our annual panel regressions are for panels of around fifty years.

A.3 Instrumenting for pre-democracy inequality as well.

In Table A3 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 is strongly negatively correlated with \overline{Gini} (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:

$$D(0,1)_{i,t} = \zeta Gini_{t-1} + \eta_1 Z_{i,t} + \eta_2 Z_{i,t} \times Ag_ratio_i + \theta GDP_{i,t-1} + \gamma_i + \delta_t + e_{i,t} D(0,1)_{i,t} \times \overline{Gini}_i = \zeta Gini_{t-1} + \eta_1 Z_{i,t} + \eta_2 Z_{i,t} \times Ag_ratio_i + \theta GDP_{i,t-1} + \gamma_i + \delta_t + e_{i,t}.$$
(A.1)

We use the fitted values from equations (A.1) 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 income inequality (especially net). Nevertheless, the results in Table A3 are encouraging. In column 1 Ag_ratio is used to instrument for the interaction term with our baseline coding for the initial inequality level, whereas in column 2 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} in the interaction term, but we note that the joint effect remains statistically significant. In columns 3 – 4, 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 3 – 4 are the following:

$$D(0,1)_{i,t} = \zeta Gini_{t-1} + \eta_1 Z_{i,t} + \eta_2 Z_{i,t} \times Ag_ratio_i + \theta GDP_{i,t-1} + \gamma_i + \delta_t + e_{i,t} D(0,1)_{i,t} \times Ag_ratio_i = \zeta Gini_{t-1} + \eta_1 Z_{i,t} + \eta_2 Z_{i,t} \times Ag_ratio_i + \theta GDP_{i,t-1} + \gamma_i + \delta_t + e_{i,t}.$$
(A.2)

	C	DLS	2	SLS		
	(1)	(2)	(3)	(4)		
democracy breakdown $_{t-1}$	-0.1486 (0.131)	-0.8642** (0.359)	-0.4055 (0.591)	-4.7924 (4.033)		
$breakdown_{t-1} imes \overline{gini}$		0.0177* (0.009)		0.1403 (0.147)		
log GDP per capita $_{t-1}$	0.3506* (0.184)	0.3379* (0.184)	0.4070* (0.235)	0.0870 (0.519)		
Country & year fixed effects	yes	yes	yes 4	yes		
Excluded instruments within-R ²	- 0.8923	- 0.8854	2	3		
Joint F-test p-value	-	0.0278	_	0.2066		
C-D F-stat on excluded IV's Hansen J-stat p-value	_	_	36.322 0.4920	4.139 0.7464		
N Countries	3253 143	3253 143	3197 141	3197 141		
Democratic breakdowns Long-run effect at 10th p. Gini	54 -1.25	54 -3.41	54 -3.35	54 -8.74		
Long-run effect at 90th p. Gini	-1.25	1.47	-3.35	24.20		

Notes: Robust standard errors clustered by country are in parentheses. Stock-Yogo weak identification test for the set of instruments has critical values for 10% / 25% maximal IV size are 13.43 / 5.45 for 2SLS specifications with three excluded instrument and are 19.93 / 7.25 for 2SLS specifications with two excluded instruments. Referring to the Cragg-Donald (C-D) F-statistic, the test's null hypothesis that the set of instruments is weak is easily rejected. The Hansen J-statistic tests for exogeneity of the set of instruments. The null of the Hansen test, that the set of instruments is exogenous, cannot be rejected. The panel runs from 1964 - 2010for all specifications. *** / ** / * represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.

	OLS		2	SLS	
	(1)	(2)	(3)	(4)	
democracy $_{t-1}$	-0.0780 (0.112)	1.2751*** (0.285)	0.1980 (0.285)	1.7194*** (0.476)	
$democracy_{t-1}\times \overline{gini}$		-0.0335*** (0.007)		-0.0426*** (0.010)	
log GDP per capita $_{t-1}$	0.3138* (0.180)	0.1948 (0.187)	0.3409* (0.201)	0.1863 (0.207)	
$democracy_{t-1} \times military_{t-2}$	-0.1953 (0.194)	-0.0986 (0.197)	-0.3169 (0.233)	-0.1029 (0.236)	
$democracy_{t-1} \times monarchy_{t-2}$	0.0144 (0.146)	-0.0387 (0.223)	-0.0968 (0.193)	-0.0862 (0.267)	
$democracy_{t-1} \times party_{t-2}$	-0.1108 (0.323)	-0.1517 (0.309)	-0.2499 (0.334)	-0.2005 (0.325)	
Country & year fixed effects Lagged DV's	yes 4	yes 4	yes 4	yes 4	
Excluded instruments within-B ²	_ 0 8851	_ 0 8865	2	3	
Joint F-test p-value	-	0.0000	_	0.0001	
C-D F-stat on excluded IV's Hansen J-stat p-value	_		171.518 0.5047	102.360 0.5309	
N	3248	3248	3198	3198	
Countries	143 -0.67	143 3 29	141 1 70	141 5.06	
Long-run effect at 90th p. Gini	-0.67	-6.21	1.70	-7.11	

Table A2: Effects of democracy on the Gini coefficient - controlling for regime type

Notes: Robust standard errors clustered by country are in parentheses. Stock-Yogo weak identification test for the set of instruments has critical values for 10% / 25% maximal IV size are 13.43 / 5.45 for 2SLS specifications with three excluded instrument and are 19.93 / 7.25 for 2SLS specifications with two excluded instruments. Referring to the Cragg-Donald (C-D) F-statistic, the test's null hypothesis that the set of instruments is weak is easily rejected. The Hansen J-statistic tests for exogeneity of the set of instruments. The null of the Hansen test, that the set of instruments is exogenous, cannot be rejected. The panel runs from 1964 – 2010 for all specifications. *** / ** / * represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.

		Two Stage L	east Squares	
		Net Gini		Gross Gini
	Baseline interaction	Simple interaction	Reduced form	Reduced form
	(1)	(2)	(3)	(4)
democracy _{t-1}	17.6085* (9.861)	21.0661 (13.894)	10.4580** (4.413)	29.0401*** (9.041)
$democracy_{t-1}\times \overline{gini}$	-0.3983 (0.273)	-0.4678 (0.365)		
$democracy_{t-1} {\times} gini \ IV$			-0.1823* (0.105)	-0.6725*** (0.231)
log GDP per capita $_{t-1}$	0.3023 (1.408)	0.4418 (1.529)	1.2695 (1.070)	2.8576* (1.650)
Country & year fixed effects Joint F-test p-value Excluded instruments C-D F-stat on excluded IV's Hansen J-test p-value N	yes 0.0701 3 27.592 0.3704 3706	yes 0.1321 3 25.638 0.3321 3696	yes 0.0336 3 99.882 0.2308 3696	yes 0.0048 3 88.012 0.1122 3623
Countries Democratic changes Long-run effect at 10th p. Gini Long-run effect at 90th p. Gini Years	147 67 6.88 -5.96 1961 – 2010	147 65 8.47 -6.61 1961 – 2010	147 65 5.55 -0.33 1961 – 2010	147 64 10.93 -10.75 1961 – 2010

Table A3: Effects of democracy on the Gini coefficient, instrumenting for initial inequality levels as well

Notes: Robust standard errors clustered by country are in parentheses. Stock-Yogo weak identification test for the set of instruments has critical values for 10% / 25% maximal IV size are 13.43 / 5.45 for the 2SLS specifications. Referring to the Cragg-Donald (C-D) F-statistic, the test's null hypothesis that the set of instruments is weak is easily rejected. The Hansen J-statistic tests for exogeneity of the set of instruments. The null of the Hansen test, that the set of instruments is exogenous, cannot be rejected. The panel runs from 1964 – 2010 for all specifications. *** / ** / * represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.

	Column (5)	Column (6)	Colur	(Z) UL	Colu	mn (8)
	democracy $_{t-1}$	democracy $_{t-1}$	democracy $_{t-1}$	$dem_{t-1}\times \overline{gini}$	democracy $_{t-1}$	$dem_{t-1}\times \overline{gini}$
regional share $_{t-1}$	0.2584**	0.2877**	0.4162	-27.5589**	0.4959	-25.2796*
1	(0.103)	(0.111)	(0.309)	(11.306)	(0.355)	(12.898)
regional share $_{t-2}$	0.4715***	0.4197***	0.4733***	16.6709***	0.4218***	14.9829***
	(0.099)	(0.098)	(0.099)	(3.941)	(0.098)	(4.127)
regional share $_{t-1} imes \overline{gini}$			-0.0040	0.9460***	-0.0053	0.8957***
			(0.007)	(0.283)	(0.008)	(0.315)
log GDP per capita $_{t-1}$	-0.0074	-0.0258	-0.0172	-1.1936	-0.0396	-2.2648
	(0.094)	(0.114)	(0.096)	(3.904)	(0.116)	(4.693)
$gini_{t-1}$	-0.0030	-0.0047	-0.0022	-0.2030	-0.0036	-0.3372
	(0.003)	(0.005)	(0.004)	(0.177)	(0.007)	(0.280)
$gini_{t-2}$		0.0010			0.0013	0.0379
		(0.003)			(0.003)	(0.142)
$gini_{t-3}$		-0.0005			-0.0008	-0.0205
		(0.002)			(0.002)	(0.117)
$gini_{t-4}$		0.0015			0.0015	0.1437
		(0.004)			(0.004)	(0.185)
Country & year fixed effects	yes	yes	yes	yes	yes	yes
within-R ²	0.2893	0.2959	0.2901	0.2820	0.2973	0.2915
Joint F-test p-value on excl.IV's	0.0000	0.0000	0.0000	0.0000	0.0001	0.0000
Excluded instruments	0	N	ო	ო	ო	ო
Z	3700	3198	3700	3700	3198	3198
Countries	151	141	151	151	141	141
Years	1962 – 2010	1964 - 2010	1962 – 2010	1962 – 2010	1964 - 2010	1964 – 2010
Notes: Robust standard errors cluster	ed by country are in I	parentheses. *** / **	/ * represent significa	nce at the 0.01 / 0.05	5 / 0.10 levels, respec	tively.

Table A4: Effects of democracy on the net Gini coefficient – First stage

	Colu	ımn (1)	Colt	10 (Z) umr	Col	umn (3)	Col	umn (4)	Col	umn (5)	Colt	mn (6)
	dem_{t-1}	$dem_{t-1}\times \overline{gini}$										
regional share $_{t-1}$	0.1865	-33.2939**	0.1749	-33.7304**	0.4990	-24.7203*	0.5016	-26.1423*	0.4493	-25.5686**	0.1787	-34.6670**
	(0.380)	(13.697)	(0.374)	(13.606)	(0.346)	(12.584)	(0.363)	(13.575)	(0.319)	(12.352)	(0.348)	(13.702)
regional share $_{t-2}$ (0.3955***	14.2895***	0.3920***	14.1692***	0.4270***	15.0028***	0.3997***	14.4238***	0.3936***	14.3398***	0.3865***	14.4691***
	(0.100)	(4.258)	(0.099)	(4.252)	(0.097)	(4.046)	(0.094)	(4.074)	(960.0)	(3.864)	(0.092)	(3.912)
regional share $_{t=1} imes \overline{gini}$	0.0016	1.0712***	0.0019	1.0826***	-0.0052	0.8921***	-0.0047	0.9275***	-0.0039	0.9142***	0.0022	1.0977***
	(0.00)	(0.342)	(600.0)	(0.341)	(0.008)	(0.312)	(0.008)	(0.332)	(0.008)	(0.305)	(0.008)	(0.339)
log GDP per capita $_{t-1}$	-0.0851	-4.0231	-0.0907	-4.2392	-0.0271	-2.0188	-0.0625	-3.4302	-0.0446	-2.4730	-0.0711	-3.8851
	(0.130)	(5.190)	(0.130)	(5.223)	(0.112)	(4.660)	(0.118)	(4.801)	(0.116)	(4.727)	(0.128)	(5.233)
$exports_{t-1}$	0.0013	0.0415									-0.0070**	-0.2749
	(0.001)	(0.055)									(0.004)	(0.172)
total trade $_{t-1}$			0.0014*	0.0473							0.0051 **	0.1912*
			(0.001)	(0.034)							(0.002)	(0.101)
$population_{t-1}$					-0.0000	-0.0000					-0.0000**	-0.0000
					(0000)	(0000)					(000.0)	(0000)
regional conflict $_{t-1}$							0.2681	8.3261			0.4245*	13.3930
							(0.249)	(9.487)			(0.238)	(9.157)
regional mean gini $_{t-1}$									0.0084	0.1481	0.0020	-0.0262
									(00.00)	(0.310)	(600.0)	(0.361)
Country & year FE's	yes	yes										
Gini coeff. lags	4	4	4	4	4	4	4	4	4	4	4	4
within-R ²	0.2699	0.2839	0.2731	0.2859	0.2999	0.2918	0.2999	0.2903	0.2992	0.2909	0.2920	0.2956
Joint F p-value on excl.IV's	0.0006	0.0001	0.0006	0.0001	0.0001	0.0000	0.0001	0.0001	0.0001	0.0000	0.0003	0.0001
Excluded instruments	e	ო	e	ო	ო	ю	ო	ო	ო	ო	ო	ო
Z	3086	3086	3086	3086	3198	3198	2901	2901	3198	3198	2789	2789
Countries	140	140	140	140	141 141	141	141	141	141	140	140	

Table A5: Effects of democracy on the net Gini coefficient with other time varying controls- First stage

	Fixed initial inequality interaction		Once lagged initial inequality interaction	
	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)
democracy _{t-1}	1.0001***	1.6216***	0.9333***	1.4171**
	(0.348)	(0.552)	(0.348)	(0.583)
$democracy_{t-1} imes \overline{gini}$	-0.0268*** (0.008)	-0.0385*** (0.013)		
$democracy_{t-1} \times gini_{t-1}$			-0.0252*** (0.008)	-0.0330** (0.014)
log GDP per capita $_{t-1}$	0.2325	0.2170	0.2380	0.2358
	(0.191)	(0.217)	(0.194)	(0.221)
Country & year fixed effects	yes	yes	yes	yes
Lagged DV's	4	4	4	4
Excluded instruments	–	3	–	3
within-R ² Joint F-test p-value	0.8858 0.0014	- 0.0081 101.034	0.8857 0.0035	- 0.0425 102 574
Hansen J-stat p-value	_	0.5218		0.5289
	3251	3198	3251	3198
Countries	143	141	143	141
Democratic changes	52	52	52	52
Long-run effect at 10th p. Gini	2.60	5.72	2.38	5.05
Long-run effect at 90th p. Gini	-5.51	-6.45	-5.15	-5.09

Notes: Robust standard errors clustered by country are in parentheses. Stock-Yogo weak identification test for the set of instruments has critical values for 10% / 25% maximal IV size are 13.43 / 5.45 for the 2SLS specifications. Referring to the Cragg-Donald (C-D) F-statistic, the test's null hypothesis that the set of instruments is weak is easily rejected. The Hansen J-statistic tests for exogeneity of the set of instruments. The null of the Hansen test, that the set of instruments is exogenous, cannot be rejected. The panel runs from 1964 – 2010 for all specifications. *** / ** / * represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.

Table A7: Effects of democracy on the net Gini coefficient with alternative democracy indicators

		Ordinary L	east Squares		
Δ Polit	ty IV \ge 2	Machin	e binary	Machine of	continuous
(1)	(2)	(3)	(4)	(5)	(6)
-0.0027	1.4176**	0.0198	0.9532***	-0.0219	1.2315**
(0.140)	(0.642)	(0.098)	(0.307)	(0.173)	(0.518)
	-0.0344** (0.015)		-0.0225*** (0.008)		-0.0313** (0.013)
0.3166*	0.3003	0.7349***	0.7235***	0.7364***	0.7005***
(0.186)	(0.185)	(0.217)	(0.219)	(0.216)	(0.216)
yes	yes	yes	yes	yes	yes
4	4	4	4	4	4
0.8853	0.8851	0.9037	0.9045	0.9037	0.9042
	0.0706	—	0.0076	—	0.0576
3129	3129	2656	2656	2656	2656
134	134	142	142	142	142
-0.02	4.29	0.16	2.73	-0.06	0.46
-0.02	-5.40	0.16	-2 94	-0.06	-0.73
	$\begin{tabular}{ c c c c c }\hline & Δ Polition $$ (1)$ \\\hline (1)$ \\\hline (0.0027$ (0.140)$ \\\hline (0.140)$ \\\hline (0.186)$ \\\hline yes$ \\ 4$ \\0.8853$ \\ -$ \\3129$ \\134$ \\-0.02$ \\-0.02$ \\\hline (0.02)$ \hline\hline (0.02)$ \\\hline (0.02)$ \hline\hline (0.02)$ \\$	$\begin{tabular}{ c c c c }\hline Δ Polity $IV \ge 2$ \\\hline (1) (2) \\\hline -0.0027 1.4176^{**} \\\hline (0.140) (0.642) \\\hline -0.0344^{**} \\\hline (0.015) \\\hline 0.3166^{*} 0.3003 \\\hline (0.186) (0.185) \\\hline 0.3166^{*} 0.3003 \\\hline (0.186) (0.185) \\\hline 0.3166^{*} 0.3003 \\\hline (0.186) (0.185) \\\hline 0.3166^{*} 0.3003 \\\hline (0.185) 0.8851 \\\hline $-$ 0.0706 \\\hline 3129 3129 \\\hline 134 134 \\\hline -0.02 4.29 \\\hline -0.02 -5.40 \\\hline \end{tabular}$	$\begin{tabular}{ c c c c } \hline & & Ordinary L\\ \hline \Delta \ \mbox{Polity IV} \ge 2 & Machin \\ \hline (1) & (2) & (3) \\ \hline (0.0027 & 1.4176^{**} & 0.0198 \\ (0.140) & (0.642) & (0.098) \\ & & -0.0344^{**} \\ & (0.015) & \\ \hline 0.3166^* & 0.3003 & 0.7349^{***} \\ (0.186) & (0.185) & (0.217) \\ \hline yes & yes & yes \\ 4 & 4 & 4 \\ 0.8853 & 0.8851 & 0.9037 \\ \hline & & - & 0.0706 & - \\ 3129 & 3129 & 2656 \\ 134 & 134 & 142 \\ -0.02 & 4.29 & 0.16 \\ -0.02 & -5.40 & 0.16 \\ \hline \end{tabular}$	$\begin{tabular}{ c c c c c } \hline Ordinary Least Squares \\ \hline \Delta \ {\sf Polity} \ {\sf IV} \ge 2 & Machine \ binary \\ \hline (1) & (2) & (3) & (4) \\ \hline (0.0027 & 1.4176^{**} & 0.0198 & 0.9532^{***} \\ (0.140) & (0.642) & (0.098) & (0.307) \\ & & -0.0344^{**} & -0.0225^{***} \\ & (0.015) & (0.008) \\ \hline 0.3166^{*} & 0.3003 & 0.7349^{***} & 0.7235^{***} \\ & (0.186) & (0.185) & (0.217) & (0.219) \\ \hline yes & yes & yes & yes \\ 4 & 4 & 4 & 4 \\ \hline 0.8853 & 0.8851 & 0.9037 & 0.9045 \\ \hline - & 0.0706 & - & 0.0076 \\ \hline 3129 & 3129 & 2656 & 2656 \\ \hline 134 & 134 & 142 & 142 \\ \hline -0.02 & 4.29 & 0.16 & 2.73 \\ \hline - & 0.02 & -5.40 & 0.16 & -2.94 \\ \hline \end{tabular}$	$\begin{tabular}{ c c c c c } \hline Ordinary Least Squares \\ \hline \Delta \ \mbox{Polity IV} \ge 2 & Machine binary & Machine of (1) (2) & (3) (4) & (5) \\ \hline (1) (2) & (3) (4) & (5) \\ \hline (0,0027 & 1.4176^{**} & 0.0198 & 0.9532^{***} & -0.0219 \\ (0.140) & (0.642) & (0.098) & (0.307) & (0.173) \\ & & -0.0344^{**} & -0.0225^{***} \\ & & (0.015) & (0.008) \\ \hline 0.3166^{*} & 0.3003 & 0.7349^{***} & 0.7235^{***} & 0.7364^{***} \\ (0.186) & (0.185) & (0.217) & (0.219) & (0.216) \\ \hline yes & yes & yes & yes & yes \\ 4 & 4 & 4 & 4 & 4 \\ 0.8853 & 0.8851 & 0.9037 & 0.9045 & 0.9037 \\ & - & 0.0706 & - & 0.0076 & - \\ & 3129 & 3129 & 2656 & 2656 & 2656 \\ & 134 & 134 & 142 & 142 & 142 \\ & -0.02 & 4.29 & 0.16 & 2.73 & -0.06 \\ & -0.02 & -5.40 & 0.16 & -2.94 & -0.06 \\ \hline \end{tabular}$

Notes: Robust standard errors clustered by country are in parentheses. For the Polity IV results in the first two columns, the panel runs from 1965 – 2010. For the machine learning indicator results in the last four columns, the panel runs from 1982 – 2010. For the continuous measures of democracy, the long-run effects are calculated for a within-sample one-standard deviation of the machine learning indicator. *** / ** / * represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.



Figure 5: 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 2 of Table 5. Dashed lines represent 90% confidence intervals.

		Ten year panels				
	Ol	LS	2SLS			
	(1)	(2)	(3)	(4)		
democracy _{t-10}	-0.4789 (1.150)	7.8428*** (2.639)	2.3254 (2.985)	10.3052** (4.444)		
$democracy_{t-10}\times \overline{gini}$		-0.2065*** (0.072)		-0.2191** (0.111)		
log GDP per capita $_{t-10}$	1.3282 (1.412)	0.5873 (1.336)	2.7215 (2.031)	1.8854 (1.854)		
$gini_{t-10}$	0.0888 (0.060)	0.1392** (0.063)	0.0501 (0.071)	0.0797 (0.075)		
Country & year FE's Lagged DV's Excluded instruments within-R ² Joint F-test p-value C-D F-stat on excluded IV's Hansen J-stat p-value	yes 1 - 0.0531 - - -	yes 1 - 0.1119 0.0138 - -	yes 1 2 - 3.877 0.5232	yes 1 3 - 0.0653 2.139 0.5262		
N Countries Democratic changes Long-run effect at 10th p. Gini Long-run effect at 90th p. Gini Years	274 125 40 -0.53 -0.53 1975 – 2005	274 125 40 2.65 -5.08 1975 – 2005	200 68 35 2.45 2.45 1975 - 2005	200 68 35 4.79 -2.89 1975 – 2005		

Table A8: Effects of democracy on the net Gini coefficient – Alternative method for estimating the long-run effect

Notes: Robust standard errors clustered by country are in parentheses. Stock-Yogo weak identification test for the set of instruments has critical values for 10% / 25% maximal IV size are 13.43 / 5.45 for for 2SLS specifications with three excluded instrument and are 19.93 / 7.25 for 2SLS specifications with two excluded instruments. Referring to the Cragg-Donald (C-D) F-statistic, the test's null hypothesis that the set of instruments is weak is easily rejected. The Hansen J-statistic tests for exogeneity of the set of instruments. The null of the Hansen test, that the set of instruments is exogenous, cannot be rejected. *** / ** / * represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.

0	LS	2SLS	
(1)	(2)	(3)	(4)
0.0260 (0.725)	5.9936*** (1.773)	2.4996 (1.837)	9.5323*** (3.134)
	-0.1490*** (0.044)		-0.2000*** (0.064)
1.8089** (0.813)	1.3627* (0.799)	1.6603 (1.115)	0.9358 (1.080)
0.3609*** (0.067)	0.3819*** (0.064)	0.3543*** (0.070)	0.3764*** (0.067)
yes _ 0.1756 _	yes _ 0.2040 0.0554	yes 2 -	yes 3 - 0.0049
_ _ 659	_ _ 659	28.853 0.4060 589	17.198 0.3398 589
141 60 0.04 0.04	141 60 3.21 -4.56	115 57 3.87 3.87	115 57 6.65 -3.69
	O (1) 0.0260 (0.725) 1.8089** (0.813) 0.3609*** (0.067) yes - 0.1756 - - - - 659 141 60 0.04 0.04 0.04	OLS (1) (2) 0.0260 5.9936*** (0.725) (1.773) -0.1490*** (0.044) 1.8089** 1.3627* (0.813) (0.799) 0.3609*** 0.3819*** (0.067) (0.064) yes yes - - 0.1756 0.2040 - 0.0554 - - 659 659 141 141 60 60 0.04 3.21 0.04 -4.56	$\begin{array}{c c c c c c c c c c c c c c c c c c c $

Table A9: Effects of democracy on the net Gini coefficient, 5-year panels

Notes: Robust standard errors clustered by country are in parentheses. Stock-Yogo weak identification test for the set of instruments has critical values for 10% / 25% maximal IV size are 13.43 / 5.45 for for 2SLS specifications with three excluded instrument and are 19.93 / 7.25 for 2SLS specifications with two excluded instruments. Referring to the Cragg-Donald (C-D) F-statistic, the test's null hypothesis that the set of instruments is weak is easily rejected. The Hansen J-statistic tests for exogeneity of the set of instruments. The null of the Hansen test, that the set of instruments is exogenous, cannot be rejected. The panel runs from 1965 – 2005 for all specifications. *** / ** / * represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.

	0	LS	2SLS		
	(1)	(2)	(3)	(4)	
democracy _{t-5}	0.0136 (0.847)	8.1863*** (2.490)	3.0088 (2.178)	9.0517*** (2.876)	
$democracy_{t=5} imes \overline{gini}$		-0.1858*** (0.055)		-0.1789*** (0.067)	
log GDP per capita $_{t-5}$	2.6325** (1.325)	2.2126* (1.207)	2.4604 (1.628)	1.8433 (1.373)	
$gini_{t-5}$	0.5268*** (0.055)	0.5508*** (0.051)	0.5151*** (0.055)	0.5407*** (0.050)	
Country & year fixed effects Excluded instruments within-R ²	yes _ 0.3186	yes _ 0.3446	yes 2 —	yes 3	
Joint F-test p-value	_	0.0039	_	0.0063	
C-D F-stat on excluded IV's Hansen J-stat p-value	_		27.986 0.4459	13.337 0.4793	
N	650	650	584	584	
Democratic changes	59	59	56	56	
Long-run effect at 90th p. Gini	0.03	4.88 -7.13	6.21	9.22 -3.33	

Table A10: Effects of democracy on the gross Gini coefficient, 5-year panels

Notes: Robust standard errors clustered by country are in parentheses. Stock-Yogo weak identification test for the set of instruments has critical values for 10% / 25% maximal IV size are 13.43 / 5.45 for for 2SLS specifications with three excluded instrument and are 19.93 / 7.25 for 2SLS specifications with two excluded instruments. Referring to the Cragg-Donald (C-D) F-statistic, the test's null hypothesis that the set of instruments is weak is easily rejected. The Hansen J-statistic tests for exogeneity of the set of instruments. The null of the Hansen test, that the set of instruments is exogenous, cannot be rejected. The panel runs from 1965 – 2005 for all specifications. *** / ** / * represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.

	Excluding former USSR		Excl. USSR	& Warsaw Pact	Excluding never autocratic	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
democracy $_{t-5}$	6.0018***	9.4806***	5.7519**	10.7989**	7.9126***	14.4268***
	(1.779)	(3.135)	(2.785)	(5.130)	(1.833)	(3.937)
$democracy_{t-5} imes \overline{gini}$	-0.1490***	-0.1992***	-0.1443**	-0.2354**	-0.1794***	-0.2286**
	(0.044)	(0.064)	(0.063)	(0.100)	(0.045)	(0.090)
$\log \text{GDP} \text{ per capita}_{t-5}$	1.4198*	0.9780	1.3963*	0.7216	1.1977	0.6986
	(0.823)	(1.090)	(0.841)	(1.088)	(0.937)	(1.472)
Country & year FE's	yes	yes	yes	yes	yes	yes
Lagged DV's	1	1	1	1	1	1
within-R ² Joint F-test p-value	0.2023 0.0034	_ 	- 0.1832 0.0677	- 0.0583	0.2421 0.0002	- 0.0011
C-D F-stat on excl. IV's Hansen J-stat p-value	-	17.056 0.3495	-	17.076 0.5682	-	6.446 0.8529
N	626	581	594	551	433	390
Countries	126	111	120	106	100	78
Democracy changes	58	56	54	52	60	57
L-R effect at 90th p. Gini	3.18	6.58	3.01	7.41	4.78	12.05
	-4.50	-3.69	-4.48	-5.20	-4.19	1.31

Table A11: Effects of democracy on the net Gini, alternative samples, 5-year panels

Notes: Robust standard errors clustered by country are in parentheses. Stock-Yogo weak identification test for the set of instruments has critical values for 10% / 25% maximal IV size are 13.43 / 5.45 for the 2SLS specifications. Referring to the Cragg-Donald (C-D) F-statistic, the test's null hypothesis that the set of instruments is weak is easily rejected. The Hansen J-statistic tests for exogeneity of the set of instruments. The null of the Hansen test, that the set of instruments is exogenous, cannot be rejected. The panel runs from 1964 – 2010 for all specifications. *** / ** / * represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.

	Composite – more stringent		Polity IV binary		Polity IV continuous	
_	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)
democracy $_{t-5}$	5.2617*** (1.739)	9.9253*** (3.577)	7.7584*** (1.664)	10.4040*** (3.186)	0.4987*** (0.129)	0.5777*** (0.208)
$democracy_{t-5} imes \overline{gini}$	-0.1386*** (0.040)	-0.2052*** (0.074)	-0.1891*** (0.042)	-0.2089*** (0.064)	-0.0131*** (0.004)	-0.0109** (0.004)
log GDP per capita $_{t-5}$	1.4789* (0.775)	0.9642 (1.053)	1.1980 (0.777)	1.1384 (1.052)	1.2568 (0.947)	1.4980 (1.213)
Country & year FE's	yes	yes	yes	yes	yes	yes
Lagged DV's	1	1	1	1	1	1
Excluded instruments	-	3	-	3	-	3
within-R ²	0.2007	-	0.2214	-	0.1861	-
Joint F-test p-value	0.0033	0.0133	0.0000	0.0028	0.0008	0.0149
C-D F-stat on excl. IV's	-	12.946	-	19.907	_	17.149
Hansen J-stat p-value	-	0.2812	-	0.4043	-	0.6523
Ν	659	589	634	569	596	533
Countries	141	115	133	109	133	105
Democracy changes	46	43	46	44	_	_
L-R effect at 10th p. Gini	2.48	7.02	4.45	7.85	1.47	2.75
L-R effect at 90th p. Gini	-4.75	-3.53	-5.71	-3.21	-2.74	-0.64

Table A12: Effects of democracy on the net Gini coefficient with alternative democracy indicators, 5-year panels

Notes: Robust standard errors clustered by country are in parentheses. Stock-Yogo weak identification test for the set of instruments has critical values for 10% / 25% maximal IV size are 13.43 / 5.45 for the 2SLS specifications. Referring to the Cragg-Donald (C-D) F-statistic, the test's null hypothesis that the set of instruments is weak is easily rejected. The Hansen J-statistic tests for exogeneity of the set of instruments. The null of the Hansen test, that the set of instruments is exogenous, cannot be rejected. The panel runs from 1965 – 2010 for columns 1 – 2 and from 1970 – 2010 for columns 3 – 6. For the continuous measures of democracy, the long-run effects are calculated for a within-sample one-standard deviation of the Polity IV index. *** / ** / * represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.

	Two-stage least squares – Second stage					
	(1)	(2)	(3)	(4)	(5)	(6)
democracy _{t-5}	10.9492*** (3.909)	10.9624*** (3.949)	9.3728*** (3.019)	9.7436*** (3.258)	8.7369** (3.603)	10.0514** (3.963)
$democracy_{t-5}\times \overline{gini}$	-0.2313*** (0.073)	-0.2295*** (0.073)	-0.2012*** (0.063)	-0.2026*** (0.066)	-0.1923*** (0.068)	-0.2228*** (0.076)
log GDP per capita $_{t-5}$	1.4654 (1.285)	1.4248 (1.308)	0.7420 (0.967)	0.9283 (1.112)	0.9192 (1.046)	1.2780 (1.187)
exports/GDP $_{t-5}$	-0.0296 (0.026)					-0.0150 (0.073)
total trade/GDP $_{t-5}$		-0.0151 (0.014)				-0.0062 (0.040)
$population_{t-5}$			0.0000 (0.000)			0.0000 (0.000)
regional share civil $conflict_{t-5}$				1.6665 (3.355)		1.6622 (4.003)
regional mean gini $_{t-5}$					0.0852 (0.112)	0.0851 (0.113)
Country & year FE's Lagged DV's	yes 1	yes 1	yes 1	yes 1	yes 1	yes 1
Excluded instruments	3	3	3	3	3	3
Joint F-test p-value	0.0066	0.0075	0.0043	0.0059	0.0186	0.0130
C-D F-stat on excl. IV's	16.884	16.745	18.031	15.314	15.886	15.999
Hansen J-stat p-value	0.3239	0.3432	0.5296	0.4778	0.2703	0.3729
N	555	555	589	544	589	510
Countries	111	111	115	106	115	102
Democracy changes	54	54	5/	56	57	53
L-R effect at 10th p. Gini L-R effect at 90th p. Gini	7.71 -4.47	7.80 -4.27	6.35 -4.07	6.85 -3.58	5.59 -4.15	6.47 -5.00

Table A13: Effect of democracy on the Gini coefficient, with other time-varying controls, 5-year panels

Notes: Robust standard errors clustered by country are in parentheses. Stock-Yogo weak identification test for the set of instruments has critical values for 10% / 25% maximal IV size are 13.43 / 5.45 for the 2SLS specifications. Referring to the Cragg-Donald (C-D) F-statistic, the test's null hypothesis that the set of instruments is weak is easily rejected. The Hansen J-statistic tests for exogeneity of the set of instruments. The null of the Hansen test, that the set of instruments is exogenous, cannot be rejected. The panel runs from 1965 – 2010 for all specifications. *** / ** / * represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.

	Yearly	panels	5-year panels		
	Net Gini	Gross Gini	Net Gini	Gross Gini	
	(1)	(2)	(3)	(4)	
democracy $_{t-1}$	3.1627***	2.3017**	5.9014*	5.9085*	
	(0.860)	(1.125)	(3.295)	(3.298)	
$democracy_{t-1}\times \overline{gini}$	-0.0760***	-0.0474**	-0.1414*	-0.1218	
	(0.020)	(0.023)	(0.081)	(0.076)	
log GDP per capita $_{t-1}$	-0.0712	0.6331***	-0.3396	2.5608	
	(0.209)	(0.191)	(1.554)	(2.139)	
Year fixed effects	yes	yes	yes	yes	
Joint F-test p-value	0.0012	0.1254	0.1991	0.2036	
Number of instruments	272	272	221	220	
AR(2) p-value	0.268	0.078	0.666	0.916	
N	3781	3706	659	650	
Countries	154	154	141	141	
Number of democracy changes	66	65	60	59	
L-R effect at 10th percentile Gini	32.49	28.22	5.35	7.01	
L-R effect at 90th percentile Gini	-38.90	-22.16	-6.29	-5.51	

Table A14: Effects of democracy on the the Gini coefficient, GMM regressions

Notes: Robust standard errors clustered by country are in parentheses. The panel runs from 1965 - 2010 for all specifications. *** / ** / * represent significance at the 0.01 / 0.05 / 0.10 levels, respectively.