European Journal of Political Economy 70 (2021) 102048

Contents lists available at ScienceDirect

European Journal of Political Economy

journal homepage: www.elsevier.com/locate/ejpe

Inclusive institutions, unequal outcomes: Democracy, state capacity, and income inequality

Hector Bahamonde[b,*], Mart Trasberg[a]

[a] Tulane University, USA
[b] University of Turku, Finland

ARTICLE INFO

Keywords:
Income inequality
Democracy
State capacity
Financial Development
FDI

ABSTRACT

Although the relationship between democratic rule and income inequality has received important attention in recent literature, the evidence has been far from conclusive. In this paper, we explore whether the redistributive effect of democratic rule is conditional on state capacity. Previous literature has outlined that pre-existing state capacity may be necessary for inequality-reducing policies under democratic rule. In contrast to that intuitive view, this study argues that democratic rule and high state capacity combined produce higher levels of income inequality over time. This relationship operates through the positive effect of high-capacity democratic context on foreign direct investment and financial development. By making use of a novel measure of state capacity based on cumulative census administration, we find empirical support for these claims using fixed-effects panel regressions with the data from 126 industrial and developing countries between 1970 and 2013.

1. Introduction

Median voter and selectorate theories posit electoral democracy as fundamentally equalizing (Acemoglu and Robinson, 2006; Boix, 2003; Bueno de Mesquita et al., 2003; Meltzer and Richard, 1981).[1] However, these redistributive propositions have not received support in recent, more empirically-minded literature (Acemoglu et al., 2015; Scheve and Stasavage, 2017; Timmons, 2010; Wong, 2016). The skeptics of the inequality-reducing effects of democratic institutions have noted that deficiencies in mechanisms of responsiveness and accountability, clientelism, interest group capture, and the institutional legacies of authoritarianism may pose serious obstacles to redistributive policies under democratic rule (Albertus and Menaldo, 2018). It has also been suggested that such effects might be heavily context dependent (Dorsch and Maarek, 2019; Soifer, 2013).

Looking at context-conditionality could be a new way forward to clarify both the theoretical and empirical relationship between

* Corresponding author.
E-mail addresses: hibano@utu.fi (H. Bahamonde), tmart@tulane.edu (M. Trasberg).

[1] We both thank the two anonymous reviewers, Martin Dimitrov, Patrick Egan, Robert Kaufman, Victor Menaldo, Thomas Oatley, Andres Sandoval, Xander Slaski, Hillel Soifer, and the participants of the XVI meeting of The Italian Association for the History of Economic Thought and the "Social Sciences Seminar" at Universidad de O'Higgins for their insightful comments. We also thank Jonathan Hanson for sharing the data on census administration. Javiera Tobar provided excellent research assistance. Authorship is in alphabetical order. The usual caveats apply.

https://doi.org/10.1016/j.ejpoleco.2021.102048
Received 26 June 2020; Received in revised form 29 April 2021; Accepted 22 May 2021
Available online 4 June 2021

democracy and inequality. In this paper, we explore whether democracy's impact on inequality is conditioned by state capacity. It might be expected that pre-existing state capacity, in the form of functioning bureaucracies and territorial penetration, would be necessary for redistributive policies under democratic rule (Ziblatt, 2008).[2] For example, Soifer (2013) focused on the effect of inequality on democratization and argued that inequality-induced redistributive conflict only ensues in contexts of considerable levels of state capacity, which allows the implementation of redistributive taxation and transfer policies.

This study found no empirical support for these intuitive claims. Using fixed-effects panel regression models with data from 126 industrial and developing countries from 1970 to 2013, we show that democratic rule combined with high state capacity leads to increasing income inequality over time. This study's theoretical argument centers on the idea that democracy and high state capacity combined provide a context of optimal property rights protection and contract security. We argue that the high-quality investment environment backed by democratic and high-capacity state institutions increases income inequality through two transmission channels: the higher inflow of foreign direct investment (FDI) and the development of sophisticated financial sectors, which have been associated in recent literature with increasing income inequality. While income concentration in high-capacity democratic environments occurs through market inequality, we contend that fiscal policy in these contexts is not able to offset these changes. Multinational corporations and transnational elites become more relevant actors in national politics with increasing FDI flows and financial development, which allows them to exert downward pressure on labor-protecting regulations, redistributive taxation, and transfers.

Evaluating the effect of regime type on inequality at different levels of state capacity poses significant empirical challenges. Both regime type and state capacity tend to be endogenous to inequality levels and other socio-economic variables associated with economic development. Most importantly, democratic rule might create incentives to increase state capacity in order to levy more tax revenue and provide more public goods to citizens. We mitigate these concerns via a careful construction of a state capacity measure based on cumulative census administration, which is unlikely to reflect government policy priorities that are endogenous to democracy levels. We also use instrumental variables to relieve endogeneity concerns by instrumenting regime type with regional democratic diffusion. Our results, based on annual panel data, are robust to the system generalized method-of-moments estimator (GMM) analysis, alternative measurements of democracy, various sub-sample restrictions, such as excluding industrial and former Warsaw Pact countries from the analysis, and different lag structure specifications.

This paper joins several recent contributions in stressing the importance of conditional factors in the regime-inequality relationship. Our contribution is parallel to that of Dorsch and Maarek (2019), who argue that the effect of democracy on inequality is conditioned by the initial level of inequality. According to their argument, democratization tends to bring initial high or low levels of inequality to the "middle ground," through either redistributive social policy or market reforms. We point to another factor that conditions the effect of democratic rule on inequality: state capacity, which principally affects inequality in the market income phase. This might well illuminate the institutional underpinnings of increasing within-country inequality in the last four decades in many parts of the world.[3] The institutionalist literature has implicitly assumed that "inclusive institutions" do not only promote development but also more equal income distribution, at least in the long term (Acemoglu et al., 2001). In this paper, we provide evidence that this conclusion may not be warranted and that "inclusive" institutions—captured in the combination of democratic regime type and high-capacity state institutions—might well lead to a trend of steady increases in income inequality through different policy mechanisms.

The paper is structured as follows. In Section 2, we give an overview of the recent literature on democracy and inequality. In Section 3, we present our theoretical argument on the interactive relationship between democratic rule, state capacity, and inequality. Then, in Section 4, we present our research design and address issues of the measurement of key variables. In Section 5, we present our results from fixed-effects panel models and the corresponding robustness checks. In Section 6, we test the transmission channels behind this relationship. In Section 7, we provide concluding remarks.

2. Democracy and inequality

Democratic institutions have been conceptualized as a major source of responsiveness and accountability in the political economy literature, providing electoral incentives to redistribute income. Leaders in democratic nations need widespread support to achieve and sustain power and are, therefore, more likely to move beyond their narrow set of personal interests by appealing to a wider public through public policies (Meltzer and Richard, 1981). Compared to authoritarian polities, widespread enfranchisement in democracies is likely to result in higher public goods provision, which may help the poor to benefit from economic growth via investments in human capital (Baum and Lake, 2003; Lindert, 2004;). These policies are expected to produce more equal income distribution over time.

Despite these plausible theoretical mechanisms, empirical evidence has not offered solid support for the inequality-reducing effects of democracy. Several empirical studies incorporating various regions of the developing world find that democracy does not induce lower income inequality (Acemoglu et al., 2015; Gradstein and Milanovic, 2004; Dorsch and Maarek, 2019; Timmons, 2010; Wong, 2016), more progressive taxation (Scheve and Stasavage, 2012), or pro-poor social policies (Mulligan, Gil, and Sala-i-Martin, 2004; Pagalayan, 2020; Ross, 2006). The causes of this "democratic unresponsiveness" have constituted a major puzzle for researchers. At the

[2] We define "state capacity" as state institutions' ability to collect and manage information and to effectively execute policies in different areas, notably including market regulation and contract enforcement ("legal capacity") and resource extraction ("fiscal capacity") (Besley and Persson, 2009: 1219).

[3] There have been major exceptions to that trend, especially in Latin America, where inequality has declined since the end of 1990s, albeit very slowly.

same time, some democracies might affect inequality more than others, and a focus on the social and institutional contexts in which democracies operate could offer a new way forward for fruitful theorizing.

In this paper, we concentrate on the question of whether democracy's effect on inequality is conditioned on state capacity. We define "state capacity" as state institutions' ability to collect and manage information and to effectively execute policies in different areas, notably including market regulation and contract enforcement ("legal capacity") and resource extraction ("fiscal capacity") (Besley and Persson, 2009: 1219). High state capacity implies a monopoly on violence over a territory and a cohesive and competent civil service and courts operating on the basis of well-established rules and routines. State capacity has received important attention recently as an explanatory variable in determining development outcomes (Hanson, 2015; Knutsen, 2013).

The previous literature has hinted that inequality reduction is more likely when both the political-electoral incentives stemming from regime characteristics and the state capacity to redistribute exist. In low-capacity states, democratization should not matter for redistributive outcomes given their inability to collect taxes and implement social policy. Revenue extraction and policy implementation—both crucial for income redistribution—are dependent on the state's ability to penetrate its territory and implement decisions (Ziblatt, 2008). In low-capacity states, elites are able to escape taxation, lowering the state's ability to provide public goods and transfers (Scott, 1988). For example, income taxation requires identifying individual incomes both within the national territory and offshore, assessing value, and collecting payments. The implementation of redistributive policies, such as basic education, healthcare, social assistance, and insurance policies, is also likely to be dependent on the pre-existing capacity of the state institutions (Ziblatt, 2008).

3. Democracy, state capacity, and investor confidence

In contrast to that intuitive account, we present a more nuanced understanding of the relationship between democracy, state capacity, and income inequality. Counterintuitively, we argue that democratic rule in the context of high state capacity is associated with increases in income inequality. It is plausible to think that democracy and high state capacity provide the context for optimal property rights and contract security, which favors investor confidence through lower-risk capital investments. The high-quality investment climate in a democratic, high-capacity setting increases inequality through two policy channels, financial development and larger FDI flows, that affect market income inequality. For several reasons—which we further introduce below—we believe that fiscal redistribution is not able to offset these inequality-concentrating mechanisms.

Under low state capacity, we would expect neither democratic nor authoritarian regime types to make much difference in terms of distributive outcomes, given that the state lacks the ability to undertake both redistributive policies and the provision of contract and property rights security. We also anticipate that in autocratic regimes, the level of state capacity does not matter for inequality. This is because different sub-types of authoritarian regimes are inherently diverse and have very different policy priorities in terms of property rights, contract security, and redistribution (Dorsch and Maarek, 2019). For instance, communist regimes in Eastern Europe and Asia led to extremely egalitarian outcomes over time, while many right-wing dictatorships in Latin America and Sub-Saharan Africa presided over the most unequal distributive outcomes in modern history. Our interactive theory therefore makes only the modest prediction that democratic rule is associated with increasing inequality in the context of high preexisting state capacity.

Democratic regimes have been widely portrayed as more likely to respect private property rights and provide greater rule of law, incentivizing capitalist investor confidence (North and Weingast, 1989; Olson, 2000). An influential argument has connected democracy with higher FDI inflows precisely because of greater investment security (Busse and Hefeker, 2007; Jensen 2003, 2008). At the same time, democracy alone is not enough to secure investor confidence. Contract enforcement—based on state capacity to enforce the rule of law among private agents—is likely to be crucial for business confidence and attracting foreign investors, along with the protection of private property from arbitrary government involvement. Pre-existing state capacity clearly underlies this positive contractual environment (Besley and Persson, 2009). The "watchman" capacities of the state—Weberian-like central- and local-level bureaucracies, impartial courts, uniform weights and measures, and effective law enforcement institutions—are crucial for reducing uncertainty and transaction costs (Coase, 1960; Williamson, 1985). Therefore, it could be hypothesized that nations combing high state capacity with democratic rule achieve the highest FDI inflows (Li and Resnick, 2003).

In addition to fomenting FDI inflows, democratic high-capacity contexts offer an especially nurturing context for financial development. The checks and balances inherent to a democratic system reduce the government's leverage in both expropriating assets and threatening property rights in the financial sector (Haber et al., 2008; Menaldo and Yoo, 2015). Yet these positive effects might not be achieved without pre-existing state capacity reducing important market failures that might otherwise result from information asymmetries and obstruct contract enforcement. For example, the creation of accurate property registers by the state allows banks to know who owns which assets, which facilitates the creation of contracts (Haber et al., 2008). The enforcement of bankruptcy law and the diffusion of the modern accounting standards underlying credit expansion may depend on the quality of bureaucracy and its ability to penetrate the reaches of the state territory. At the same time, stock market expansion is likely to depend on stronger corporate governance and the capacity to enforce bankruptcy laws (Becerra et al., 2012; Menaldo, 2016).

Thus far, we have argued that democratic and high-capacity state institutions are more likely to attract more FDI and help to develop sophisticated financial sectors. The second step of our argument connects these two variables with increasing income inequality. First, considerable recent evidence has pointed out that FDI flows may increase income inequality in both the developed and developing worlds (Basu and Guariglia, 2007; Jaumotte et al., 2013; Reuveny and Li, 2003). FDI inflows lead to an increased demand for skilled workers, associated with growing wage differentials between skilled and unskilled jobs, which is likely to increase income inequality (Decreuse and Maarek, 2015; Feenstra and Hanson, 1997; Kratou and Goaied, 2016). For example, investment by multinational corporations often creates a small sector of high wage earners and a large low-wage backward sector (Nafziger, 1997).[4]

The development of a sophisticated financial system is another transmission channel by which investor confidence in high-state-capacity democracies produces higher income inequality. On the one hand, scholars have long recognized the growth-promoting and poverty-reducing effects of financial development through incentivizing and channeling savings (Beck and Demirguc-Kunt, 2008). According to this view, financial development is likely to happen in the "extensive margin," which is likely to be associated with more equal income distribution. On the other hand, financial development could be produced in the "intensive margin" through improvements in the quality and range of financial services available to those who already enjoy access to the financial system, which has an important potential to widen inequality and perpetuate intergenerational differences in economic opportunity (Greenwood and Jovanovic, 1990). Financial instruments, such as bonds and stocks, are likely to provide higher rates of returns to pre-existing capital, providing a basis for the concentration of financial assets (Piketty, 2014).

In addition, the un-equalizing effects of the financial system could work through a labor income channel. Financial sector employees are strongly concentrated at the top of the income distribution, and their earnings exceed those of employees with similar profiles (in terms of age, gender, and education) in other sectors. Asymmetric compensation schemes for bank managers may especially contribute to this un-equalizing dynamic (Denk and Cournède, 2015).[5] Empirically, recent literature has provided evidence for both positive and negative associations between financial development and inequality. Severalrecent papers find a positive association between financial sector size—usually proxied by private credit as a percentage of GDP—and an increase in income inequality, both in cross-national and subnational contexts (Dabla-Norris et al., 2015; Denk and Cournède, 2015; De Haan and Sturm, 2017; Jaumotte et al., 2013). Other studies find that countries with higher levels of financial development have less income inequality (Hamori and Hashiguchi, 2012; Kunieda et al., 2014; Naceur and Zhang, 2016).

It might be expected that fiscal policy would offset the increase in market inequality in democratic high-capacity settings in the post-redistribution stage. However, while the context of high state capacity in democracies establishes preconditions for progressive taxation or social policy, the redistributive capacity does not automatically translate into policy outcomes. Inequality-increasing market processes also put pressure on fiscal policy, making it difficult to increase redistribution via taxes and transfers (Egan, 2010). With increasing FDI flows and more developed financial sectors, domestic and international corporate and financial elites become more relevant actors in national politics and are likely to exert downward pressures on labor-protecting regulations and redistributive taxation and transfers (Wong, 2016).

A high concentration of income at the top increases potential resources for elite lobbying activities, augmenting their already disproportionate influence on policy making even in countries where considerable redistributive capacity exists (Acemoglu and Robinson, 2006). Starting in the mid-1970s, most industrial nations have experienced considerable reductions in marginal tax rates on income, which has contributed to higher inequality in the disposable income phase (Atkinson, 2015; Bartels, 2008; Gilens and Page, 2014). Egan (2010) shows that, in the Latin American context, accumulated FDI levels are associated with a greater likelihood of market economic reforms, such as lower tax burdens and domestic financial liberalization. Although further work needs to be done in this domain, it is likely that similar patterns of reinforcing elite dominance could be at play in other parts of the developing world, where economic elites enjoy similar political opportunities to concentrate capital.

To summarize, our theoretical propositions have the following empirical implications. Our main hypothesis is that democratic rule in a high-state-capacity context increases both market and post-redistribution inequality over time. We also posit that a democratic, high-capacity context is associated with larger annual FDI inflows and faster growth of the financial sector. For these reasons, we do not expect high-capacity democracies to experience larger fiscal transfers or redistribution, holding all else equal. Lastly, we also expect a positive association between FDI stock, financial development, and income inequality.

4. Research design, methods, and data

We use annual fixed-effects panel regression models to test our propositions. We use unit fixed effects because we are particularly interested in changes within individual countries over time.[6] Country-fixed effects allow us to account for country-specific omitted factors that are stable over time. The inclusion of a lagged dependent variable controls for autocorrelation. The model takes the following form:

$$Inequality_{i,t} = \alpha_0 + \beta_0 Inequality_{i,t-1} + \beta_1 Democracy_{i,t-1} + \beta_2\, State\ Capacity_{i,t-1} + \beta_3 Democracy{*}State\ Capacity_{1,t-1} + Controls_{i,\ t-1} + \gamma_i + \lambda_t + \mu_{i,t}$$

Our main theoretical interest is the interaction term between the lagged values of democracy and the lagged values of cumulative state capacity; (β_3). γ_i and λ_t are the country- and year-fixed effects, respectively, while $\mu_{i,t}$ is the estimated residuals.

[4] Decreuse and Maarek (2015) show that FDI stock is negatively associated with the labor share in the host countries, though this effect is non-linear.

[5] In addition, large financial sectors contribute to moral hazard problems. Given bailout expectations by the government, sophisticated financial instruments encourage the pursuit of high returns through risk-taking behaviors, benefiting members of the financial elite compared to other sectors of the economy (Korinek and Kreamer, 2014).

[6] List of countries is provided in the Appendix in Table A2.

4.1. Variables and measurement

Inequality: Our outcome variable is income inequality as measured by the Gini index. The Gini index ranges from 0 (perfect equality) to 100 (one person has all the income). We use the Standardized World Income Inequality Database (SWIID) (Solt, 2016) for our inequality measure. Using the Luxembourg Income Study (LIS) as the methodological standard for comparability, the SWIID incorporates data from various sources. The SWIID uses "model-based multiple imputation estimates of the many missing observations in the LIS series" (Solt, 2016, p. 1271), maximizing both comparability and sample size. Incomparability is reflected in the standard errors of the SWIID estimates, where the Gini estimates and their associated uncertainty are represented by 100 draws from the posterior distribution. The data set provides 100 imputations for each country-year observation (ibidem).[7] The drawback of the SWIID data is therefore the reliance on estimation to fill in missing data points.

The SWIID is composed of four indicators—disposable income inequality (post-tax and -transfer), market income inequality (pre-tax and -transfer), absolute redistribution (the difference between the market income and disposable income Gini indexes), and relative redistribution (the percentage by which market income inequality is reduced). We expect a democratic high-capacity context to affect both market and disposable (net) income inequality. While we anticipate the inequality-increasing processes to work mostly through market income concentration, they also put a strain on fiscal redistribution, as we have argued above. Therefore, we present results with both net and market income inequality in our empirical analysis.

Democracy: We adopt the Boix et al. (2013) and Polity indicators as our main democracy measures. Boix et al.'s (2013) measure is based upon two principal components: 1) the use of elections to choose the legislature and, directly or indirectly, the chief executive, and 2) a minimum threshold of participation rights. The Polity democracy index consists of six component measures that record key qualities of executive recruitment, constraints on executive authority, and political competition (Marshall et al., 2017). It provides an ordinal ranking of political regimes on a scale of 10 to −10 (democracy to authoritarian regimes). Both of these measures offer almost universal country coverage over time. We further test the robustness of our results with the democracy indicators of Cheibub et al. (2010) and Dorsch and Maarek (2019) (based on the democracy measure initially developed by Papaioannou and Siourounis, (2008) and Acemoglu et al., [2019]).

Given that inequality is likely to affect the prospects of democratic consolidation in different nations, issues of endogeneity must be discussed. Indeed, the level of inequality has figured as a crucial explanatory variable in previous studies of democratization (Acemoglu and Robinson, 2006; Ansell and Samuels, 2014; Boix, 2003). To mitigate reverse causality concerns, we make use of an instrumental variable strategy. Relying on previous work (Acemoglu et al., 2019; Dorsch and Maarek, 2019), we use regional waves of democratization as a source of exogenous variation in domestic democracy (Dorsch and Maarek, 2019). It is very unlikely that within-country inequality or other domestic economic and political variables could have an influence on the timing of regional democratization processes, while democratization waves clearly affect domestic democratization (Acemoglu et al., 2019; Huntington, 1991). It is implausible that democratic or autocratic waves have a direct effect on inequality in a particular country except through their effect on domestic political institutions. This instrument allows us to plausibly isolate an exogenous variation in democratic institutions.

We construct our instrument through the following strategy. For our binary indicator of democracy (Boix et al., 2013), we calculate the fraction of countries with democratic institutions in the region that shared the same regime type at the beginning of the panel. For instance, for country *i*, we add up the number of countries sharing regime type in the same region that are democratic at the time, excluding country *i*. For our continuous Polity indicator, we calculate the average democracy score in a region to instrument Polity scores in a given year, excluding the country itself.[8]

A possible violation of the exclusion restriction is that democratic transitions in neighboring countries affect domestic economic growth rates, which could affect economic variables domestically—especially if regional economies are integrated—which in turn affects both inequality and the likelihood of domestic democratic transition (Acemoglu et al., 2019). To mitigate these concerns, we control for log of GDP per capita in all models.

State Capacity: Operationalizing state capacity in the context of our analysis is a complicated task. Similar to regime type, state capacity tends to be endogenous to inequality levels and other socio-economic variables associated with economic development. In addition, democratization might affect state capacity by creating incentives to gather more tax revenue and provide more public goods and services to citizens (Acemoglu et al., 2011). Most existing measures used in the literature—based on fiscal capacity or levels of public goods provision—reflect the policy preferences of governments and are likely to be directly endogenous to regime type and inequality levels. In addition, as we explain below, expert survey-based indicators, such as the Bureaucratic Quality Index of the International Country Risk Guide (ICRG) or the World Bank (WB) Worldwide Governance Indicators, are likely to be affected by expert biases of different types (Kurtz and Schrank, 2012).

We make use of a novel measure of state capacity that is less vulnerable to these problems: the regular ability to conduct national

[7] We make use of multiple imputation (MI) regression tools provided by Stata, as recommended by Solt (2016). We perform our main regressions over each of the 100 imputations in order to provide a reliable estimate of the coefficients, taking into account the standard errors across the 100 imputations. This allows the uncertainty of the SWIID to be reflected in MI regression estimates. Given that the MI estimation is computationally intensive, some MI regression tools are not available (e.g., 2LS2); we therefore chose to present the majority of our models with non-imputed estimates, calculating the mean of imputed series for each country-year, and performed the regressions on that single point estimate.

[8] Following the definition of Dorsch and Maarek (2019), we define the regions as follows: Africa, Central Asia, Eastern Europe, Europe/U.S., Middle East, South-East Asia, South/Central America.

population censuses (Hanson, 2015; Soifer, 2013). The capacity to undertake periodic censuses captures the ability of the central state to gather information about its subjects, proxying well for functioning central and local bureaucracies and effective law enforcement institutions (Mann, 1984). In addition, censuses also provide the state the necessary information for the construction of tax registers, cadastral maps and other forms of systematization (Soifer, 2013). Census administration therefore captures the capacity to collect and manage information and effectively execute policies in different areas, including market regulation, property rights protection, and contract enforcement ("legal capacity") and the ability to extract resources ("fiscal capacity") across national territory (Besley and Persson, 2009; Knutsen, 2013).

Even if nations have incentives to manipulate the timing, reach, and coverage of the censuses—concerns which we discuss below—there are no major political incentives to avoid them altogether. They are infrequent in time—conducted usually every five or 10 years—and take up relatively few resources compared to the implementation of welfare policies or infrastructure programs. However, they serve multiple purposes for both democratic and autocratic nations of different development levels (Christopher, 2008). Censuses not only provide information to identify subjects for taxation, military conscription, and government programs but have also figured as crucial nation-building devices for nations in the developing world while contributing to social control and surveillance for authoritarian regimes (Anderson, 1991; Lieberman and Singh, 2017).[9] It is therefore plausible to believe that the absence of a census acts as a direct signal of extreme state weakness, while its presence indicates meeting a minimal threshold of state organization. Where states cannot conduct censuses regularly, they are surely unable to undertake property rights and contract enforcement, even if they have political incentives to do so (Centeno, 2002).

We use data from the United Nations Social and Housing Statistics Section database (United Nations, 2015) on national population censuses,[10] which documents information on the presence or absence of a standard national census for every country-year during the period 1945–2015, which covers 13,466 country-years, as compiled by Hanson (2015).[11] An intuitive approach would be to create a lagged indicator measuring whether nations conducted a census in the past five or 10 years (Hanson, 2015; Soifer, 2013). Yet this measure is more likely to be endogenous to contemporary socio-economic situation and regime type, and the occurrence and timing of censuses could be manipulated by governments according to different policy priorities. For instance, it is possible that governments might determine the timing of censuses according to their electoral calendar to influence the boundaries of electoral districts (e.g., to exclude or include some particular ethnic and regional groups) or to show favorable population sizes in order to achieve more development aid from donors (Lieberman and Singh, 2017).

To mitigate these concerns, we construct a simple continuous indicator that counts the cumulative number of decades in which countries have conducted periodic censuses since 1950 for every country-year. The national censuses are conducted at either 10- or five-year intervals, and the absence of a census in a decade is likely to signal considerable weakness of the central government due to a lack of control over sub-national areas, absent bureaucracies, and inability to control national territory to its full extent. For instance, if a government were unable to conduct censuses in the 1950s and 1960s but able to do so in the 1970s and 1980s, the country receives a score of 2 for the whole decade of 1980–1990. The indicator has a global mean of 3.40 and standard deviation of 1.36. Fig. 1 displays the cumulative census scores in 2010. In 2010, industrial countries have unanimously maximum values (6) on this indicator, while Somalia, Eritrea, Chad, Yemen, and Afghanistan possess the lowest values with only 1–2 census iterations.

This procedure—while rather blunt—creates an indicator that is largely unaffected by both expert coding bias and the policy priorities of governments, relieving inherent endogeneity bias (Soifer, 2013). This long-term measure—which captures the effect of censuses conducted in previous decades—is likely to "wash out" all temporary shocks resulting from the timing of elections, foreign aid priorities, or other time-variant policy agendas. These political incentives might explain why censuses are conducted in one particular year versus another but are unlikely to affect whether the census was conducted over a long time frame such as several decades. It is important to note that our measure captures all censuses conducted in the national territory since 1950 by any state—even those that were conducted under colonial administrations and nations that formed part of other countries. In this way, our measure accounts for pre-statehood state capacity. We demonstrate this point in the Appendix in Table A3., which lists all censuses in our sample that were undertaken under colonial administrations and parts of other countries.[12]

However, several potential criticisms of our measure merit discussion. First, our census indicator does not allow us to capture more subtle differences between countries that do not miss censuses, nor does it take into account more gradual increases or declines in state

[9] For instance, colonial independence movements, initially concerned with the censuses' surveillance role, coopted them as a means of promoting national identity through the definition of a national population, akin to the definition of a national territory (Anderson, 1991).

[10] The United Nations Social and Housing Statistics Section database excludes all censuses defined as "urban, administrative," or "sample," as well as all those described only as "scheduled," since these censuses do not provide the government with systematic information about its entire population. This leaves two types of censuses in the sample: the standard census, as carried out in most countries, and the rolling census, carried out on an annual basis for a portion of the population in a small set of countries, including Iceland, Sweden, and Denmark (Hanson, 2015; Soifer, 2013).

[11] The data for 1990–2020 is available in the United Nations Social and Housing Statistics Section https://unstats.un.org/unsd/demographic-social/census/censusdates/. The data for the period 1950–1990 is available at the United Nations Social and Housing Statistics Section. 2003. Ethnicity: A Review of Data Collection and Dissemination. Demographic and Social Statistics Branch, United Nations Statistics Division. At http://unstats.un.org/unsd/demographic/sconcerns/popchar/Ethnicitypaper.pdf.

[12] Table A3 in the Appendix shows that states that emerged from other states with high capacity (had regular census administration) also had high capacity post-independence (i.e., had achieved a high cumulative census score by 2010), given that they had accumulated a high census score. As demonstrated by this table, the Soviet Union, Yugoslavia, and Czechoslovakia gave their high-capacity levels to successor states, given that all these censuses are captured by our measure. The table also shows that censuses conducted under colonial empires (British, French, and Portuguese) in other parts of the developing world are accounted for by our census measure as well.

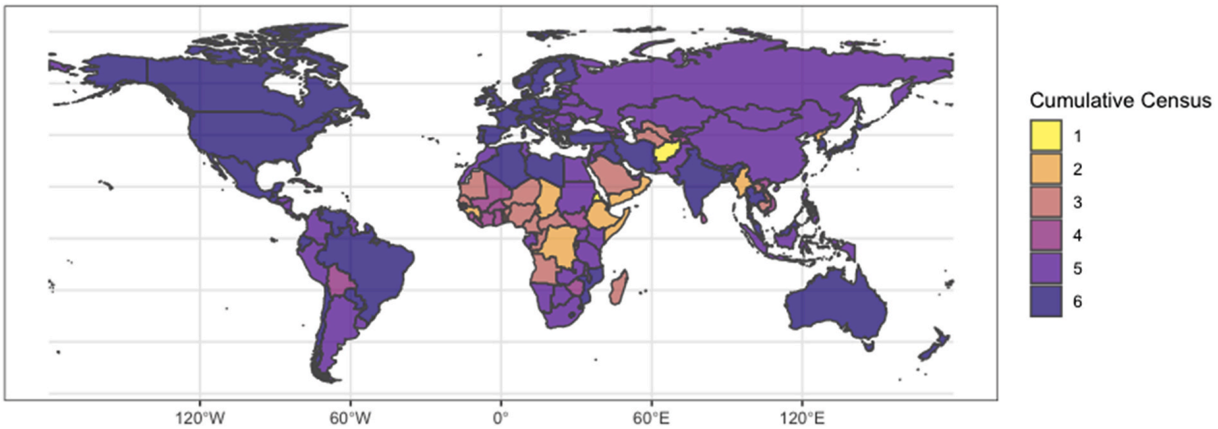


Fig. 1. Cumulative census variable in 2010.

capacity occurring yearly.[13] Despite these potential shortcomings, we argue that our indicator is considerably less susceptible to measurement error than the existing measurements based on expert evaluations, such as the Bureaucratic Quality Index (BQI) from the ICRG dataset and the WB World Governance Indicators, which are able to capture more subtle country-differences within regions.[14] The "expert scores" are indeed likely to meaningfully reflect differences between countries in the same region (say between Sweden, Italy, and Albania in Europe).[15]

Yet these evaluations start to face enormous measurement validity issues—stemming from expert biases possibly inducing considerable measurement error—when comparing the aforementioned nations with countries outside of their political and cultural regions. Given that what "high-quality bureaucracy" means in different countries is regionally and culturally specific, we cannot expect that quantitative gradations in expert scores for various dimensions meaningfully reflect differences in state strength (Kurtz and Schrank, 2012). A great advantage of our measure—besides its wide availability—is the fact that census occurrence is based on "hard" institutional data, which is not vulnerable to coding biases of the type that stem from expert evaluations (Knutsen, 2013; Kurtz and Schrank, 2012). Our simple measure is largely free of these measurement problems, as national censuses always include enumeration of the whole population (as defined by the UN), although their quality might vary considerably. The absence of a census gives a powerful indication of the weakness of state institutions in the developing world, despite the measure's bluntness.

Second, another potential disadvantage of our measure, which sums up censuses over decades cumulatively, is that it cannot decline over time. To mitigate that concern, we have devised another cumulative census variable that introduces a penalty to the cumulative census score when countries miss a census in a decade. It is very plausible that when countries miss a census, state capacity is likely to decline. We believe that missing a census is likely to be a sign of inherent state fragility, which leads to a decline in state capacity. To demonstrate that our penalized measure is robust to different sizes of penalty, we create two versions of that variable. For every version, we subtract either 1 or 0.5 points from the accumulated state capacity variable when countries miss a census in a decade and find our results identical to the simple cumulative variable (Table 4).

Third, while all nations could potentially have a minimum of zero and a maximum of six censuses, colonial legacies could influence census administration and state capacity, confounding our results. Scholars have argued that former British colonies inherited higher institutional capacity and human capital stock at the time of independence compared to nations under French, Portuguese, and Belgian colonial rule (Cogneau and Moradi, 2014; La Porta et al., 1998; Landes, 1998).[16] While former French, Belgian, and Portuguese colonies tend to have lower cumulative census scores, we show that these varying colonial legacies do not drive our results in any meaningful way. We capture the effects of different colonial legacies on different countries through sample restrictions and present evidence that our results are not driven by these trends in Table A6 in the Appendix.[17]

[13] We thank one of the anonymous reviewers for this comment.

[14] For instance, our indicator does not allow us to capture any meaningful variation between industrial countries in Western Europe and North America, given that they have not missed censuses in any decade since the 1950s.

[15] For example, Kurtz and Schrank (2012: 542) explain that measurements that rely on surveys, particularly of foreign investors or domestic firms, wrongly assume that "the interests of investors [...] and the interest of the state institutions are essentially coterminous." In some instances where the state is strong and able to levy taxes and impose regulations, for example, the state will most likely "be judged 'burdensome' and 'growth-inhibiting' by many businesspersons" (Kurtz and Schrank, 2012: 542).

[16] For instance, the British established a system of indirect rule—enabling local autonomy and self-governance—while the French relied on direct colonial rule accompanied by repression (Landes, 1998). Others have contrasted liberal British policies regarding missionary schooling to restrictive systems of state education in French dependencies, which led to higher human capital outcomes in British colonies (Cogneau and Moradi, 2014).

[17] Table A3 I shows that British colonies (with a cumulative census score of 4.5 in 2010) seemed to conduct more censuses than the French (3.38 in 2010), Belgian (3 in 2010) and Portuguese (4 in 2010) ones in the 1950s, and also have a higher cumulative census score in 2010. Yet, as we show in Table A6, varying colonial legacies do not confound our results.

Lastly, it might be that civil wars confound the relationship between cumulative census administration, democracy, and inequality. Civil wars are indeed the principal reason why nations are not able to take on censuses. Yet, while having a dreadful short-term effect on state capacity, not all civil wars lead to a deterioration of state capacity in the long term, which our measure intends to capture. As suggested by Pagalayan (2020), civil wars might incentivize central states to cater more public goods to sub-national regions that had been neglected by central governments before the conflict. In some cases, states have indeed started to conduct censuses rather quickly after civil wars with the aim of gathering information on citizens to provide better public services to them (Verpoorten, 2012). This discussion prescribes that we should not expect a clear relationship between civil wars and census administration proxying for state capacity. We account for these concerns by introducing control variables for civil conflict and ethnic fractionalization, variables commonly connected to domestic conflicts.

Our cumulative census measure is correlated to a reasonable degree with other proxies of state capacity. It has a .55 correlation with GDP per capita, a 0.33 correlation with tax revenue (as a percentage of GDP), and a 0.45 correlation with school enrollment. This suggests that the cumulative census variable is a reasonable proxy for "fiscal" state capacity. Our indicator has a slightly weaker association with "legal capacity"–property rights protection and contract enforcement. Our measure has a 0.39 correlation with Fraser Institute's Economic Freedom Index, and 0.35 and 0.40 correlations with Property Rights Protection and Enforcement of Legal Contracts indicators in the same data base, respectively. Our indicator is not correlated with the Gini index (0.03), which relieves the concern that censuses might be especially likely to be absent in low- or high-inequality nations.

Control Variables: Besides country- and year-fixed effects, we add a series of control variables to account for alternative factors that might be associated with inequality changes (in our baseline models). We add the log of GDP per capita to control for the level of economic development. We include trade openness as an indicator of economic openness, measured as imports and exports as a percent of GDP (Reuveny and Li, 2003). Inflation captures the macroeconomic situation of the country. Finally, we include the urban share of the population to account for the structure of the economy. Our control variables come from the World Development Indicators (WDI) of the World Bank. Descriptive statistics for these control variables are presented in the Appendix (Table A1.) We discuss the measurement of FDI, financial development, and other control variables in Section 6.

5. Results

We start the presentation of our results with models without the interaction term (Table 1). Our results directly replicate previous studies of inequality (Dorsch and Maarek, 2019; Gradstein and Milanovic, 2004; Timmons, 2010; Wong, 2016). Models 1 and 2—using the MI approach, as suggested by Solt (2016)—show a lack of association between the Boix (Model 1) and Polity (Model 2) indicators of democracy and net inequality, respectively, while controlling for covariates typically used in the literature and considering country- and year-fixed effects.

In order to capture the conditional effect—and following (Dorsch and Maarek, 2019)—we first add an interaction between our binary Boix democracy indicator and the cumulative census score prior to democratization, therefore using a fixed state capacity variable for these interaction terms (Models 3 and 5).[18] Model 3 presents results with MI estimation, Model 5 with imputed series. The results from columns 3 and 5 directly support our counterintuitive theoretical contentions: democratization and state capacity interact positively in producing higher inequality levels.[19] Very similar coefficients are produced using the means of the imputed series.[20]

These effects are substantively meaningful. The dichotomous democracy indicator allows us to calculate the long-run effect of democratic transitions under different levels of state capacity. The shift from democratic to authoritarian under the highest value of state capacity (six censuses) results in an increase in future inequality of 3.7 Gini points (Model 3), holding all other variables constant at their means. We follow the advice of Berry et al. (2012) and present the conditional effect of democracy at different levels of state capacity graphically, using the results from Model 5 (Table 1). Fig. 2 shows that democracy has a positive effect on income inequality when state capacity is high at the moment of democratization (approximately two census iterations). To get a better sense of these results, Table 1 also provides the marginal impacts of the interaction term.

We obtain a similar result using the Polity continuous indicator as our democracy indicator (Models 4 and 6). While the continuous indicator does not allow us to calculate long-run effects, it allows us to evaluate the marginal impact of more gradual shifts in regime type, conditional on lagged levels of cumulative censuses (lagged in one period). Under the maximum level of state capacity (six censuses), the shift from full authoritarianism to full democracy (from −9 [10th percentile] to 10 [90th percentile] in Polity scores) would result in a 4.5-Gini-point increase in inequality in 10 years, holding all other variables constant at their means. The marginal effects of Polity are presented in Fig. 3.

[18] The advantage of this approach is that it allows us to calculate the long-run effects of shifts from autocracy to democracy.

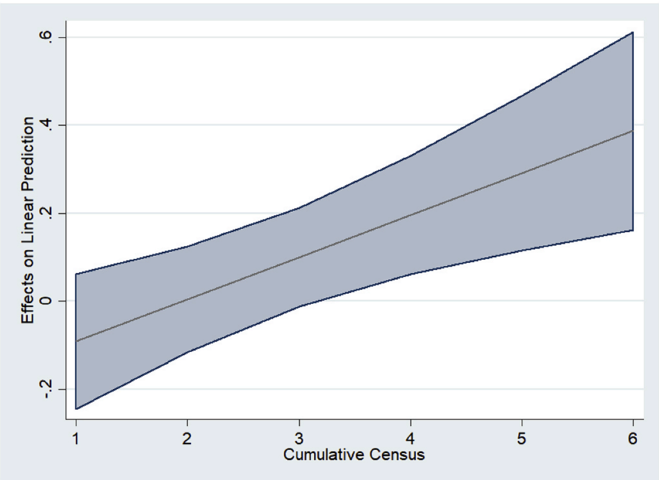
[19] For Models 5 and 6, the stationarity of residuals were tested using conventional panel unit root tests. Thus, Fisher's tests with both the Augmented Dickey Fuller and the Phillips-Perron specifications were implemented. We report these tests in Table A4 in the Appendix. All variables in the main models were tested (models 5 and 6 in Table 1) i.e., dependent independent ones. All tests also included parameters to test for trend and drift. All tests included one lag, which is the same lag structure used in the estimation procedures. These tests were implemented using the "xtfisher" routine in Stata (v. 15). As the table suggests, most tests indicate stationary residuals at conventional levels of statistical significance.

[20] We prefer the median imputed series specification, as it is not as computationally intensive as MI models, while results are identical.

Table 1
Effect of democracy and state capacity on the net gini index.

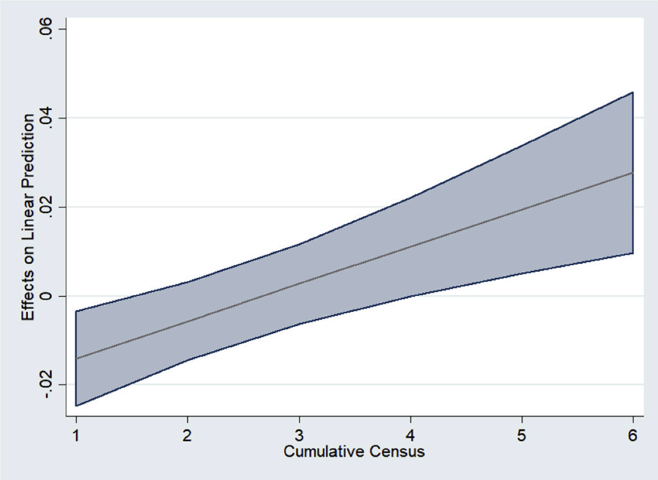
	Multiple Imputation				Mean Imputed series	
	(1) Boix	(2) Polity	(3) Boix	(4) Polity	(5) Boix	(6) Polity
Gini Lagged	0.919***	0.920***	0.917***	0.921***	0.937***	0.941***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.006)	(0.006)
Boix Democracy	−0.065		−0.337***		−0.187*	
	(0.065)		(0.124)		(0.102)	
Polity Democracy		−0.004		−0.021**		−0.022***
		(0.006)		(0.009)		(0.007)
Cumulative Census			−0.036	0.004	0.088**	−0.043
			(0.048)	(0.086)	(0.040)	(0.064)
Boix Democracy*Cumulative Census			0.107***		0.096***	
			(0.037)		(0.031)	
Polity*Cumulative Census				0.007**		0.008***
				(0.003)		(0.002)
GDP (log)	0.279**	0.278**	0.246*	0.259*	0.382***	0.426***
	(0.134)	(0.137)	(0.129)	(0.133)	(0.085)	(0.083)
Inflation	0.000***	0.000***	0.000***	0.000***	0.000*	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Urban Population	−0.013	−0.013	−0.008	−0.010	0.002	−0.001
	(0.008)	(0.008)	(0.007)	(0.008)	(0.005)	(0.005)
Trade	−0.001	−0.001	−0.001	−0.001	0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
LR effect at Census = 6			3.67		6.17	
LR effect at Census = 3			−0.19		1.6	
Marg. impact at Census = 6			0.305	0.021	0.389	0.026
Marg. impact at Census = 3			−0.016	0.000	0.101	0.002
Country and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4190	4006	4019	3962	3778	3962
R-squared	0.99	0.99	0.99	0.99	0.99	0.99

Notes: The dependent variable is the net Gini coefficient and the main explanatory variables are one-period-lagged democratic scores from Boix et al. (2013) and Polity IV by Marshall et al. (2017) interacted with the cumulative census variable. The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. All independent variables are lagged one year. Standard errors are clustered at the country level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.



Note: Conditional Effect of Democracy and State Capacity on Inequality. The panel shows the predicted change in the Gini index from democratic transitions using Boix et al.'s (2013) democracy index at different pre-democracy cumulative census scores according to the estimates in Table 1, Model 5. The blue lines represent the 95% confidence intervals.

Fig. 2. Marginal Effect of Boix Democracy Indicator on Gini Index for Different Pre-Democracy Cumulative Census Values.(For interpretation of the references to colour in this figure legend, the reader is referred to the Web version of this article.)



Note: Conditional Effect of Democracy and State Capacity on Inequality. The panel shows the predicted change in the Gini index from a one-unit change in democracy (Polity) at different lagged cumulative census values according to the estimates in Table 1, Model 6. The blue lines represent the 95% confidence intervals.

Fig. 3. Marginal Effect of Polity Democracy Indicator on Gini Index for Different Lagged Cumulative Census Values. (For interpretation of the references to colour in this figure legend, the reader is referred to the Web version of this article.)

5.1. 2SLS and GMM estimations

Table 2 (Models 1–4) presents results from 2SLS instrumental variable regressions for both of our democracy indicators (Boix and Polity) with the means of imputed series. We consider both democracy and its interaction term with cumulative census values as endogenous and instrument for them with regional democracy share/scores and an interaction of the latter with the cumulative census indicator. We perform these analyses for our two democracy variables. We present the first stage's results in the Appendix (Table A5), where we demonstrate a positive association between our regional instruments, our democracy indicators, and second-stage results, with required statistics, in Table 2. In order to have an over-identified specification, as a third excluded instrument, we also use the regional wave measure from five years before our one-year lagged democratization/democracy score regressor (the sixth lag of the share of a country's region that is democratically governed, in the case of the Boix indicator).

In addition, we present F-statistics of excluded instruments for the first-stage regressions in Table 1. Cragg–Donald F-statistics give evidence that the set of instruments is strong (above the rule of thumb of 10). The large p-values in the Hansen J statistics also confirm that the excluded instruments are exogenous. The results from the 2SLS procedure are similar to those in Table 1, with coefficients of interaction term larger in size. The shift from autocratic to democratic rule under the highest value of state capacity results in an 8-Gini-point increase in inequality in the long run, holding all other control variables constant at their means (Model 1).

In Models 3 and 4, we further probe the validity of our instrumental variable strategy by including various control variables that might confound the conditional effect of democracy and state capacity on inequality. First, given that civil conflicts are likely to affect both census administration and inequality, we control for lagged events of civil conflict by including an indicator from the Uppsala Conflict Data Program and the Peace Research Institute (UCDP/PRIO) Armed Conflict Dataset. Second, left governments are more likely to engage in redistributive policies and investments in state capacity. We grasp this effect through the left partisanship indicator from the Database of Political Institutions. Third, we also control for Official Development Aid (ODA) as a percentage of GDP to account for its possible effects on census administration. Fourth, ethnic diversity could hinder redistributive policies (Pleninger and Sturm, 2020), which we account for by controlling for the Herfindahl index from the Ethnic Power Relations (EPR) Core Dataset. Lastly, we control for the Fraser Institute Index of Economic Freedom, a summary index constructed from five components (size of government, legal system and property rights, sound money, freedom to trade internationally, and regulation) (Krieger and Meierrieks, 2016). In both models, our results remain unaltered, for both the Boix et al. (2013) and Polity measures.

To further bolster the robustness of our conclusions, we also provide results using the system method-of-moments estimator (GMM) introduced by Arellano and Bond (1991) (Models 5 and 6). The GMM system uses lagged explanatory variables in levels and differences as instruments. AR(1) and AR(2) in Table 2 report the p-values for first- and second-order autocorrelated disturbances in the first differences equations, where the null denotes no correlation. The first-order serial correlation AR(1) is expected since we are including lags as instruments. However, a correlation at higher orders than 1 would lead to an inconsistent estimator. Hence, the null should not be rejected for AR(2). Our results are completely robust to GMM estimation.

5.2. Robustness tests

In Table 3, we present further robustness tests considering some intuitive sample restrictions. In Models 1 and 2, we replicate our

Table 2
Effect of democracy and state capacity on the net gini Index—2SLS and GMM.

	2SLS				GMM	
	(1) Boix	(2) Polity	(3) Boix	(4) Polity	(5) Boix	(6) Polity
Lagged Gini	0.937***	0.944***	0.944***	0.941***	0.995***	0.997***
	(0.006)	(0.006)	(0.006)	(0.009)	(0.001)	(0.001)
Boix Democracy	−0.117		−0.096		−0.216**	
	(0.245)		(0.243)		(0.087)	
Boix*Cumulative Census	0.108**		0.085*		0.039*	
	(0.053)		(0.055)		(0.024)	
Polity Democracy		−0.055***		−0.062**		−0.020***
		(0.015)		(0.031)		(0.007)
Polity*Cumulative Census		0.007*		0.011**		0.005**
		(0.004)		(0.003)		(0.002)
Cumulative census	0.097*	−0.082	0.133**	0.012	0.001	0.012
	(0.056)	(0.064)	(0.057)	(0.018)	(0.017)	(0.018)
GDP (log)	0.392***	0.034	0.298**	0.017	0.201**	0.032*
	(0.088)	(0.045)	(0.093)	(0.021)	(0.179)	(0.017)
Inflation	0.000*	0.000	0.000	0.000	0.000***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Urban Population	0.001	0.006	0.002	0.006	−0.001*	−0.002**
	(0.006)	(0.006)	(0.006)	(0.009)	(0.001)	(0.001)
Trade	0.001	0.002	0.002*	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)		
Ethnic Diversity			8.099	1.613		
			(6.531)	(2.836)		
Civil Conflict			−0.091	−0.117*		
			(0.060)	(0.068)		
ODA (log)			−0.006	−0.007*		
			(0.004)	(0.004)		
Left			0.008	0.099*		
			(0.041)	(0.057)		
Growth			−0.604*	−0.069		
			(0.350)	(0.438)		
Fraser Index			0.013	0.015		
			(0.013)	(0.014)		
LR effect at Census = 6	8.43		7.39		3.6	
LR effect at Census = 3	3.29		2.84		−19.8	
Marg. impact at Census = 6	0.531		0.414		0.018	
Mar. impact at Census = 3	0.207		0.159		−0.099	
C-D F stat on excl. IVs	84.26	58.56	73.14	56.78		
Hansen J-stat p-value	0.11	0.14	0.12	0.26	0.38	0.9
Excluded Instruments	3	3	3	3		
Number of Instruments					98	124
AR(1)					0.000	0.000
AR(2)					0.888	0.640
Observations	4034	4020	3999	3894	3160	3042
R-squared	0.741	0.742	0.837	0.769	0.769	0.818

Notes: The dependent variable is the net Gini coefficient and the main explanatory variables are one-period-lagged democratic scores from Boix et al. (2013) and Polity IV by Marshall et al. (2017) interacted with the cumulative census variable. The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. All independent variables are lagged one year. Standard errors are clustered at the country level. ***p < 0.01, **p < 0.05, *p < 0.1.

results with 5-year panels. The effect of political variables on distributive outcomes is usually slow-moving, and democracy might take time to produce results, so longer panel lengths may capture more substantive variation in the variables between each observation (Dorsch and Maarek, 2019). We take the variables' values in the first year of each five-year time period, starting from 1970 (independent variables are lagged by one panel period). We demonstrate that identical results to annual panels are obtained using 5-year panels, interacting Boix (Model 1) and Polity IV (Model 2) variables with our cumulative census indicator.[21] A shift from autocracy to democracy under the highest cumulative census value (6) results in a 7-Gini-point increase in inequality in the long term.

In Models 3 and 4, we replicate our main results, excluding industrial countries from the sample. We replicate our results with both democracy measures. Therefore, we are certain that our results are not driven by an increasing inequality trend in the industrial world since the 1970s but can be generalized more widely to other regions (Atkinson, 2015; Piketty, 2014). In Models 3 and 4, we find an identical effect when excluding former Warsaw Pact nations, where inequality increased after democratization in a relatively high-capacity context. Both long-run and marginal effects look similar to those of previous models. The colonial origins of the countries

[21] Lagged variables are thus lagged by one panel period.

Table 3
Effect of democracy and state capacity on the net gini index: Robustness.

	Mean Imputed series					
	5-year Panels		OECD Excluded		Warsaw Pact Excluded	
	(1) Boix	(2) Polity	(3) Boix	(4) Polity	(5) Boix	(6) Polity
Gini Lagged	0.714***	0.689***	0.942***	0.935***	0.937***	0.936***
	(0.032)	(0.034)	(0.007)	(0.008)	(0.006)	(0.007)
Boix Democracy	−1.177**		−0.112		−0.175*	
	(0.555)		(0.135)		(0.105)	
Polity Democracy		−0.082**		−0.024***		−0.003
		(0.041)		(0.008)		(0.010)
Cumulative Census	0.024	0.448	0.153***	−0.068	0.092**	−0.081
	(0.237)	(0.352)	(0.049)	(0.077)	(0.042)	(0.075)
Boix *Cumulative Census	0.515***		0.080**		0.093***	
	(0.170)		(0.040)		(0.032)	
Polity*Cumulative Census		0.041***		0.003*		0.009***
		(0.013)		(0.002)		(0.003)
Inflation	0.000	0.001	0.000	0.000	0.000	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Urban Population	−0.006	0.001	0.011*	0.007	0.001	−0.003
	(0.028)	(0.029)	(0.007)	(0.007)	(0.005)	(0.006)
Trade	0.013**	0.002	0.002**	0.000	0.001	0.000
	(0.005)	(0.005)	(0.001)	(0.001)	(0.001)	(0.001)
GDP (log)	0.501***	1.692***	0.486***	0.540***	0.383***	0.460***
	(0.101)	(0.481)	(0.103)	(0.101)	(0.089)	(0.103)
LR effect at Census = 6	6.69		6.34		6.08	
LR effect at Census = 3	1.29		2.21		1.65	
Marg. impact at Census = 6	1.913	0.450	0.368	0.01	0.383	0.05
Marg. impact at Census = 3	0.368	0.198	0.128	−0.02	0.104	0.02
Observations	692	665	3170	2987	3782	3469
R-squared	0.967	0.969	0.984	0.985	0.990	0.990

Notes: The dependent variable is the net Gini coefficient and the main explanatory variables are lagged democratic scores from Boix et al. (2013) and Polity IV by Marshall et al. (2017) interacted with the cumulative census variable. Models 1 and 2 consider 5-year panels, while for Models 3–6, the unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. Standard errors are clustered at the country level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

could also affect our results. In Table A6., we exclude French and British developing world colonies from our sample, and replicate our results. This gives evidence that different colonial legacies influencing state capacity do not affect our results.

We also test the robustness of our results using variables that introduce a penalty to the cumulative census score when countries miss the census in a decade in Table 4. In Models 1 and 2 we subtract 1 and Models 3 and 4 0.5 points from the accumulated state capacity score when countries miss a census, interacting these variables with our Boix et al. and Polity democracy indicators. These interaction terms remain significant and the substantive effects are similar to our baseline models in Table 1. In addition, to probe the robustness of our results to other democracy indicators, we show identical results to Table 1 with the democracy variables developed by Cheibub et al. (2010) and Dorsch and Maarek (2019)) (based on Papaioannou and Siourounis, (2008) and Acemoglu et al., [2015; 2019]; see Appendix [Table A7]).

Lastly, we also explore if the inequality-increasing effect of democracy under high capacity works primarily through market (gross) inequality, as we have hypothesized in Section 3, rather than redistribution. In Models 1 and 2 (Table 5), we document a positive interactive effect of Boix et al. (2013) and Polity variables and the cumulative census variable on market inequality. In Models 3 and 4, we find no interactive effect of democracy variables and state capacity on fiscal redistribution (measured as an absolute difference between market and net inequality). This provides evidence that the impact of high-capacity democracy on the net Gini mostly occurs through changes in market income distribution rather than redistribution. Fiscal redistribution is not greater in high-capacity democracies, showing that it is not likely to offset inequality-increasing changes occurring in the market phase. Next, we turn to a test of concrete policy channels through which high-capacity democracies promote higher inequality.

6. Mechanisms

In this section we empirically test the causal mechanisms underlying our theory. We expect democratic rule to be positively associated with FDI inflows and the size of the financial sector only when state capacity exceeds a minimum level. We do not expect democratic rule to have an effect on these variables under low capacity, given the state's inability to provide contract and property rights security. For reasons outlined in Section 3, we do not anticipate a positive effect of democracy on fiscal redistribution and government spending in high-capacity contexts. Lastly, we also expect to see a positive association between FDI stock and financial development and inequality.

We measure financial development with an indicator commonly used in studies of financial development: private credit by deposit

Table 4
Effect of democracy and state capacity (penalized variables) on the net gini index.

	Mean Imputed series			
	(1)	(2)	(3)	(4)
	Boix Penalized 1	Polity Penalized 1	Boix Penalized 0.5	Polity Penalized 0.5
Lagged Gini	0.937***	0.941***	0.938***	0.940***
	(0.006)	(0.006)	(0.006)	(0.006)
Boix Democracy	−0.209**		−0.184*	
	(0.092)		(0.105)	
Polity Democracy		−0.023***		−0.024***
		(0.007)		(0.008)
Cumulative Census	−0.011	−0.100***	0.048	−0.026
	(0.037)	(0.038)	(0.041)	(0.046)
Boix Democracy*Cumulative Census	0.069***		0.060**	
	(0.025)		(0.027)	
Polity*Cumulative Census		0.007***		0.007***
		(0.002)		(0.002)
GDP (log)	0.441***	0.429***	0.443***	0.435***
	(0.084)	(0.082)	(0.084)	(0.082)
Inflation	0.000*	0.000**	0.000*	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Urban Population	−0.003	−0.001	−0.003	−0.002
	(0.005)	(0.005)	(0.005)	(0.005)
Trade	0.001	0.000	0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Constant	−1.846**	−1.870**	−1.930**	−1.958**
	(0.901)	(0.878)	(0.900)	(0.879)
Long-run effect at Census = 6	3.25	0.32	2.84	0.3
Long-run effect at Census = 3	−0.03	−0.03	−0.06	−0.05
Marginal impact at Census = 6	0.205	0.019	0.176	0.018
Marginal impact at Census = 3	−0.002	−0.002	−0.004	−0.003
Observations	4034	3977	4034	3977
R-squared	0.990	0.991	0.991	0.991

Notes: The dependent variable is the net Gini coefficient and the main explanatory variables are one-period-lagged democratic scores from Boix et al. (2013) and Polity IV by Marshall et al. (2017) interacted with the penalized cumulative census variable. The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. All independent variables are lagged one year. Standard errors are clustered at the country level. ***p < 0.01, **p < 0.05, *p < 0.1.

money banks and other financial institutions to GDP. This measurement captures the ratio of claims on the private sector by deposit money banks and other financial institutions to GDP (Beck et al., 2010). FDI is measured through annual FDI inflows to a country as a percentage of GDP, retrieved from WDI. We proxy fiscal policy through the fiscal redistribution measure introduced above and public goods provision through healthcare and education spending (as a percentage of GDP).

To investigate these policy mechanisms driving our theory, we present a series of split-sample regressions. For each policy area, we split the sample with respect to the level of state capacity at which the estimated impact of the Polity index on the Gini coefficient switches from positive to negative (at 3 from Model 6 of Table 1). We test these relationships using similar fixed-effects regressions as in the main analysis while controlling for GDP per capita, GDP annual change, and country- and year-fixed effects. To facilitate exposition, we have plotted the Polity coefficients for each of these 10 regressions in Fig. 4. Lines around the point estimates represent 95% confidence intervals. Tables A8 and A9 in the Appendix present the fixed-effects panel regressions that underlie the coefficient plots presented in Fig. 4.

Fig. 4 provides evidence that democratic rule favors FDI inflows and financial sophistication only in contexts where minimal state capacity has been met. Under low values of state infrastructural power, democracy lacks a relationship with these variables. This suggests that a minimal level of state capacity is necessary for democratic rule to improve the investment climate. By contrast, we find little evidence that democratic rule promotes fiscal redistribution and public goods provision in high-infrastructural power contexts. Democratic rule has a positive effect on health expenditure only in low-capacity contexts. As Knutsen (2013) and Hanson (2015) have argued, democratic rule may operate as a substitute for a capable state in providing better public goods in low-capacity contexts. This is explained by the special propensity of dictatorial rulers to choose non-welfare-promoting policies under low state capacity (Wintrobe, 1998). According to this rationale, a shift from dictatorship to democracy produces greater investments in redistributive social policy under low state capacity compared to democratization under high state capacity.

Lastly, we expect to see a positive association between FDI inflows and financial development and inequality. To test this relationship, we use our baseline model (Model 3 in Table 1) and add lagged private credit and FDI stock (as percentage of GDP) as independent variables, while excluding democracy and state capacity variables (Table 6). Using net inequality as the outcome variable, Model 1 demonstrates a statistically significant relationship with Private Credit, while Model 2 displays a significant association between FDI stock and the Gini index. Models 3 and 4 produce similar results using system GMM analysis.

Table 5
Effect of democracy and state capacity on the market gini index and redistribution.

	Mean Imputed series			
	(1) Boix	(2) Polity	(3) Boix	(4) Polity
	DV: Market Gini	DV: Market Gini	DV: Redistribution	DV: Redistribution
Gini Lagged	0.985***	0.986***		
	(0.003)	(0.003)		
Redistribution Lagged			0.867***	0.871***
			(0.009)	(0.008)
Boix Democracy	−0.224***		−0.242**	
	(0.035)		(0.099)	
Polity Democracy		−0.023***		−0.013*
		(0.002)		(0.007)
Cumulative Census	−0.004	0.021	−0.099**	0.091
	(0.014)	(0.022)	(0.039)	(0.062)
Boix*Cumulative Census	0.066***		0.028	
	(0.010)		(0.030)	
Polity*Cumulative Census		0.007***		0.001
		(0.001)		(0.002)
GDP (log)	0.025	0.021	−0.385***	−0.433***
	(0.028)	(0.028)	(0.081)	(0.079)
Inflation	0.000***	0.000***	−0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Urban Population	−0.005***	−0.004**	−0.016***	−0.013***
	(0.002)	(0.002)	(0.005)	(0.005)
Trade	−0.000	−0.001**	−0.002**	−0.002*
	(0.000)	(0.000)	(0.001)	(0.001)
Long-run effect at Census = 6	11.47		−0.07	
Long-run effect at Census = 3	−1.73		−0.16	
Marginal impact at Census = 6	0.17	0.019	−0.07	−0.007
Marginal impact at Census = 3	−0.026	−0.002	−0.16	−0.01
Observations	4019	3962	4019	3962
R-squared	0.998	0.998	0.988	0.988

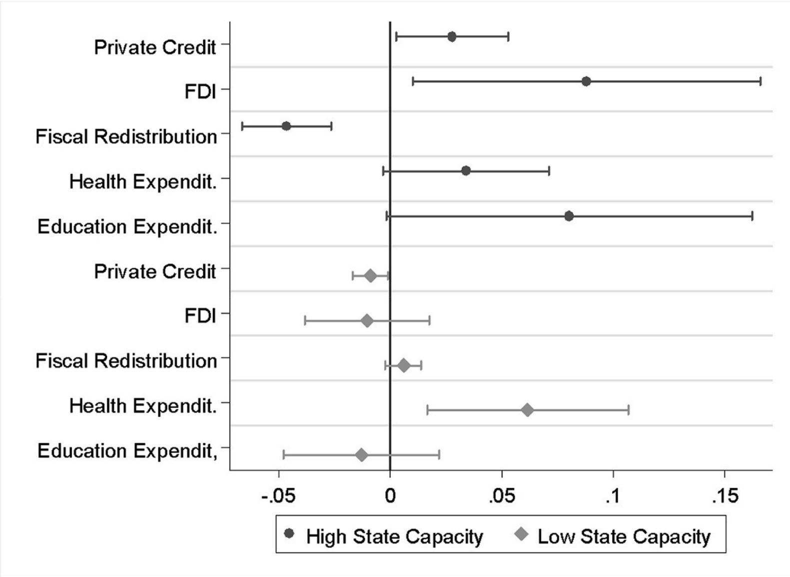
Notes: The dependent variable for Models 1 and 2 is the market Gini coefficient and, for Models 3 and 4, redistribution. The main explanatory variables are one-period-lagged democratic scores from Boix et al. (2013) and Polity IV by Marshall et al. (2017) interacted with the cumulative census variable. The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. All independent variables are lagged one year. Standard errors are clustered at the country level. ***p < 0.01, **p < 0.05, *p < 0.1.

7. Conclusion

In this paper, we have explored whether the effect of democratic rule on income inequality is conditional on state capacity. Counterintuitively, we argue that democratization and democratic rule in the context of high state infrastructural power is associated with increases in income inequality. Larger financial sectors and FDI inflows favor income concentration through market incomes. To test our hypothesis, we introduced a novel measure of state capacity based on cumulative census administration. Our empirical results are robust to instrumental variable and GMM estimation and various alternative measures of democracy, and they apply beyond the context of the industrial world, a high-capacity democratic context where inequality has increased sharply in recent decades. In addition, we also test the mechanisms of our theory, finding consistent support for our claim that the interactive effect of democratic rule and infrastructural power posited in our main analysis might operate through financial development and FDI.

We join the recent literature in exploring the conditional relationship between democracy and inequality. Our contribution is parallel and complementary to that of Dorsch and Maarek (2019), who argue that the effect of democracy on inequality is conditioned by the level of inequality at the moment of democratization. Consistently with our findings, they show that inequality tends to increase after democratic transitions in autocratic nations that had developed a strong state to deliver public goods to the poor, given that their policies tend to move towards the “middle ground” after democratization. By contrast, in regimes that democratize under low capacity, inequality is usually high, which leads to greater catering to demands by median voter for larger redistribution, resulting in a decrease in income inequality after democratization.

Albertus and Menaldo (2018) stress another set of factors—authoritarian constitutions and other institutional legacies—that pose obstacles to fiscal redistribution after democratization and affect inequality that way. In this paper, we stress the inequality-increasing mechanisms associated with democratic rule in high-state-capacity contexts. We believe our conclusion speaks directly to recent scholarship on increasing inequality in the developed world and many regions of the developing world, reflecting the natural tendency of well-functioning capitalism to produce higher income concentration (Piketty, 2014). Institutionalist literature has implicitly assumed that “inclusive institutions” promote not only development but also more equal income distribution, at least in the long term (Acemoglu et al., 2001). In this paper, we have provided evidence that this conclusion may not be warranted and “inclusive” institutions—captured in the combination of democratic regime type and high-capacity state institutions—might well lead to a trend of steady increases in income inequality, which we argue happens through financial development and FDI inflows. Further research



Note: This figure shows the estimated marginal effect of Polity on a series of policy areas for the subsamples with high state capacity (black dots) and low state capacity (gray diamonds), where the subsample cutoff is a cumulative census score of 3. The lines around the point estimates represent 95% confidence intervals.

Fig. 4. Policy Channels of Democratic Effect on Inequality at Different Levels of State Capacity.

Table 6
Effect of private credit and FDI stock on net and market gini indexes.

	Mean Imputed Series			
	(1)	(2)	(3) GMM	(4) GMM
Lagged Gini	0.937***	0.934***	0.996***	0.996***
	(0.006)	(0.007)	(0.002)	(0.002)
GDP per capita (log)	0.371***	0.377***	−0.007	0.009
	(0.092)	(0.097)	(0.017)	(0.015)
Urban Population	−0.005	−0.004	−0.001	−0.001
	(0.005)	(0.005)	(0.001)	(0.001)
Inflation	0.000***	0.000***	0.001**	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Trade	0.001	0.001	0.000	0.000
	(0.001)	(0.001)	(0.000)	(0.000)
Private credit	0.003***		0.001*	
	(0.001)		(0.001)	
FDI stock		0.044**		0.029**
		(0.022)		(0.012)
Constant	−1.417	−0.782	0.054	−0.063
	(0.978)	(1.071)	(0.180)	(0.185)
Hansen J-stat p-value			0.23	0.11
Number of Instruments			120	78
AR(1)			0.00	0.00
AR(2)			0.173	0.037
Observations	3840	3898	3013	2964
R-squared	0.991	0.991	0.991	0.991

Notes: The dependent variable is the net Gini coefficient. The main explanatory variables are one-period-lagged Private Credit and FDI stock. Models 3 and 4 present results from system GMM analysis. The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. All independent variables are lagged one year. Standard errors are clustered at the country level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

should clarify the additional pathways through which high-capacity state institutions and democratic regime type affect inequality.

Data availability

Data available upon request.

Appendix

Table A1
Descriptive Statistics

Variable	Obs.	Mean	Std. Dev.	Min	Max
Net Gini	4636	38.59	8.70	20.43	60.88
Market Gini	4622	45.56	6.41	22.23	70.54
Redistribution	4622	6.99	7.39	−13.86	25.43
Cumulative Census	4449	3.42	1.36	0	6
Polity	4445	3.80	6.59	−10	10
Boix Democracy	3843	0.58	0.49	0	1
GDP per capita	4225	12167.37	16971.26	182.71	111968.30
Urban Population share	4464	53.72	23.51	4.99	100.00
Trade	7144	70.59	47.25	0.02	441.60
Inflation	4344	41.62	410.73	−98.70	15444.38
Private Credit	4127	40.42	36.20	0.85	262.46
FDI Inflows	4089	3.64	13.48	−58.32	451.72
Health Expenditure	2616	3.53	2.05	0.27	10.05
Education Expenditure	2528	4.39	1.66	0	13.21957
Ethnic Diversity	4479	0.42	0.25	0.01	0.93
Civil Conflict	4636	0.22	0.42	0	1
ODA	2969	5.20	8.50	−0.68	181.10
Left	4636	0.56	0.50	0	1
Fraser Index	2105	6.59	1.07	2.47	8.88
Democracy Cheibub (2010)	3928	0.59	0.49	0	1
Democracy Dorsch and Maarek (2019)	4374	0.66	0.47	0	1

Table A2
List of Countries

Afghanistan, Albania, Argentina, Armenia, Australia, Austria, Bahamas, Bangladesh, Barbados, Belarus, Belgium, Benin, Bhutan, Bolivia, Bosnia, Botswana, Brazil, Bulgaria, Burkina Faso, Cambodia, Cameroon, Canada, Chad, Chile, China, Colombia, Costa Rica, Croatia, Cyprus, Czech Rep., Denmark, Djibouti, Ecuador, Egypt, El Salvador, Estonia, Ethiopia, Finland, France, Gambia, Georgia, Germany, Ghana, Greece, Guatemala, Guinea, Haiti, Honduras, Hungary, Iceland, India, Indonesia, Iran, Ireland, Israel, Italy, Japan, Jordan, Kazakhstan, Korea, Rep., Latvia, Lebanon, Lesotho, Liberia, Lithuania, Luxembourg, Macedonia, Madagascar, Malawi, Malaysia, Maldives, Malta, Mauritania, Mauritius, Mexico, Moldova, Mongolia, Morocco, Myanmar, Namibia, Nepal, Netherlands, New Zealand, Nicaragua, Niger, Nigeria, Norway, Pakistan, Panama, Paraguay, Peru, Philippines, Poland, Portugal, Qatar, Romania, Russia, Rwanda, Senegal, Seychelles, Sierra Leone, Singapore, Slovakia, Slovenia, South Africa, Spain, Sri Lanka, Sweden, Switzerland, Taiwan, Tajikistan, Tanzania, Thailand, Togo, Tunisia, Turkey, Uganda, Ukraine, United Kingdom, United States, Uruguay, Vanuatu, Venezuela, Zambia, Zimbabwe.

Table A3
Censuses in Non-Independent Countries.

Country	Independence year	Prior ruling country/empire	Censuses under other countries
Armenia	1991	Soviet Union	1959, 1970, 1979, 1989
Bangladesh	1971	Pakistan	1951, 1961
Belarus	1991	Soviet Union	1959, 1970, 1979, 1989
Benin	1960	France	–
Bosnia	1992	Yugoslavia	1953, 1961, 1971, 1981, 1991
Botswana	1966	Britain	1946, 1964
Burkina Faso	1960	France	–
Cameroon	1960	France	–
Chad	1960	France	–
Croatia	1991	Yugoslavia	1953, 1961, 1971, 1981, 1991
Cyprus	1960	Britain	1960
Czech Rep.	1992	Czechoslovakia	1950, 1961, 1970, 1980, 1991
Djibouti	1977	France	1967

(*continued on next page*)

Table A3 (*continued*)

Country	Independence year	Prior ruling country/empire	Censuses under other countries
Eritrea	1993	Ethiopia	1984
Estonia	1991	Soviet Union	1959, 1970, 1979, 1989
Gambia	1965	Britain	1963
Georgia	1991	Soviet Union	1959, 1970, 1979, 1989
Ghana	1957	Britain	–
Guinea	1958	France	–
Kazakhstan	1991	Soviet Union	1959, 1970, 1979, 1989
Latvia	1991	Soviet Union	1959, 1970, 1979, 1989
Lesotho	1966	Britain	1956
Lithuania	1991	Soviet Union	1959, 1970, 1979, 1989
Macedonia	1991	Yugoslavia	1953, 1961, 1971, 1981, 1991
Madagascar	1960	France	–
Malawi	1964	Britain	–
Malaysia	1963	Britain	1951, 1960
Mauritania	1960	France	–
Morocco	1956	France	–
Namibia	1990	South Africa	1960, 1970, 1981
Niger	1960	France	–
Nigeria	1960	Britain	–
Russia	1991	Soviet Union	1959, 1970, 1979, 1989
Rwanda	1962	Belgium	–
Senegal	1960	France	–
Seychelles	1976	Britain	1960, 1971
Sierra Leone	1961	Britain	–
Singapore	1965	Malaysia	1957
Slovakia	1992	Czechoslovakia	1950, 1961, 1970, 1980, 1991
Slovenia	1991	Yugoslavia	1953, 1961, 1971, 1981, 1991
Tajikistan	1991	Soviet Union	1959, 1970, 1979, 1989
Tanzania	1961	Britain	1948, 1957
Togo	1960	France	–
Tunisia	1956	France	–
Uganda	1962	Britain	1948, 1959
Ukraine	1991	Soviet Union	1959, 1970, 1979, 1989
Vanuatu	1980	France	1967
Vietnam	1976	France	–
Zambia	1964	Britain	1950, 1963
Zimbabwe	1980	Britain	1962, 1969

Table A4
Unit Root Tests for Main Models (5) and (6) in Table 1

Variable	Fisher			Phillips-Perron	Conclusion
	No trend/drift	Trend	Drift		
Gini	0.6670	0.1793	0.0000	0.0000	Stationarity
Boix Democracy	1.0000	1.0000	0.0000	1.0000	Unit root
Polity Democracy	0.0063	0.0000	0.0000	0.0000	Stationarity
Cumulative Census	1.0000	0.0000	0.0000	1.0000	Stationarity
GDP (log)	0.9702	0.0978	0.0000	0.0860	Stationarity
Inflation	0.0000	0.0000	0.0000	0.0000	Stationarity
Urban Population	0.0000	0.0000	0.0000	0.0000	Stationarity
Trade	0.0007	0.0000	0.0000	0.0000	Stationarity

Note: Fisher's tests with both the Augmented Dickey Fuller and the Phillips-Perron specifications were implemented. The values in the table are combined p-values from different independent unit root tests (one per panel). All variables in the main models were tested (models 5 and 6 in Table 1). All tests also included parameters to test for trend and drift. All tests included one lag (same lag structure as the estimation strategy). These tests were implemented using the "xtfisher" routine in Stata (v. 15). The alternative hypothesis is stationarity. As the table suggests, most tests indicate stationary residuals at conventional levels of statistical significance.

Table A5
First-Stage Results

	(1)	(2)	(3)	(4)	(5)	(6)
	DV: Democracy (Boix)	DV: Democracy (Boix)	DV: Boix*Cumulative Census	DV: Democracy (Polity)	DV: De-mocracy (Polity)	DV: Polity*Cumulative Census
L. Boix Regional share	0.934***	1.130***	0.067			
	(0.045)	(0.048)	(0.142)			
L6. Boix Regional share	0.030	0.085**	−0.007			
	(0.036)	(0.037)	(0.109)			

(*continued on next page*)

Table A5 (*continued*)

	(1)	(2)	(3)	(4)	(5)	(6)
	DV: Democracy (Boix)	DV: Democracy (Boix)	DV: Boix*Cumulative Census	DV: Democracy (Polity)	DV: De-mocracy (Polity)	DV: Polity*Cumulative Census
Boix Regional share *Cumulative Census		−0.101***	0.754***			
		(0.007)	(0.022)			
L. Polity Region Average				1.223***	1.375***	1.349***
				(0.120)	(0.126)	(0.358)
L6. Polity Region Average				1.451***	0.128	0.124
				(0.312)	(0.317)	(0.380)
Polity Region Average *Cumulative Census					−0.029***	0.765***
					(0.009)	(0.027)
GDP (log)	−0.124***	−0.078***	0.002	−2.738***	−2.578***	−2.002***
	(0.013)	(0.014)	(0.042)	(0.200)	(0.205)	(0.601)
Inflation	−0.000	−0.000	0.000	0.000***	0.000***	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Urban Population	0.001	−0.003***	−0.019***	0.100***	0.088***	−0.013
	(0.001)	(0.001)	(0.003)	(0.012)	(0.013)	(0.037)
Trade	0.001***	0.001***	0.001*	0.014***	0.019***	0.062***
	(0.000)	(0.000)	(0.001)	(0.003)	(0.003)	(0.009)
Observations	6543	6139	6039	5676	5589	5589
R-squared	0.70	0.56	0.67	0.53	0.71	0.49

Notes: The dependent variables are Boix et al. (2013) (Models 1 and 2) and Polity IV by Marshall et al. (2017) (Models 4 and 5) and their interaction with cumulative census values (Models 3 and 6). The key independent variables include lags of regional democracy share/average indicators and their interaction with cumulative census values. The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. Standard errors are clustered at the country level. ***p < 0.01, **p < 0.05, *p < 0.1.

Table A6

Effect of Democracy and State Capacity on the Net Gini Index: Sample restrictions

	Mean Imputed Series			
	(1)	(2)	(3)	(4)
	Boix French Colonies Excluded	Polity French Colonies Excluded	Boix British Colonies Excluded	Polity British Colonies Excluded
Gini Lagged	0.946***	0.945***	0.953***	0.959***
	(0.006)	(0.006)	(0.006)	(0.006)
Boix Democracy	−0.240**		−0.384***	
	(0.108)		(0.101)	
Polity Democracy		−0.021***		−0.027***
		(0.007)		(0.007)
Cumulative Census	0.096**	−0.037	0.036	0.015
	(0.041)	(0.073)	(0.039)	(0.064)
Boix Democracy*Cumulative Census	0.107***		0.146***	
	(0.032)		(0.030)	
Polity Democracy*Cumulative Census		0.007***		0.009***
		(0.002)		(0.002)
Inflation	0.000	0.000*	0.000	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Urban Population	0.004	−0.002	0.006	0.001
	(0.005)	(0.005)	(0.005)	(0.005)
Trade	0.002**	0.001	0.003***	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
GDP (log)	0.388***	0.412***	0.314***	0.329***
	(0.081)	(0.090)	(0.076)	(0.080)
Constant	1.584***	−1.878**	1.391***	−1.747**
	(0.522)	(0.946)	(0.483)	(0.846)
Long-run effect at Census = 6	7.44	0.38	10.47	0.66
Long-run effect at Census = 3	1.5	0	1.15	0
Marginal impact at Census = 6	0.402	0.021	0.492	0.027
Marginal impact at Census = 3	0.081	0	0.054	0
Observations	3775	3602	3372	3205
R-squared	0.991	0.991	0.993	0.993

Notes: The dependent variable is the net Gini coefficient and the main explanatory variables are one-period-lagged democratic scores from Cheibub et al. (2010) and Dorsch and Maarek (2019) interacted with the cumulative census variable. The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. All independent variables are lagged one year. Standard errors are clustered at the country level. ***p < 0.01, **p < 0.05, *p < 0.1.

Table A7
Effect of Democracy and State Capacity on the Net Gini Index: Alternative Measures of Democracy

	Mean Imputed Series	
	(1) Cheibub	(2) Dorsch and Maarek
Lagged Gini	0.943***	0.935***
	(0.020)	(0.018)
Democracy Cheibub	−0.251	
	(0.154)	
Democracy Dorsch and Maarek		−0.183
		(0.169)
Cumulative Census	0.050	0.035
	(0.067)	(0.079)
Democracy Cheibub*Cumulative Census	0.127***	
	(0.047)	
Democracy Dorsch & Maarek (2019)*Cumulative Census		0.096*
		(0.055)
GDP (log)	0.427**	0.409**
	(0.189)	(0.190)
Inflation	0.000	0.000
	(0.000)	(0.000)
Urban Population	0.004	−0.002
	(0.008)	(0.008)
Trade	0.000	−0.000
	(0.002)	(0.002)
LR effect at Census = 6	8.96	6.05
LR effect at Census = 3	2.28	1.62
Marginal impact at Census = 6	0.51	0.39
Marginal impact at Census = 3	0.13	0.105
Observations	3445	3905
R-squared	0.993	0.991

Notes: The dependent variable is the net Gini coefficient and the main explanatory variables are one-period-lagged democratic scores from Cheibub et al. (2010) and Dorsch and Maarek (2019) interacted with the cumulative census variable. The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. All independent variables are lagged one year. Standard errors are clustered at the country level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A8
The Effect of Democracy on FDI and Financial Development

	(1)	(2)	(3)	(4)
	DV: Private Credit	DV: Private Credit	DV: FDI inflows	DV: FDI inflows
	High Capacity	Low Capacity	High Capacity	Low Capacity
Lagged DV	0.883***	0.957***	−0.117***	0.319***
	(0.008)	(0.008)	(0.023)	(0.019)
Polity	0.028*	−0.009*	0.088*	−0.010
	(0.015)	(0.005)	(0.047)	(0.017)
GDP per capita	0.123***	0.080***	0.086	0.171***
	(0.024)	(0.014)	(0.078)	(0.058)
% Change GDP	−0.603***	−0.227***	0.762**	0.113
	(0.089)	(0.076)	(0.310)	(0.271)
Constant	−0.199	−0.048	0.140	−0.383***
	(0.167)	(0.033)	(0.566)	(0.134)
Wald p-value	0.06	0.06	0.05	0.54
Observations	1950	3096	2105	2802
R-squared	0.987	0.971	0.378	0.367

Notes: The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. All independent variables are lagged one year. Standard errors are clustered at the country level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A9
The Effect of Democracy on Fiscal Redistribution

	(1)	(2)	(3)	(4)	(5)	(6)
	DV: Redistribution	DV: Redistribution	DV: Health Expenditure	DV: Health Expenditure	DV: Education Expenditure	DV: Education Expenditure
	High Capacity	Low Capacity	High Capacity	Low Capacity	High Capacity	Low Capacity
Lagged DV	0.871***	0.830***	0.762***	0.709***	0.612***	0.805***
	(0.015)	(0.012)	(0.016)	(0.026)	(0.035)	(0.020)

(continued on next page)

Table A9 (*continued*)

	(1)	(2)	(3)	(4)	(5)	(6)
	DV: Redistribution	DV: Redistribution	DV: Health Expenditure	DV: Health Expenditure	DV: Education Expenditure	DV: Education Expenditure
	High Capacity	Low Capacity	High Capacity	Low Capacity	High Capacity	Low Capacity
Polity	−0.046***	0.006	0.034	0.062**	0.080	−0.013
	(0.012)	(0.005)	(0.023)	(0.027)	(0.050)	(0.021)
GDP per capita	−0.012	0.005	0.089**	0.235	−0.008	0.118**
	(0.019)	(0.017)	(0.044)	(0.146)	(0.107)	(0.058)
% Change GDP	−0.172**	0.049	−0.624***	−0.514	−0.344	−0.556**
	(0.082)	(0.099)	(0.115)	(0.335)	(0.239)	(0.245)
Constant	0.159***	0.084	0.170	0.750***	0.124	0.030
	(0.053)	(0.076)	(0.117)	(0.103)	(0.268)	(0.118)
Wald p-value	0.00	0.23	0.13	0.02	0.11	0.54
Observations	2020	2056	1770	874	612	1099
R-squared	0.989	0.989	0.972	0.904	0.935	0.935

Notes: The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. All independent variables are lagged one year. Standard errors are clustered at the country level. ***p < 0.01, **p < 0.05, *p < 0.1.

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