Strong Theories, weak Evidence

The Effect of Economic Inequality on Democratization

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This paper summarizes the current state of the theoretical and empirical debate on the potential effect of economic inequality on the odds of democratization. This debate is reconstructed by first focusing on the seminal ‘new structuralist’ theories put forward by Boix as well as Acemoglu and Robinson. Subsequently, pre-existing quantitative studies on the potential inequality-democracy nexus are reviewed. It turns out that new structuralist theories are at best weakly supported by the empirical evidence. This may be partially explained by problems of methodology and variable operationalization. On a more theoretical level, this paper discusses nine potential problems of the new structuralist approaches that have been discussed in recent scholarly work on the impact of inequality on the likelihood of democratization.

Introduction

How does economic inequality affect the emergence and stability of democratic regimes? This question has kept political analysts busy for centuries. Aristotle (1962: 173) once wrote that “where one set of people possesses a great deal and the other nothing, the result is either extreme democracy (mob rule) or unmixed oligarchy or tyranny due to the excesses of the other two.” Alexis de Tocqueville (2004: 266) similarly praised an “equality of conditions” as the most important precondition for democracy: “Between these two extremes [very few rich men and few poor ones] of democratic communities stands an innumerable multitude of men almost alike, who, without being exactly either rich or poor, possess sufficient property to desire the maintenance of order, yet not enough to excite envy.”

The modern social sciences have also extensively stressed the importance of a relatively equal distribution of economic assets for the emergence of democracy. In his classic account of democratization, Lipset (1959: 75) points at the moderating effect of a large middle class and argues in almost Aristotelian fashion that a “society divided between a large impoverished mass and a small favored elite would result either in oligarchy (…) or in tyranny.” Robert Dahl (1971), in his seminal book Polyarchy, spends a whole chapter on the destabilizing effects of extreme inequalities on democratic regimes. However, it took several decades until researchers have explicitly formulated plausible theoretical accounts and derived unambiguous empirical hypotheses on how economic inequality might affect democracy.

Carles Boix (2003) as well as Daron Acemoglu and James Robinson (henceforth AR) have presented path-breaking accounts of democratization that, for the first time, explicitly shift economic inequality to the heart of the analysis (Boix 2003, Acemoglu and Robinson 2000, 2001, 2002, 2006). Due to their main focus on macro-economic variables, their theories have been dubbed “new structuralist” (Ziblatt 2006: 319).

The present paper first presents these two most important contributions to the ‘new structuralist’ literature and then moves on to an empirical and theoretical discussion of this relatively new strand of research. In doing so, it summarizes the existing quantitative studies on the relationship between inequality and democracy and points to their potential methodological weaknesses. It turns out that ‘new structuralist’ theories gain at best limited support when confronted with quantitative empirical evidence. Subsequently, nine theoretical problems that have been raised in the on-going scholarly debate on the ‘new structuralist’ theories are discussed. The final part of the paper concludes and identifies promising areas for future research.

New Structuralist Theories of Democratization

Both Boix and AR use rational choice theory and formal game theoretic modelling to exploit the full potential of Dahl’s (1971: 14–16) cogent intuition that regime choice, in its essence, depends on the relevant actors’ evaluation of the cost of repressing opposition under pre-existing authoritarian institutions vs. the expected costs of tolerating the opposition and/or rule by other social groups under democracy. In both accounts, the relevant actors are conceived of as antagonistic social groups engaged in distributional conflict over scarce resources: “the wealthy” and “the poor” in Boix and “the citizens” and “the elites”, who are also assumed to be divided along income lines, in AR (Boix 2003: 22; Acemoglu and Robinson 2006: 22).

Carles Boix’s ‘Democracy and Redistribution’

In Boix’s account, inequality enters on the ‘cost of toleration’ side of the equation as faced by the wealthy. In relating inequality to the cost of democracy, he heavily relies on the political economy explanation of the size of government put forward by Meltzer and Richard (1981) who in turn build on the median voter theorem (Hotelling 1929; Downs 1957). The observation that income distributions are usually right-skewed (i.e. the mean income is higher than the median income), leads
Meltzer and Richards to conclude that under majority voting, the self-interested decisive voter would have an interest in redistribution. The amount of redistribution the median voter chooses increases with the distance between the median and the mean income, or, put simply, with income inequality within the voting population. By implication, this can also come as a result of extending the franchise to people, who earn below-median incomes (Meltzer and Richard 1981: 924). Boix takes this insight as the central element of a functionalist explanation of why or why not democracy is established in the first place. In more equal societies, extending the franchise only leads to limited redistribution from the rich to the poor and thus the wealthy face lower ‘costs of toleration’ than in the case of democratization in unequal societies. Provided that the expected costs of redistribution fall below the repression costs needed to uphold authoritarian rule, the rich will accept a transition to democracy (Boix 2003). By implication, lower levels of inequality should *veteris paribus* be associated with higher probabilities of transitions to democracy and lower probabilities of reversions back to authoritarianism (Boix 2003: 37).

In his basic model and several extensions to it, Boix also considers a host of additional variables conditioning the impact of inequality on the odds of democratization. The most important one is what Boix calls “asset specificity” – a concept denoting the degree to which wealth is tied to a given country and cannot be moved out in order to avoid taxation (Boix 2003: 38–40). Capital mobility implies lower asset specificity and makes democracy less threatening for the wealthy. If tax rates were too high, the rich would just exit the country. Anticipating this, the relatively poor median voter will set a rather moderate tax rate in order to benefit from the modest amount of redistribution that is feasible under conditions of capital mobility (Boix 2003: 24–25). Thus, democratization becomes more likely, even if inequality is quite substantial. A high income elasticity to taxation similarly reduces the impact of inequality on democratization (ibid.). On these grounds, Boix argues, economic development is conducive to democratization since it transforms “societies that rely on the exploitation of mines and agricultural land to economies based on manufacturing industries and human capital-intensive businesses” and thus makes capital less taxable. While globalization in the form of financial integration has similar effects (Boix 2003: 41), the impact of trade is ambiguous: If labour-abundant economies open for trade, inequality decreases and democratization becomes more likely, whereas the reverse holds true for capital-abundant countries (Boix 2003: 170).

Absent extremely low levels of inequality or asset specificity, repression costs become a crucial variable. Boix (2003: 26) regards repression cost as a “function of population shares, the economic resources of each class, the extent to which individual actors have overcome any collective action problems, landscape, international aid and so on” but does not explicitly model it. Instead he assumes that the costs of repression can be either “high” or “low.” According to Boix (2003: 28–30), high repression costs are often the consequence of information on “defeats in war, the death of the dictator, or internal struggles in the ruling elite ... prompting citizens to update their beliefs on the probability of survival of the existing political arrangement” and thus facilitating their collective action. In the context of moderate levels of inequality and asset specificity, high repression costs can trigger peaceful democratization. With high inequality and asset specificity, however, the wealthy have too much to lose and choose repression. Drawing from Fearon’s *Rationalist Explanations for War* (Fearon 1995), Boix predicts revolutions and civil wars if “the poor underestimate the repressive technology of the wealthy while the wealthy play down the organizational capability of the poor” (Boix, 2003: 28). Depending on who wins, the result will either be stable authoritarianism or a communist dictatorship (Boix 2003: 45).

Note that the poor primarily enter the model as a factor that determines the repression costs on behalf of the rich and thus affects their cost-benefit calculus of whether or not to grant democracy. Against this backdrop, Boix’s model has been criticized as being too supply-centric and neglecting the “demand side of democratization” (Ansell and Samuels 2010: 1570). Boix assumes the rational poor individual to be always in favour of democracy due to its expected benefits from redistribution. Thus the mobilization of the poor, which is by assumption either absent or present, becomes the crucial factor. The insights from Boix’s merely verbal discussion of the informational sources of mass mobilization remains tentative, to say the least. He neglects the possibility that both mass preferences for democracy and the ability of the poor to overcome collective action problems might vary across countries with similar levels of inequality for other reasons than updated information (Welzel and Inglehart 2008: 36). Even worse, both factors might be systematically linked to key variables of his model.

**Acemoglu and Robinson’s ‘Economic Origins of Dictatorship and Democracy’**

Acemoglu and Robinson partially circumvent this problem. Just as Boix, they assume inequality to make democracy more costly for the elites due to higher levels of taxation (Acemoglu and Robinson 2006: 36). However, they also argue that elites will only make democratic concessions if they are faced with an effective revolutionary threat from below – “the spark that ignites” the whole process (ibid.). In order to pose such a threat “the masses need to solve the collective action problem necessary to organize themselves, they need to find the momentum to turn their organization into an effective force against the regime, and the elites – who are controlling the state apparatus – should be unable to use the military to effectively suppress the uprising” (Acemoglu and Robinson 2006: 25). AR expect this mobilizational capacity of the citizens to increase with inequality, since they have more to gain from revolution or democratization the more redistribution is possible. In other words, a certain amount of inequality is required to spur the demand for democracy in societies under authoritarian rule. (ibid.: 36) Combining these two opposing effects of inequality, AR predict it to exhibit an “inverted U-shaped ... relationship” when plotted against the probability of transition to democracy and conclude that “democracy has the best chance to emerge in societies with middle levels of inequality” (ibid.: 37).

Apart from this theoretically more elaborate treatment of inequality, the AR model offers further innovations. First, instead of looking at a static picture, as Boix does, AR take inter-temporal dynamics into account. They regard the “*de facto* political power” that citizens must have accumulated to be able to mount a revolutionary threat to be transitory in nature. Being aware of this, citizens do not only claim redistribution, which is the actual policy change they are fighting for. In order to also secure redistribution in the future, when their *de facto* political power may be gone, citizens push for more permanent “*de jure* political power” through the installation of democratic political
institutions \( (\text{ibid.:} \ 21-26) \). In fact, AR argue that democratic concessions represent the only way for elites to credibly commit themselves to “future pro-majority policies” \( (\text{ibid.}) \).

Second, AR explicitly model not only transitions to democracy but also democratic consolidation \( (\text{Acemoglu and Robinson} \ 2006: \ 221–253) \). Their account of consolidation is basically their democratization model with reverse sign. Now, the citizens have de jure political power and the elites from time to time accumulate sufficient de facto political power to mount an anti-democratic coup and change political institutions back to authoritarian rule \( (\text{ibid.:} \ 30–31) \). As coups are a rather unilateral affair – no one has to ask the citizens to ‘grant’ a coup – their likelihood merely depends on the elites’ calculus of their costs vs. their benefits. Quite clearly, the benefits of coups increase with inequality, which should therefore exhibit a linear and negative correlation with democratic consolidation \( (\text{Acemoglu and Robinson} \ 2006: \ 37–38) \).

Third, the structure of wealth is not only assumed to influence the odds of democratization through the channel of capital mobility but also through its impact on repression costs. The costs of repression and coups increase the more an economy relies on physical and human capital instead of agricultural land \( (\text{ibid.:} \ 31–32) \). In short, we see that although starting from very similar premises, Boix and AR arrive at fundamentally different conjectures when it comes to the impact of inequality on democratization. With a clear understanding of these competing hypotheses and their theoretical derivations, we can now take a first look at which model fares best when confronted with empirical evidence.

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Quantitative Studies and their Problems

As far as quantitative evidence is concerned, Table 1 lists 17 studies that have been conducted over the past decades and gives information on the respective operationalization of inequality and democracy as well as the basic thrust of their results. In total, Table 1 reveals an inconsistent picture. Income inequality has been found to negatively (9 studies), positively (2 studies), curvilinearly (2 studies) not at all (3 studies), or only conditionally on a third variable (2 studies) affect democracy. Land inequality also fails to produce consistently negative correlations with various measures of democratization. In this sense, the quantitative evidence on a potential inequality-democracy nexus is similarly mixed as the evidence on the relationship between inequality and political conflict (cf. Lichbach 1989).

The ambiguity in results is not the only drawback of these quantitative studies. Upon closer scrutiny, most quantitative studies on the potential inequality-democracy nexus suffer from severe limitations in terms of methodology and/or data quality.

Following Wucherpfennig and Deutsch (2009), let me point to five common pitfalls in the quantitative study of democratization and their specific implications for estimating the role of inequality.

1) Most studies might be subject to selection bias. Data on income inequality usually covers only limited numbers of countries and time periods. Data availability is usually not randomly distributed. Houle (2009: 600) shows that the sample of observations included in the most popular inequality dataset is systematically biased towards rich democracies. This might bias any estimation results derived with such data: if e.g. authoritarian states exhibiting intermediate levels of inequality – many Middel Eastern countries share these characteristics – are systematically underrepresented in the data, any finding of an inverse-U shaped relationship between inequality and democratization looses credibility (ibid.).

2) The choice of the most appropriate indicator for inequality is difficult. The most commonly used Gini index comes with a whole bunch of problems, even beyond data availability. Most inequality datasets combine information from different sources that may be problematic in terms of comparability. Sources differ with respect to whether data on income or on spending is collected, whether the household or the individual is the unit of observation, whether net or gross income is reported and whether the data stems from one particular region or is representative for the whole country (Gradstein and Milanovic 2002: 10); Houle 2009: 597). The use of net or disposable income data for testing hypothesis relating to democratization is particularly questionable. This data captures income after redistribution through taxes and government has taken place (Milanovic 2009: 3). Provided that democracies indeed tend to redistribute more than non-democracies, a finding that lower inequality is statistically associated with the odds of democracy would risk being tautological. Moreover, as Acemoglu and Robinson (2006: 35–36) point out, individual- or household-level inequality data might miss the theoretical target of their analysis. Instead, inter-group inequalities, for which almost no systematic data is available are more relevant. Using more indirect proxies for economic inequalities, like e.g. the percentage of family farms or capital shares is a viable alternative strategy but requires careful theoretical justification and should be in line with the causal mechanisms one postulates.

3) Choosing the right indicator for democracy is also not straightforward. The existing measures differ quite substantially when it comes to conceptual questions regarding which definitional attributes to include, how to aggregate various indicators and whether democracy is a dichotomous or a continuous concept (Munck and Verkuilen 2002; Bollen and Paxton 2000). Moreover, the decision to analyze democracy levels, transitions to or reversions from democracy should be in line with the theoretical predictions of the respective model. For studies of democratization, the latter are usually preferable, since democratization is about temporal dynamics in democracy levels and thus about As cited in Russett (1964: 442) the presence of transitions and absence of reversions (i.e. democratic consolidation).

4) The older studies mainly use mere cross-sectional designs with a limited number of countries, whereas the newer studies resort to time-series- cross-section analyses, in which they are able to capture temporal dynamics and actual regime changes. This further reduces the comparability of results.

5) Finally, the form of the relationship between inequality and democracy matters. Before Acemoglu and Robinson published their book, most studies only tested for a linear relationship but neglected the possibility of a non-monotonic correlation (e.g. an inverted U). Only recently, scholars started to take this possibility into account, however with quite ambiguous results. Houle (2009) finds no and, if anything, U-shaped effect of income inequality on the odds of transition, Ansell and Samuels (2010) report a monotonically positive effect, whereas Freeman and Quinn (2012) find an inverted U, but only in financially closed autocracies.

In sum, therefore, neither Boix (2003) nor Acemoglu and Robinson (2006) receive overwhelming empirical support from the pre-existing quantitative studies. Neither is income or land inequality systematically linked to lower levels of democracy, nor do they consistently increase the probabilities of democratic transition and/or consolidation. The inverted U-shaped relationship postulated by Acemoglu and Robinson also does not appear to hold in general.

Theoretical Problems and their Empirical Consequences

While the weak evidence for Boix’s and AR’s theoretical predictions may to a large part be traced back to the methodological and data problems just outlined, the possibility
remains that theoretical misspecifications bear their share of the brunt. In the following paragraphs, I discuss the plausibility of a series of implicit or explicit assumptions underlying the game theoretic models of both Boix and Acemoglu and Robinson and how problems with these assumptions might explain the weak fit between the theoretical models and empirical data.

Inequality, the demand for democracy and costs of repression

Above, I have praised AR for partially integrating the demand for democracy in their model. A certain level of inequality is needed to confront the elites with an effective revolutionary threat that sets the democratization process in motion (Acemoglu and Robinson: 120–126). Below that threshold level, autocracy is assumed to be stable. However, as Houle (2009: 594) points out, the intensity of protest might be assumed to vary continuously with inequality in such a way that even at low levels of inequality, the regime needs to deal with some sort of low-scale protest implying non-zero repression costs. In order for AR’s predictions to hold, one has to further assume that at low levels of inequality, maintaining autocracy is always cheaper than redistribution under democracy. This might not necessarily be the case. In a very equal society, redistribution might as well be less costly than repression of low-scale protest that fails to reach the revolution threshold and thus result in Boix-style democratizations at low levels of inequality (ibid.). Both repression and redistribution need to be financed through taxation. Hence, it is a priori unclear, which one comes at a lower cost.

AR’s conceptualization of protest or threat intensity is not only problematic at low levels of inequality. If protest intensity was assumed to systematically increase with inequality, which does not seem implausible, repression costs would most probably also rise with inequality. If this is the case, the relationship between inequality and democratization crucially depends on the positions and slopes of both the cost of redistribution and the cost of repression curve plotted against inequality. In order to arrive at an inverted U-shape, one has to implicitly assume the redistribution cost curve to ‘start’ below the repression cost curve and to have a higher slope in order to ensure that both curves cross at some point. Only under such circumstances it can be argued that at high levels of inequality, elites prefer repression to democratization, whereas at medium levels the reverse holds true. Furthermore, one has to conceive of a threshold level of inequality below which no substantial repression costs accrue in order to rule out Boix-style democratizations at extremely low levels of inequality. Those are indeed rather strong assumptions. Quite evidently, one could easily come up with constellations of repression and redistribution costs implying democratization to be most likely at low and/or high or even at all or no levels of inequality, without necessarily being less plausible than Acemoglu and Robinson.

Hence, the demand for democracy and its effect on the repression costs faced by the elites could be better specified not only in Boix but also in the more sophisticated AR model. The same criticism however does not apply to AR’s theory of democratic consolidation. As coups are a rather unilateral affair – forceful seizure of political power by the elites – it is their cost-benefit calculus that matters. Citizens are not in a position to ‘grant’ the elites a coup. Therefore, the ‘supply side’ of coups is negligible. This makes it less ambiguous to gauge the effect of inequality on the likelihood of democratic breakdown. Inequality unequivocally increases the benefits the elites draw from the re-establishment of authoritarian rule, since democratic redistribution is more severe than in equal societies. On these grounds, Houle (2009) hypothesizes that inequality does not affect the likelihood of democratic transitions but only hinders democratic consolidation. These hypotheses are borne out in his dynamic probit analysis covering 116 countries between 1960 and 2000 (ibid.). In this sense, Houle confronts the theories by Boix and AR with the same sort of criticism that classic modernization theory has been confronted with by Przeworski and his co-authors.

The mystery of collective action

On a more general level, but still on the demand side of the equation, both Boix and AR regard collective action on behalf of the masses as crucial element. Yet both accounts have been criticized for largely failing to explain why, when and under which circumstances citizens manage to mobilize and kick-start the process of democratization (Keefer 2009; Houle 2009: 595; Haggard and Kaufmann 2012: p. 513). To be fair, Boix (2003: 28–30) and AR (2006: 36) briefly discuss the role of information and inequality in citizens’ mobilization. Moreover, AR (2006: 123–128) discuss three standard economic solutions to collective action problems – ideological indoctrination, providing selective private benefits and excluding non-participants from benefits – and some empirical examples, where they have been put into practice in the context of revolutions. Nonetheless, things appear to be more complex.

Haggard and Kaufman (2012) point at three potential factors that might determine the degree to which citizens can overcome barriers to collective action and should therefore be seen as preconditions for inequality to affect the odds of democratization. First, economic development and modernization, especially in the forms of industrialization and urbanization, may be at the basis of collective organization, e.g. in the form of organized labour movements (Haggard and Kaufman 2012: p. 513). Second, institutional preconditions of the respective authoritarian regime may matter (Geddes 1999). Previous experiences with democracy or other forms of “institutionalized opportunities for collective action” (Haggard and Kaufman 2012: 513) as well as the existence and organization of political parties (Keefer 2009) or trade unions (Collier and Collier 1991) are likely to play a role. Third, non-rationalist accounts rooted in the literature on ‘social movements’ and ‘contentious politics’ stress more sociological aspects and political resources such as cultural reference frames, nationalism and ethnic identities (Haggard and Kaufman 2012: 513; McAdam, Tarrow and Tilly 2003). Against this backdrop, the inconsistent empirical findings might spring from the authors’ failure to properly specify the appropriate collective-action-related scope conditions under which inequality may exert its influence.
Are unequal democracies really that threatening?

The theories of both Boix and AR crucially rest on Meltzer and Richard’s proposition that greater inequality leads to more redistribution under majority voting and thus makes democracy more threatening for the elites. While this appears intuitively plausible, whether it holds empirically, is debated quite controversially. Some studies do find that inequality is associated with higher redistributive spending (Milanovic 2009; Gradstein and Milanovic 2004), whereas others fail to find a statistically significant relationship (Moene and Wallerstein 2001, 2003; Perotti 1996). If this key relationship does not hold, the models of Boix and AR are very unlikely to be of any empirical value.

Soifer (2009) argues that, at the very least, the effect of inequality on redistribution and thus on the odds of democratization appears to be conditional on the respective state apparatus’ capacity to effectively collect the taxes needed to finance redistribution. Thus, he posits, democracy should be less threatening in states with weak extractive capabilities, regardless of the level of inequality. In his empirical analysis, Soifer uses the state’s ability to conduct a national census as a proxy for its capacity to tax. As expected, he finds a negative effect of inequality on the chances of democratic transition in administratively strong state, but no effect in states with weak extractive capacities (Soifer 2009).

Who acts?

The “new structuralist” theories of Boix and Acemoglu and Robinson have been deservedly praised for combining the strengths of modernization theory’s focus on macro-structural socio-economic variables with an explicit specification of political actors and political dynamics that have been stressed by rather process-driven accounts. As Ziblatt (2006: 322) puts it, “their accounts improve upon the agentless structural functionalism implicit in modernization theory by reasserting the primacy of collective actors’ resources, preferences, and strategies.” However, Ziblatt (2006) criticizes the conceptualization of the relevant actors as “the rich, the poor, and the middle class” as not sufficiently justified in terms of empirical relevance. With these abstract categories of actors, Ziblatt (2006: 322) fears, the ‘new structuralists’ “risk suffering precisely the same absence of agency that mars modernization theory.” In this respect, scholarship in the tradition of comparative historical analysis might have an advantage by focussing on actor categories that are more concrete in terms of historical substance: Be it landholders and the rising bourgeoisie in Moore’s (1966) classic account, or landowners and organized labour as argued by Rueschemeyer, Stephens, and Stephens (1992). On the downside, however, these social classes are likely to be more context-bound and it is far more difficult to make any general claims about their preferences and interactions than it is the case with the mere income-based actor categories employed by Boix and AR (see Huntington 1991: 39).

Why democracy and not mere redistribution?

If citizens rise up mainly for material gain, why do they fight for democracy and not only for the specific redistributive policies they want in the first place? Acemoglu and Robinson regard democracy as a credible commitment to present and future redistribution. Under authoritarianism, they claim, elites would face an extremely salient incentive to renege on any promise made in the face of a revolutionary threat as soon as the de facto power of the citizens fades away. Anticipating this, the citizens will thus regard any such promise as cheap talk and require a more credible commitment to redistribution. In this framework, democracy is the only incentive-compatible solution, since it permanently equips the citizens with de jure political power and therefore deprives the elites of any opportunity short of a military coup to reverse their agreement to redistribution (Acemoglu and Robinson 2006).

Is democracy really the only solution? Smith (2008) argues otherwise and claims that authoritarian rulers can sometimes employ strategies of redistribution that are sufficiently credible to buy off revolutionary threats. He illustrates his point with the example of Singapore, which for Acemoglu and Robinson (2006: 44) represents a country in which wealth is distributed so equal that no effective revolutionary threat ever emerged. First, Smith (2008: 19) demonstrates that Singapore exhibits consistently higher levels of economic inequality than Argentina during its democratic transition in 1973. In AR (2006: 5–8), Argentina is portrayed as an extremely unequal society oscillating between democracy and autocratic coups. Second, Smith (2008) shows that upon Singaporean independence, economic inequality was substantial. He argues that high inequality indeed spurred Socialist revolutionary challenges to the ruling PAP. Instead of granting democratic concessions, however, the PAP resorted to large-scale and long-term spending on housing, education and health policies. These policies sharply reduced inequality and successfully staved off revolutionary threats. From this, Smith (2008: 19) concludes that “authoritarian ruling parties can often provide a credible commitment to social actors sufficient to retain their support for long periods of time.”

Inequality of what? The differences between land and capital income

Both Boix (2003) and Acemoglu and Robinson (2006) argue that land is less mobile and easier to tax than physical and human capital. Hence, landed elites are expected to be especially wary of redistribution under democracy. However, they do not systematically test the implications of this argument for the inequality-democracy nexus. Boix’s interpretation of his empirical findings suggests that, if anything, land and income inequality have similarly negative effects on the odds of transitions and consolidation. While a closer look at some of his regression models yields a somewhat more inconsistent pattern – land inequality surprisingly appears to be related with higher chances of transition – Boix at no point reflects on the possibility that land and income inequality could be theoretically expected to have different effects.
The notion that land inequality is particularly inimical to democracy can be traced back at least to Gerschenkron’s (1989[1943]) *Bread and Democracy in Germany*, in which he attributes the concentration of land ownership in the hands of the Prussian Junkers as a key obstacle to democratization. Moore (1996) as well as Rueschemeyer, Stephens and Stephens (1992) also regard a landed aristocracy, controlling large shares of agricultural land, as detrimental to democracy.

Ziblatt (2008) tests the potentially differing effects of income and land inequality in an innovative quantitative analysis of a Prussian parliament vote in May 1912 on a proposal to replace the inequalitarian three-class voting system with a more democratic alternative. He finds that “representatives from electoral constituencies marked by high levels of landholding inequality were more likely to vote against democratic reforms”, whereas income inequality had no effect (Ziblatt 2008). Do these results hold beyond the case of pre-WW I Prussia? Ansell and Samuels (2010) develop a formal model, in which not the poor masses, but instead a disenfranchised yet economically rising bourgeoisie à la Moore is the key driver of democratization. Referring to Douglass North (1990) and Mancur Olson (1993), they argue that for disenfranchised elites, autocracy is more costly than democracy, since they fear the constant threat of being expropriated by the authoritarian leaders.” In their view, disenfranchised elites push for limited extensions of the franchise in order to secure a credible commitment from the incumbent elites not to violate their property rights. Elites grant democratic concessions if they face a realistic possibility of loosing a potential revolutionary confrontation with their so far disenfranchised competitors. How is this possibility related to inequality? Assuming that a landed elite is in power, land inequality is detrimental to democratization. It decreases out-groups’ motivation to mobilize for the protection of property rights, as they control less property in need of protection. In addition, land inequality lowers the out-groups’ chances of winning in a confrontation with the well-equipped incumbents. As a result, no substantial demands for democratization arise (Ansell and Samuels 2010: 1553).

Extensions of the franchise only become an option if the wealth of disenfranchised industrial elites is growing. This increases their need for property rights protection and their chances to prevail in a violent confrontation with the incumbent landed elites. The latter grant democracy in order to avoid a likely defeat. Ansell and Samuels (2010) argue that the rise of industrial elites and a bourgeois class is typically associated with higher levels of income inequality and thus hypothesize a positive correlation between income inequality and democratic transitions.” These expectations are confirmed in their empirical analysis.

Midlarsky (1992), who surprisingly finds a positive relationship between land inequality and democracy tries to rationalize this finding by arguing that inequality provided a basis for selectively allocating political rights that were later gradually extended. He further speculates that inequality might spur the demand for democracy and revolutionary violence or that it even sets in motion processes of urbanization, which then fosters democracy (Midlarsky 1992: 457–462).

**Open or closed economies?**

While AR’s basic model assumes a closed economy, they later extend their analysis and model the effects of international trade and financial integration (Acemoglu and Robinson 2006: 321–348). They argue that the effect of trade depends on its impact on the income distribution. In labour-abundant countries, trade can be expected to reduce inequality, whereas the reverse holds true for capital-abundant economies. The effect on the odds of democratization thus ultimately depends on where the respective country is on the inverted U between inequality and transition probability (ibid.: 41). As far as financial integration is concerned, AR postulate a similar effect. In labour-abundant countries, capital inflows reduce the returns to capital and increase the returns to labour, thus attenuating income inequality. Moreover, the increased ease at which capital can be moved across borders sets a limit on the levels of taxation that are possible in a financially open democracy. In sum, economic openness is argued to make democracy in labour-abundant countries less threatening for the elites (ibid.: 338–343). Given that inequality is sufficiently high to motivate citizens to demand democratization, this should increase the odds of transition.

With respect to financial integration, Freeman and Quinn (2012) argue otherwise. They claim that free capital flows increase income inequality, but still foster democratization. They base their argument on three interrelated mechanisms:

1) In open autocracies, elites as rational profit-maximizing investors would construct an internationally diversified portfolio of assets, regardless of whether democratic redistribution is a realistic threat or not. With only a part of their wealth remaining in the country, the introduction of democracy is less costly and coups do not tend to be worth the effort (Freeman and Quinn 2012: 62).

2) Substantial parts of the respective country’s capital stock will be owned by foreigners, who also hold diversified portfolios and have only small stakes in the domestic politics of one out of many countries in which they are invested. In addition, financial openness allows domestic owners of immobile assets to sell these to foreign investors seeking diversification. This implies that even landowners have less to fear from democracy, since they can trade their possessions into more mobile forms of wealth. As a corollary of these asset sales and the higher returns of diversified portfolios, the income gap between native capital-owning elites and citizens can be expected to increase (ibid.).

3) As in AR, international capital mobility is assumed to limit the feasible level of taxation but not to such an extreme extent that citizens would prefer revolution to democratization (ibid: 61).

In their empirical analysis, Freeman and Quinn (2012) find that inequality positively affects the probability of transitions in financially open autocracies, whereas in financially closed autocracies the relationship resembles the inverted U as postulated in AR.
When is inequality politically salient?

Furthermore, the reasoning of Boix (2003) and Acemoglu and Robinson (2006) as well as all cross-country studies on a potential link from inequality to democracy may be subjected to a more sociologically oriented criticism, concerning the question under what circumstances economic inequality emerges as a politically relevant factor in democratization processes. Critical levels of inequality from which on the demand for democratic reforms is sufficiently strong or the supply is denied are likely to vary across spatial and temporal contexts. Different societies might show different levels of toleration when it comes to economic inequalities. Bollen and Jackman (1985: 152) argue that some societies might emphasize “equality of results”, whereas others are more concerned with “equality of opportunity”. In short “the political interpretation” of inequality matters (Bollen and Jackman 1995: 984). Ziblatt (2006: 321) makes a similar point in claiming that revolutions might occur at levels of inequality that are relatively unsuspicious in cross-country comparison, provided that there are rapid changes in inequality. Hence, longitudinal within-country comparisons might be more appropriate than mere cross-country analyses. In sum, it appears that “what matters just as much as any ‘measurable levels’ of inequality are the perceptions and possible misperceptions of real individual and collective actors embedded in changing social structures” (ibid.).

Notwithstanding the intuitive validity of this line of reasoning, it is not without plausibility to assume that inequality as perceived by the relevant actors is somehow systematically linked to objective underlying fundamentals. Hence, this point provides and insufficient basis for a wholesale rejection of the ‘new structuralist’ theories. At the very least, however, economic inequalities seem to be particularly relevant for democratization processes if they reinforce what Tilly (2003) dubs “categorical inequalities”. This term refers to “organized differences in advantages based by gender, race, nationality, ethnicity, religion, community, and similar classification systems” (Tilly 2003: 37). In Tilly’s account, the reduction of such categorical inequalities is a crucial precondition for democracy. By stressing the importance of “inter-group inequality” and at least verbally allowing for other than income-based cleavages, Acemoglu and Robinson (2006: 35, 42) appear to partially acknowledge this.

Many roads may lead to democracy

One of the main strengths of the new structuralist theories is that they use a parsimonious framework build on clear assumptions to derive testable hypothesis that may hold across a variety of cases. On the other hand, however, their generalist, if not universalist aspirations have also been criticized as their main weakness (see e.g. Haggard and Kaufman 2012, Houle 2009). It might well turn out to be an illusionary endeavor to search for an explanation that covers most or even all instances of democratization. This point has been raised before by scholars such as Linz, Stepan and (1978), O’Donnell and Schmitter (1986), Huntington (1991), Collier (1999) and Przeworski et al. (2000).

Huntington (1991: 35) sees one of the main reasons for this in the simple fact that democratization is a complex phenomenon involving “(1) the end of an authoritarian regime; (2) the installation of a democratic regime; and (3) the consolidation of the democratic regime. Different and contradictory causes may be responsible for each of these three developments.” The potentially differing effects of inequality on each of these stages have been empirically illustrated by Houle (see above). Huntington (1991: 38) further argues that:

1) No single factor is sufficient to explain the development of democracy in all countries or in a single country.
2) No single factor is necessary to the development of democracy in all countries.
3) Democratization in each country is the result of the combination of causes.
4) The combination of causes producing democracy varies from country to country.
5) The combination of causes generally responsible for one wave of democratisation differs from that responsible for others.
6) The causes responsible for the initial regime changes in one wave are likely to differ from those responsible for later regime changes in that wave.

While recent scholarship has definitely made progress and the state of the art of democratization studies today arguably looks somewhat less sobering than back in 1991, a basic insight remains: The problems of potential equifinality and conjunctural causation remain a formidable challenge for every student of democratization.

Haggard and Kaufman (2012) propose categorization schemes for both transitions to and reversions from democracy. First, they distinguish between distributive and non-distributive transitions. Distributive transitions conform to the new structuralist theories and require the “mobilization of redistributive grievances on the part of economically disadvantaged groups” as well as “authoritarian withdrawal” in response to increasing costs of repression (Haggard and Kaufman 2012: 500). Non-distributive transitions come in one of three main forms: They may be the results of (1) international pressures and/or intervention, (2) intra-elite rivalries, or (3) elite-driven reforms, implemented in the belief that control over the process can be maintained (ibid.). Then, Haggard and Kaufman classify democratic reversions according to whether they are distributive or non-distributive in nature. Among the distributive reversions, they distinguish between “elite-reaction reversions”, in which elites intervene in order to restrict redistribution as predicted by Acemoglu and Robinson and “populist reversions”, in which the incoming authoritarian ruler promises more instead of less redistribution. Among non-distributive reversions, there are cross-cutting challenges exploiting “wide disaffection with the performance of democratic incumbents and invoke broad valence issues,
such as economic performance and corruption” and intra-elite reversions such as military coups with no clear distributive aims (Haggard and Kaufman 2012: 508).

In order to empirically assess which types of transitions and reversions exhibit the greatest empirical relevance, Haggard and Kaufman have compiled a qualitative dataset including causal process observations of all transitions and reversions between 1980 and 2000. They show that depending on which democracy measure they use, only 55 to 58% of democratic transitions have been distributive in nature. Even worse, they do not find any systematic relationship between the level of inequality and the probability of distributive transitions – they appear to occur at any level of inequality (ibid.). When it comes to democratic breakdown, they find that 63% of reversions are non-redistributive and that among the remaining 37% that are distributive reversions, even 16% are populist and thus contradict AR’s predictions (ibid.). While reversions indeed tend to be more likely at medium and high levels of inequality, the reason for this pattern does not seem to be explained by new structuralist theories. Along the lines of Huntington (1991: 39), these theories and more generally, income inequality might fare better in explaining instances of democratization in the first and/or second wave. However, this proposition has still to be substantiated by systematic empirical evidence.

At any rate, the theories of Boix and Acemoglu and Robinson are unlikely to hold in general. Thus, we can conclude that the effect of economic inequalities, if they affect democratization at all, is likely to depend on a host of conditioning variables as well as on specific spatial and temporal contexts. The further specification of plausible scope conditions and alternative routes to democratization remain promising avenues for further research.

Conclusion

As the present literature review has revealed, economic inequality has recently emerged as one of the most prominent structural variables in the study of democratization. The potential detrimental effects of inequality on the odds of democratization have been frequently stressed and distributional conflict has long been an important element in the classic accounts of democratization from Lipset and Moore to Dahl. However, it needed the groundbreaking contributions of Boix and Acemoglu and Robinson to shift economic inequality to the centre of analysis. Based on clear assumptions and parsimonious theoretical frameworks, these accounts have derived plausible, empirically testable hypotheses. In this respect, new structuralist theories represent a quantum leap in terms of theoretical rigour.

When it comes to the empirical relevance of these theories, the picture looks less benign. The predictions of Boix and Acemoglu and Robinson have only found limited support in quantitative studies. This may in part be explained by difficulties in appropriately measuring both inequality and democracy. Future research should therefore focus on collecting better data on inequality. A particularly promising point of departure can be seen in pioneering datasets on inter-group inequalities. Apart from issues of data quality and empirical methodology, recent research has highlighted some theoretical problems of the new structuralist theories.

These problems include a neglect of the demand for democracy, a lacking explanation under which circumstances barriers to collective action can be overcome, a potentially too abstract conceptualization of political actors, a possible overstatement of the redistributive consequences of democracy, a possible overstatement of commitment problems, an overly simplistic treatment of economic openness, potentially differing effects of land and income equality as well as a neglect of the question how political actors perceive inequality. On a more general level, new structuralist theories might be overly ambitious in terms of external validity and fail to take the problems of conjunctural causation and equifinality into account. A whole bunch of more recent articles has raised these issues, specified additional variables that condition the effect of inequality on the odds of democratization, and empirically demonstrated the relevance of such scope conditions. Continuing along these lines provides ample opportunities for both theoretical and empirical innovation in the fascinating study of democratization.

Literature


of Theoretical Economics 12(1).


As cited in Russett (1964: 442)

In both books, the analysis is later extended to models that include the middle class as a third actor. The basic insights, however, remain unaltered.

For prior yet not as fully developed accounts see Romer (1975) and Roberts (1977)

Boix uses „asset specificity“ and „capital (im)mobility“ interchangeably. The latter would be the more unambiguous term, since the concept of asset specificity has already been defined by Williamson (1981: 555) in a somewhat different context (the transaction-specificity of an investment).

See also Houle (2009: 595–598)

However, effects not significant and consistent across all of Boix’s models (see e.g. models 1A and 2A in Boix (2003: 79), where inequality remains insignificant or positively affects consolidation). For the results of land inequality, see p. 91.

Wucherpfennig and Deutsch themselves loosely base their 5-point list of problems on Przeworski and Limongi (1997) as well as Sirowy and Inkeles (1990).

For recent and pioneering data collection efforts on economic inequalities across ethnic groups see Cederman, Weidmann and Gleditsch (2011) and Alesina, Michalopoulos and Papaioannou (2012)

Burkhart (1997) is a rare exception.

See Houle (2009: 594) for a similar criticism.

Or, as Huntington (1996: 9 as cited in Houle, 2009: 596) puts it “With only one or two possible exceptions, democratic systems have not been ended by popular vote or popular revolt.”

Przeworski and Limongi (1997); Przeworski et al. (2000). Their refusal of „endogenous democratization“ approaches has however convincingly been criticized as resting on dubious premises and erroneous interpretation of their own results: see Boix and Stokes (2003), Welzel and Inglehart (2005), Epstein et al. (2005).

See also Rueschmeyer, Stephens and Stephens (1992); Collier (1999)

Iversen (2006: 4) even claims that the hypothesis that „inegalitarian societies redistribute more ... has been soundly rejected by the data“

Or vice versa: “the stakes of the regime outcome are much higher when the state is able to enforce and administer the policies chosen by its leaders.” (Soifer 2009: 27).

See Kitschelt (1992) for a distinction between structure- and process-oriented approaches in the study of democratization.

Boix (2003: 75–97) uses an income inequality index for his analysis of the post-war era and a land inequality measure for his longer-term analysis. However, he never enters both measures in the same regression and is not concerned with potentially diverging effects of land and income inequality.

See especially Boix (2003: 90–92)

“History provides not even a single example of a long and uninterrupted sequence of absolute rulers who continuously respected the property rights of their subjects” (Olson 1993: 572). North (1990: 51) sees democracy as a tool to „eliminate the capricious capacity of a ruler to confiscate wealth, and to develop third-party enforcement of contracts.” Both cited in Ansell and Samuels (2010: 1547)

In an extension of their argument, Ansell and Samuels (2010) show that under certain circumstances, the rising bourgeoisie has an incentive to ally forces with the excluded masses and push for full democratization. The main implications with respect to different types of inequality remain the same.

The coincidence of economic inequalities and cultural differences also plays a major role in Michael Hechter’s (1975, 1978) work on the „cultural division of labour“ and “internal colonialism”

See Boix (2011) or Gleditsch and Ward (2006) for recent accounts of how international factors influence democratization.

Schmitter and O’Donnell (1986: 19) claim: “There is no transition whose beginning is not the consequence – direct or indirect – of important divisions within the authoritarian regime itself.” Llavador and Oxoby (2005) present a formal model of how intra-elite competition can drive democratization.