Democracy and the Class Struggle[1]

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Why do societies today distribute political power more equally than before? Most scholars believe that this transition is explained by the rise of capitalism but have long disagreed about why it mattered. The author argues that dominant models fail to capture why capitalist development helps key actors win what they seek. Drawing on comparative and historical work, the author introduces a model of the democratic transition that centers on the concept of disruptive capacity. He collects data on employment structures for much of the modern period to study democratization over the same period. In cross-national regressions, the author finds evidence that the disruptive capacity of nonelites drives democratic gains, and the finding that landlord capacity stymies it is reproduced. Counterfactual exercises show that slightly more than half of the democracy gap between the developing and developed world can be explained by the fact that late development bolstered landlords while handicapping nonelites.

INTRODUCTION

As recently as 150 years ago, ordinary individuals had little influence over the decisions that most affected their lives. Economic elites paid scant notice to those who tilled their fields or operated their machines. Political elites

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launched wars and enacted taxes without regard for those under their writ. Nonelites were disenfranchised.

Eventually these regimes crumbled. The democratic transition swept Western Europe and, over the course of the 20th century, refashioned most of the world's governments in its image. Democratization revolutionized authority, transforming subjects into citizens, autocrats into politicians, and barons into employers. Although authoritarian regimes linger, and political democracy has not eliminated inequalities of income and wealth, these changes amount to a remarkable transformation in the exercise of power.

What explains democracy? In what follows, I reconsider how ordinary individuals wrested political concessions from their economic and political masters. Most work on the democratic transition begins from the observation that this sea change in political life followed epochal transformations of the economy. For some, this correlation licenses the causal claim that capitalist development has itself incubated democratic government. Classic accounts argue that modernization breeds democracy because it raises standards of living, extends the division of labor, and transforms individual values (Lipset 1959; Cutright 1963; Welzel and Inglehart 2005).

Recent scholarship has challenged this focus on what are benign and conflict-free mechanisms (Boix 2003; Acemoglu and Robinson 2006; Ansell and Samuels 2014). These authors do not dispute that democracy is endogenous to development, but they argue that capitalism matters via the class struggle that it detonates. Specifically, development shapes patterns of inequality, which in turn structure the contest between contending classes over the form of government. Where this struggle tilts in favor of ordinary individuals, democracy is more likely.

In my view, these scholars are right to object that democratization is riddled with conflict. They are also right to agree that it is endogenous to economic development. Other accounts may explain much that is interesting about the variation in democracy across countries and across time, but these arguments complement rather than supplant the endogenous tradition. That said, the endogenous view needs reformulating. The new inequality-based accounts have either ignored or misunderstood the conditions under which actors acquire the capacity to credibly demand (or oppose) democracy. This casts doubt on the evidence that has thus far been used to test that view.

In this article, I argue that development matters because it shapes the ability of ordinary people to disrupt the routines on which economic and political elites depend. As I argue below, these disruptive capacities determine the balance of power between elites and nonelites, which in turn shapes the character of government. Here I draw on an existing and distinct tradition, which has argued that development matters because it empowers democracy's proponents and defangs its detractors (Stephens 1989; Rueschemeyer, Stephens, and Stephens 1992).

I make two contributions to this literature. First, I reconstruct the underlying theory in a way that helps identify the errors of the reigning conflict-centered paradigm. Key to my formalization is the concept of disruptive capacity, which helps explain why elites are more powerful than nonelites, why this balance of power shifts with economic development, and why these shifts usher in democracy. Second, I propose an empirical test. Existing evidence for the account is based mostly on case studies, several of which are contested. In fact, a recent review of the scholarship finds little quantitative evidence for what the author labels the "social forces" view (Teorell 2010, p. 101). Drawing on an original data set documenting employment structures from the 19th century to the present, spanning almost 200 countries, I subject this thesis to its first adequate quantitative test. Panel regressions show significant support for the hypothesis that nonelite disruptive capacity drives democratic change, and I reproduce the well-known finding that landlord power imperils it.

While the capacity-enhancing consequences of development may explain democratization, this does not mean they are the only explanation. I find some support for both the inequality-centric and modernization arguments, as well as a diffusion-based explanation, although none of these findings are as robust as my main results. To illustrate the political significance of the social forces account, I explore how well it explains differences in levels of democracy between the advanced and developing worlds. I find that slightly more than half of this gap is explained by the structural facts that handicapped nonelites while empowering landlords. Had capacities there accumulated as they did in the developed world, the 20th century would have been a remarkably more democratic time.

WHY DEMOCRATIZATION?

Modernization

Perhaps the most well-established fact about democracy is that richer countries are also more democratic. By any measure, citizens in the developed world enjoy a wider array of rights than their counterparts in the developing world.[2] Elections are more competitive and more regular. The rule of law is more transparent. Authority is more accountable.

Why are richer countries more democratic? According to Lipset (1959), development bred democracy because it incubated a tolerant middle class. According to Cutright (1963), the reasons were functional: modernization bred complexity, and this complexity demanded a correspondingly more specialized, representative government. This latter account comes closest to the text-

[2] In this paper I use the terms "developed world" and "advanced world" interchangeably and contrast this group to the "developing world" or "late developers."

book modernization view and has obvious affinities to foundational works in sociology (Parsons 1964; Durkheim 2014). More recent versions are not identical (Welzel and Inglehart 2005; Inglehart and Welzel 2010), but they share the characteristic feature of the modernization account, which is to propose that development breeds democracy by mechanisms that are relatively conflict-free.

If the virtue of this account is to capture one of the most durable correlations in social science, its weakness is that the evidence in its favor is mostly correlational. The majority of quantitative evidence illustrating a link between development and democracy is cross-national, but cross-national associations are weak grounds for causal inference. This argument was made in detail by Robinson (2006) and Acemoglu et al. (2008, 2009), who found that the association did not survive the addition of country-level fixed effects to standard models.[3] While it is true that more developed countries are more democratic, they found little evidence that countries become more democratic as they developed. More recent scholarship has recovered support for the impact of development on democracy by extending the sample backward in time, using newer GDP per capita data and employing more suitable estimators (Boix 2011; Benhabib, Corvalan, and Spiegel 2011; Miller 2012; Murtin and Wacziarg 2014; Treisman 2015). However, the overall lesson is that development itself has more complicated and less immediate consequences than previously believed. There are, of course, other mechanisms by which modernization might incubate democracy. In the analysis that follows I measure three ways in which modernization is believed to matter (aggregate income, educational attainment, urbanization) and find weak support for only the second of these three channels.

This empirical issue also betrays a theoretical weakness. The theory has underdeveloped microfoundations. Staple accounts do not give much thought to the actors who actually fashion or obstruct the democratic transition. Richer citizens may be more tolerant, for instance, but they may be more interested in stabilizing a regime that raises their incomes than in democratizing it. Education might encourage breadth of outlook, but it may also produce incentives to lock the less educated out of the political process.

Inequality and the Class Struggle

Recent work by political scientists and economists addresses this lacuna by modeling the interests and capacities of the key actors in the democratic transition. These scholars propose game-theoretic formalizations in which elites

[3] Earlier, Przeworski (2000) and Przeworski and Limongi Neto (1997) showed that development predicted democratic survival much more strongly than it did transition, which is also somewhat at odds with the conventional account (Miller 2012).

and nonelites struggle over the organization of political authority. Each of the three dominant models predicts that it is not wealth but its *distribution* that determines whether democracy will flourish (Boix 2003; Acemoglu and Robinson 2006; Ansell and Samuels 2014). These authors are not the first to foreground inequality (Bollen and Jackman 1985; Muller 1988), but their work is the clearest expression of the view that it should matter.

For Boix and Acemoglu and Robinson, this conclusion follows from the premise that majority-rule democracies will follow the lead of the median voter, whereas dictatorships face no such pressure (Meltzer and Richard 1981). They will thus redistribute more where inequality is higher, since there the median voter earns less than the mean. For Boix, this establishes that inequality inhibits democracy because it raises the costs of a redistributive state to asset-rich elites. Acemoglu and Robinson argue that the probability of transition takes the shape of an inverted U: at low inequality nonelites will realize that the costs of attempting to overthrow elites will outweigh benefits gained, and at high inequality elites are more likely to dig in their heels. Democracy is thus most likely at middling levels of inequality.

Both these models have been criticized by Ansell and Samuels (2014), who instead foreground intraelite competition. Ansell and Samuels model a two-sector economy in which three actors compete over control of the state, which requires them to distinguish among three types of inequality (industrial, landed, and intersectoral). Their flagship prediction is that industrial inequality (which they measure as income inequality) actually makes democracy *more* and not less likely, because it empowers a prodemocratic bourgeoisie (Ansell and Samuels 2014, p. 80). Landed inequality empowers landlords, endangering democracy. Importantly, the model makes ambiguous predictions about the role of nonelites, who thus drop out of their interpretations of their empirical evidence.[4]

This work has obvious virtues. Formalization mandates clarity. The modernization account did not carefully consider how (i.e., through whom) development led to democracy. These scholars have thus been able to propose more suitable empirical tests of their theories. For instance, both Boix and Ansell and Samuels emphasize the antidemocratic preferences of landlords. This has mandated indicators of landlord capacity, one of which I describe and employ in this article.

However, while they draw attention to the actors demanding or opposing transition—to the class struggle over democracy—these accounts fail to capture the circumstances under which nonelites pose a threat. Boix and Ace-

[4] By their reasoning, nonelites seek full democracy (where they set policy), which can be achieved either by acting alone or by rebelling jointly with the bourgeoisie. They predict that only higher intersectoral inequality (i.e., a shift from land to industry) has unambiguous effects on nonelite action, but in their empirical work they acknowledge no way of measuring this dimension of the resource distribution (Ansell and Samuels 2014, p. 100).

moglu and Robinson assume that the capacity of nonelites to rebel (and thus win democracy) cannot be modeled. As a result, income inequality matters because it makes democracy more or less *desirable*, not because it shapes capacities for collective action.[5] For Ansell and Samuels, inequality bears on both incentives *and* capacities, but collective action is a straightforward consequence of the resources actors possess. Put another way, more income, more bargaining power.[6]

Both of these approaches are mistaken. Others have noted this (Slater 2009; Haggard and Kaufman 2016), but my article proposes a revision that retains the view that the class struggle is endogenous to capitalist development. The ability of nonelites to act collectively is not a function of their resources, conventionally understood (whether absolutely or relative to elites). Collective action is not a commodity. Rather, nonelites are more likely to organize when they are positioned to disrupt the routines on which elites depend for their wealth and power. To furnish evidence for this view, I draw on an original data set of the labor force composition of almost 200 countries over most of the modern period.

Most of the evidence thus far has centered on the prediction that levels of inequality should be somehow associated with democratization (Boix 2003; Houle 2009; Soifer 2013; Ansell and Samuels 2014; Slater, Smith, and Nair 2014). But if this prediction is the result of a flawed model of the transition game, we need new or at least additional evidence. To test the conflict-centered view, we need an alternative measure based in a better theory.

Capacity and the Class Struggle

An earlier tradition of mostly comparative and historical scholarship argued that democratization had its origins in a contest featuring elites (landlords and industrialists) and nonelites (peasants, workers, and perhaps the middle class). Granted, there was no unanimity about the shape that this struggle took. Moore (1966) identified the democratic path to the modern world with the bourgeois revolutions (famously, "no bourgeois, no democracy"). These transitions (the English, the French, and the American) had disparate origins—in England, the early commercialization of agriculture weakened the aristocracy; in France and the United States, landed upper classes were decimated by revolutions—but the different paths to democracy were paved by competition between the classes.

[5] To be clear, this is not to argue that either believe the capacity of nonelites to threaten elites is irrelevant to the probability of democratization. The point is simply that they do not model this capacity. It is given from without (as the costs of repression in Boix [2003] and the costs of revolution in Acemoglu and Robinson [2006]).

[6] "A group's likelihood of prevailing in conflict is proportional to the resources it holds" (Ansell and Samuels 2014, p. 71).

Stephens (1989) criticized Moore for ignoring the role of nonelites in forging democracy, a criticism that would inspire the exemplary work of this tradition: Rueschemeyer et al. (1992), which surveyed several democratic transitions in early- to mid-20th-century Europe, Latin America and the Caribbean. While agreeing with Moore (1966) that landlords were democracy's key opponents, they disputed Moore's corollary claim that the task of advancing democracy had thus fallen to nonlandlord elites. In their view, the principal agents of democratization were nonelites, and industrial elites were more frequently their enemies than their allies. Although Rueschemeyer et al. (1992) find that this pattern was muted in Latin America and the Caribbean, their paradigmatic cases fit this pattern, and many other scholars have applied variants of this argument to other cases and other time periods (Therborn 1977; Paige 1990; Keck 1995; Bermeo 1997; Wood 2001; Eley 2002; Yang 2007; Kraus 2007).[7]

This work exhibits all the strengths of the comparative historical approach. Its partisans present fine-grained evidence of the role of different class actors in advancing or stymieing democratization. However, it also suffers the weaknesses that accompany it. While the hypotheses are general ones, the evidence is not.

Recent quantitative work has furnished general evidence for the first hypothesis (Boix 2003; Ansell and Samuels 2014; Albertus 2017). I reproduce this finding in what follows. But quantitative evidence for the second hypothesis that nonelites forged democracy is less impressive. The work that exists relies on measures that are limited in availability and also likely to be endogenous to democracy (Przeworski 2009; Teorell 2010; Aidt and Jensen 2014; Haggard and Kaufman 2016; Kadivar and Caren 2016). The principal contribution of this article is to propose a cross-national, long-run measure of nonelite capacity.

Exogenous Democratization

The modernization, inequality-centered, and social forces accounts all share the view that democracy is endogenous to development. But several authors have argued that much of the variation in levels of democracy cannot be explained by reference to patterns in development or to the resulting contours of the class struggle.

What else matters? Some dispute the obsession with specifically economic cleavages. Somers (1993), for instance, argues that democracy emerges in only a subset of institutional settings. Similarly, in their criticism of the mod-

[7] As one reviewer noted, these arguments have a long lineage in the world outside academia, as well (Marx 1852; Rosenberg 1939; Kautsky 1996).

ernization view, Acemoglu et al. (2008) attribute long-run trajectories of democratization to divergent institutional environments (which were the actual cause of divergent development paths). For their part, Higley and Burton (1989) argue that successful democratization requires elite unity, which is itself the result of elite settlements or transformations that have little to do with patterns of socioeconomic development. Summarizing, Haggard and Kaufman (2016, p. xviii) complain that the "structural proclivities" of traditional approaches have crowded out institutional and political explanations.

Other authors emphasize the agency of political actors and parties. A rich literature attributes democratic consolidation to good choices made by key political actors at key times (Linz and Stepan 1978; O'Donnell, Schmitter, and Whitehead 1986; Diamond and Linz 1995). As Karl (1990) argues, this tradition of "transitology" was led to its focus on agency by the concern that key cases (especially Latin America in the 1980s) defied staple theories. Some recent work has resurrected this view that the fate of democracy is governed by the choices of parties and politicians (Bermeo 2010; Ziblatt 2017).

Can these accounts of democratization be reconciled with the modernization, inequality-centered, and social forces schools? And if so, how? Endogenous theories seek to explain why economic development is associated with democracy, while other accounts identify variation that this generalization ignores (Wejnert 2005). Indeed, partisans of these theories often motivate their work by emphasizing variation that the endogenous tradition leaves unexplained (e.g., Bermeo 2010, pp. 1120–21). This article is motivated by the same observation, but in reverse.

Certainly, there are factors unrelated to capitalist development that explain much that is interesting about democracy's origins. But this fact should not blind us to the impact of capitalism on democracy. In short, my argument is not at all that the endogenous view should supplant the exogenous one, but that, with some clarity about the questions each one answers, they can profitably coexist.

The Democracy Gap

Of course, sometimes these accounts address the same question. Consider, for instance, one of the obvious puzzles on which this scholarship might shed some light: Why have developing countries been persistently less democratic than countries in the developed world? By the modernization account, the developing world is less democratic simply because it is less developed. Both of the other endogenous accounts maintain that the issue is not the underdevelopment of these countries, but either concomitant patterns of inequality (see Ansell and Samuels 2014, pp. 17–35) or an unpropitious distribution of class capacities (weaker nonelites, stronger landlords). Exogenous explana-

tions of this gap are various: whether institutional (Acemoglu et al. 2008), cultural (Woodberry 2012), or world systemic (Alavi 1972; Bollen 1983). In counterfactuals presented at the end of this article, I illustrate the explanatory power of the social forces account specifically. A little more than half of the democracy gap between the developing and advanced worlds is explained by the muted capacity of nonelites and the elevated power of landlords. This illustrates the utility of some generalizations, even as the remainder invites other explanations.

THE TRANSITION GAME

> Those who against the public weal have power cannot be expected to yield save to superior power.
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> —W. E. B. Du Bois ([1935] 1998)

In my view, the existing game-theoretic tradition has lost sight of important insights from comparative and historical work on democratization. As a consequence, the dominant models mistheorize the conditions under which nonelites are best positioned to demand democracy from recalcitrant elites. In this section, I propose to remedy this error by outlining what I will call the social forces model.

I am not the first to highlight these failings. Slater (2009, pp. 214–17) and Haggard and Kaufman (2012, 2016) both criticize Boix and Acemoglu and Robinson for failing to model the conditions under which nonelites overcome collective action problems. Slater, specifically, argues that capacities arise from "emotive collective identifications" (Slater 2009, p. 220), the force of which depends on a prior, path-dependent political history and not on any broad correlates of economic life. His criticism thus deliberately abandons what he calls "rationalist" models of collective action (Slater 2009, p. 221).

I propose an alternative, "rationalist" remedy to the same problem. I draw on a different literature to identify the economic conditions that incubate nonelite capacities over the long run. My argument is not novel, but recent work on democratization seems to have forgotten it. In the early 1970s, both Charles Tilly (with collaborators) and Walter Korpi lambasted reigning theories of revolution for concluding that revolutions happen when people become discontented (Snyder and Tilly 1972; Lodhi and Tilly 1973; Tilly 1973; Korpi 1974). Tilly contrasted this approach to theories that study the *capacities* of ordinary individuals to exercise leverage over the powerful, rather than merely their incentives to do. In his words, the "effects of large-scale change on conflict run through the structure of power, especially by shaping the organizational means and resources available to different . . . contenders" (Tilly 1973, p. 429). Below, I build on Tilly's observation and other work to establish three propositions.

1. Elites have more power than nonelites because they have greater capacities to disrupt economic life.
2. This gap in disruptive capacities narrows or widens depending on the character of the economic roles filled by nonelites.
3. Where this gap narrows, democracy is more likely.

The Capacity Gap

Under what circumstances are dictatorships likely to become more democratic, or democracies likely to backslide toward authoritarianism? To answer this, we require an account of who is likely to demand democracy and who is not. The game-theoretic tradition has adopted several simplifying judgments about the factors that bear on democratic outcomes. It has focused on agents as defined by the economic assets they own. This has an a priori motivation, since the significance of democracy lies in the fact that it distributes political power equally in societies in which economic resources are unequal (Rueschemeyer et al. 1992, p. 44). I follow their lead here. To be clear, the justification of these simplifications is empirical. They help derive testable hypotheses about the conditions under which we should see democratization or its reverse.

Consider a society of two types of actors: a tiny minority of elites, who command assets that they rely on to generate an outsized share of total income, and a large majority of nonelites, who are propertyless.[8] Nonelites rely on their capacity to work for a source of income. Because assets are scarce but the capacity to labor is plentiful, this income is inferior to that earned by elites. Last, assume a government that presides over this society, about which three facts are most important. First, this state is formally autonomous. The actors who administer the state have no a priori ties or allegiances.[9] Second, this state has no independent resource base. To pursue its own agenda, the

[8] In this article I will refer to these groups as elites (below, industrial elites or landlords) and nonelites, rather than "the bourgeoisie" or "the working class." These latter terms are encumbered by the fact that they have been used differently by different authors. I will sometimes refer to the conflict between elites and nonelites as a class struggle, which implies no more than the fact that I have defined these groups by their relationship to economic assets.

[9] While history suggests that elites often staff the offices of the state, there are two good reasons to assume that the state is staffed by extraeconomic actors. First, while state actors are drawn from the elite, most of the elite are not state actors. To understand why elites defend dictatorship, as I argue here, we must appreciate (1) the leverage that *all* elites have over the state and (2) the interests that *all* elites have in restricting nonelite access to the state, even if they are not state actors themselves. Second, as Slater et al. (2014) argue in a criticism of redistributivist models of the transition game, many 20th-century dictatorships were led by obviously autonomous actors (e.g., the military). I thank a reviewer for pushing me to clarify this point.

state depends on revenue drawn from taxing economic activity. Third, this state has the authority to affect the distribution of wealth, whether through tax-and-transfer policies or through regulating exchange between elites and nonelites. Naturally, elites and nonelites will clash over the direction of government policy. Crucially, their incentives to do so are indistinguishable. Nonelites want redistribution, but no more than elites hope to prevent it. However, while both sides have equal incentives, they do not have equivalent capacities. Why? Elite capacity is a feature of the property structure. This follows from the assumptions above. The state depends on a healthy economy to yield the revenues it requires. The health of the economy is a function of the health of investment, which means that it is determined first and foremost by the decisions of those who own the economy's commanding heights. If elites refuse to put their scarce assets to work, state revenues suffer greatly. Critically, the significance of this impact is a function of the scarcity of these assets. Substitutes for their assets are not readily available, which gives elites leverage over the state. As a consequence, when considering different policy options, the state will show deference to elites (Lindblom 1977, 1982; Offe 1984; Przeworski 1986; Bowles and Gintis 1987; Swank 1992; Winters 1996).

Nonelites, on the other hand, rely on an asset that is effectively universal, and for which substitutes are typically available. Withdrawing participation as an individual or as a small group is thus ineffective. Instead, nonelites exercise capacity in numbers. They seek to sway the state by means of collective action—by calling protests, organizing unions, forming parties, and more. They have to coordinate withdrawal from the everyday routines to which they are collectively indispensable and on which a healthy economy depends (Piven and Cloward 1978; Schwartz 1988). As with elites, the effectiveness of this action depends on the extent to which it threatens the resource base of the state.

Both elites and nonelites thus wield capacity in proportion to the amount of disruption they threaten, but the reasoning above has only established that elites and nonelites do this differently. Why might one side have an advantage? Capacities are uneven because achieving a fixed level of disruption is far easier for elites than for nonelites. Nonelite exit depends on the coordination of massive collective action, but elites can achieve the same level of disruption with far fewer participants.

Narrowing the Gap

This said, history furnishes innumerable examples of nonelites winning concessions from economic elites and the state. Why might the balance of capacities between elites and nonelites tilt in their favor? It is here that the dominant formalizations prove unhelpful. For Boix and Acemoglu and Robin-

son, nonelites acquire capacities for unpredictable reasons. For Ansell and Samuels, groups wield capacity in proportion to their resources, conventionally understood. Here I present my reasons for preferring an alternative. In the next section, I present evidence that this alternative explains patterns of nonelite organization and mobilization over the sweep of modern history.

I assume that the capacity of elites is stable. Where relative capacity varies, it does so because of shifts in the ability of nonelites to act collectively in disruptive ways. So, why might nonelite capacity vary?

Nonelite capacity varies because not every nonelite role has equivalent disruptive capacities. Consider at least two reasons that this might be the case. First, some nonelites have greater leverage over economic life. Some work in industries that produce more of other industries' inputs or consume more of other industries' outputs (Perrone, Wright, and Griffin 1984; Wallace, Griffin, and Rubin 1989). Others have scarce skills or are located far afield from population centers, which means that elites will have trouble obtaining replacements (Kimeldorf 2013). The refusal of nonelites to fill these roles inflicts greater costs than the refusal of other nonelites to fill theirs. Second, in some roles, nonelites find it easier to coordinate collective action. Some work in densely clustered workplaces, while others work alone or in small groups. Constant contact facilitates collective action. For these reasons, nonelite capacity (and by my earlier assumption, relative capacity) will vary with the distribution of nonelites into these differentially empowering roles.

When the discrepancy in the disruptive capacities of elites and nonelites is large, the state will be solicitous toward elites. However, as this gap narrows, the state's attentions will shift. The specifics are arbitrary: what is important is that the state's optimal strategy is less elite friendly than the maximally friendly one. And thus, as the gap narrows, so grows the influence of nonelites on the state.[10] This line of reasoning thus concludes in a familiar proposition: the balance of power shapes the balance of policy (Stephens 1979; Korpi 1983; Esping-Andersen 1990; Hicks 1999; Kristal 2010; Volscho and Kelly 2012; Hung and Thompson 2016).[11]

[10] Why, if elites are always more capable of disruption than nonelites, should the state ever listen to nonelites? If the state is committed to minimizing disruption, why not attend only to the interests of elites? To establish that it must, I assume that the intensity of collective action is a function not just of existing capacities, but also of time spent acting collectively. The longer nonelites spend organizing against grievances that go unfulfilled, the more threatening their collective action is likely to be. Thus, for the state, ignoring the interests of nonelites risks greater disruption and perhaps calamity in the future. Minimizing disruption thus requires a middle-of-the-road strategy aimed at placating both elites and nonelites.

[11] Power-resources theory is primarily concerned with explaining policy outcomes in advanced capitalist democracies, and its focus is on organizational and political rather than structural sources of actors' power Korpi (2006, p. 187). This being said, to the extent that

Democracy or Dictatorship?

Suppose that political authority can be organized democratically, where by democracy we mean that the state is subject to regular, competitive elections involving elites and nonelites alike. Elites will have incentives to oppose or obstruct democratization, and nonelites will have incentives to demand or safeguard it. This is because nonelites are numerous and elites a minority. All else equal, the extension of suffrage to propertyless nonelites should lead to less elite-friendly policy, since state actors interested in retaining their position now have to accommodate the preferences of nonelites. This does not mean that they are free to ignore elites, who still have control of scarce assets, and thus investment. It means only that democracy introduces important, countervailing pressures.

Thus, we can now ask, When are we likely to observe democratization or its reverse? Between equal incentives, capacity decides. I expect democratization to be associated with a narrowing of the capacity gap between elites and nonelites. Where nonelites accumulate capacity, we should see progress toward democracy. This insight—foundational to the social forces tradition, and based on priors that should be familiar to scholars of social movements and the welfare state—has been absent from the resurgent literature on democracy's origins. This article proposes to reintroduce it.

Landed Elites

As mentioned earlier, the social forces tradition makes *two* predictions about democracy: first, that nonelites are the protagonists of the democratic transition, and second, that landlords are the antagonists. I have explained the hypothesis that stronger nonelites make democracy more likely, but what about landlords? Why should we expect them to be particularly keen defenders of dictatorship?

Until now I have assumed that elites are undifferentiated. Suppose, however, that some elites rely on land to generate their income and others rely on industrial and commercial property. By the reasoning above, democracy reduces the returns to *all* assets because it enhances the capacity of nonelites to sway the state (which regulates their use) and to bargain privately with elites.

However, not all assets will be affected in the same way. First, returns to land are *particularly* dependent on the willingness of the state to guarantee

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the structural facts are broadly correlated with organizational outcomes (see below), the foregoing can be considered a more abstract view of what those power resources actually entail. In this sense, the social forces account of democratization is a close cousin of power-resources theory.

a docile and immobile workforce. Juridical controls associated with labor immobility and repression are more difficult to maintain under democracy (Gerschenkron 1966; Moore 1966; Zeitlin 1984; Stephens 1989; Rueschemeyer et al. 1992). Second, as Boix (2003) argues, because landed assets are fixed in place, landlords will be less able than other elites to flee the prying hands of the newly democratic state. For both of these reasons, landed elites are especially vulnerable to democratization, and they will therefore have strong incentives to oppose it. Note that nothing in this model predicts that elites not based in land will promote democracy. But countries in which power has shifted from the landed to the industrial elite should prove more amenable to democratization.

DISRUPTIVE CAPACITY

To assess the social forces account, we require a measure of the capacity of nonelites to engage in disruptive action. Where this capacity increases, the balance of power shifts in favor of nonelites, and democracy should be more likely.

An ideal measure would marshal information about the different properties of nonelite positions in the economy (e.g., the economic importance of the various kinds of work, the difficulties of replacing any given worker, and the density of worksites). It may be possible to construct something like this in the most data-rich countries in the most data-rich years, but for inferential reasons, this approach is not of much use. Restricting the sample to these countries would limit us to units and time in which there is no substantial variation in the level of formal democracy. Thus, we need an alternative. I propose to use long-run measures of industry-level employment to construct a substitute index. I measure nonelite disruptive capacity as the proportion of the working-age population who are employed in manufacturing, mining, construction, or transport, as against those in agriculture, services, commerce, other occupations or those who are otherwise outside of the labor force or unrecorded in surveys.

$$\text{disruptive capacity} = \text{high-capacity workers}/\text{working-age population}.$$

The reasoning behind this index is straightforward. I consider manufacturing, mining, construction, and transport to be high-capacity industries. It is a well-known fact that workers employed in these industries have consistently exhibited higher levels of organization and mobilization than workers employed elsewhere (Rubin 1986; Wallace et al. 1989; Southworth and Stepan-Norris 2009; Stepan-Norris and Southworth 2010; Kimeldorf 2013; Kristal 2013). To the extent that a greater share of all nonelites are employed in these industries, it should raise overall disruptive capacity.

I justify my measure on empirical grounds. Figure 1 presents evidence that the measure is associated with three leading indicators of nonelite capacity and mobilization: the proportion of the working-age population in unions (union membership), the number of strikes per 100,000 working-age people (strike frequency), and the number of days lost to strikes for every 1,000 working-age people (strike volume).[12] Across specifications, my measure of disruptive capacity is a statistically significant predictor of trends in union membership (at $\alpha = 0.10$), as well as the frequency and volume of strike activity at (at $\alpha = 0.01$ and $\alpha = 0.05$, respectively).[13] I have standardized the estimates by the average within-country standard deviations to ease interpretation. As figure 1 shows, in each case the estimated impact of disruptive capacity is sizable. A standard deviation increase in disruptive capacity implies almost as large a change in union membership (0.91 SDs), a change of about 0.24 standard deviations in strike frequency, and a change of 0.08 standard deviations in strike volume.[14]

Would it be better to measure disruptive capacity by using these indices, or other measures of organization or mobilization? Many scholars have used direct measures of workers' associational power, whether they be actual measures of the state of worker organization (Garrett 1998; Kristal 2010; Haggard and Kaufman 2016) or, more recently, an index that captures the degree to which workers' rights are protected (Mosley and Uno 2007; Dean 2015; Kerrissey 2015). Some researchers of democratization have employed measures of nonelite mobilization or organization (Przeworski 2009; Teorell 2010; Aidt and Jensen 2014; Kadivar and Caren 2016; Haggard and Kaufman 2016). Separately, Rudra (2002) has proposed a measure of labor capacity that, much like my own, is an attempt to capture the underlying capacity of nonelites to organize (see also Caraway, Rickard, and Anner 2012). In this vein, as an alternative measure of nonelite capacity, Haggard and Kaufman (2016) use the

[12] For a discussion of the concepts of "strike frequency" and "strike volume," see Hibbs (1978). In the supplementary online appendix to this paper, I give additional information about data sources. I also discuss the estimation method, the counterfactuals, model fit, and the robustness tests in greater detail than I do in this article.

[13] To model these three variables, I adopt the strategies that I discuss in more detail below. That is to say, all models include country fixed effects to account for unobserved country-level heterogeneity, year dummies to capture common time trends, and one or more lags of the dependent variable in order to capture dynamics. All samples are restricted to countries with 20 or more observations to mitigate Nickell's bias.

[14] Strike frequency and volume are typically low, but some countries record some years in which strikes are frequent and/or very large. Disruptive capacity explains why the expected level of strike activity is higher in some years than in others, but it cannot, as a slow-moving series, explain these outlying years. This is reflected in the fact that these models generally fit the data less well than the models of union membership. It also helps explain why the implied change in strike frequency or volume is small, as a share of observed movements (i.e., the average within-country standard deviation is large, due to these outlying years).

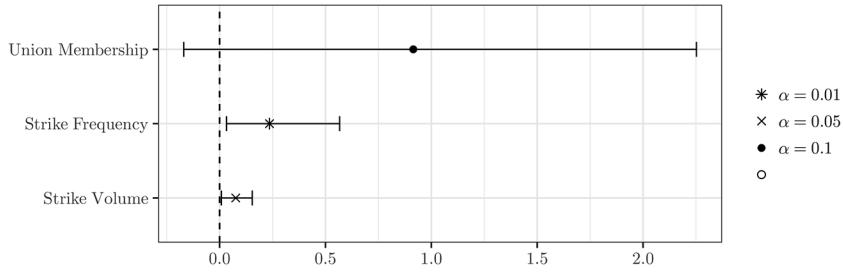

FIG. 1.—This graph plots the estimated long-run impact of disruptive capacity on each of three standard measures of nonelite capacity or mobilization. These estimates are standardized (i.e., divided by the average within-country standard deviation of the dependent variable and multiplied by the average within-country standard deviation of disruptive capacity). As expected, each of the three estimates is positive and statistically noteworthy at conventional levels (in the case of the union membership ratio, at $\alpha = 0.10$ in a two-sided test).

manufacturing share of GDP. Yet for at least three reasons, my approach is preferable.

First, country-year coverage for all other measures is vastly inferior. As table 1 shows, the next most comprehensive indicator is the number of strikes per capita, but this is only available in 4,378 country-years for which democ-

TABLE 1
INDICATORS OF NONELITE CAPACITY

	N	*N* (Polity2)	Δ Average Polity2	Countries	Date Range
Disruptive capacity	8,668	7,610	+15	179	1754–2012
Manufacturing share	6,203	4,947	+14	191	1960–2016
Strikes per capita	5,282	4,378	+27	152	1881–2008
Strike frequency	5,030	4,343	+27	138	1881–2008
Strike frequency per worker	4,486	4,012	+30	123	1881–2008
Strike volume per capita	4,444	3,709	+29	139	1890–2008
Strike volume	4,283	3,684	+29	129	1890–2008
Strike volume per worker	3,833	3,450	+32	113	1890–2008
Labor rights index	3,425	2,848	+11	201	1985–2002
Potential labor power	2,874	2,746	+14	145	1970–2003
Union members per capita	2,629	2,448	+42	63	1885–2012
Union membership	2,581	2,426	+42	59	1885–2012
Union density	2,506	2,387	+42	59	1890–2012

NOTE.—This table shows that my measure of nonelite capacity has two advantages. First, it is available in a far larger sample of countries and over a far longer period. Second, it is available in a substantially less biased sample (Δ Average Polity2 denotes the difference between the sample Polity2 average and the Polity2 average for all sovereign countries in the modern period) than are all other measures except for Potential Labor Power (Rudra 2002), the Labor Rights Index (Mosley and Uno 2007), and the manufacturing share of GDP (Haggard and Kaufman 2016), all of which have inferior coverage.

racy data are also present (i.e., only 60% of the coverage of disruptive capacity). More importantly, table 1 shows that measures of mobilization and especially organization bias the sample, since they are much more likely to be available in democratic than authoritarian countries. Where data on the number of union members are available, the average democracy score is 42 points higher (on a 0–100 scale) than the average score in all countries and years for which data exist. Some bias is unavoidable, but table 1 shows that disruptive capacity minimizes this problem while maximizing coverage.[15]

Second, more explicit measures of organization or mobilization raise the likelihood that the chosen measure is endogenous to the dependent variable or something closely associated with it. More democratic countries are likely to make it easier for workers to join unions or organize protests. My measure is not obviously exogenous, but the extent of any endogeneity bias is likely to be less severe.

Third, my measure has important theoretical advantages. As outlined earlier, where nonelites win democracy, this is because state administrators concede it when they realize that nonelites are *capable* of significant disruption. All else equal, this capacity should coincide with strikes and protests of which we have a record. But, sometimes, we might expect that concessions will be offered in anticipation of nonelite unrest, which will thus never transpire. Mobilization is thus only ever likely to be imperfectly related to capacity: strong nonelites might not need to mobilize, and weak nonelites may mobilize out of desperation. Thus, it makes sense to measure capacity rather than mobilization. And none of the other capacity measures identify the correct protagonists. Rudra (2002) proposes a measure rooted in the advantages of high-skill workers inside manufacturing, but this ignores the relatively high disruptive capacities of *all* manufacturing workers and workers in other sectors (mining, construction, and transport). Haggard and Kaufman (2016) use the share of manufacturing in GDP, but this has two problems: for gauging capacity, it is the wrong measure of significance (i.e., value-added rather than employment), and it again ignores the relative capacities of workers in other industries.

Finally, even if existing alternatives are unfeasible, my decision to approximate disruptive capacity by the share of the working-age population employed in traditionally disruptive industries raises a conceptual concern. Does variation in this index simply register economic development? As discussed already, modernization theorists argue that development incubates democracy by raising standards of living and incubating a middle class. This is not my hypothesis, but one might worry that my index is testing their mech-

[15] Both Rudra's PLP and Mosley's Labor Rights Index are available in less biased samples than disruptive capacity but over a time period that is far too truncated to include in models of long-run democratization.

anism rather than my own. However, note that the share of the working-age population employed in high-capacity industries is not a straightforward correlate of economic development. Over time in a single country, as is well-known, the relationship is variable: positive at earlier levels of development and then negative at higher levels. There is also important cross-country variation. Recent work has shown that, in later developing countries, peak industrial employment occurs earlier on the development path and at lower levels of industrial employment than in earlier developers (what Rodrik [2016*b*] calls "premature deindustrialization"). In short, economic development does not yield equivalent disruptive capacities at all times or in all places. Of course, as with other variables that this literature has previously employed, disruptive capacity is imperfectly correlated with economic development ($\rho = 0.62$). I approach this difficulty conventionally, which is to say that I control for levels of development in the regressions I introduce later. My preferred models estimate the impact of disruptive capacity on democracy, conditional on the confounding consequences of GDP per capita.

ESTIMATION

Variables

As described, the principal contribution of this article is to propose a quantitative test of the social forces account—and, specifically, of the hypothesis that nonelite capacity drives democratic gains. To do so, I run panel regressions over the span of the modern history of democracy. As dependent variables, I use two continuous measures of the quality of a country's democracy. First, I use the 21-point Polity2 score, rescaled to lie between 0 and 100 in order to ease interpretation. It is derived from measures of six dimensions of a country's political state, which together evaluate executive recruitment, political participation, and checks on executive authority (Marshall, Gurr, and Jaggers 2014). A score of 0 on my rescaled index (–10 on the original index) denotes a hereditary monarchy or its equivalent, and a score of 100 (+10 on the original index) denotes full democracy. In 2014, only Saudi Arabia, Bahrain, North Korea, and Bhutan received the lowest score on this index, and there were 35 countries that received the highest. In all, data are available for 201 sovereign states, some as early as 1800. Table 2 shows in-sample descriptive statistics for this and all other variables.

Reliable measurement of the level of democracy is no easy task. Some of the relevant properties are very difficult to quantify, especially since we seek consistency across long spans of time and an array of countries. The Polity IV project is a credible attempt to this end, but it has its critics (Cheibub, Gandhi, and Vreeland 2010). To ensure that my results are not sensitive to its measurement decisions, I also make use of an alternative measure from the Va-

TABLE 2
Descriptive Statistics

Variable	Average	SD	Within-Country SD
Dependent:			
Polity2 score	67.32	34.66	19.71
Electoral democracy	52.35	29.17	15.85
Independent:			
GDP per capita (log)	8.13	.93	.44
Growth rate	1.96	5.17	4.69
Disruptive capacity	19.90	8.37	3.35
Educational attainment	6.13	2.97	1.63
Urbanity	229.47	142.19	69.26
Landlord power	27.37	19.72	10.20
Income inequality	36.71	9.95	3.00

Note.—Descriptive statistics from my preferred sample, where Polity2 is the dependent variable (except for the electoral democracy variable, which is from its own preferred sample). SD refers to the overall standard deviation, while within-country SD refers to the average of all the within-country standard deviations.

rieties of Democracy data set (hereafter V-Dem). V-Dem solicits answers to a wide range of evaluative questions from several thousand country experts and pools these responses via a model that accounts for differences in interpretation and reliability across coders (Coppedge et al. 2016; Pemstein et al. 2015). This procedure yields estimates of the level of democracy in five different dimensions (as well as several subdimensional measures). In this analysis, I use the single measure in V-Dem that best captures overall progress in the prevalence of electoral democracy (v2x_polyarchy, hereafter Electoral Democracy).

As discussed already, my measure of nonelite capacity is a count of the total number of workers in manufacturing, mining, construction, and transport divided by the size of the working-age population. Data on the sectoral breakdown of the labor force comes from three sources. For the early period, I used the various volumes of Mitchell's *International Historical Statistics* (Mitchell 2013).[16] For the post–World War II period, I updated and expanded coverage using two other data sets, one available via the International Labour Organization and the other curated by the Groningen Growth and Development Center (Timmer, de Vries, and de Vries 2014). Both the Mitchell and International Labour Organization data sets sometimes report categories that straddle the high-capacity and low-capacity classifications (e.g., transport and commerce together). In these cases, I estimate the proportion of these

[16] The data originate in national censuses that are available at irregular intervals, so I linearly interpolated the missing values.

composite categories that are high-capacity from the nearest observation for that country in which the relevant categories are reported separately. Otherwise, I considered the Groningen data set the most reliable and Mitchell's the least. Accordingly, if two or more data sets covered the same country-year, I relied on them in this order. The working-age population is defined as those between the ages of 15 and 65. These data come from two sources. For the early period, Mitchell's *International Historical Statistics* collates population censuses and reports totals by age and gender. Occasionally, they report categories that straddle the working-age classification, in which case I proceed as above and estimate the proportion of these categories that are working-age by drawing information from the nearest observation in which no straddle categories are present. For the post-1960 period, annual and complete estimates are available through the United Nations.

The unconditional effect of disruptive capacity may partly capture the negative impact of the other half of the social forces account. I emulate Ansell and Samuels (2014) and estimate landlord capacity by the interaction of the percentage of farmland cultivated by family farms and the percentage of the population that lives on the land. Landlords should be most powerful where their chief asset is highly concentrated in their hands *and* where large numbers of people still depend on land for their livelihood. Both of these measures come from Vanhanen (2003). To ease interpretation, I invert each measure (i.e., subtract the original by 100) and rescale their product to lie between 0 and 100. Low values of this index thus denote country-years in which few people live on the land and most land is in the hand of family farmers. High values of this index denote country-years in which most people live on the land and the distribution of land is skewed.

My main concern in this article is to demonstrate that these two variables help explain democratization, net of likely confounders, so in my preferred specification I include measures of the two other theories of endogenous democratization. I measure the modernization account in three ways. Most importantly, I control for a country's level of development. I measure this by the log of GDP per capita, which requires combining data from the Penn World Tables and Angus Maddison's long-run data set. From the same data, I calculated the logarithmic growth rate of GDP per capita, which I also include. This variable controls for the impact of antecedent growth rates on subsequent democratization—whether the rate of economic growth between time $t - 2$ and $t - 1$ affects a country's democracy score at time t. Separately, I include a measure of educational attainment. Whether because it incubates tolerance, or because it is a proxy for the growth of a middle class, higher levels of educational attainment should be associated with subsequent democratization (Murtin and Wacziarg 2014). These estimates are taken directly from a recent long-run data set published by Morrisson and Murtin (2009). Otherwise, modernization might matter because it clusters

people into large cities. The growth of cities is an index of functional complexity, which some link to democratic government (Cutright 1963). I thus include a measure of the proportion of a country's population that lives in cities with more than 100,000 residents (Banks and Wilson 2015). Measurement of the inequality-centric account is more straightforward. As discussed already, the relationship between antecedent levels of inequality and subsequent democratization outcomes has been the subject of much recent research. Income inequality is the key variable in all leading models of the transition game. I followed Ansell and Samuels (2014), and for the early period used Bourguignon and Morrisson's historical estimates of the income distribution (Bourguignon and Morrisson 2002). I combined these with more recent, country-specific inequality estimates from Babones and Alvarez-Rivadulla (2007) and Solt (2014). Finally, to ensure that my results are not simply an artefact of ignoring the diffusion of democracy across borders (Wejnert 2005), I measure the average level of democracy in all of a given country's regional neighbors in the previous year.

Model

I assess the hypothesis that nonelite capacity drives democratization on samples containing as many as 104 sovereign countries and in the date range from 1821 and 2013 and the hypothesis that landlord power obstructs it on a sample of as many as 145 countries in the date range between 1859 and 2008. When drawing inferences from within-country, over-time variation, as I do here, the advantages of long-run coverage are considerable (Scheve and Stasavage 2009, pp. 216–17). The addition of several controls means that the samples for my preferred models contain fewer countries and span less time (see tables 3 and 4 for specifics). The specification I employ is very similar to the models estimated in Acemoglu et al. (2008, 2009), Boix (2011), and Treisman (2015), where

$$d_{it} = \alpha d_{it-1} + \gamma DCAP_{it-1} + \zeta LAND_{it-1} + x^{l}it - 1\beta + \mu_t + \delta_i + E_{it}, \quad (1)$$

in which d_{it} represents either the Polity2 or electoral democracy score of country i at time t, and d_{it-1} is thus a single lag of the dependent variable, which is included to account for the fact that the democracy score is correlated with itself over time (Keele and Kelly 2006; De Boef and Keele 2008; Beck and Katz 2011). The raw coefficients from this specification yield the immediate impact of the independent variables on the outcome. These understate the consequences of a change in the independent variables. Due to the inclusion of a lagged term among the estimators, any given change in an independent variable also has knock-on effects on the outcome in future time periods. The

TABLE 3
LONG-RUN ESTIMATES, POLITY2

	(1)	(2)	(3)	(4)	(5)	(6)
Long-run multiplier:						
Disruptive capacity	2.978**		2.624**			4.168**
	[1.01, 5.08]		[.68, 4.68]			[1.35, 6.94]
Landlord power		−4.904**	−5.505**			−5.774**
		[−7.55, −2.4]	[−9.04, −2.46]			[−10.07, −2.44]
GDP per capita (log)				2.449		−1.219
				[−2.9, 8.48]		[−8.77, 6.76]
Growth rate				−.003		.094
				[−2.71, 2.53]		[−4.02, 3.72]
Educational attainment				−3.273		8.681
				[−10.67, 4.64]		[−2.16, 19.67]
Urbanity				3.156*		−2.287
				[.26, 6.04]		[−5.23, .81]
Income inequality					.516	2.061
					[−2.07, 2.89]	[−.99, 5.16]
Polity2 score (regional average)						5.471**
						[1.18, 9.69]
Model information:						
Observations	7,127	10,656	6,427	6,995	9,501	4,437
Countries	104	145	96	90	121	64
Date range	1821–2013	1859–2008	1859–2008	1871–2003	1821–2014	1871–2003
Average N_i	68.5	73.5	66.9	77.7	78.5	69.3
Adjusted R^2	.880	.885	.881	.878	.914	.869

NOTE.—This table reports the estimated long-run effects across the six specifications I estimate: bivariate models with my two key variables (cols. 1 and 2), a social forces model (col. 3), a modernization model (col. 4), an inequality-centric model (col. 5), and my preferred model (col. 6), which includes all variables. The dependent variable here is the Polity2 score. All models include one lag of the Polity2 score and country- and year-level fixed effects. As hypothesized, the long-run impact of disruptive capacity is positive and statistically significant, while the long-run impact of landlord power is negative and statistically significant.

TABLE 4
LONG-RUN ESTIMATES, ELECTORAL DEMOCRACY

	(1)	(2)	(3)	(4)	(5)	(6)
Long-run multiplier:						
Disruptive capacity	1.406^{+}		1.489^{+}			2.812**
	[−.13, 3.15]		[0, 3.27]			[.69, 5.08]
Landlord power		−3.014**	−3.351**			−6.022**
		[−5.08, −1.11]	[−6.07, −.79]			[−8.82, −3.31]
GDP per capita (log)				3.336		−.675
				[−1.62, 8.69]		[−7.52, 5.7]
Growth rate				1.924		2.361
				[−1.12, 5.23]		[−2.52, 7.24]
Educational attainment				2.532		11.938*
				[−4.98, 10.56]		[.67, 25.13]
Urbanity				1.377		−2.809^{+}
				[−1, 3.69]		[−5.72, 0]
Income inequality					1.493^{+}	2.160
					[−.16, 3.34]	[−.49, 5.11]
Electoral democracy (regional average)						7.274**
						[1.65, 12.46]
Model information:						
Observations	6,691	8,837	5,855	6,093	7,246	4,013
Countries	102	138	93	89	116	64
Range	1901–2013	1901–2008	1901–2008	1901–2003	1901–2014	1901–2003
Average N_i	65.6	64	63	68.5	62.5	62.7
Adjusted R^2	.930	.929	.931	.926	.934	.926

NOTE.—This table reports the estimated long-run effects across the same six specifications shown in table 3, but when the dependent variable is the V-Dem measure of electoral democracy. My main findings are the same, and other findings are similar, which shows that my results are robust to considering an alternative measure of democracy.

total effect, which is sometimes referred to as the long-run multiplier, is thus the sum of these effects across each subsequent time period. Because these long-run multipliers are a better gauge of effect size than the raw coefficients, in the main text I discuss these estimates only.[17]

The variables γ and ζ are the main parameters of interest. They represent the estimated impact of nonelite and landlord capacity on democracy. The variable x^{l}_{it-1} is a row vector containing all the other covariates, and β is a column vector containing the corresponding coefficients. Note that all independent variables are lagged one time period to account for the fact that the impact of capacity on democracy is not likely to be instantaneous. The term μ_t denotes a year fixed effect for year t; its purpose is to account for shocks and time trends common to all countries. The term δ_i denotes a fixed effect for country i, included to account for time-invariant and country-specific confounders that are otherwise unobserved.[18] This means that estimation relies only on within-country covariance, ignoring all cross-sectional variation (Firebaugh, Warner, and Massoglia 2013). Last, E_{it} represents the estimated error for the observation in country i and time t, adjusted for clustering at the country level.[19]

There is no foolproof way to draw causal inferences from observational data of this kind, but as one imperfect strategy, I also estimate all models on samples of varying intervals: an annual sample, on which most of the results discussed below are based, as well as a sample split by 5-, 10-, 15- and 20-year intervals. These longer interval samples defend against the possibility that results are muddied by the reverse causal relationship. If a country's democracy score affects disruptive capacity, the specification in equation 1 yields biased estimates of the impact of disruptive capacity on democracy. Since the Polity2 score is correlated with past values of itself, lagging disruptive capacity one period is no protection (Bellemare, Masaki, and Pepinsky 2017). However, observing the same result at a 5- or 10-year remove

[17] My choice to focus on the long-run multipliers does not affect any conclusions I draw in this paper about the statistical significance of my estimates. In the results I discuss, short- and long-run estimates yield the same conclusions.

[18] By construction, the lagged dependent variable and the effective error term are correlated, which leads to biased and inconsistent parameter estimates. This is known as Nickell's bias. As Nickell (1981) shows, the resulting bias is on the order of $1/T$, which means that for longer panels this need not be a serious concern. Regardless, to assuage concerns that my results are an artefact of this bias, I follow Papaioannou and Siourounis (2008) and restrict my annual sample to countries in which at least 20 years of data are available. The longer interval panels are then estimated on the same span of observations, which means, for instance, that the minimum T for the five-year panel is 4.

[19] A prerequisite of specifications such as the one in eq. (1) is that all constituent variables be stationary—that they not have unit roots (Box-Steffensmeier and Helgason 2016). I test all series for unit roots and conclude that there is enough evidence of stationarity to justify leaving all variables in their levels.

improves our confidence in the ordering underlying this association. Autocorrelation at this long a remove is significantly less severe.[20]

RESULTS

Panel Regressions

If my argument is correct, capitalist development incubates democracy by weakening antidemocratic landlords and strengthening prodemocratic nonelites. Across specifications and samples, I find strong support for both of these hypotheses. Landlord capacity inhibits democratization, and nonelite disruptive capacity advances it. Tables 3 and 4 present estimates from the one-year panel, and figure 2 shows my estimates from my preferred specification on the longer interval panels.[21] Note that while the estimates in tables 3 and 4 are semistandardized, I reserve discussion of this dimension of the results to a later section. Here I focus on their statistical rather than political significance.

By now, a plethora of work has argued that strong landlords generally inhibit democratization (Moore 1966; Stephens 1989; Paige 1990; Rueschemeyer et al. 1992; Boix 2003; Ziblatt 2008, 2009; Ansell and Samuels 2014; Albertus 2017), and to my knowledge there is no scholarship that argues the opposite. As Mahoney (2003, p. 148) argues, "It is fair to say that the weakening of landed elites is currently the most plausible explanation for why capitalist development is associated with . . . democracy in cross-national quantitative studies." The first of these findings is thus not particularly surprising, although its robustness bears mentioning. The negative long-run impact of landlord capacity is statistically significant at $\alpha = 0.01$ in the bivariate, social forces, and preferred specifications (cols. 2, 3, and 6 of tables 3 and 4). As figure 2 shows, this relationship holds whether we look 1, 5, 10, 15, or 20 years into the future.

The second result, however, is novel. Quantitative work on democratization has not yet tested the hypothesis that nonelite capacity yields democracy, due either to theoretical confusion or to the absence of a credible measure of long-run disruptive capacity. This article has sought to fill that gap. The bivariate association of disruptive capacity with a country's subsequent Polity2 or Electoral Democracy score (col. 1) is unambiguous: the higher disruptive capacity moves above its country-specific mean, the higher a country's

[20] Granted, reverse causation is just one threat to identification. Both variables may also covary with some time-varying unobserved variable. This strategy is no defense against that possibility.

[21] Tables 3 and 4 include measures of fit, but because the samples are not consistent across specifications, the statistics cannot be compared.

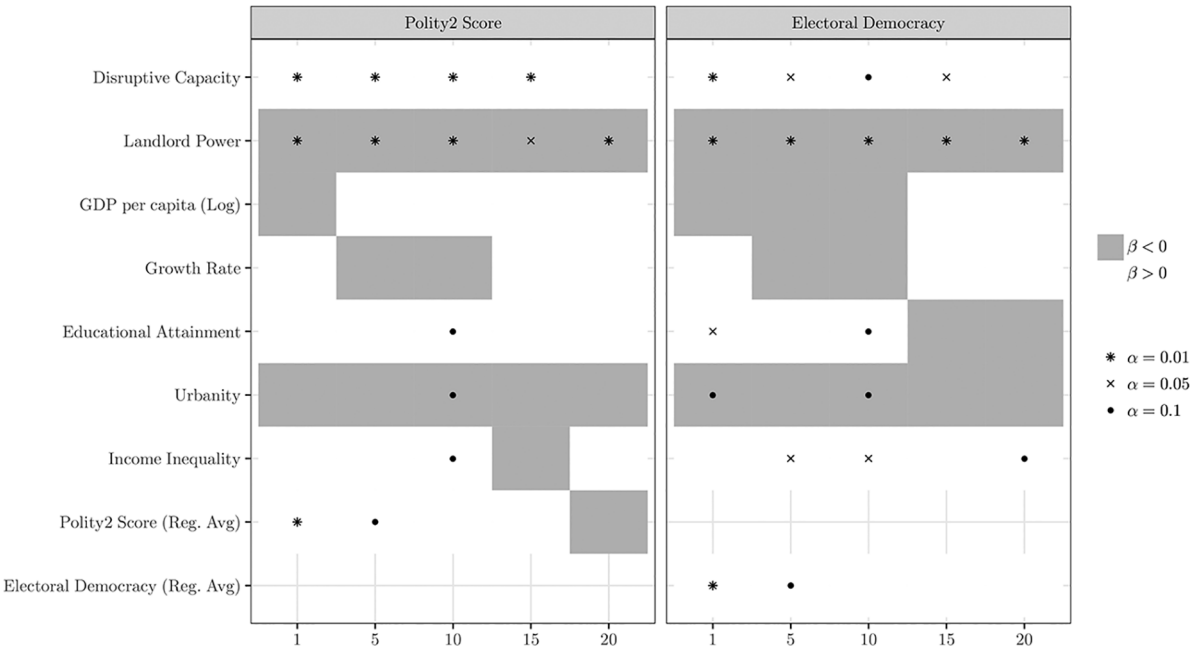

FIG. 2.—This graph illustrates the statistical significance of all variables in my preferred specification, in samples of varying remove. The two panels correspond to the two different measures of democracy that I examine in this paper. The *y*-axis denotes the independent variable in question. The *x*-axis denotes the interval (in years) between observations. The purpose of this exercise is to see whether results survive at long remove (e.g., whether disruptive capacity at time $t - 10$ predicts the Polity2 score at time t). If so, this is better (although not at all dispositive) evidence that the main associations I discuss are causal. Disruptive capacity is positively associated with subsequent levels of democracy at all except a 20-year remove. The statistical significance of this result fluctuates slightly when Electoral Democracy is the dependent variable, but only in the 20-year sample does it drop below $\alpha = 0 : 10$. Landlord power is a consistent predictor of subsequent democratization at short and long remove.

democracy score in subsequent years. Columns 3 and 6 of tables 3 and 4 show that this association is not obviously spurious—either when I control for just the capacity of landed elites or when I include all other variables. In fact, for both outcomes, when controlling for all likely confounders for which there are adequate data, the long-run estimate of disruptive capacity is larger and significant at $\alpha = 0.01$. As figure 2 shows, this is mostly true in the longer interval samples as well. Whether we look 1, 5, 10, or 15 years into the future, the long-run impact of disruptive capacity is positive and statistically significant (at $\alpha = 0.05$ in all but the 10-year Electoral Democracy sample, where it is significant at $\alpha = 0.10$). At 20 years, the estimate is still positive but statistically imprecise.

These are my key results, and the second is new, so one might wonder whether they are robust to alternative estimation choices. I find that both results are statistically significant (at $\alpha = 0.10$, at least) when each specification is estimated on a consistent rather than unrestricted sample, when I vary the cutoff for the minimum number of nonmissing observations that must be present in a country to merit inclusion in the sample, when I use alternative measures of disruptive capacity,[22] when I add measures of involvement in war to the model, when I employ random rather than country fixed effects, or when I omit one country at a time (i.e., testing whether results were driven by an outlying country). Separately, I find no evidence that adding interactions between key variables changes any of the conclusions I draw. When I use dichotomous measures of democracy to model democratic transitions or consolidation rather than democratization, I find the class capacities matter as expected, except that the impact of disruptive capacity on consolidation, specifically, is quadratic rather than linear. Results do change if I omit either random or fixed effects or a lag of the dependent variable. In both cases, the effect of disruptive capacity on democracy is positive but no longer statistically significant. However, given the properties of these data, neither of these decisions is the right one. Countries in my sample are heterogeneous in ways that cannot be measured, and democracy is a strongly dynamic process. Thus, the omission of fixed or random effects or of a lag of the dependent variable invites bias.

How does the modernization account of democratization fare? I find little support for its staple mechanism. The long-run impact of GDP per capita is sometimes negative and sometimes positive, but never statistically significant at conventional levels. Most specifications furnish no evidence that ei-

[22] Except that the estimated negative impact of landlord power is ambiguous when I add a count of public sector workers to the numerator of disruptive capacity (although only in the Polity2 models), perhaps because these data truncate the sample considerably.

ther the level or antecedent growth rate of GDP per capita is positively associated with levels of democracy. I find better evidence, however, for a different channel by which modernization is supposed to matter: not by raising average incomes, but by increasing the average level of education. This result is not statistically significant in models of the Polity2 score (or in col. 3 of either table, in which I only include the modernization variables), but the coefficient estimate is sizeable, and the same estimate is statistically significant (at $\alpha = 0.05$) in the model of Electoral Democracy. The result surfaces at 10 years' remove, too (in both models), although not in any other panel. If this estimate is correct, it suggests that, to the extent that development yields educational gains (but not income gains), it is associated with democratization.[23] What of the inequality-based account of democracy? I find no support for the redistributivist account, which supports recent work showing the same (Houle 2009; Haggard and Kaufman 2012; Soifer 2013; Slater et al. 2014). Whether in the simple inequality-based specification (col. 4) or in the preferred specification (col. 6), every estimate of the impact of the income inequality is positive, not negative. As figure 2 illustrates, this estimate is relatively more precise at longer remove: the long-run effect of income inequality is statistically significant at $\alpha = 0.10$ in the annual panel (Electoral Democracy) but significant at conventional levels at 5- and 10-year removes. All of this might be considered weak support for the view that higher income inequality incubates democracy (Ansell and Samuels 2014). Yet, although these results fit Ansell and Samuels's argument, by my reasoning earlier it is not clear what a positive estimate implies. Ansell and Samuels (2014) argue that income inequality strengthens the hand of a prodemocratic industrial elite, but I presented theoretical reasons to doubt that these elites have prodemocratic preferences. As I argued, Ansell and Samuels (2014) derive the industrial elite's preference for democracy by assuming that they have no influence over autocratic states. But industrial elites have their hands on the levers of the economy. They do not need to possess the state to influence it. Neither is it clear that their collective action capacities are a function of the income distribution. What about other explanations? My results buttress the view that democracy tends to diffuse across borders (Wejnert 2005). Across specifications (and, note, net of the lagged value of democracy), the average democracy score of a country's regional neighbors predicts subsequent democratization. This coefficient is attenuated at longer remove, which may suggest that democracy diffuses in the short but not long run.

[23] Of course, this approach gives us no ability to determine why: whether it is because educational attainment is an index of middle-class growth or because it incubates tolerance (or the second via the first, or both).

Political Significance

Of all the explanations I consider, the twin workhorses of the social forces account are the most consistently statistically significant results. However the model is specified, or whether the association is examined at annual or extended remove, the impact of disruptive capacity (positive) and landlord power (negative) is unmistakable. Other explanations figure, but evidence of their importance is weaker. This said, the fact that these associations are statistically noteworthy is not proof of their political significance. How much do landlord power and disruptive capacity really matter? As noted already, the estimates presented in tables 3 and 4 are semistandardized, but the magnitudes are still not intuitive. How can one interpret a 4.1 increase in the Polity2 score, or a 6-point decline in Electoral Democracy?

To better convey their political significance, I consider the impact of a substantively important change in these two key variables. My measures suggest that developing world nonelites have always been weaker than their developed world counterparts and landed elites stronger than theirs. This is not surprising, given what we know about the economic history of the developing world, but it invites a ready test of the significance of the social forces account. Had the developing world not been handicapped in this way, how much more democratic might it have been? By asking this question, I shed light on the puzzle posed earlier. By examining how much of the observed gap between the developed and developing world is closed under this counterfactual, I offer one estimate of how well the social forces account explains the authoritarian bent of the developing world's political history.[24]

To aid interpretation, I present this counterfactual in two stages.[25] First, figure 3 plots this counterfactual trajectory in a single country, Brazil, and for only one of the two outcomes (Polity2).[26] In the main inset, the bottom line illustrates Brazil's predicted Polity2 score in the observed world. The topmost line plots counterfactual levels of democracy in a world in which

[24] The use of counterfactuals in this sense should not be confused with the use of counterfactuals in the service of causal inference (Elster 1978; Lewis 2001; Morgan and Winship 2014). In fact, this kind of counterfactual simulation *assumes* accurate inference. Its purpose is to aid interpretation.

[25] Note that the difference between the counterfactual trajectory of democracy and its predicted trajectory (i.e., the estimated trajectory of democracy when independent variables are held at their observed values) registers two different magnitudes: (1) the coefficients associated with any independent variables that take different values under the counterfactual and (2) the difference between the counterfactual trajectory of any independent variables that I change and its observed trajectory.

[26] Brazil makes for a useful representative case because data are widely available over the span of my panel, and its estimated fixed effect is very close to the sample median.

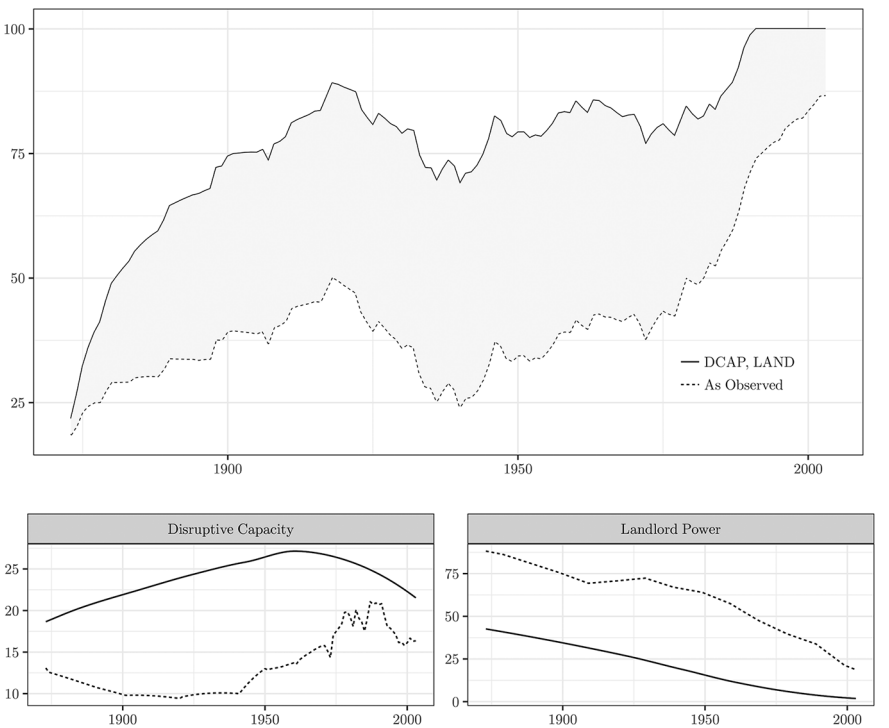

FIG. 3.—This graph illustrates the estimated democratic gains in a counterfactual Brazil, in which disruptive capacity and landlord power follow developed world trajectories. The results affirm that the estimates from the main models are practically and not just statistically significant: a meaningful change in the key independent variables implies large democratic gains. Had Brazil escaped the democracy-disabling consequences of late development, it would have experienced 46 additional years of democracy.

Brazilian nonelites accumulated disruptive capacities on an advanced world pattern (the dotted line in the bottom left inset) and landlords lost power as they did in the average developed country (the dotted line in the bottom right inset). As is evident, the estimated consequences are significant: Brazil would have been considerably more democratic. The most intuitive way to summarize these gains is to quantify the area shaded in figure 3. This area represents the additional years of democracy that Brazil would have enjoyed had its landed elites been weaker and its nonelites stronger.[27] In these terms, the shaded gains amount to about 46 democracy-years. This represents roughly an 83% improvement over its observed history, which is equivalent to observing that Brazil would have been close to twice as democratic.[28]

Is Brazil idiosyncratic? Figure 4 plots the consequences of the same counterfactual across all developing countries and also extends the exercise to V-Dem's alternative measure. Gains are comparably significant. The average developing country gains around 29 democracy-years (Polity2),[29] which represents about a 54% improvement over the predicted trajectory in the observed world.[30] Figure 4 also makes it possible to estimate what this improvement represents, as a proportion of the gap between the developed and the advanced world. As the graph shows, at every point in the modern period, the developing world (the bottommost line) is less democratic than the developed world (the topmost). As before, I quantify this gap by calculating the area between the two curves, which suggests that the average developing country experiences around 49 fewer years of democracy (35 fewer, by V-Dem's measure). We can thus ask, How much of this gap is closed, in a counterfactual world in which all developing world elites and nonelites lost or gained capacities by developed world patterns? By this measure, around 57% of the gap between the advanced and developing worlds is explained by

[27] These areas cannot be computed analytically, but there are several numerical methods that can substitute. I used Simpson's rule, which approximates the integral using quadratic polynomials over short intervals.

[28] How much of this improvement is due to the strengthening of nonelites and how much to the weakening of landed elites? Results from these counterfactuals run separately suggest that about 63% are due to the early demise of landlords and about 37% to the earlier and more significant accumulation of disruptive capacity.

[29] If the outcome is V-Dem's Electoral Democracy measure and not the Polity2 score, these numbers are 19 democracy-years and 52%, respectively. Remember that the Electoral Democracy sample is truncated by V-Dem's later starting date, so a given percentage gain translates to smaller raw gains in this sample.

[30] To summarize trends across all developing countries, I fit a locally weighted nonparametric smooth to the data. This choice is arbitrary, but other choices do not make any difference to the conclusions here. If I fit the data with simple linear best-fit lines, or if estimate the average score in a given year by the average across all countries, or if I vary the weights in the locally weighted smooth, neither this estimate nor the estimate discussed at the end of this paragraph changes substantially.

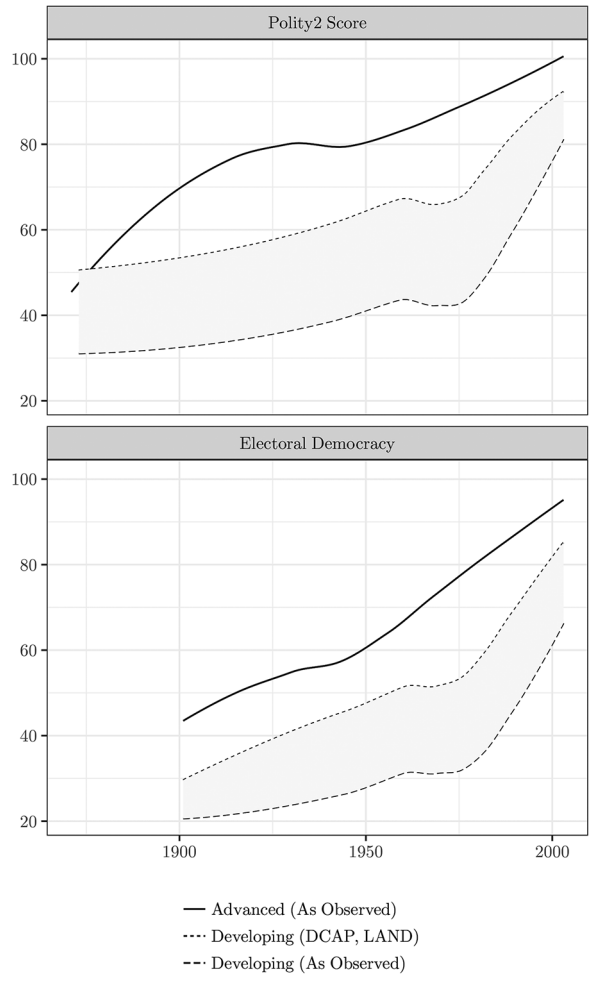

FIG. 4.—This graph illustrates the estimated democratic gains in a counterfactual world in which all developing countries escaped the disabling features of their late development (i.e., in which disruptive capacity and landlord power all followed developed world trajectories). It generalizes the results plotted in figure 3 beyond Brazil (and beyond the Polity2 score). These gains can be compared with the actual trajectory of the advanced world over this period. As discussed in the main text, roughly 50%–60% of the gap between the developing and advanced world is closed under this counterfactual.

the capacity handicaps of late development (54%, by V-Dem's measure). Put another way, however I measure it, slightly more than half of the developing world's democratic deficit is explained by the diminished power of its nonelites and the longevity of its landed classes.

CONCLUSION

The rise of democracy transformed the distribution of power, and in this sense inaugurated a new epoch in human history. Social scientists have long believed that this new epoch had something to do with the rise of capitalism, but the specific arguments have varied. One wing of this endogenous tradition argued that capitalist development mattered because of the class capacities it eroded and incubated—because it weakened landlords, democracy's most intransigent opponents, and strengthened nonelites, democracy's most committed partisans. Yet the existing scholarship that argues this point has been case study based, and its theoretical foundations have been largely ignored by recent quantitative work.

In this article, I made two main contributions. First, I reintroduced the social forces view into the contemporary debate by presenting a capacity-centered model of the democratic transition. Existing models of the transition game either assume away the capacities of social actors to win what they seek or they rely on an ingenuous account of collective action. I proposed a better model, which is centered on the concept of disruptive capacity. This helps explain why elites always have more power than nonelites, why the balance of power might shift with development, and why this shift leads to democracy. Second, I proposed the first long-run quantitative test of the social forces account. I gathered data on the composition of the labor force in almost 200 countries between the late 19th century and the present, which I used to construct an aggregate measure of nonelite disruptive capacity. This measure is not perfect, but barring significant progress in the theory and measurement of collective action, it is the best that students of cross-national democratization can do. I showed strong support for the hypothesis that nonelites drove democratization and confirmed existing wisdom that landlords imperiled it.

To be clear, it is not exactly correct to advertise my results as evidence that class capacities *caused* democratization. I have used this language as shorthand, but my results are consistent with two other explanations. It could be that countries on the verge of democratic transitions are more likely to adopt policies that cluster nonelites in high-capacity employment or that undermine landlords. In other words, democracy might explain capacity rather than capacity, democracy. The fact that antecedent levels of disruptive capacity predict democracy at a longer remove is encouraging, but this is not proof. Alternatively, it is possible that an omitted variable explains the coincidence of transition and antecedent capacity. Acemoglu et al. (2008) argued that both development and democracy were the consequence of antecedent institutional changes, which are thus the real cause. Note that the implications of this argument for the social forces account need not be dramatic. If the point is just that favorable institutional changes detonate capitalist de-

velopment, capacities may still be the proximate drivers of democratization. With no obvious way of capturing these institutional conjunctures quantitatively, I have to assume that they are residual to the patterns I explain. Nonetheless, partisans of the inferential revolution are correct to demand better. This is surely a fruitful area for future research.

Otherwise, future work might also explore improvements to the model of the transition game I proposed. Consider two. First, I assumed that nonelites always prefer democracy to dictatorship and that the intensity of these preferences is invariant. In my view, the incentive-centered accounts of democratization understate the benefits of democratization to nonelites, but assuming that they always demand democracy with equal intensity might make the opposite mistake. Of course, the measurement of incentives is an enormous challenge. Existing work uses measures of national-level inequality, which requires that we make heroic assumptions about what nonelites know about their relative well-being.[31] Second, I assumed that elite capacity was invariant. This allowed me to argue that nonelite disruptive capacity was the sole determinant of the balance of power, but this may not be realistic. My model assumed that elites exercise power through disinvestment (or exit), but there is a long tradition of scholarship that emphasizes explicit forms of organization (or voice). Future research might consider whether there is some way of capturing the facility with which elites organize.

As for the fate of democracy today, what can this article offer? For one, recent work has disputed the credentials of the democracies that now pepper the developing world. These governments are formally democratic, they argue, but democracy may flourish only because it is no longer bears on the distribution of power (Huber, Rueschemeyer, and Stephens 1997; Schmitter 2013; Roberts 2016; Rodrik 2016*a*). If recent democratizations have not yielded substantive gains, why? One possibility is that democratization in the postcolonial world has taken a different form than democratization in the first, as several authors have argued or implied (Soifer 2013; Slater et al. 2014; Rodrik 2016*a*; Haggard and Kaufman 2016). Future work might relax the typical assumption that the effect of all covariates is homogeneous; or, more ambitiously, it might synthesize and test explanations of why postco-

[31] That is to say, for the level of inequality to be a good proxy for nonelite incentives, nonelites should be aware of how unequal their country is relative to past years and even, depending on the specification, to other years in other countries. At least two facts about income inequality make this tricky. First, it does not usually vary very dramatically over time. Relying on this axis of variation requires believing that individuals discern movements of a few points in the Gini coefficient. Second, for all but the most recent period, we have known very little about patterns in cross-national inequality. Using cross-national variation to test the hypothesis requires that we believe nonelites had data that social scientists did not. See Bermeo (2010), who makes a similar argument when disputing the redistributivist account of the Portuguese transition to democracy

lonial democratization has been different. Moreover, if certain democracies have proved more empowering, the explanation may lie in the character of antecedent democratization. Collier (1999) and Haggard and Kaufman (2012) have noted that transitions take many paths, but there is surely more to learn about the consequences of this heterogeneity (Fishman and Lizardo 2013; Albertus and Menaldo 2014; Haggard and Kaufman 2016; Kadivar 2018). Second, and separately, recent events have led some to doubt the future of democracy in the *developed* world (Foa and Mounk 2016). The rise of populist authoritarians in these mature democracies is, some argue, a sure sign of their brittleness. Optimistically, my results imply that these challenges are destined to be short-lived. The landlord class is dead, the citizenry are educated, and democracy reigns in their neighbors. Yet there may be grounds for pessimism too. The collapse of the labor movement has surely contributed to the oligarchic and populist flavor of present-day politics. The health of democracy may depend on that movement's ability to rebuild currently defunct class capacities. Whatever the errors of my approach in this article, I hope what is clear is that key insights from the comparative-historical tradition have been missing from the resurgent literature on democracy's origins. The main casualty of this omission has been our approach to the problem of nonelite collective action. Leading scholars have proposed models that make ingenuous assumptions (Ansell and Samuels 2014), or they have abjured modeling nonelite capacity altogether (Boix 2003; Acemoglu and Robinson 2006). Others have noticed this problem, but my article is the first to propose both an abstract solution and a strategy for testing it over the long run. Future work will no doubt show better ways to both ends, but in doing so this article will have succeeded in its aim, which is to reintroduce scholars of the transition game to enduring sociological insights about the ability or inability of nonelites to remake the world around them.

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