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Capitalist Development and Democracy: The Controversy

In the comparative study of macro-social phenomena two radically different research traditions coexist with each other – cross-national statistical work and comparative historical studies. This may be – and is often – seen as just another instance of the age-old opposition between quantitative and qualitative inquiry or, more radically, between social science and humanistic scholarship.

A minority of scholars – among them Jeffery Paige (1975), John Stephens (1979c), and Charles Ragin (1987) – have insisted that the two research modes should complement and be integrated with each other rather than treated as irreconcilable opposites. In research on development and democracy, the opposition between the two modes of work has a particularly intense character. Here we encounter not just a divergence of method but sharply contradictory findings. As noted, this constitutes a difficult dilemma unless one is prepared to dismiss one mode of research out of hand as inadequate. This chapter describes and evaluates the two bodies of research and seeks to lay the basis for a way to reconcile and transcend the contradictions.

Comparative Historical and Cross-national Quantitative Research on Development and Democracy

The contradictions between these two research traditions did not often lead to actual controversy and debate. Many who worked in the more qualitative, comparative historical mode hardly became aware of the other side, because they simply could not conceive of a research strategy that claims to come to valid results by taking a few pieces of information about many countries and subjecting them to statistical, correlational analysis. In turn, the emphasis on historical particularity and the small number of cases to which the historical approach could be applied, appeared radically unsuited for an exploration of the causal conditions of democracy in the eyes of students who conceived of the

quantitative analysis of a large number of cases as the only viable substitute for the experimental approach that is impossible in macro-social analysis; they therefore searched for ways of studying the conditions of democracy through statistical inference from cross-national research covering many countries. The gulf between these methodological conceptions was – and is – so deep that the work of the other side was easily dismissed if it was noticed at all.

Ignorant dismissal was especially frequent among the historically minded students of democracy. The quantitative analysts paid more attention to the comparative historical work, though, as we will see, they often disregarded the more sophisticated theoretical ideas advanced by the other side, especially if these could not be tested with the crude measures suitable for macro-quantitative statistical analysis. This asymmetry has little substantive justification. Both sides grapple with difficult, yet fundamental methodological issues that are hard to do justice to at the same time; and each side makes different strategic decisions on which issues are to be given the most attention and which are to be treated with relative neglect.

We will first offer a selective account of both research traditions. This will introduce not only the problems at issue in the contrast between comparative historical and statistical investigations of the conditions of democracy but also a large number of ideas used in our own account. The chapter then turns to reflections on the questions raised by the divergence of the two research traditions. We will offer methodological arguments for an approach that can transcend the impasse and reconcile the contradictory findings.

Early quantitative cross-national studies

Seymour Martin Lipset published in 1959 a now classic paper linking democracy to economic development. It opened a long line of increasingly sophisticated quantitative cross-national studies. Lipset's theoretical position derived from the nineteenth-century classics of social theory, especially from Durkheim and Weber but also from Marx, combining a systemic conception of society with a revised version of social evolutionism. In many ways, his approach to the problems of development resembled that of modernization theory. At the same time, Lipset did not subscribe to the value determinism and the equilibrium assumptions that came to characterize especially later versions of modernization theory as well as his own later work. He combined a systemic view of social change with a resolute focus on divergent class interests and conflict.

Lipset begins with the observation that greater economic affluence in a country has long been thought of as a condition favorable for democracy: "The more well-to-do a nation, the greater the chances that it will sustain democracy" (Lipset 1959/1980: 31). He then proceeds to put this idea to the test by cross-national comparison.

He compares European and Latin American countries on the interrelated dimensions of wealth, industrialization, education, and urbanization and demonstrates that European stable democracies scored on average higher in all

of these dimensions than European dictatorships. Examples of the indicators he uses are per capita income, telephones per 1,000 persons, percent of people employed in agriculture, percent literate, and percent living in cities of different sizes. A comparison of democracies and unstable dictatorships with stable dictatorships in Latin America comes to very similar results at a lower level of development.

In his theoretical account for these relationships, Lipset focuses on moderation and tolerance. Education, he contends, broadens one's outlook, increases tolerant attitudes, restrains people from adopting extremist doctrines, and increases their capacity for rational electoral choice. Increased wealth moderates the lower classes and thus makes them more prone to accept gradual change. Actually, it is the discrepancy in wealth rather than its overall level that is decisive, but since there is generally more inequality in poorer countries these two factors are closely related. In countries with great inequality of wealth, the poor are more likely to be a threat to the privileged and the established order. The rich in turn tend to be hostile to democracy, both because they feel threatened and because they often view it even as morally wrong to let the poor and the wretched participate in political decisions – an arrogant attitude which in turn feeds the resentment of the poor. Thus, the middle class emerges as the main pro-democratic force in Lipset's analysis, and this class gains in size with socioeconomic development. In sum, Lipset argues that industrialization leads to increases in wealth, education, communication, and equality; these developments are associated with a more moderate lower and upper class and a larger middle class, which is by nature moderate; and this in turn increases the probability of stable democratic forms of politics.

Subsequent studies employed far more refined statistical techniques. But they confirmed the positive relation between development and democracy. While they explored alternative as well as complementary hypotheses and sought to detail the causal mechanisms underlying the connection between development and democracy, they added little to a more comprehensive interpretation of this relationship.

Phillips Cutright (1963) brought correlational – and more generally multivariate – analysis to bear on these problems. He argued that averages of different social and economic indicators are far too crude a measure of development, discarding the more precise information available. Furthermore, differences in the character of the political order must not be just crudely classified because they then cannot be related with any precision to the quantitative information on social and economic conditions: "It makes little difference that in the verbal discussion of national political systems one talks about shades of democracy if, in the statistical assessment, one cannot distinguish among nations" (Cutright 1963: 254).

Cutright constructed scales of economic development, of "communications development" as well as of "political development" or, in effect, democracy, each combining several specific measures.¹ He then subjected these quantitative scores for 77 countries to a correlational analysis.

The correlation between the indices of communication development and democracy (or political development) was $r = 0.81$, while the correlation of

democracy with economic development was 0.68, significantly lower. Cutright concluded that his main hypothesis – that political institutions are interdependent with the level of social and economic development – was confirmed.

The theoretical account Cutright offered for these findings is simple and not fully developed. More strongly than Lipset's it reflects the assumptions of modernization theory – of evolutionism and functional system integration. National societies are conceived as interdependent systems with strong equilibrium tendencies. Greater division of labor and structural differentiation in economy and society demand more complex and specialized political institutions, if the system as a whole is to be in equilibrium. He considers representative democracy as the form of government sufficiently complex to deal with a modern, increasingly heterogeneous social order. This identification of representative democracy with political differentiation is also the reason why the title of his paper speaks of "political development" rather than democracy.

In any less than perfect correlation, many countries will stand significantly above or below the regression line. Relative to its level of social or economic development a country may have "too much" or "too little" democracy. Commenting on this, Cutright offered on the one hand a number of ad hoc hypotheses explaining such "deviations" from a presumed equilibrium. For instance, he speculated, democracy may have flourished in the western hemisphere more than in Europe because of the absence of large-scale international conflict. And he suggested that case studies focus on deviant cases in order to gain further insights into the particular conditions favoring or hindering "political development".

On the other hand, he turned the mathematical equation representing the overall relations between social, economic and political development in all 77 countries into a "prediction equation:"

The concept of interdependence and the statistical method of this study (lead us to consider the existence of hypothetical equilibrium points toward which each nation is moving. It is possible for a nation to be politically overdeveloped or underdeveloped, and we suggest that either political or non-political changes will occur to put the nation into equilibrium. (Cutright 1963: 264)

This prediction presupposes an extremely tight integration of national systems. It furthermore implies the assumption that the social and economic development indicators represent the structural conditions that in the long run are decisive for the chances of democracy. However, these factors cannot explain on their own why any deviations from the predicted configuration should exist in the first place. Other conditions, such as those considered in the ad hoc hypotheses, become then by implication merely temporary obstacles to representative democratic forms of government or passing favorable circumstances.

Six years later, Cutright and Wiley (1969) published a study that responded to a number of questions raised by critics. It constituted a significant advance in quantitative comparative research on democracy. They selected 40 countries that were self-governing throughout the period from 1927 to 1966, thus excluding the effects of foreign occupation and colonial rule on the form of

government. This represents a small, but significant advance toward the ideal of employing units of analysis that are independent of each other – a technical presupposition of causal inference from correlational analysis that can never be fully met for human societies, especially in the twentieth century.

With this sample of countries they studied democracy in relation to social and economic development in four successive decades, 1927–36, 1937–46, 1947–56, and 1957–66. In this way they were able not only to examine the same relationships in four different periods but also to subject the question of causal direction to a “cross-lagged” correlational test. Their conclusion: the positive association between social and economic development and democracy holds for all four decades, and the data suggest a causal priority especially for economic development.

The analysis then turned to the conditions of change in political representation over time. What accounts for stability of regime form in the face of social and economic change? And which factors are associated with declines in political representation, which occur in spite of the fact that literacy rates and energy consumption, the indicators of social and economic development, hardly show similar declines? Here a simple measure of social security provisions, based on the age and number of national social security programs, proved illuminating.

Changes in political representation were virtually confined to nations that rated low in the provision of social security and at the same time high in literacy. This led Cutright and Wiley to a revision of Cutright's earlier equilibrium theorem which predicted that countries with a political representation “too high” or “too low” in view of their level of social and economic development would decline or increase in political representation. Only nations high in literacy and low in social security provisions conformed to this expectation. Where literacy as well as social security were low, little or no change was observed. Neither did any significant political change occur in countries with high social security, whatever their levels of literacy.

The interpretation of these results given by Cutright and Wiley stayed as close as possible to the original equilibrium model: economic development entails division of labor and social differentiation to which representative democracy is the most adequate constitutional response. This functionalist argument is now complemented by a causal hypothesis concerning *social* development: increasing literacy and related aspects of social change foster a population's interest and capability in political participation and thus engender pressures for democratization.

The stabilizing effect of social security provisions, which constitutes the main new finding, is explained by two ideas, the second of which is only obliquely hinted at. First, satisfying major economic interests of the population strengthens people's allegiance to the political status quo, independent of constitutional form. Demands for democracy, in this view, derive their strength from unmet economic needs.

The second explanation can be combined with the first, but it is a sharply distinctive argument once fully developed. The capacity of a government to deliver social security programs can be taken as an indication of a strong and

effective state apparatus, and – so we interpolate the argument – such state apparatuses may be strong enough to maintain the constitutional status quo: strong enough to defend itself against forces in society demanding a voice in collective decision-making, effective enough to “bribe” them into quiescence, and even powerful enough to crush them.

Retreat from comprehensive theoretical interpretations

Subsequent studies changed and refined the indicators for democracy² as well as the measures of social and economic development; they analyzed different samples of countries, and examined constitutional change over time. More important, however, was a subtle but significant shift in the relation of these studies to issues of theory. Typically, they explored propositions derived from alternative theoretical views of the relation between development and democracy, considering now in addition to modernization theory also the more conflict-oriented ideas of world-system and dependency theories. At the same time, they tended to refrain from such broader theoretical interpretations as offered by Lipset and Cutright and focused more and more on specific testable hypotheses.

Ken Bollen’s work, arguably the most careful of this type, brought further methodological refinements together with confirmation of the basic empirical generalizations. Bollen also responds to a wider range of theoretical arguments. His paper on “Political Democracy and the Timing of Development” (Bollen 1979) takes off from the skepticism about any clear-cut relationship between socioeconomic development and democracy that we will encounter when we turn to the comparative–historical studies. To anticipate, this view sees favorable conditions for democracy rooted in the particular historical constellation of early capitalism and it maintains that such favorable conditions are not going to be repeated.

Bollen formulated this as the hypothesis that “the earlier a country begins to develop, the higher its level of democracy,” noting that one could well argue the opposite by virtue of a diffusion of the democratic ideal over time which would exert more pressures for democracy in late developing countries. Using two different measures for the “beginning” of development, he found no significant association between the timing of development and political democracy. The interpretation of this negative finding is carefully left open. It could, for instance, be the result of the opposite – and mutually canceling – effects of different factors associated with the timing of development.

His analysis demonstrates again a rather robust association between economic development and democracy. This is especially significant because he examines a very large sample of 99 countries and because he employs a different set of indicators for political democracy. The association between political democracy and economic development was fundamentally unaffected by this different operationalization.

Bollen’s study also throws light on the role of cultural factors and on the impact of state strength on democracy. He found political democracy to be

positively associated with the proportion of Protestants in a country and negatively with the fraction of domestic economic production used for government expenditures. In subsequent publications, Bollen explored the interrelations between political democracy and income inequality as well as between democracy and the degree of a country's dependence on other countries in transnational economic relations. To these issues we now turn.

The relation between income inequality and democracy had been a central issue for Lipset (1959/1980) as well as for some of the classic authors on whose work he built his own argument. If these arguments lead us to expect chances for democracy to be more favorable under conditions of reduced inequality, the reverse direction of causation is equally plausible: wherever democracy is more than a mere formal sham, it should over time contribute to a reduction of inequality. Either or both of these relationships between inequality and democracy should result in fairly clear-cut negative correlations, although both are likely to involve time-lags the length of which is not easily specified. Empirical analyses have had trouble identifying clear-cut patterns.

A number of early quantitative studies came to contradictory results. Bollen and Jackman (1985b) concluded from their review of these studies as well as their own analysis that no relationship could be established once the level of development was taken into account. Muller (1988) argued that this was true only if democracy and inequality are measured at a single point in time. He found that the length of a country's experience with democracy has a significant negative impact on income inequality – independent of level of development, position in the world system, and the population's age structure. This is a gradual impact, measurably effective only after about two decades of democratic experience. Conversely, while the degree of income inequality does not seem to affect the *inauguration* of democracy, it does show a close relation to the chances of *maintaining* a democratic form of government.

The issue is far from settled. Muller's findings were challenged by Weede (1989) who introduced literacy in addition to level of development and the age structure of the population as control variables; this eliminated the central finding of Muller – the negative correlation between inequality and democratic experience. In turn, Muller (1989) replicated his earlier findings even with literacy as a control, using new measures for democratic experience and literacy.³

The exploration of the relation between democracy and economic dependence led to results one must judge ambiguous. Various authors had argued, on the basis of qualitative assessments and theoretical considerations, that the impact of advanced "core" countries on the political economy of dependent, "peripheral" countries would diminish the chances of democracy. "Outside the core, democracy is a rarity" (Chirot 1977: 22). Thomas and others (1979) drew from several, not altogether consistent, empirical tests the conclusion that economic dependence was indeed associated with political centralism.⁴ However, Bollen (1983: 476, n.13) found no significant effect modifying the relation between democracy and level of development when he introduced such variables as penetration by multinational corporations, foreign trade concentration and US foreign aid. A complex classification distinguishing countries at the

core of the capitalist world system, peripheral countries and semiperipheral countries, which was based on political as well as economic considerations – on treaty memberships, military interventions, and diplomatic relations as well as trade flows – did lead to the conclusion that democracy had, independent of a country's level of economic development, worse chances in peripheral and – to a lesser extent – in semiperipheral nations.⁵ This finding suggests that geopolitical international configurations may be more important than economic dependency as determinants of the chances of democracy. Shifting the focus to political international relations is also suggested by the results of Muller (1985), who found no evidence that breakdowns of democracy were the result of economic dependence, but observed a significant negative relation between aid, especially military aid, by the United States, and the stability of democracies.

Quite a few other cross-national statistical studies have also dealt with specific conditions or consequences of democracy – investigating further its relation to economic inequality and to a country's dependence on other countries in transnational economic relations or examining for instance the impact of democratic rule on economic performance. The details of these complex and often contradictory research findings need not detain us here. research findings need not detain us here.

A last quantitative study to be reviewed here departs from the cross-sectional mode of analysis of earlier work. Hannan and Carroll (1981) seek to identify social and economic correlates of *transitions* from one formal political structure to another. This “event–history method” partially confirms, partially modifies and complements the findings of cross-sectional research. Hannan and Carroll found that in the 90 countries studied for the period from 1950 to 1975, only a few of the variables examined had significant effects on the transitions from one of four political forms to another. High levels of economic production were negatively, ethnic diversity positively associated with overall rates of change in political form. The most stable political structures were multi-party systems: of the 39 countries with multi-party political structures in 1950, 28 had such a system still (or again) in 1975. In line with what one would expect from cross-sectional analyses, Hannan and Carroll's event–history analysis showed that richer countries are less likely to move from multi-party politics to political centralism, but the same holds for transitions away from centralized political forms: “Stated loosely, successful countries retain their political strategies.” (Hannan and Carroll 1981: 30–1). Ethnic diversity was not only found to destabilize formal political structures in general, but had a particularly negative effect on democracy: it was especially associated with transitions *out of* multi-party systems and with changes *into* one-party regimes.

The whole gamut of quantitative cross-national research was dismissed by many and attacked as inadequate by a few. Its empirical conclusions as well as its – generally sparse – theoretical grounding, primarily in modernization theory, were sharply contradicted by investigations that focused on the histories of a few countries and analyzed them in the light of more complex theoretical arguments. These studies were critical of the a-historical quasi-evolutionary generalizations that informed modernization theories. Their own common ground in theoretical conception has been characterized by a focus on

long-term effects of past conflicts and historical structures, by a search for the critical collective actors in historical change, and by an emphasis on the changing world historical environment of national histories. We offer a sketch of some of these comparative historical works before turning to an evaluation of both strands of research.

Early comparative historical investigations

Karl de Schweinitz (1964) formulated a theoretical position that sharply contradicts the notion that today's advanced capitalist countries represent the future state toward which less developed countries will travel on roads roughly similar to the paths taken by the "early developers". Democracy as known in the West was in his view the privilege of the original capitalist countries. Here economic development was slow. Its decentralized character encouraged liberal political conceptions and ideals. The working class was not yet mobilized. There was no demonstration effect from neighboring more advanced countries that would have stimulated individual and collective consumption demands. Thus, it was far easier than in today's developing nations to impose the disciplines of consumption, of work, and of public order that are necessary for economic development.

Later developing countries need a stronger state also for a number of other reasons – among them a very different international economic environment, which is likely to trap the less advanced countries in unfavorable positions in the transnational division of labor, and new technological options that can be exploited only with larger lumps of investment than private savings can sustain. The pressures toward greater centralization go beyond economic considerations and necessities. States in late developing countries also have more reason to intervene repressively because their rapidly changing societies are more mobilized. At the same time, they have more effective means – military and police technology, modern systems of communication and transportation, as well as better forms of organization – to impose the three disciplines of consumption, work, and public order. If that imposition succeeds, democracy is not very likely since democratization now depends largely on the values and intentions of the ruling groups. If it does not succeed, neither development nor democracy have good prospects. De Schweinitz concludes (1964: 10–11): "The development of democracy in the nineteenth century was a function of an unusual configuration of historical circumstances which cannot be repeated. The Euro-American route to democracy is closed. Other means must now be devised for building new democratic states." The remainder of the book makes clear that he sees the possibilities of developing democratic political structures as limited indeed.

Two generations earlier, in 1906, Max Weber voiced an opinion on the chances of bourgeois democracy in Russia that is similarly skeptical and roughly akin in its reasoning. While his passionate sympathies lay with the struggle of the liberal democrats in Russia, his analysis of the impact of capitalism on the

Russian economy and especially on the Russian agrarian structure led him to a rather negative prognosis.

True, the bureaucracy of the autocratic regime of the Tsar would hardly survive the tensions and conflicts of capitalist transformation: "As far as the negative side of the problem is concerned, the view of the 'developmental theorists' will be right. The Russian autocracy of the past has ... by any human estimate no choice but to dig its own grave" (Weber 1906: 350). But that does not mean that it will be replaced by a democratic regime. The project of democratization would have to rely primarily on the power of Western ideas, while it faces overwhelming structural obstacles. These obstacles are in Weber's view firstly grounded in the conditions of the Russian political economy, particularly in its agrarian problems. But the progress of democratization is also not favored by the character of advanced capitalism itself, which begins to penetrate the Russian economy. Capitalism in the twentieth century represents in Weber's judgement an increasingly hostile environment for freedom and democracy: "It is completely ridiculous to attribute to today's advanced capitalism an elective affinity with 'democracy' not to mention 'freedom' (in *any* meaning of the word)." Successful democratization in Russia now has to overcome obstacles that derive from the political and economic problems of late and uneven capitalist development as well as from the changed character of capitalism anywhere. Its only hopes are in Weber's view the *ideals* of bourgeois liberal reform – a slender reed to lean on.⁶

An even more skeptical view of the relation between capitalism and democracy that applies to early capitalism as well can be inferred from his analysis of the role of law and bureaucracy in the rise of capitalism. Here Weber (1922/1968) argues for a functional correspondence or "elective affinity" between early, competitive capitalism and the predictability of formally rational law and bureaucratic administration. Formal rationality and thus predictability are compromised by substantive demands of justice. Democracy, however, is in Weber's view precisely the institutional arrangement through which such substantive demands are invading and transforming the pure formalism of law. In critical ways, then, democracy and even early capitalism were at odds with each other.

More recent comparative historical work

Guillermo O'Donnell (1979b) sought to explain authoritarian developments in South America during the 1960s and 1970s that seemed at odds with the optimism implied in modernization theory. Argentina, Brazil, Uruguay, and other countries turned away from democratic constitutional forms at fairly high levels of development and, he argued, for reasons precisely related to their comparatively advanced stage of development. O'Donnell's analysis was based on a political economy framework, roughly comparable to that of Max Weber and de Schweinitz. He gave particular attention to the economic and political dependence of a late developing country on the developed core of the capitalist

world economy and to the responses of the state and of class-based politics to the problems engendered by this dependency.

Import substitution industrialization (ISI) had expanded the urban middle and working classes and brought to power populist coalitions which deliberately activated popular forces, particularly through labor organization, and included them in the political process. Economic growth underwrote the costs of social welfare policies. However, the progress of “easy,” or “horizontal,” i.e. consumer goods import substitution behind high tariff walls depended on growing imports of capital goods, paid for by exports of primary goods. This development strategy ran into trouble when the foreign exchange reserves accumulated during World War II were exhausted and both prices and demand for Latin America’s primary exports declined in the 1950s. The severe balance of payments problems caused domestic inflation. Attempts to impose stabilization policies hurt the popular sectors, divided the populist coalitions, and created political crises.

The growth of ISI had also enlarged the number and range of technocratic roles in the public and private sectors. Prominent on the minds of these technocrats was the “deepening” of industrialization, i.e. the creation of a capital goods industry. However, successful pursuit of this strategy entailed reduction of popular consumption in order to generate higher domestic investment levels (as taxation of the wealthier sectors was not even considered as a realistic alternative), and attraction of foreign capital. The crucial obstacles in this path were militant labor movements and populist politicians. This constellation led to the formation of a coup coalition among civilian and military technocrats and the big bourgeoisie. They discarded democracy as incompatible with further economic development and installed bureaucratic-authoritarian regimes. These regimes insulated economic policy makers from popular pressures and deactivated unions and left-wing political parties, by force if necessary. Thus, it was exactly in the more advanced of the Latin American countries that particularly harsh authoritarian rule was imposed in the 1960s and 1970s.

O’Donnell asserted on the basis of these findings an “elective affinity” between advanced capitalist development in dependent political economies and bureaucratic authoritarian rule. Though the wider and longer-term significance of such developments is treated with caution, his perspective is radically different from the optimism of much of modernization theory: “It is impossible to say, without systematic comparative research, but it is a disquieting possibility that such authoritarianisms might be a more likely outcome than political democracy as other countries achieve or approach high modernization (O’Donnell 1979b: 90).

O’Donnell places great emphasis on a country’s dependent position in the international economic system. Dependency theory – as well as its close cousin, world system theory (see Wallerstein 1974 and 1976) – generally tends to see economic dependence as creating pressures toward authoritarian rule (see, e.g. Chirot 1977; Thomas 1984).

Seven years before O’Donnell’s book, Barrington Moore, Jr., had published *The Social Origins of Dictatorship and Democracy* (1966). This was without doubt the most important comparative historical research on development and

political form, and it achieved paradigmatic influence in the field. Through historical case studies of six countries – England, France, the United States, Japan, India, and China – and extensive research on two more, Germany and Russia, Moore identifies three distinct paths to political modernity, each characterized by specific conditions: the path to parliamentary democracy, the path to fascist dictatorship, and the path to communist dictatorship. These three routes, he argues, are not alternatives that are in principle open to any society. Rather, they are tied to specific conditions characteristic of successive phases of world history. Thus he sees the conditions favorable for democracy – like Weber and de Schweinitz – bound up with the historical constellation of early capitalism: “the route that ended up in capitalist democracy ... was itself a part of history that almost certainly will not be repeated” (Moore 1966: 5).

A strong concern with historical particularity and process leads Moore to a principle that informs all of his interpretations and explanations: past conflicts and institutional structures have long-term effects and are of critical importance for later developments. Any attempt to explain current change without attention to these continuing effects of past history – any “presentist” analysis – is doomed to fail.⁷

Moore’s specific analyses proceed in the by now familiar political economy framework: economic change, state structures and state actions, and social classes are the central categories. Basically marxist in orientation, the study focuses on peasants and lords, though the bourgeoisie is given a critical role as well. Moore’s emphasis on the role of the rural classes derives, of course, from the principle of long-term effects of past history. It is noteworthy, however, and it will occupy us later, that the working class is virtually absent from the picture he gives of the rise of democracy.

In his conceptions of rural class conflict, the distinction between labor-repressive and market-dominated modes of labor control plays a crucial role. This has found striking support in a study of agrarian social movements in contemporary developing countries by Jeffery Paige (1975). Paige found that the most radical agrarian movements emerged when a landlord class relied on coercive labor policies while facing a cultivating class that derived its income primarily from wages rather than directly from the land and that was able to organize for collective action.

Moore asks of his cases a number of central questions, and it is these questions that constitute the core of his theoretical framework. The analysis focuses (1) on the strength of the state in relation to the power of landlords and bourgeoisie, (2) on the incidence of repressive agriculture for which the landlords need the help of the state, (3) on the relative strength of the rural and the urban dominant classes, (4) on the alliances of domination among the crown and the dominant classes, alliances shaped by the relative strength and the interests of these partners in power, and (5) on the chances of the peasantry to come to collective action depending on the presence or absence of solidary village and work structures.

The conditions for the route to communist revolution can now be listed in skeletal fashion: a highly centralized state, a weak bourgeoisie, a land owning class that relies on political means of labor repression, and a peasantry with

good chances of collective action that are due to solidary village communities and weak ties to the – often absent – landlords. This picture bears a striking similarity to the sketch of the factors Weber considered relevant in the early stages of the Russian revolution. The communist take over occurred in Russia only after the system of domination broke down in the revolution at the end of World War I, which was fueled by peasant discontent.

Moore's view of the conditions for the reactionary revolution from above that ends in fascist dictatorship can be put in similarly apodictic form as follows: a coalition led by a strong state and powerful landowning classes includes a bourgeoisie that is not without some strength but depends on the support of the state through trade protectionism, favorable labor legislation and other measures that in different combinations characterize top-down, state-sponsored industrialization. Agricultural labor remains significantly controlled by repressive means rather than primarily through the market. Owing to village and work structures that do not favor solidarity, the peasant revolutionary potential is low. The internal tensions and contradictions of industrialization under reactionary sponsorship lead to experiments with democracy that do not, however, yield results acceptable to the dominant classes. Fascist repression is the final outcome. The similarity of this path to the developments in Argentina and Brazil in the 1960s and 1970s did not escape the notice of O'Donnell. In fact, he explores the broader theoretical implications of his own analysis precisely by linking it to Moore's work and by extending Moore's ideas beyond the cases of Japan and Germany (O'Donnell 1979b: 88–90).

The emergence of parliamentary democracy represents the oldest route to modernity. The picture Moore offers here is more complex than in the case of the other two routes. Conflict and a fairly even balance of power between the lords and the crown are a first condition. A strong bourgeoisie, at odds in its interests with the rural dominant class and even able to entice landlords into commercial pursuits, is of critical importance: "No bourgeoisie, no democracy" (Moore 1966: 418). Moore also notes that in all three cases of democratic development studied there was a revolutionary, violent break with the past, unsettling the established domination of landlords and crown. Other conditions that emerged as significant in the rise of communist revolution and fascist dictatorship show, however, no clear-cut pattern in the histories representing the democratic route: while labor repressive agriculture was present in France and the United States, English agriculture relied rather exclusively on the market. The capacity of rural labor for collective action – the revolutionary potential of the peasantry – was high in France but low in England and the United States.

On the case of India, Moore takes a similar position as de Schweinitz: there are complex conditions that allow the institutional legacy of post-colonial democracy to survive. But due to the limited compatibility of freedom and efficiency under current conditions, Indian leaders have to face cruel choices between effective democracy and effective development. The argument reveals a conviction that informs Moore's analysis of all routes into the modern world: "The tragic fact of the matter is that the poor bear the heaviest costs of modernization under both socialist and capitalist auspices." Therefore, moder-

nization is not possible without "either masked coercion on a massive scale, as in the capitalist model including even Japan, or more direct coercion approaching the socialist model" (Moore 1966: 410).

In her incisive review of *Social Origins*, Skocpol (1973) takes Moore to task on a number of points, among them his neglect of the variably autonomous role of states and his lack of an intersocietal perspective.⁸ She argues that a strong state with a capacity for repression – something absent in England for example, due to the reliance on the navy for military power – is an essential element of the authoritarian class coalition. Her argument for the importance of the intersocietal perspective is, in part, a plea for integrating Moore's domestic class analysis with the central ideas of dependency theory and Wallersteinian world-system theory. But at the same time, it is an argument for the importance of interstate relations in analyzing domestic politics and the variably autonomous role of the state. Conceptualizing states as standing at the intersection of domestic and international power relations proved to be exceptionally fruitful in her own comparative study of social revolutions (Skocpol 1979).

Moore's analysis is open to quite important other criticisms. One was briefly noted earlier and will occupy us later at some length: the role of the working class in democratization is rather radically neglected. This is in part a consequence of Moore's focus on long historical gestation periods. In addition, it follows from his definition of democracy which focuses on public contestation of political issues rather than on inclusive participation in the political process. The democratic struggles of the working class then appear only to extend an otherwise already largely established pattern. We will argue for a very different view.

Another important critique takes off from the apparently innocuous fact that the time periods taken into account for the different countries vary considerably in length. While the cases of democratization are pursued over very long time periods, the discussion of Japan and Germany breaks off with the establishment of Fascism. This can be defended only by arguing that post-war democratization in these two countries was exclusively a result of foreign imposition, which in turn is – like all questions of international context in Moore's analysis – excluded from the explanatory framework.

If this exclusive focus on domestic developments is modified and if the time periods considered are adjusted in theoretically meaningful ways, it is possible to argue that the reactionary path to political modernity has some potential for leading – by tortuous detours – to democratic political forms. This argument goes far beyond the cases of Japan and Germany. France came at various points in the nineteenth century quite close to the reactionary path model, yet it rightly figures as one of the main cases of democratization.⁹ Spain, Portugal, and Greece as well as Argentina and Brazil may well be seen as instances of a similar development toward democracy in the twentieth century (Rueschmeyer 1980).

Yet, these as well as other critiques notwithstanding, Moore's book represents a towering achievement. It helped transform the social sciences by reestablishing the comparative historical mode of research as the most appropriate way of analyzing macro-social structures and developments.

Two modes of research – contradictory results

Our review of quantitative cross-national and comparative historical studies on the relation between capitalist development and democracy has shown us results that rather consistently contradict each other. We are faced with a serious dilemma because the two research traditions are separated by two things at once: by opposite findings and by different methods.

The first research tradition covers many countries, takes for each country only a minimum of standardized, aggregate, but not always reliable information into account, and translates that information – on occasion not with great delicacy – into numerical expressions in order to subject it to complex mathematical operations. It sees the quantitative analysis of a large number of cases as the only viable substitute for the experimental approach that is impossible in macro-social analysis.

The other tradition studies only a few countries at a time, and while the complexity of such analyses far exceeds the possibility of testing the explanatory propositions with so small a number of cases, these works are attentive to many factors suggested as relevant by common sense and theoretical argument, they treat historical particularity with care, they give weight to the historical genesis of social and political structures and developments, and they betray an attractive awareness of long-term historical developments in different parts of the world.

Taken together, the two research traditions highlight fundamental methodological issues that are hard to do justice to at the same time. Faced with difficult dilemmas, each makes different strategic decisions on what to give priority.

The quantitative cross-national research, which we respect for its breadth of coverage, the objectivization of analysis, and the quantitative testing of specific hypotheses, has come to a number of consistent results. The outstanding finding is that there exists a stable positive relationship between socioeconomic development and democracy.

The comparative historical tradition of research, which we respect for its analyses of historical process and for the sophistication of theoretical argument, is by contrast extremely skeptical of the chances of democracy in contemporary developing countries. These authors do not only deny that there exists a consistent and theoretically plausible relationship between democracy and development, capitalist or otherwise, but they also see the odds of democracy especially in developing countries as extremely unfavorable. They find the main reasons for this world historical change since the first rise of capitalism in the different and more powerful role of states (including the expansion and transformation of the military forces) in both less developed and advanced industrial countries, in the different balance of power between dominant and subordinate classes and different patterns of class alliance in less developed countries, and in the different transnational environment in which late-coming nations have to advance their projects of development.

How can this dilemma – created by contradictory results of different research methods – be resolved? Before that question is approached, one point should be made clear. This is not a conflict between divergent quasi-

philosophical, “meta-theoretical” positions, as was argued for different theories of the state by Alford and Friedland (1985). In that case the conflicting analyses would simply talk past each other. The contradictory results at issue here can in our view be confronted with each other much more directly; they are in principle open to resolution on the basis of empirical evidence. This, too, is the way in which they have been treated in the past – by Max Weber no less than by the quantitative methodologists of today. We will first turn to some methodological arguments and reflections, giving emphasis to those that challenge the widely accepted monopoly of the quantitative cross-national methodology, and then seek to arrive at a judgement about the best foundations of a strategy of resolving the contradictions.

Methodological Reflections

Critique and countercritique

A convenient starting point for examining the contradictions between the two research traditions is O'Donnell's critique of cross-national statistical research, one of the rare responses from a comparative historical scholar to the other side. O'Donnell argues, first, that causal inferences from quantitative cross-national evidence imply the assumption that the causal conditions which affect the chances of democracy today are the same as those which shaped democratic developments during the early rise of capitalism, an assumption that may well be wrong. This, of course, invokes the fundamental claim made by all the comparative historical analysts we reviewed – that democratic developments were rooted in a historical constellation not likely to be repeated. However, quantitative research results make it difficult to sustain the lines of argument that have been advanced so far. Bollen (1979), as we have seen, found no consistent relationship between the timing of development and democracy or, more precisely, none that overrides the association between democracy and level of development. Furthermore, the statistical association between democracy and level of development holds even if the most advanced industrial countries are excluded from the analysis (see e.g. Cutright 1963: 258; Marsh 1979: 238). That means it cannot be “explained away” by a strong association between democracy and the highest levels of development achieved by the early modernizers.¹⁰

Next O'Donnell charges that if “deviations” from the central tendency identified by multivariate analysis are dismissed as due to idiosyncratic obstacles, “the basic paradigm is rendered immune to empirical falsification” (O'Donnell 1979b: 5). This objection seems rooted in the comprehensive interest in each case characteristic of comparative historical research; rather than dismissal, the deviant case deserves special attention. The objection is plausible in the context of comparative historical analysis. It is not convincing as a critique of the statistical approach, which focuses on a number of variables while randomizing the effects of others. True, in the early work of Cutright

(1963) we encountered interpretive arguments, wedded to the neo-evolutionism and the equilibrium assumptions of modernization theory, that fit O'Donnell's charge rather exactly. However, Cutright himself adduced the evidence for very important modifications of the assumed equilibrium tendency (see Cutright and Wiley 1969). And later studies no longer viewed the statistical associations as confirming complex macro-trends, but used them rather to test specific hypotheses.

O'Donnell also charges the quantitative studies with what he calls the "universalistic fallacy" – the assumption that since in a set of all or most contemporary countries "some positive correlation between socio-economic development and political democracy can be found, it may be concluded that this relationship holds for all the units (say, regions) included in that set" (O'Donnell 1979: 6). This raises the same question about uniform conditions of democracy across different regions as we just considered for different periods of time. The argument is central to O'Donnell's view of South America, where it seemed at the time that "political authoritarianism – not political democracy – is the more likely concomitant of the highest levels of modernization" (O'Donnell 1979: 8). Though nothing is wrong with this idea of regionally variant conditions in logic or theoretical principle, it is contradicted by the evidence of quantitative studies that varied in regional inclusiveness but not in the dominant result of a positive association between level of development and democracy. Given our present knowledge, it may be more reasonable to warn regionally specialized scholarship – such as Latin American studies – against the "particularist fallacy" of disregarding the results of more comprehensive analyses than to press the dangers of a universalist fallacy against the claims of quantitative cross-national research.

O'Donnell makes a quite valid point when he argues that variations within a country are not taken into account when cross-national analyses are based on average per capita figures for domestic production, educational attainment etc. It is quite true, for instance, that the growing wealth of some segments of the population affects national averages quite strongly even though nothing may have changed in the economic condition of the vast majority; in fact, such a development renders the groups that do not participate in the higher standard of living even less – rather than more – capable of making their interests count in political decisions.

However, one may see such inattentiveness to intra-country variation as a discrepancy between the indicators used and the theoretically relevant variables – an error in measurement. And it is well known that measurement error, unless it systematically favors the hypothesis under review, has the counter-intuitive effect of deflating correlations. This also applies to the – often quite debatable – indicators of social and economic development and political democracy. Bad measures make it harder, not easier, to confirm a hypothesis.

Another argument of O'Donnell constitutes, however, a powerful critique with far-reaching consequences: it is highly problematic to draw *diachronic* conclusions – about changes over time and thus about causation – from *cross-sectional* analyses. The same idea – that genetic, causal questions require historical information about processes rather than cross-sectional data on a

given point (or short period) of time – was the starting point of a seminal paper by Dankwart Rustow (1970) that developed a simple process model of democratization whose phases moved from prolonged and inconclusive struggle through elite compromise to habituation. The systematic exploration of causal conditions through comparative analysis of historical sequences is a cornerstone of our own approach.

It is true that several quantitative cross-national studies did take the historical dimension into account, however minimally and crudely (Cutright and Wiley 1969; Bollen 1979; Hannan and Carroll 1981). The findings of these studies are suggestive for further analyses that search for genetic, processual explanations. Nevertheless, there is little doubt that causal explanations cannot be tested directly with cross-sectional studies and that it is diachronic propositions and studies of historical sequence that are needed for settling the issues of a causal interpretation of cross-sectional findings.

Where, then, do these rather complex arguments leave what we may take as established conclusions of the quantitative cross-national studies? One massive result of these studies still stands: there is a stable positive association between social and economic development and political democracy. This cannot be explained away by problems of operationalization. A whole array of different measures of development and democracy were used in the studies under review, and this did not substantially affect the results.

This result cannot be invalidated either by arguing that it may not apply to certain regions of the world. Nor can it be explained by diffusion from a single center of democratic creativity, though some associations of democracy with former British colonial status as well as the proportion of Protestants were found by Bollen (1979). It also cannot be explained by a particularly close correlation between development and democracy at the highest levels of development, because samples consisting only of less developed countries exhibited substantially the same patterns. Finally, the close concatenation of level of development and democracy cannot be accounted for by a special association between early modernization and democracy since the explicit inclusion of measures of the timing of development did not significantly affect the relationship between level of development and democracy.

Yet as the tale of storks and babies often told by statisticians suggests, any correlation – however reliably replicated – depends for its meaning on the context supplied by theory and accepted knowledge. The relation between statistical finding and theoretical account is decidedly asymmetrical. The theoretical explanations we encounter in the cross-national studies do not gain any particular credibility from the sturdiness of the findings for which they give an account. They are, to put it most starkly, pure conjecture. This is so by logical necessity, though it also finds support in well-founded reservations about the theoretical models most often used. In sum, the quantitative findings are compatible with a wide range of explanatory accounts.

The causal forces that stand behind the relationship between development and democracy remain, in effect, in a black box.¹¹ The explanations offered in the early quantitative research adopted the then prevailing assumptions of modernization theory. But nobody can maintain that this in any way followed

from the statistical results. The correlations between development and democracy constitute an *empirical generalization* – not more and not less. In regard to the theoretical account of the conditions of democracy, this empirical generalization plays a role that is critically important and at the same time strictly limited: It has a veto power over certain explanations – those that are at odds with it; but it does not determine the choice between various theoretical accounts that are compatible with it.

If we must consider the association between development and democracy a fundamental given in any theoretical argument about the conditions of democracy, the quantitative cross-national research has yielded also a number of results that have less definite and often quite ambiguous implications. We can best treat them as important suggestions for further analysis, because the relationships emerged only in one or a few studies and were contradicted by others or because it is not clear what exactly is measured by the empirical indicators used. Among the more important suggestive findings are the following:

- the possible negative impact of state strength on the chances of democracy,
- the association between stability of political form and the provision of social security,
- the negative relation between central control of the economy and democracy,
- the negative effects of ethnic and linguistic fragmentation on democracy.
- the possible role of cultural tradition and diffusion (British influence, percent Protestant),
- the supportive relation of literacy and literacy gains to democracy,
- the possible impact of economic and especially geo-political dependency,
- and the long-term mutually supporting relation between democracy and lowered economic inequality.

There are no similarly explicit and refined critiques of the comparative historical approach as O'Donnell mounts against the cross-national quantitative work. That does not mean that comparative historical research is generally held to stand above such criticism. To the contrary, the very self-understanding of many quantitative social scientists is built on a dismissal of qualitative evidence as merely anecdotal – interesting for illustration and perhaps inspiration, but worthless when it comes to establishing results. The critical claims about comparative historical research implied by this view are easily listed. Comparative historical research, while theoretically often very complex, covers too few cases to come to any definitive results about these theoretical arguments. The choice of cases is often arbitrary, and there is no protection against a case selection that favors the author's line of theoretical argument. In fact, theories are rarely tested in any meaningful sense, because they are typically developed from facts known in advance. Finally, the lack of methodological self-consciousness in much comparative historical research is taken as the symptom not only of a profound unconcern but also of fatal substantive flaws.

We will take up some of the specifics of this critique in our discussion below. Here it is sufficient to make only a few fundamental points. The first, already

made earlier, is excellently developed in Ragin's recent examination of comparative methods, both quantitative and qualitative (Ragin 1987): both comparative historical case studies and variable-oriented quantitative research must answer to the same fundamental standards, and both meet them – imperfectly – in different ways and with different strengths. The second is that the near-consensus of the comparative historical studies on the extremely limited chances of democracy after a favorable phase in the course of world history is at odds with the most robust finding of cross-national quantitative research. That consensus opinion must be dismissed, and the contrary result of the quantitative studies must be considered an established empirical generalization with which all accounts of democratization have to come to terms. This does not, however, follow from inherent flaws of comparative historical research; rather, it is our considered judgement after comparing the two traditions of research. Our third and final claim is that in principle comparative historical research is equally able to come to similarly pivotal results.

The comparative advantage of historical analyses

How are we going to develop an empirical theory about development and democracy that is credible in the light of general sociological knowledge, capable of accounting for the central relationship between development and democracy established by the cross-national quantitative research, and promising for further research into the conditions of democracy and for the interpretation of ambiguous and opaque findings? It is our conviction that we must turn to the richer theoretical reasoning of the comparative historical tradition if we want to lay the groundwork for an adequate theory of the conditions of democracy. We take this position in spite of the fact that so many of the qualitative historical works came to conclusions about the relation of democracy to development in today's world that are at odds with the quantitative empirical evidence. That their conclusions went far beyond the evidence actually examined in these studies may or may not be taken as an indictment; it does point to the problem inherent in theory-oriented comparative history just mentioned: the number of cases is too small for the number of variables considered. The contrast between intellectual complexity and the limited number of cases is indeed a basic dilemma of the comparative historical search for explanation and theory.

There are several reasons why nevertheless the comparative historical tradition of research on democracy appears to offer the best foundation for constructing a satisfactory theoretical account of the conditions of democracy. First, it is far richer in theoretical argument and analysis than the macro-quantitative studies. This is true whether we compare it with the quantitative studies that – like Cutright's – seek to support a broad systemic interpretation or with the later research trying to test specific hypotheses. This theoretical richness is not an accident: "One of the most valuable features of the case-oriented approach ... is the fact that it engenders an extensive dialogue

between the investigator's ideas and the data" (Ragin 1987: 49). Second, and more specifically, the political economy orientation of the works reviewed has proved fruitful in a number of similar areas of inquiry – for instance in comparative work on inequality, on socioeconomic development, and on state intervention in civil society.

Finally, these studies developed their explanatory ideas grappling with historical sequences; and we are convinced that it is in sequences of change that we will find the key to the black box that mediates the relation between development and democracy. Historical sequence studies are generally best attuned to the necessities of a genetic, causal explanation. This claim will appear to many social scientists at first sight counterintuitive. Further reflection will perhaps make it more plausible.

What are the specific chances of insight, which the particular blind spots of the two modes of research? Our basic position on the methodological side of the impasse between them was already stated: neither side has an obvious superiority in principle, and neither can be dismissed. Rather, each has made choices when confronted with a situation that did not allow obedience to all mandates of methodology – not even to all major mandates – at the same time. Each side had to pay for its peculiar strengths with equally characteristic weaknesses.

Further reflection may usefully begin with the theoretical implications of a single case pursued over time. All too often it is taken for granted that the theoretical utility of studying one single case is extremely limited. It can inspire hypotheses, this argument says, but so can sheer imagination. It can perhaps force a reconsideration of those propositions contradicted by this singular set of unique facts, but it cannot go beyond that. This view overlooks that a particular sequence of historical development may rule out a whole host of possible theoretical accounts, because over time it typically encompasses a number of different relevant constellations. The continuity of a particular system of rule can for instance invalidate – by its very persistence under substantially changing conditions – quite a few claims about the conditions of stable domination. Such an effect presupposes, of course, that there are reasoned expectations, that the interrogation of the historical record is theoretically informed. This impact of a single case analysis is strengthened by the fact that for one (or a few) cases it is possible to match analytic intent and empirical observations much more precisely than in an analysis covering many cases with the help of standardized indicators. Case-centered research can examine the particular context of seemingly simple facts and take into account that their analytic meaning often depends on that historic context. It is these two features of historical analysis that led E. P. Thompson to insist on the "epistemological legitimacy of historical knowledge ... as knowledge of causation" and to speak – somewhat obliquely and perhaps extravagantly – of "history as a process inscribed with its own causation" (Thompson 1978: 225, 226).

Yet if the theoretical utility of the narration and analysis of even a single case must not be dismissed, a focus on historical lines of change does carry its own problems. Studying change within the same society implicitly holds constant those structural features of the situation that do not actually change during the

period of observation. It is for this reason that process-oriented historical studies – even if they transcend sheer narrative and are conducted with theoretical, explanatory intent – often emphasize the role of voluntary decision and tend to play down – by taking them as givens – structural constraints that limit some options of historical actors and encourage others.

This consideration may throw a revealing light, for instance, on the recent controversy about the relation of German big business to the rising National Socialist party. Turner's analysis (1985) is a good example of the focus on process and agency and he comes to rather innocuous results about the role of big business in the rise of Hitler, while Abraham (1986) who uses a theoretical framework centered on structural analysis comes to very different conclusions. Similar questions are raised if O'Donnell and Schmitter (1986: 19) claim that in recent transitions from authoritarian rule "what actors do and do not do seems much less tightly determined by 'macro' structural factors during the transitions we study here than during breakdowns of democratic regimes". They may indeed offer us a fascinating empirical generalization. But the fact that their conclusion to the studies of "Transitions from Authoritarian Rule" (O'Donnell, Schmitter and Whitehead 1986) emphasizes themes congenial with a voluntarist perspective – such as political divisions within the authoritarian regime, pacts of "soft-liners" in government with parts of the opposition, and the sequences and turns of liberalization that could have taken a different course – may also derive from the design of this project, which had at its center a series of country monographs covering a relatively short period of time.¹²

If we entertain serious reservations about the voluntaristic bias that seems associated with the study of single instances of historical process or with analyses covering relatively short time periods, we do not intend to counterpose the focus on the *longue durée* of the French *Annales* school to a dismissed *histoire événementielle*. Our aim is rather to construct a framework of inquiry that is in principle equally well attuned to the study of process and to the recognition of structural constraints. This does not seem a utopian goal. Within this broader framework, our own interests do center on the structural conditions of democracy rather than on a process analysis of regime transitions.

Ragin claims that comparative historical case studies are generally inhospitable to structural explanations while "wide-ranging cross-national studies, by contrast, are biased in favor of structural explanations" (Ragin 1987: 70). There is little doubt about the latter assertion. In fact, cross-national statistical research has no choice but to be structurally oriented.¹³ The former, however, truly holds only for single-case historical accounts. The voluntaristic bias of case oriented research is counterbalanced by comparison. Even in single-case studies comparative awareness and especially a longer time span of investigation can – logically analogous to cross-country comparisons – make the structural conditions of different event sequences more visible.

It is, however, actual comparison of cases featuring different structural conditions that really turns things around. Even a few comparisons have a dramatic effect in disciplining explanatory accounts. Moore's (1966) classic study does not stand alone as a case-oriented comparative inquiry that illuminates the role of structural constraints. In fact, most of the comparative

historical studies we have reviewed share a strong focus on structural conditions. Clearly the strategy of case selection acquires critical importance here, because the range of cases examined determines which outcomes and which potential causal conditions can be comparatively studied. Certain case selections and choices of time horizon can also favor a focus on process and agency. This is demonstrated by O'Donnell and Schmitter's (1986) work on redemocratization and Linz's (1978) work on breakdown of democracy. Linz's extended essay compares cases in which the democratic regime collapsed and focuses on the events which led up to the demise of the regime. His emphasis on process (e.g. "the constriction of the political arena") and agency (e.g. mistakes made by the supporters of the democratic regime) are direct results of the short time horizon and the case selection. Had he compared breakdown cases with those in which democracy survived and/or selected a longer time horizon, for example comparing the breakdown with later returns to democracy, structural differences would have appeared as much more important in the analysis. Precisely the same observations could be made about O'Donnell and Schmitter's essay on redemocratization.

Case selection is a more important concern in comparative historical research than in quantitative cross-national studies because the latter typically reach for the largest number of cases for which the relevant information is available. Rational case selection depends primarily on a sound theoretical framing of the issues.

Ragin (1987) sees the special strength of comparative historical research in its particular aptitude to deal with two phenomena – multiple causal paths leading to the same outcome and different results arising from the same factor or factor combination, depending on the context in which the latter operates. He sees this as a powerful advantage because he considers multiple and "conjunctural" causation as the major reasons for the peculiar complexity of social phenomena and especially of large-scale social phenomena.

Why should the comparative case strategy have a special strength in dealing with this causal complexity? Since each case is viewed both on its own terms and in comparison, alternative causal conditions for the same or similar outcomes stand out with special clarity in comparative historical work, while macro-quantitative studies tend to view their cases as a causally homogeneous population of units. This is closely related to what we observed about the relation between indicator and analytical concept. The case-oriented approach has a strong comparative advantage in taking context into account – both in assessing the character of an event – say an insurgent social movement – and in evaluating its causal impact within a historical situation. Again, it is clear that good, theoretically guided case selection is critical for making full use of these advantages.

Finally, the comparative historical method allows the exploration of sequence and this, as claimed earlier, is indispensable for causal analysis. The claim deserves more comment. While a causal condition obviously has to precede its result in time, historical depth is not so obviously required. It is logically quite conceivable that the outcomes we wish to explain result from conditions located in the most immediate past. However, macro-social research has taught us two

lessons, which make it problematic to take this logical possibility for granted. We have learned that (1) sequence often matters and (2) structural conditions, once settled, often resist transformation. It may, for instance, matter a great deal for the outlook and the organization of the working-class whether universal suffrage came early or late in the process of industrialization (Katznelson and Zolberg 1986), and – once set – different patterns of class consciousness and readiness to organize may be hard to change.

Neither sequence effects nor historical persistence can be counted on a priori. We need to know much more about the conditions under which lasting patterns form, change and break down before we can use historical persistence as an *explanatory* principle; and the same goes for sequence effects. We do, however, have sufficient knowledge to treat them as *heuristic* principles. As heuristic principles they privilege certain research strategies and cast doubt on others. What we know about sequence effects and structural persistencies in large scale social change make “presentist” explanations profoundly problematic. Therefore causal exploration in macro-social analysis requires the study of fairly long time periods, it requires comparative *historical* work.

Our insistence on the importance of comparative historical sequence studies for developing and testing genetic and causal theories will not go unchallenged. There is not only the argument of “too few cases, too many variables”. There are also arguments presenting cross-sectional quantitative studies as particularly suitable for causal inference. These consider the factors that in a large cross-sectional set of cases are associated with a dependent variable as those most important in the longer run (see Bollen 1979: 583; also Bollen and Jackman 1989). If the number of cases is large enough for “accidental” variations to balance each other out, this argument maintains, it is precisely a cross-sectional analysis that will best reveal the major structural determinants of variation in the dependent variable – here democracy.

It is clear that this assertion presupposes a causal homogeneity of the universe of cases as well as long-run equilibrium tendencies. It also assumes a close correspondence of diachronic and synchronic relations among variables. Without such premises, which make the sharp differentiation between short-run and long-run, “accidental” and “major” causal factors possible, the goal of “reading off” the major causal factors from cross-sectional statistical patterns is logically impossible. Even with these presuppositions, that project remains deeply problematic. If there is more than one way to account for the same results, we encounter again the black box character of these findings. Quantitative research can sometimes help to adjudicate between competing theories (which more often than not were developed and given credible standing in qualitative research), but often this hypothesis testing runs into tremendous difficulties because such research must work with crude and ambiguous indicators the context of which is necessarily excluded from the analysis.

All this is not to deny the very considerable value of quantitative research results. It is certainly true – and bears repetition – that established cross-sectional results represent limits with which any genetic, causal explanation has to be reconcilable. This must be added to the obvious and powerful argument that cross-sectional studies – the prime case of available large-scale quantitative

work – reduce even if they do not fully avoid the perennial problem of macro-social research that the number of cases is small and the number of potentially relevant variables large. This remains a major difficulty of the comparative historical strategy, a difficulty put into perspective but not eliminated by the arguments just developed.

A methodological strategy outlined

We can now describe the design of our own project. We will employ a strategy that takes the results of cross-national studies seriously but gives more weight to comparative historical research. It will be informed by a theoretical framework that builds on past research and theoretical argument and focuses attention on structural constraints as well as on process and decision. We want to develop a theoretically adequate account of the causal conditions of democracy that is sensitive to the insights of comparative historical research and capable of explaining the persistent statistical relationship between development and democracy. This account is to be further tested and developed in a series of comparative investigations that seek to combine a relatively large number of cases with qualitatively adequate information on each case. The cases and comparisons are chosen so as to elucidate critical questions about the relationship between development and democracy. We are confident that this combination of theory and research strategies will render the implications of both comparative historical findings *and* cross-national quantitative results more far-reaching.

Our strategy will be the strategy of “analytic induction,” a strategy that can be observed in practical use in several of the comparative historical works reviewed.¹⁴ It breaks with the conventional view that research based on one or a few cases can at best stimulate some hypotheses, while only research on a large number of cases can test them. In this view, case studies – even careful comparative case studies – are irrelevant for the validation of theoretical ideas. They belong to the “context of discovery” rather than the “context of validation” – along with anything else that might stimulate intelligent ideas, from reading novels and philosophical treatises to the enjoyment of food, wine and bright conversation. Yet this radical separation of validation from an essentially arbitrary process of “discovery” is manifestly at odds with the ways we come to reasonably reliable knowledge in everyday life or to historical knowledge that transcends the single case at hand and can be used in historical explanation.

Analytic induction employs in a self-conscious and disciplined way the same strategies we see used in everyday life and in sophisticated historical explanation. Yet it has a more explicitly analytic orientation. It begins with thoroughly reflected analytic concerns and then seeks to move from the understanding of one or a few cases to potentially generalizable theoretical insights capable of explaining the problematic features of each case. These theoretical generalizations are then tested and retested in other detailed case analyses.

Committed to theoretical explanation and generalization, analytic induction builds its arguments from the understanding of individual histories. The complex features of successive cases – with each factor remaining embedded in its historical context and therefore more adequately interpretable – serve as empirical “road blocks” that obstruct arbitrary speculative theorizing. In the overall process of theory building, they are the logical equivalent of the standardized coefficients relating a few selected variables in large-scale quantitative research.

The speculative element, and even arbitrariness, can never be fully eliminated from such case-based theory building. But neither does the opposite strategy, quantitative cross-national research, ever really lose its black box character when it seeks to account for its findings. In our own analysis, we will include as many cases as possible in the same analytically inductive project. In addition, building as we do on the results of both research traditions, we incorporate the empirical generalizations of quantitative cross-national studies into the premises of our own project.

A critical feature of successful analytically inductive research is the initial theoretical reflection. This may take the form of an explicitly developed theoretical framework of concepts, questions, guiding ideas and hypotheses. Yet even if the theoretical foundation is not announced with special fanfare, we can usually identify it with little difficulty. Barrington Moore, for instance, clearly worked with a consistent conceptual grid centered on economic change, the state, and social classes (and especially rural social classes); he used such ideas as the long-term consequences of past conflicts and developments as orientations for all his case analyses; he asked of each case a set of theoretically grounded questions: about the relative strength of the major historical actors and about their pacts and conflicts; and he deployed certain hypotheses – for instance about the chances of revolutionary collective action of peasants – repeatedly as he then turned to the main task: the case-by-case analyses from which he arrived at the three models of political routes into the modern world.

In her justly famous critique of “Origins,” Skocpol, a student and critic of Moore, made these intellectual structures visible and subjected them to a searching evaluation. In her own book on social revolutions (Skocpol 1979), she begins with a critical assessment of alternative theoretical approaches and in effect constructs a full-scale theoretical framework that insists on a structural rather than voluntarist explanation of revolutions, on the salience of international and world-historical contexts, and on the potentially autonomous role of the state. It is with this set of concepts and theoretical premises that she then enters the analysis of the French, the Russian, and the Chinese revolutions as well the non-revolutionary developments in Britain, Prussia/Germany, and Japan.

Such a theoretical foundation of analytically inductive research has not only the function of stating explicitly which questions are asked, how they are framed conceptually, and what the theoretical premises of the analysis are. By giving reasons – preferably empirically grounded reasons – for these decisions and premises, it establishes continuity with earlier scholarship. It is critical to fully

appreciate this point, because here lies one reason why the credibility of analytic induction is far greater than one could possibly justify with the few cases studied. As in everyday life we can gain powerful insights from a few encounters because these are assessed against the experience of a life-time, so the theoretical framework – when informed by previous thought and research – provides the background against which the picture of the cases studied yields more telling results. To put it slightly differently, a carefully developed theoretical foundation also eases the thorny problems of any macrosocial analysis that derive from the small number of cases; for it taps the results of earlier inquiries.¹⁵

The theoretical framework does not represent unchangeable assumptions. It does not constitute a “metatheory” in the sense of a set of premises upon which the validity of any finding is contingent. True, any theoretical framework, whether explicitly recognized or not, structures analytic attention and thus is more open to some findings than others. But we certainly do not wish to claim for the framework developed below a privileged status by which our findings would be protected from criticism that is based on other premises. Developing our theoretical framework in self-conscious detail should in fact make it easier to identify possible blind spots in the subsequent analyses.

The theoretical framework, once developed on the basis of earlier research and argument, then informs the comparative case investigations, and it will in turn be specified and modified through these analyses. The result is, on the one hand, a set of historical cases accounted for with a coherent theory and, on the other, a set of propositions about the conditions of democracy that have been progressively modified and are consistent with the facts of the cases examined as well as with the preceding research taken into account. We will develop our own theoretical framework for the study of democracy in the next chapter before we turn to three comparative analyses – of South America and Mexico, Central America, and the Caribbean, and advanced capitalist societies – in the main body of this volume.¹⁶

Our case comparisons are far-flung, stretching to the limits what can be done by comparative case analysis, but they are not exhaustive. The case selection – while it inevitably derives in part from the particularity of our intellectual journeys – seeks to accomplish specific analytic purposes. The set of cases examined, focusing on advanced capitalist societies, Latin America and the Caribbean, represents the areas with the most extensive democratic experience. At the same time, there are many examples of stable non-democratic regimes as well as of breakdowns of democratic political systems that can be analyzed comparatively side by side with instances of democratization and stable democratic rule, giving ample opportunity to use both John Stuart Mill’s “method of agreement” as well as his “indirect method of difference.”¹⁷ The advanced core countries and South America also offer long stretches of recorded and analyzed history in which the question of democracy was a live issue. They thus give us the chance to explore the conditions of democracy in some historical depth.

The chapter on the advanced capitalist societies takes as its central problems a comparative review of democratization processes and the question of which

democracies broke down in the interwar period and which did not. In taking on the latter question, it directly confronts Moore's analysis on some of the same cases which he studied. Since European democratization has been studied most extensively, it is most directly reflected in our theoretical framework. This gives the chapter on South America a special significance for the further development of our theoretical account of democracy and development. South America is also of special interest, because political independence here came earlier than in other parts of the Third World and liberal ideas had a strong political appeal in this area during the nineteenth century, while the fate of democracy was very different from the liberal centers of Europe. This gives an opportunity to explore the relevance of factors that could not be studied in a more limited set of comparisons. The chapter on Central America and the Caribbean, finally, analyses a startling contrast between the Spanish- and the English-speaking countries; yet it comes to conclusions quite different from a simplistic explanation in terms of the difference in cultural heritage.