earlier counterparts. The basic problem is that many of the theoreticians have lost the capacity to bring into focus the important fine-grained detail while some area specialists only seem to have the capacity to focus narrowly and myopically upon that detail. The future seems to require the development of approaches which proceed coherently and rigorously with the comparison of limited numbers of political systems. In the process, the American political system must be seen as an important and integral case within the laboratory of study of comparativists.

In the end, the consensus of the participants (and audience) attending this roundtable was that comparative politics is alive and well. Led by those who study Latin America, new concepts and approaches are constantly being born. The revolution that marked the field in the 1950s and 1960s has quietly institutionalized itself. An important reason for the relatively negative image of comparative politics in the discipline in recent years rests in the self-criticism engaged in by scholars of comparative politics themselves. This self-criticism is in fact a healthy sign and one that promises continuing breakthroughs and transformations in the field in the years ahead.

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Area studies and comparative political analysis are inextricably intertwined with one another. The experiences of nation-states across the world provide the material and substance for analysis. Methodological tools and theoretical approaches must have data to organize and interpret. This is the stuff of the area specialist. Increasingly, the tools of the area specialist and the theoretician are found in the kits of the leading scholars of comparative politics. And these scholars must be in continuing communication with one another across countries, cultures, areas, and methodological approaches. □

Internal vs. External Factors in Political Development: An Evaluation of Recent Historical Research

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Has recent historical research left any role for *domestic* causation in political development? That subversive question was addressed, and answered, rather differently by David Abraham of Princeton University, Gabriel Almond of Stanford University, David Collier of the University of California, Berkeley, and Peter Katzenstein of Cornell University in a Saturday morning roundtable.

The historiography at issue, I suggested at the outset, seemed to fall into three broad categories: (a) the *dependency* debate and its echoes (including world-systems theory and the bureaucratic-authoritarian model); (b) investigations of the rise, form, and strength of the *modern nation-states,* including those by Tilly, North and Thomas, Skocpol, Anderson, and now Rasler and Thompson; and (c) work on the impact of *trade,* which comprises not only the contributions of Keohane, Krasner, Cameron, Gourevitch, and Katzenstein, but of a small army of recent historians of Imperial and Weimar Germany: Wehler, Winkler, Boehme, Feldman, Eley, Maier, and Abraham. Within these literatures, moreover, the question of external influence in five broad areas of development has emerged as crucial: (1) state strength; (2) (geographical) state size; (3) the strength and intransigence of Right; (4) styles of social and political decision; and (5) susceptibility to authoritarianism.

Almond, summarizing the draft of a large review essay that he had circulated well in advance of the session, denied that the new work represented any radical departure. Such earlier historians and social scientists as Seeley, Hintze, Gerschenkson, Hirschman, Rosenau, Eckstein, and Lijphart—not to mention Almond, Flanagan, and Mundt had amply recognized the importance of external factors, often in a clearer and more convincing way (and here Hintze's work deserved par-

https://doi.org/10.1017/S0030826900624827 Published online by Cambridge University Press

ticular praise) than in some of more recent contributions. At the same time, one must allow that the recent efforts—e.g., those of Tilly, Gourevitch, Katzenstein, Tony Smith, Kahler, and Gereffi—represent "a record of substantial accomplishment, and even greater promise."

Collier maintained that the popularity of external explanation has peaked, at least so far as Latin American research was concerned. It has now been demonstrated, for example, that the tendency toward "populist" regimes antedated the trade crisis and the growth of import-substituting industry in the 1930s. Dependency theory can hardly explain U.S. development, with its escape from dependency. Whatever inclination toward monocausal external causation might previously have prevailed—and he emphasized that O'Donnell's work in particular had been far subtler than that—it has now yielded to a more nuanced approach, in which external factors are seen as intervening variables of uncertain weight.

To Katzenstein, the answer to the question of "internal vs. external causation" was obvious: "It is both." International vulnerability, or more accurately leaders' *perceptions* of vulnerability, is a variable that can not be ignored; but demonstrably states' responses to those perceptions has differed, depending on such internal factors as the strength of the traditional Right and the quality of domestic leadership. To him this issue is part of a much larger one, namely that of determinism vs. voluntarism.

Abraham proposed to confine himself to a single, if plainly central, issue of the debate: whether the liberal, capitalist, democratic form of rule can survive only in a congenial international environment. Recent historiography on Imperial and Weimar Germany suggested overwhelmingly that the answer was "yes," but Abraham has increasingly entertained doubts. Some of the crucial intervening factors can be linked only tenuously to the international environment; and it is hard to distinguish Weimar convincingly from the small-state cases that Peter Katzenstein had studied in the same period.

Why, for example, had a "Red-Green" coalition proven impossible in Germany, particularly in the 1930s? That had to do with workers' having historically defined themselves as consumers, with the unions' links to progressive capital, with the continuing strength of the Right and with the Right's dominance of agriculture; yet none of those factors had really been determined by external events. (Even the strength of the Right, we now see, was no automatic consequence of the tariff decision of 1879.) Similarly, the implacable hostility of the German petite bourgeoisie to labor was crucial, but crucially affected by the split between SPD and KPD and by the strength of Communism in Germany—again, something that no student has been able to tie convincingly even to structural variables, let alone to international factors.

The students of the state are "not a school but a church."

Finally, was Weimar Germany's external situation so very different from that painted by Katzenstein for the smaller European states in the 1920s? Surely Weimar's leaders all saw the Republic as vulnerable internationally; and the economy depended extremely on trade, exporting fully one-third of industrial production. Why then had the outcome differed so tragically?

In the course of these discussions two important subsidiary issues surfaced. Almond doubted the wisdom of (in Skocpol's phrase) "bringing the state back in." Surely a major service of the newer historiography has been to disaggregate the "black box" of the state, to see its actions as products of external and internal factors. Why did some adherents of the newer school want now to re-introduce this "opaque, almost metaphysical entity"? Katzenstein responded that the students of the state are "not a school but a church," albeit quite a broad one; state-centric analysis is only "a way of framing a question."

Katzenstein and Abraham both ad-

https://doi.org/10.1017/S0030826900624827 Published online by Cambridge University Press

dressed political scientists' use of history. For Katzenstein, the lessons of his own research were frankly (a) to bastardize history recklessly, even as Gerschenkron confessedly did; and (b) at all costs to avoid the "dirty work" of the historians, especially archival investigation, and to rely on secondary sources. Abraham found precisely this "bastardization" problematic; he saw in Katzenstein's new book a functionalism that might be difficult to reconcile with the broader European evidence.

Dependency theory is "dead in the water."

Richard Sklar of the University of California, Los Angeles, from the floor, wondered where all of this left us. Dependency theory is "dead in the water"; but what remained? What precise connections between the external and the internal can be specified? I pushed the question further; can anything still be assigned unambiguously to domestic causes? Almond, responding, largely concurred in the negative assessment of dependency theory. That he did not regard internal causation as unimportant can be inferred from other sections of his paper in which he discussed recent work on the domestic sources of foreign policy. But the precise weights to be assigned to internal and external forces are a matter for further historical, and above all for comparative, inquiry. □

Social Protest Movements: What Sociology Can Teach Us

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The social protest movements roundtable provided an opportunity for a cross-disciplinary exchange between political scientists and sociologists sharing similar research interests. Although the political science literature of the 1968-1978 period witnessed a lively and productive use of the E. E. Schattschneider tradition of examining nonelectoral forms of political activism and protest, in more recent years sociology has generated a richer and more extensive literature concerning protest movements. As I noted in two preliminary memos to interested colleagues and as several panel members reiterated at the session, the scholarly literatures in the two disciplines have to date developed in relative isolation from each other.

The New Orleans roundtable opened with University of Missouri sociologist J. Craig Jenkins providing an excellent overview of the theoretical and conceptual developments that have occurred in sociology's social movements literature since the early 1970s. A new paradigm, generally known as "resource mobilization" theory, was introduced in 1973 through the works of Anthony Oberschall and John McCarthy and Mayer Zald. Resource mobilization challenged the previously prevailing assumption that protest movements could be explained simply by reference to the psychological needs and "discontent" of mass participants. Instead, "RM" theory presumed that protesters were rational rather than irrational actors, and focused upon the organizations and resources available to potential protest participants. In succeeding years, "RM" theory increasingly split into two competing perspectives, one of which maintained an organizational focus and the other developing what is sometimes called a "political process" emphasis. The first approach increasingly focused on the appearance of "professional social movement organizations," or "SMOs," groups that had fulltime, paid staffs, cultivated "conscience constituencies," possessed largely "paper" memberships, and concentrated upon manipulating the mass media so as to influence public opinion and hopefully generate elite responses and policy changes.

Jenkins, author of the newly published *Politics of Insurgency* (Columbia University Press), explained that the "political process" approach has given primacy to indigenous protest mobilization while also acknowledging the importance of reactive external support from movement