



On Debating and Falsifying Theories of Collective Violence

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their contents in learned behavior patterns and stored memories)—are *a* and perhaps *the* crucial variable in producing violence. If environmental circumstances are sufficiently frustrating to basic expectations, which are organically rooted in the minds of men, people will turn to violence. If government is blamed for the frustrations, people will turn to political violence. Without environmentally induced frustration, there could be no violent response; without being assessed and acted on by the minds of men, environmental inputs would be of no political significance, let alone any other significance. While not sufficient to produce any human action, mental processes are a neglected but necessary element of all action.

The power-struggle theory and the data do not predict French civil violence before, during, or after the 131-year period. If we do not get to the causes of such violence, they may produce worldwide revolution or worldwide repression. In that case, there will be arrests and power struggles, but national elections will be meaningless.

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ON DEBATING AND FALSIFYING THEORIES OF COLLECTIVE VIOLENCE

Mr. Davies' objections to our article have little connection to the language or the logic of what we wrote. The 1972 research report which began this debate offered the work of Feierabend and Feirabend, Gurr and Davies as examples of arguments predicting rising levels of collective violence within a given population as a consequence of widening discrepancies between that same population's expectations and achievements. Although various members of our research group (e.g. Polen 1972, Tilly and Rule forthcoming; Tilly, forthcoming) have deliberately examined Davies' J-curve formulation elsewhere, the report in question did not concentrate on revolutions or restrict itself to the special J-curve formulation of the expectation-achievement theory. It offered measures of conflict, expectations and achievements which were arguably consistent with the measurement practices of major authors of expectation-achievement arguments, plus further measures representing variables in an alternative power-struggle formulation. It applied those measures to France as a whole for 131

single years from 1830 to 1960. It estimated and tested a series of simple models representing the expectation-achievement and power-struggle arguments. It concluded that the expectation-achievement models had almost no explanatory power for this series, while the power-struggle models worked well enough to merit further exploration.¹

The article's concentration on a long whole-country series instead of particular revolutions did not make the work irrelevant to Davies' arguments. For Davies himself had applied his scheme to student protests, draft riots, the protests of American Blacks in the 1960's, not to mention the Pullman Strike, the Dorr Rebellion, plus many other conflicts of all shapes and sizes. In any case, it is simply incorrect that France experienced no "nation-wide collective violence" during the 131 years under examination. By the conventional definition of a forcible transfer of control over the state, France went through revolutions in 1830, 1848 and 1870, with 1851 and 1958 debatable cases. If we wanted to add those years in which widespread, simultaneous collective violence and/or extensive multiple sovereignty occurred, 1851 and 1958 would certainly qualify; 1871, 1936, 1944 and 1947 would be strong candidates as well.

The year 1968 is, as Davies indicates, a good one for further trials of expectation-achievement and power-struggle arguments; we are now collecting our data on the 1960's for just such purposes. As for the French Revolution of 1789, we have not collected the relevant time series for the eighteenth century. But that is where our thinking on the

whole subject began (Tilly, 1963, 1964a, 1964b). We are quite ready to join in debate on the origins of the eighteenth-century revolution if that becomes necessary. We are sure that a J-curve is not the answer.

Davies' admonition to match the measures of expectations and achievements with the population whose behavior we are seeking to explain is more interesting and substantial. We only wish he had followed it in his own work. There, he is quite willing to reason from aggregate observations concerning Russia or Egypt as a whole to revolutions in which only a small portion of the population participated directly in collective violence or the transfer of power (Davies 1962, 1969, 1971). We have, in fact, often applied our power-struggle arguments and measurements to smaller populations and to localities immediately involved in conflict (e.g. Aminzade 1973, Lees and Tilly, forthcoming; Shorter and Tilly 1974). But the expectation-achievement literature, including published work by James Davies, provides plenty of warrant for testing the arguments on national aggregates.

What is more, that same literature generally assumes—nay, insists—that negative cases are just as important as positive ones. The usual cross-sectional test of an expectation-achievement argument is an attempt to show that for a large number of populations the smaller the gap between expectations and achievements the lower the level of conflict. In proposing that we turn from general efforts to account for variation in the level of collective violence to fitting the J-curve to particular cases of rebellion, Davies turns his back on most of the authorities he cites. He also abandons most of his chances to formulate falsifiable (hence sociologically interesting) arguments.

The arguments need not, of course, apply to national populations. They may apply instead to individual rebellions or groups of rebels. But they will hardly be falsifiable if they do not a) tell us in advance which features of the group's experience should improve in the long run, then decline in the short run, if the J-curve is to have its revolutionary effects, b) permit us to use as evidence against the arguments those situations in which the same appropriate sequence of improvement-then-decline occurs without revolutionary consequences. In Davies' procedure, the Chinese Revolution of 1949 and the Hungarian Revolt of 1956 become not negative cases, but cases for which data were "unavailable." In ours, Gurr's and the Feierabends' procedures, cases of large gap and little collective violence count against the argument, as do cases of small gap and much

¹ Actually, the data were somewhat more favorable to the power-struggle argument than the article said. Through a minor computing error, Tilly estimated an equation for 1886-1939 as:

$$\sqrt{\text{participants}} = 4838.6 + 6.60 (\text{excess arrests}) - .002 (\text{national budget}) - .027 (\text{jail-days}) + 30.97 (\text{elections}) - 106.58 (\text{time}) + .632 (\text{time}^2) \\ R^2 = .5035 \text{ s.e.} = 99.065 \text{ F}_6, 38 = 6.4238 \text{ p} < .0001$$

The correct estimate, however, was:

$$\sqrt{\text{participants}} = 5004.0 + .63 (\text{excess arrests}) - .004 (\text{national budget}) - .040 (\text{jail-days}) + 32.12 (\text{elections}) - 106.72 (\text{time}) + .633 (\text{time}^2) \\ R^2 = .6548 \text{ s.e.} = 102.67 \text{ F}_6, 43 = 5.3781 \text{ p} < .0003$$

In the corrected equation, the coefficients for national budget, jail-days and elections are still in the expected directions, but larger than before ($p = .01, .02$ and $.37$, respectively); while the previously embarrassing positive coefficient for excess arrests has declined practically to zero ($p = .86$). We are grateful to Alvin Jacobson for asking the questions which led us to reexamine our estimates.

collective violence. Davies reads our "struggles for political power" argument with surprising inventiveness. He imagines that excess arrests and man-days of detention are indicators of collective violence, and that our election-year variable is the (dependent) measure of power struggle. In the language we thought we were writing, the regression equations and accompanying text say that 1) *participants* in disturbances is the measure of collective violence (and the dependent variable) throughout the analysis; 2) excess arrests and man-days of detention are measures of repression; and 3) election year is an indicator of national political activity. We argue that such activity facilitates mobilization, contention for power and, consequently, collective violence. Davies' critique attacks some other argument than our own.²

Davies himself argues only that sufficient frustration of individual citizens generates collective violence. He never systematically considers the actions of authorities, or other power holders, in his scheme; that is a surprising omission, considering how often repressive forces appear in Davies' descriptive accounts of particular events. Nor does Davies ever specify how *individual* deprivations and frustrations translate into *collective* action. Although our principal arguments bear direct-

ly on these omissions in Davies' theory, his commentary ignores them.

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- ²Davies raises other issues which are less important to his argument, and to ours: 1) He objects to our indicators of hardship. We consider these measures elsewhere (Snyder and Tilly 1973) and show that alternative representations (e.g. real wages) yield equivalent results and interpretations. 2) He claims that our procedure of sampling three random months per year for 1861-1929 probably underestimates localized events such as the Paris Commune. In the same breath, however, Davies admits that the aggregate data show a high peak around 1871; that year is, in fact, the fourth largest in the participants series. We prefer to rely on the data. We have, however, discovered since writing the 1972 report that our initial enumeration of events in the period 1861-1929 was inefficient, with the result that the estimates for the period as a whole are too low relative to those for 1830-1860 and 1930-1960. The verification we have done so far indicates that the underenumeration is spread across the whole period rather than concentrated in any particular years. When the corrected estimates are available (as they should be some time in 1974) we intend to re-estimate our equations and to report any discrepancies with results reported earlier. Given the nature of the statistical analysis we have performed, it is unlikely that any but the time variables will be seriously affected by the corrections.
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ITEMS (*Continued*)

investigating the sexual identity and interpersonal relations of bisexuals. KATHRYN GROVES CARSSOW, JANET HALL, BARBARA HAWKINS, RONALD HOFFMAN, ERNEST ISHEM, CAROLL PALMER MAURER, DANA SPENS, JOHN TAYLOR, and D. LEE ZIMMERMAN collaborated in the study reported in this issue while undergraduates at the University of Washington.

■ SAM D. SIEBER ("Toward a Theory of Role Accumulation") is on extended leave from Columbia University. The basic version of his paper was written while he was Senior Research Associate at the Bureau of Applied Social Research (and reprints will be available through the Bureau). His research interests center on the sociology of religion, the organization of schools, the diffusion of innovations, and methodology. Presently he is writing and consulting while living on an uninhabited island in the Caribbean. He describes himself as an arch anti-role accumulator.

■ The issue ends with two sociology of science articles and a lively exchange concerning a previously published article. IAN I. MITROFF ("Norms and Counter-Norms in a Select Group of the Apollo Moon Scientists") is Associate Professor in the Interdisciplinary Doctoral Program in Information Science at the University of Pittsburgh. His research interests center around the development of a dialectical theory of decision making for policy analysis and the design of information systems, and development of an interdisciplinary, integrative philosophy, sociology, and psychology of science by means of substantive case studies. PAUL D. ALLISON and JOHN A. STEWART ("Productivity Differences Among Scientists") are doctoral candidates at the University of Wisconsin, Madison. Allison is studying the inter-organizational mobility of academic scientists, and trends in the religious behavior of university students. Stewart is investigating social processes in contemporary scientific revolutions (with Warren O. Hagstrom).

On to Canada. I hope to see many of you there!

J.F.S.