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# HARDSHIP AND COLLECTIVE VIOLENCE IN FRANCE, 1830 TO 1960

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*We challenge the standard argument which treats collective violence as an expression of the dissatisfactions felt by populations experiencing hardship after periods of relative well-being. We propose an alternative explanation in which struggles for political power are the central features. Time-series analyses of year-to-year fluctuations of collective violence in France from 1830 through 1960 fail to yield significant results for a variety of models designed to represent major arguments in the recent literature stressing the effects of short-term hardship. Similar analyses representing the effects of governmental repression and of national political activity yield results corresponding to our expectations. So far we have not been able to incorporate adequate measurements of the other major power-struggle variables into the time-series analysis. But we take the results of this preliminary investigation as a warrant to continue in that direction.*

CONSIDERING the scattered, unsystematic and contradictory character of the available evidence, the idea that hardship causes collective violence has gained surprising currency. In recent years few scholars have propounded a simple mechanical relationship between the two. Yet at least one variant of the notion has actually gained adherents; that is the explanation of collective violence (and other forms of protest or rebellion, whether violent or not) as a response to a gap between expectations and achievements. That explanation can easily be made true by definition—for example, by letting the violence itself stand as the evidence of unrealized expectations. It can also be made irrefutable but trivial, simply by authorizing an eternal search for one more gap to account for the violence at hand. There is, however, a credible, weighty and sometimes testable form of the argument which reasons from short-run hardship to protest via the accumulation of individual dissatisfactions.

We challenge the entire line of argument. Men do, indeed, often become angry when other people violate their expectations. Under some conditions short-run hardship does, we concede, precipitate rebellion. But we do not think there is any general connection between collective violence and hardship such that an observer could predict one from the other. We doubt that the diverse events

which go by the names of protest, collective behavior, rebellion and violence have anything more in common than the fact that authorities disapprove of them. And we suppose that the principal, immediate causes of collective violence are political: collective violence results from changes in the relations between groups of men and the major concentrations of coercive power in their environments.

This paper says little about the political analysis of collective violence, and much about hardship. Here we seek merely to show that plausible versions of the expectation-achievement argument fail to explain the year-to-year fluctuation in collective violence over an important span of one country's history, while two eminently political variables—the extent of governmental repression and the degree of national political activity—do provide a partial explanation of that fluctuation. Other reports of our work lay out the political analysis more fully, provide some evidence of its validity, and treat a number of alternative arguments not mentioned here.<sup>1</sup> We do not

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<sup>1</sup> This paper reports one part of a continuing study of the effects of large-scale structural change on the character of political conflict in western Europe. The study is being carried on in loose collaboration by a number of scholars at the University of Michigan, the University of Toronto, the Westfälische Wilhelms-Universität (Münster),

think for a moment that this particular investigation disposes of all possible relationships between collective violence and hardship, or that it comes close to establishing the priority of politics. At most we claim no more than to have lodged enough doubts against the expectation-achievement theories of collective violence to recommend a moratorium on their use as explanations until they have received further tests, and to have provided enough support for a political-process theory to justify investing new efforts in its elaboration and verification.

Lest we be suspected of battling straw men, let us mention a few oft-cited statements which follow the line of argument we reject. James C. Davies begins by speaking about revolutions, but soon extends his formulation to a wide variety of violent events: ". . . revolution is most likely to take place when a prolonged period of rising expectations and rising gratifications is followed by a short period of sharp reversal, during which the gap between expectations and gratifications quickly widens and becomes intolerable. The frustration that develops, when it is intense and widespread in the society, seeks outlets in violent action." (Davies, 1969:547; see also Davies, 1962, 1971). In addition to revolutions in a strict sense of the term, Davies explicitly applies the scheme to draft riots, student protests, the "Black Rebellion of the 1960s" and the Nazi seizure of power. Despite his insistence that the definitive evidence for this argument must come from observations of attitudes, he is willing to use changes in income, education, economic growth, farm productivity and civil rights as indicators of expectations and gratifications. More important

for present purposes, in analyzing the Nazis and several other cases, he offers evidence of rapid economic decline after long expansion to substantiate his argument.

Ivo and Rosalind Feierabend (1966) offer two formulations which are germane to the relationship between hardship and collective violence. First, they argue in essence that the higher the ratio of want formation to want satisfaction, the greater a country's propensity to "instability." In one study, literacy and urbanization represented want formation, GNP, caloric intake, physicians, telephones, newspapers and radios represented want satisfaction, and thirty different domestic conflict measures for 1955 to 1961—ten of them explicitly involving damage to persons or objects, and a number of others implying it—went into the index of instability. Second, they propose that "the faster (the slower) the rate of change in the modernization process within any given society, the higher (the lower) the level of political instability within that society." (Feierabend and Feierabend, 1966:263). In this case, the yearly percent rate of change from 1935 through 1962 in caloric intake, literacy, primary and postprimary education, national income, cost of living, infant mortality, urbanization and radios per thousand population served as indicators of the rapidity of modernization. This time there were two measures of instability: (a) the aggregate index mentioned earlier; (b) the variance of that index over single years from 1955 through 1961. Their formulation differs from Davies', but it clearly permits predictions from fluctuations in economic well-being to levels of collective violence.

Finally, Ted Gurr proposes that ". . . a psychological variable, relative deprivation, is the basic precondition for civil strife of any kind, and that the more widespread and intense deprivation is among members of a population, the greater is the magnitude of strife in one or another form." (Gurr, 1968: 1104; see also Gurr, 1969, 1970). Gurr's models and measurements are more elaborate than those of Davies or the Feierabends. For present purposes, the essential argument is that both persisting and short-term deprivation have direct, positive effects on the magnitude of civil strife, with

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and elsewhere. National Science Foundation grant GS-2674 currently provides the principal financial support for the study. Grants from the Canada Council made earlier phases of the work possible. Recent statements and reports of findings appear in Lees and Tilly, 1972; Lodhi, 1971; Rule and Tilly, 1971; Shorter and Tilly, 1971a, 1971b and 1971c; C. Tilly, 1970a, 1970b, 1972; L. Tilly, 1971a and 1971b; R. Tilly, 1970; R. Tilly and C. Tilly, 1971. We are grateful to Paul Siegel for criticism of an earlier paper by Snyder on the same subject, as well as for spotting a serious error in an earlier draft of this paper, and to Priscilla Cheever, Freddi Greenberg and Glen Jones for assistance in assembling the data.

allowance for the effects of legitimacy, coercive potential and social-structural facilitation. "Persisting deprivation" combines weighted measures of economic discrimination, political discrimination, potential separatism, dependence on private foreign capital, religious cleavages and lack of educational opportunity. "Short-term deprivation" combines declines in foreign trade, inflation, declining rates of growth in GNP, qualitative reports of adverse economic conditions, new restrictions on political participation and representation and new "value-depriving policies of governments." "Magnitude of civil strife" accumulates and weights information about individual conflicts, most of them involving attacks on persons or objects. (We will neglect the complicated measurements of legitimacy, coercive potential and social-structural facilitation, although they raise intriguing and serious methodological problems.) Gurr, too, reasons from short-run hardship to protest via the accumulation of individual dissatisfactions.

Although these investigations are open to serious attack on theoretical, technical and substantive grounds, we will not offer a critical assessment of them here.<sup>2</sup> Our purpose in sketching the three arguments and their implementation is to provide a rationale for our own choice of models and indexes to represent the line of reasoning we wish to challenge. We have taken one critical segment of the expectation-achievement argument, sought to represent it in terms faithful to the usual formulation of that argument, and tried to test it thoroughly against excellent data concerning year-to-year fluctuations in collective violence within one country over a long period of time. In the research reported here, we have not represented "expectations" in any direct or convincing way. We have, instead, inferred changing expectations from fluctuating "achievements" in a manner similar to that sometimes employed by Gurr, Davies and many other advocates of expectation-

achievement explanations of collective violence.

All the data are yearly aggregate measures for France during the period from 1830 through 1960. Our measure of collective violence is the estimated number of participants in disturbances in continental France as a whole. "Disturbances" are continuous interactions involving at least one group of fifty or more persons in the course of which someone seized or damaged persons or objects over resistance.<sup>3</sup> They exclude acts of international war. The disturbances studied consist of every event meeting our criteria detected by trained readers of two national newspapers for each day from 1830 through 1860 and 1930 through 1960, plus each day of a randomly-selected three months per year from 1861 through 1929. Once events qualified in this way, we collected information about them from a wide variety of sources: other newspapers, published court proceedings, annual reviews of politics, French national and departmental archives, secondary historical works, and others. We then recorded a great many characteristics of the disturbances, including estimates of the number of participants, in machine-readable form.<sup>4</sup>

<sup>3</sup> We use the commune-day as our building block. France subdivides into about 38,000 communes. If two or more events meeting our criteria occur in the same commune (in Paris, the same quarter) on the same day, with a reasonable presumption of an overlap of at least ten percent of the participants in the smaller event, we treat them as parts of the same disturbance. Similarly, if qualifying events occur in adjacent communes or on consecutive days and there is a presumption of ten percent overlap, they belong to the same disturbance. By these rules, over nine-tenths of all the disturbances fall within a single commune and a single day. Obviously, this procedure fragments large sequences like the revolution of 1848 into a considerable number of disturbances, and excludes the non-violent days of the revolution from consideration.

<sup>4</sup> We are taking two steps which should make it easier for other scholars to extend, verify and even challenge our own conclusions: (1) extending the time-series files for the 131-year period to include a far larger range of variables; (2) depositing our basic machine-readable files, including the time-series files, with the Inter-University Consortium for Political Research for redistribution; most of the files should be available by early 1973.

<sup>2</sup> Intelligent reviews of the literature appear in Bienen, 1968; Calhoun, 1970; Lupsha, 1970; Nardin, 1971; Nelson, 1970. Most of the papers cited in note 1 also contain detailed criticisms of different segments of the current literature on political conflict and collective violence.

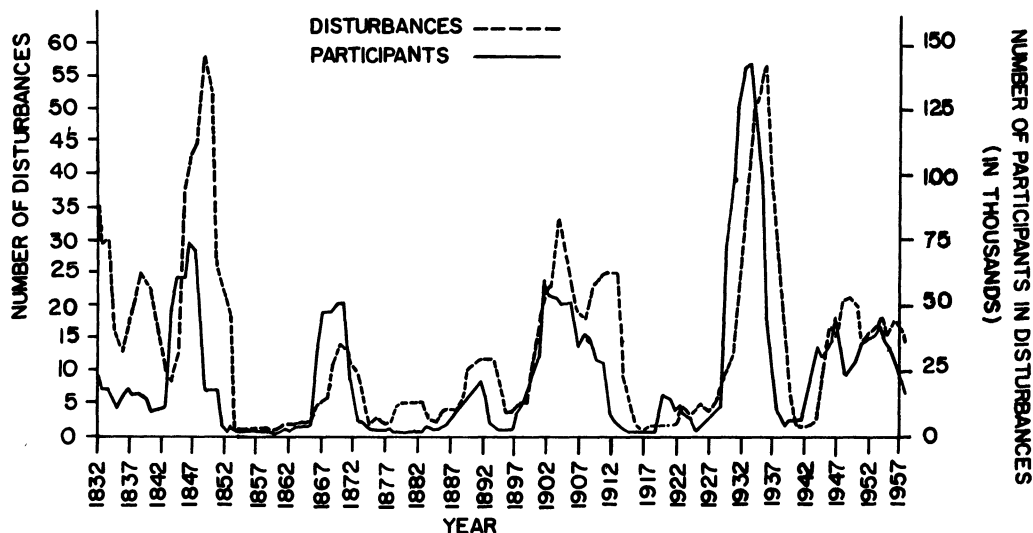


FIGURE 1. DISTURBANCES AND PARTICIPANTS IN DISTURBANCES, 1832-1958 (FIVE-YEAR MOVING AVERAGES).

In order to produce a continuous series over the 131 year period, we have performed two extrapolations which tend to reduce the variance somewhat: (1) we estimated the number of participants in the roughly 6 percent of disturbances where we had insufficient information for a numerical estimate as the mean of all those others in the same year that we *were* able to estimate numerically; (2) we quadrupled our annual figures for the period from 1861 through 1929, in which we had studied only a quarter of all the months. Altogether, then, we are dealing with 1,989 disturbances and an estimated 3.2 million participants.

As one might expect, the number of disturbances and the number of participants vary greatly from one year to another, but vary closely together.<sup>5</sup> Figure 1 represents the numbers of disturbances and of participants in five-year moving averages for easy legibility. (The analysis itself, however, uses single-year data.) As the figure shows, very high levels of collective violence came around the revolutions of 1830 and 1848, at the beginning of the twentieth century and in the mid-1930s, while exceptionally low levels prevailed in the 1850s and during the two World Wars. Sometimes the transition came abruptly. In the extreme case,

there were ninety-three disturbances and some 90,600 participants in 1851, followed by two disturbances and an estimated 950 participants in 1852. Without exception the large, abrupt shifts of this kind mark a major rearrangement of the national structure of political power in France. In 1851-1852, the crucial events were Louis Napoleon's *coup d'état*, the widespread but unsuccessful insurrection it incited, and the installation of a police state under the man who was to become Napoleon III.

Our indicators of hardship and well-being are all economic: (1) an index of food prices, (2) an index of prices of manufactured goods, (3) an index of industrial production.<sup>6</sup> Following the usual practice in expectation-achievement investigations, we take high levels on the first two variables and low levels on the third as indicating

<sup>6</sup> The industrial production index grafts the series in the *Annuaire statistique de la France, résumé rétrospectif, 1966*, p. 561 to the series for 1830 to 1898 in Lévy-Leboyer, 1968, thus covering the years 1830-1913, 1918-1938 and 1942-1960. The food index grafts the wholesale price index of the *Annuaire statistique*, p. 373, for 1830-1860 to the retail price index in Singer-Kérel, 1961: 452-453 for the years 1860-1954. The manufactured goods index grafts the *Annuaire statistique* wholesale price index for "industrial products" in 1830-1860 (p. 373) to the retail index for 1860-1940 and 1949-1954 in Singer-Kérel, 1961: 452-453. In each analysis we used the maximum number of years for which there were data for all the variables in the particular model being tested.

<sup>5</sup> Over the 131 years,  $r = .84$ . There were a mean 15.2 disturbances per year, with a standard deviation of 22.3, a mean 24,198 participants per year, with a standard deviation of 45,641.

hardship for the population as a whole. More precisely, we accept short-run *rises* in prices and *declines* in industrial production as evidence of increasing hardship. The conventional argument, which we adopt for the purposes of this inquiry, is that the population compares current experience with that of the immediate past, and therefore suffers "relative deprivation" when the economy turns down. Davies and Gurr, among many others, use that reasoning explicitly; it also seems consistent with the arguments of the group of cross-sectional studies represented here by the work of the Feierabends.

We make our test of this argument in a roundabout way: not by constructing a single model and rejecting the argument if the model fails, but by testing a *set* of models incorporating the relative deprivation argument. If none of these models fits, we can safely reject (for our data) the usual versions of the argument. Since our data are measured over time, we have employed econometric time-series techniques. Each of these series—the participants in disturbances and the economic indicators—manifests a trend verified by the non-parametric techniques described in Malinvaud (1966: 390–392). We "detrended" the series using the method of first differences ( $\Delta X = X_t - X_{t-1}$ ), for these reasons: (1) only complicated and intuitively meaningless polynomial expressions could account for the trend in these relatively long time-series; (2) detrending using first differences reduces the serial correlation of the residuals; and (3) most importantly, detrending using first differences rather than fitting a polynomial function of time is more faithful to current theories of relative deprivation. By including a polynomial expression for trend, we would in fact be treating as "deprivation years" any years (and only those years) where, for example, observations on the price index were above the predicted value. So, in effect, our deprivation measure would depend on the magnitude (and more importantly, on the sign) of the difference from the trend expression, but not necessarily on the difference from the preceding year (the measure which the theory implies). The method of first differences, by measuring relative deprivation as the change from one year to the next, erases this problem.

Our first model is one which specifies all of the economic deprivation predictors as independent variables, of the following form:

$$\Delta Z_t = b_0 + b_1 \Delta W_t + b_2 \Delta X_t + b_3 \Delta Y_t + u(t);$$

where

$$\Delta Z_t, \dots, \Delta Y_t = Z_t - Z_{t-1}, \dots, Y_t - Y_{t-1}.$$

Z = number of participants in disturbances

W = price of food index

X = price of manufactured goods index

Y = index of industrial production

t = time

u = error or residual term

These letter-variable combinations will remain constant throughout this section.

We compute the regression and correlations:

$$\text{Regression: } \Delta Z_t = 622.5 + 13.09 \Delta W_t + 0.75 \Delta X_t - 626.51 \Delta Y_t + u(t);$$

$$\text{Multiple Correlation Coefficient: } .0270$$

$$F_{3,107} = 0.3539, p < 0.55$$

$$\text{Coefficient of Determination: } .0007$$

$$\text{Standard Error of Estimate: } 57,791.4$$

Neither the analysis of variance for the multiple correlation, nor the coefficients of any of the indicators of deprivation are significantly different from zero. (We use a critical level of  $p = .05$  throughout this paper.) Furthermore, the serial correlation of the residuals is quite large (.5304), despite the fact that the first differences method is designed to reduce this correlation. By not being able to account for the serial correlation with this model, we are in effect saying that there are other (non-economic) variables which can explain some of the systematic variation left in the residuals.

A model is more than the sum of its parts. The magnitude (and even the sign) of partial slopes and the significance level often change with the inclusion or exclusion of certain independent variables. Therefore, it is possible that certain of these deprivation indicators could predict collective violence separately. With this in mind, we construct the following models, again using first differences, to ascertain whether or not any single deprivation indicator can predict signifi-

cantly the number of participants in disturbances:

- (1)  $\Delta Z_t = b_0' + b_1' \Delta W_t + u'(t)$ ;
- (2)  $\Delta Z_t = b_0'' + b_1'' \Delta X_t + u''(t)$ ;
- (3)  $\Delta Z_t = b_0''' + b_1''' \Delta Y_t + u'''(t)$ .

None of the analyses of variance for the multiple correlations in any of these models is significantly different from zero; nor are any of the coefficients significantly different from zero. In all these cases serial correlation of the residuals is relatively high (at least 0.36), indicating once again the existence of variables accounting for systematic variation which we have not yet taken into account.

On another tack, we can learn a great deal about the relationships between the indicators of hardship and our indicator of the magnitude of collective violence by looking at their intercorrelations over time. Based on the writings of the proponents of the hardship-violence linkage, we would predict the following about these relationships: (1) The correlation between changes in the price of food index and changes in the number of participants should be positive and the time lag zero or one year, unless the price of food rises rapidly over a period of years,

in which case there may be a cumulative effect. (2) The lag between changes in the price of manufactured goods and changes in the magnitude of violence might be anywhere from one to five years, since manufactured goods are not such an immediate necessity as food; the expected sign of the correlation is positive. (3) The lag in the correlation between changes in the index of industrial production (as a crude measure of business conditions) and changes in the number of participants can't be predicted in advance.

What in fact do the data look like (see Table 1)?<sup>7</sup> The correlation between changes in the price of food index and changes in the number of participants in disturbances is virtually zero for all lags/leads within the boundaries considered. Similarly, the correlation between changes in the price of manufactured goods index and changes in the number of participants is close to zero for all lags/leads except the unlagged correlation (lag zero:  $r = .1067$ ).<sup>8</sup> We have already

<sup>7</sup> Appendix tables 2 and 3 contain the full set of unlagged correlations for variables used in our analyses.

<sup>8</sup> Changes in the two price indexes are too closely associated with each other ( $r = +.91$ ) to

Table 1. Intercorrelations Over Time of Economic Deprivation Indicators and Number or Participants in Disturbances.

	Time (Lag/Lead)	$\Delta W_t$ Price of Food Index	$\Delta X_t$ Price of Man. Goods Index	$\Delta Y_t$ Index of Indus. Product.
Number of Participants in Disturbances	8-	.0115	.0016	.0967
	7-	.0008	.0015	.0687
	6-	-.0012	.0005	.0566
	5-	.0066	-.0042	.0292
	4-	-.0160	.0526	.1469
	3-	.0021	-.0211	-.1159
	2-	-.0377	.0014	-.1374
	1-	.0107	-.0496	.0757
	0	-.0270	.1067	-.0703
	1+	-.0165	.0020	-.0103
	2+	-.0640	.0000	.0170
	3+	-.0444	.0019	.0276
	4+	.0164	.0055	-.0348
	5+	.0116	-.0002	-.0433
	6+	-.0043	.0035	.0415
	7+	.0007	-.0012	.0140
8+	-.0011	.0010	-.0854	

Minus signs following entries in the "time" column indicate that the column variables ( $\Delta W_t$ ,  $\Delta X_t$ ,  $\Delta Y_t$ ) are lagged on (precede) the participants variable by the designated number of years. A plus sign following the time entry designates a lead.

Data from the years 1830-1912, 1920-1938, 1949-1954: 108 observations.

tested a model which attempted to predict the yearly difference in the number of participants in disturbances from the yearly difference in the price index of manufactured goods at lag zero, and found no significant relationship between the two variables.

The correlation between changes in the index of industrial production and changes in the number of participants is greatest in the expected (negative) direction for a lag of two years ( $r = -.1374$ ). Therefore we construct the following equation, taking account of what appears to be a two year lag in the relationship:  $\Delta Z_t = b_0 + b_1 \Delta Y_{t-2} + u(t)$ . Even then, neither the analysis of variance for the multiple correlation nor the coefficient of the independent variable is significantly different from zero (for both,  $p = .16$ ).

This last procedure has been quite artificial; where intercorrelations over time seemed to be relatively large and in the direction expected by the relative deprivation theory, we have constructed models which, by choosing the time lag with the highest correlation, have given the economic deprivation indicators the best possible chance of predicting the magnitude of collective violence. And yet, *none* of the models we have tested has yielded a relationship between our economic deprivation indicators and our indicator of the magnitude of collective violence significantly different from zero. The evidence is so clear it hardly needs laying out. The theories of a linkage between relative deprivation and collective violence propounded by Davies, Gurr and many others can safely be rejected for these data.

The alternative theories which we favor treat collective violence as a by-product of struggles for political power. We will not lay out our arguments in detail here, since we are not yet in a position to represent all the variables involved in time-series format. The central ideas are simple. Within any substantial population there is likely to be at least one structure whose members control the major means of coercion effective in that

population; to the extent that the structure is formal and differentiated, we call it a government. Within some specified period, a number of groups varying in coherence and strength collectively apply resources to influence the actions of the government; they are contenders for power.

No group contends for power without having mobilized—having acquired collective control over resources—and mobilization is a relatively rare and difficult process. Some of the contenders have routine means of influencing the government, of influencing each other, and of exerting collective control over which groups belong to their number; we consider them members of the polity. Groups enter and leave a polity through a continuous process of testing: meeting or failing to meet criteria over which the existing members of the polity exercise control. Among those criteria, the ability to mobilize extensive resources—especially manpower and coercive resources—is almost always prominent. Occasionally a revolution fragments the polity for a time; more rarely, the revolution produces a new polity by replacing some or all of the existing members, or by constituting a new government.

Collective violence, then, tends to occur when one group lays claim to a set of resources, and at least one other group resists that claim. Existing members of the polity frequently resist via agencies of the government, especially troops, police and other specialists in coercion. Where governments have substantial force at their disposal, in fact, those specialists ordinarily do the major part of the damaging and seizing which constitutes the collective violence. High levels of governmental repression, however, increase the costs of collective action. They thereby decrease the likelihood that groups will mobilize and make claims which are unacceptable to existing members of the polity. Repression thus reduces the extent of collective violence.

These extremely general statements say nothing about the conditions under which different kinds of groups mobilize and contend for power, what sorts of claims precipitate violent resistance, how the form of government matters, and so on through much of the agenda set for us by the study of struggles for power. They nevertheless

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permit us to distinguish between their effects with any confidence. However, dropping one or the other from the equations presented here does not appreciably affect the results of the multiple regressions.



point away from expectation-achievement accounts of collective violence, except to the extent that the gap between expectations and achievements for the population as a whole predicts the extent of mobilization, repression and contention for power. We do not think that extent is great; the analysis we have just reported confirms us in that belief. Our argument gives priority to conditions which facilitate or hinder mobilization, which change the frequency of contested claims, and which govern the extent and character of governmental repression.

In the present analysis, we concentrate on repression and on political activity at the national level. By *repression* we mean governmental activity which raises the cost of collective action by contenders for power. Ideally we should treat repression as a vector summing the effects of all governmental actions over all contenders.<sup>9</sup> Since many actions (as when one of several competing parties is proscribed) raise the cost of one group's action while lowering the cost for others, we would also want to have a separate vector for each contender.

In general, reinforcements of civil liberties such as assembly, due process and access to the press lower the costs of collective action, and measures like preventive detention or registration of associations raise the costs. In modern European history, one of the more striking facts is the low level of collective violence (as conventionally defined: we are perfectly aware of war and governmental terror) under such repressive regimes as those of the Nazis, the Italian Fascists or Primo de Rivera in Spain. In these cases, it is not so much that repression discourages violence itself, as that repression discourages the collective actions which would, in less repressive times, sometimes end in violent encounters with police, troops or other contenders for power.

If the government permits the organization of public gatherings, gives the press free rein, and guarantees civil liberties, all other things being equal, we expect the magnitude of collective violence should be low. In the extreme case where most forms of association (labor unions, professional organizations, political parties and the like) are outlawed, as was the case during the early part of Louis Napoleon's reign and during the German Occupation in France, then we expect virtually no collective violence. In simple terms, we expect a negative partial relationship between governmental repression and the magnitude of collective violence.

The case for national political activity as a stimulant of collective violence is easier to make. If it were not for the presence of theories emphasizing hardship, relative deprivation and other individual states only tangentially related to politics, we would hardly have to justify the introduction of political struggles into the analysis. According to our argument collective violence should tend to rise, all other things being equal, with the extent of nonviolent struggles for power. (For a given level of mobilization, we would expect a negative relationship between collective actions involving a high risk of violence and those involving a low risk of violence; in the present argument we are lumping the effects of mobilization and contention together, and neglecting the choice among alternative means.) Broadly speaking, collective violence should rise and fall with nonviolent political activity.

We propose a preliminary test of this argument for France by means of a few elementary models encompassing the period from 1830 to 1960. Our indicator of the magnitude of collective violence is again the estimated number of participants in disturbances. We employ three different indicators of repression; each presents some difficulties.

The first is "excess arrests": the deviation of the actual number of arrests of participants in collective violence from the number expected on the basis of the relationship of arrests to participants over the entire 131-year period. We compute it as the five-year moving average of the residuals from the regression of arrests on participants, lagging it so the five years end just prior to the year whose collective violence we are

<sup>9</sup> For many purposes, one would want to decompose governmental repression into *capacity* (the facilities for control of collective action available to the government), *propensity* (the extent to which those facilities are used) and *efficiency* (the amount of control achieved per unit employment of those facilities). Here we implicitly multiply the three factors together and look at their product.

seeking to explain.<sup>10</sup> Unfortunately, the index only captures the response of the government to actions which produced violence—often as a direct consequence of governmental intervention—and therefore has a degree of circularity built into it.

Our second index is the size of the national budget in the current year, which measures the sheer bulk of government. The trouble with that index, of course, is that while plausibly related to the government's repressive *capacity*, it is quite distant from the governmental action we wish it to represent.

The third measure is more clearly repressive: the man-days of detention in jails during the year. Here one might reasonably object that most of the actions for which people are jailed are not *collective* actions. Man-days of detention is no doubt the best approximation to our fundamental notion of repression, but none of the three measures gets at it very directly.

Our measure of national political activity is elementary. It is a dummy variable (1 = yes, 0 = no) representing the presence or absence of a national election in the year under examination. (We have also worked with the number of cabinet changes per year; there the results are mixed, and the shifts in the form of government make them difficult to interpret.) In a year of national elections, contention for power intensifies, coalitions form or reform, and political organization proliferates. According to our argument such a year, all other things being

equal, should produce relatively high levels of collective violence.

How can we test the argument? Our simplest model would be the following:

$$P_t = b_0 + b_1Q_t + b_2R_t + b_3S_t + b_4T_t + u(t),$$

where:

P = the number of participants per year (square root transformation)

Q = excess arrests (lagged five-year moving average residual of regression: square root of arrests on square root of participants)

R = annual expenditures, national budget, in millions of francs

S = lagged five-year moving average of man-days in jail

T = national election (1 = yes, 0 = no)

u = error term

These letter-variable combinations will remain consistent throughout this section.

Data on man-days in jail are only available for the period 1886–1939. In order to hold on to the years before and after those dates and yet be able to use that relatively good indicator of repression, we carry out two sets of analyses, one covering almost all of the years from 1830 through 1960, the other running from 1886 through 1939. We can detrend both sets of series significantly with a second-order polynomial in time ( $t^2 + t$ : a simple parabolic function of time). Our full model is therefore the following:

$$P_t = b_0 + b_1Q_t + b_2R_t + b_3S_t + b_4T_t + a_1t + a_2t^2 + u(t).$$

When the regression is computed over the years 1886–1939, we find:

$$P_t = 4838.6 + 6.60Q_t - .002R_t - .027S_t + 30.97T_t - 106.58t + .632t^2, \text{ for which}$$

$R^2 = .5035$ , standard error = 99.065,  $F_{6, 38} = 6.4238$  and  $p < .0001$ . In this case only the coefficients of R (the national budget) and of the two time variables are significant; the coefficient of S (man-days in jail) is in the predicted direction but weak ( $p = .175$ ), that of T (elections) in the predicted direction but weaker still ( $p = .395$ ) and that of Q

<sup>10</sup> At first we simply used the ratio of arrests to participants, and detrended both variables. Paul Siegel pointed out that, even after detrending, participants and arrests/participants are inversely related to each other by definition. Furthermore, since the number of participants is highly auto-correlated from year to year, lagging will not remove the difficulty. Our use of residuals eliminates the first problem, but not the second. Sources for political variables: Arrests come from our own coded descriptions of disturbances. National budget figures: *Annuaire statistique de la France, résumé rétrospectif, 1966*: pp. 484–485. Man-days in jail: *Annuaire statistique de la France, résumé rétrospectif, 1966*: p. 175. National election: compilation from a variety of political sources.

(excess arrests) positive rather than the predicted negative, and weak ( $p = .132$ ). Adding the three economic variables employed earlier—food prices, prices of manufactured goods and industrial production—to the equation produces no significant increase in explanatory power. Dropping excess arrests, on the other hand, reduces the standard error without appreciably affecting the amount of variance explained. The central relationships for the period 1886–1939 appear in the reduced equation:

$$P_t = 6104.3 - .003R_t - .050S_t + 33.3T_t - 126.7t + .728t^2$$

In that equation,  $R^2 = .4925$ , standard error = 95.41,  $F_{5,44} = 8.5395$ ,  $p < .0001$ . All coefficients except that of  $T$  (elections) are highly significant, and the removal of  $T$  raises both the standard error and the serial correlation. For this period, in sum, the bulk of the state (as represented by the national budget) and the state's use of its jails appear, as expected, to depress the level of collective violence. The effect of national political activity remains dubious and that of arrests for participation in collective violence quite unreliable.

As we stretch out to the entire period 1830–1960 we lose our measure of detention in jails but hold on to the others. Over that period the basic equation is:

$$P_t = 88.3 + 4.13Q_t - .215R_t + 45.15T_t - 1.92 + .023t^2.$$

Here  $R^2 = .172$ , standard error = 106.22,  $F_{5,120} = 4.9729$ ,  $p < .001$ . In this case, the one-year serial correlation is .306, a circumstance which lagging the relationships does not affect appreciably; we conclude that there are other variables acting on this system which we have not been able to grasp. In the equation all coefficients are significant save those for  $t$  ( $p = .14$ ) and  $T$  (elections:  $p = .07$ ). The coefficient of excess arrests is again positive instead of the predicted negative value. This time if we add the three economic variables to the equation we get a slight increase in fit to the observed distribution ( $R^2 = .211$ , S.E. = 104.69,  $F_{8,103} = 3.440$ ,  $p < .002$ , one-year serial correlation = .292), but only the coefficient of the industrial production variable takes on a significant value ( $p = .03$ ). Even though we appear to have misstated

the effect of arrests for participation in collective violence, we conclude that the basic relationships in this set are the political ones.

To close the circle, let us return to the analysis of year-to-year change with which we began. The first differences method we used in the first part of the analysis makes more sense for the expectation-achievement arguments we were examining there than it does for the political-struggle argument which the later sections of this paper have taken up; we adopt the first differences method here only to permit comparison with the earlier results. The model takes the form:

$$\Delta P_t = b_0 + b_1 \Delta Q_t + b_2 \Delta R_t + b_3 \Delta T_t + b_4 \Delta W_t + b_5 \Delta X_t + b_6 \Delta Y_t + u(t),$$

where:

- $P$  = the number of participants per year
- $Q$  = excess arrests
- $R$  = annual expenditures, national budget
- $T$  = national election
- $W$  = price of food index
- $X$  = price of manufactured goods index
- $Y$  = index of industrial production
- $u$  = error term

and all changes are measured from the previous year to the current one. Our computations produce the equation:

$$\Delta P_t = -.657 + .656(\Delta Q_t) + .001(\Delta R_t) + 35.4(\Delta T_t) - .028(\Delta W_t) + .086(\Delta X_t) - .004(\Delta Y_t),$$

for which  $R^2 = .0586$  and  $p = .3551$ . The only term of the equation which reaches statistical significance is  $T$ , which signals the shift to or from a year of national election ( $p = .04$ ); all three political variables have stronger effects than any of the expectation-achievement variables. All things considered, the political explanations of collective violence remain plausible, while the expectation-achievement arguments lose credibility.

We have not, by any means, ruled out all plausible versions of the expectation-achievement explanation. Given the multiplicity and looseness of the arguments scattered through the literature, many further analyses of these and other variables representing expectations and achievements are still possible. We recommend, and intend

to pursue, (1) tests to ascertain whether or not there is a feedback influence from violence to repression and, if so, how it operates; such feedback might account for the ambiguous results of our analysis of "excess arrests"; (2) other, more direct, representations of the "expectations" side of the argument; (3) the study of other variables representing mobilization, repression and the acquisition and loss of power by major contenders; (4) tests of both families of models on other sets of data. Until we test the same models on other times and places, some students of collective violence may prefer to hold on to expectation-achievement reasoning, arguing that France is an exception, that Frenchmen are preternaturally submissive in the face of repression, not to mention insensitive to

hardship. That way of saving the hypothesis would at least be novel. For our part, however, the result of the time-series analysis make us increasingly doubtful that the expectation-achievement arguments concerning collective violence have much explanatory value. The most promising alternatives lie in the analysis of struggles for power.

## APPENDIX

### TABLES OF UNLAGGED CORRELATIONS

Each coefficient refers to the maximum number of years for which data for both variables were available. The corresponding regression equations were, of course, confined to the years in which data were available for all variables in the equation.

Table 2. Zero-Order Correlations of Indicators of Hardship, Political Activity and Collective Violence, 1930 to 1960.

	Time	Days in Jail	Natl. Budget	Elec- tion	Indust. Produc.	Food Prices	Man. Goods Prices	Partics. in Coll. Violence	Arrests in Coll. Violence	Excess Arrests
Time	1.00									
Days in Jail	-.85	1.00								
National Budget	.51	-.58	1.00							
Election	-.17	-.02	-.11	1.00						
Industrial Production	.18	-.19	.02	-.08	1.00					
Food Prices	.20	-.55	.54	-.09	-.04	1.00				
Manufactured Goods Prices	.22	-.52	.49	-.07	-.04	.91	1.00			
Participants in Collective Violence	.15	-.25	.10	.16	-.14	-.14	.09	1.00		
Arrests in Collective Violence	.02	-.28	-.01	.23	-.12	.26	.27	.72	1.00	
Excess Arrests	-.12	-.14	-.11	.17	-.04	.24	.30	.00	.69	1.00

Table 3. Zero-Order Correlations of One-Year Changes in Indicators

	Food Prices	Man. Goods Prices	Indus. Prod.	Elec- tion	Natl. Budget	Parts. in Viol.	Arrests in Viol.	Days in Jail
Food Prices	1.00							
Manufactured Goods Prices	.91	1.00						
Industrial Production	-.04	-.02	1.00					
Election	.00	.06	.07	1.00				
National Budget	-.11	-.45	.01	-.12	1.00			
Participants in Collective Violence	.04	.07	-.02	.15	.03	1.00		
Arrests in Collective Violence	.12	.21	-.00	.23	-.20	.69	1.00	
Days in Jail	.44	.41	-.39	-.01	-.09	.13	.06	1.00

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