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Working paper #72

March 1972

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HARDSHIP AND COLLECTIVE VIOLENCE IN FRANCE 1830 to 1960

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March, 1972

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ABSTRACT

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 analysis 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 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.

HARDSHIP AND COLLECTIVE VIOLENCE IN FRANCE, 1830 to 1960

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 cumulation 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 one eminently political variable -- the extent of governmental repression -- does 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.[1] We do not 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 our most expansive, 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 the investment of new efforts in its elaboration and verification.

Lest we be suspected of battling straw men, let us mention a few much-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 or 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 as substantiation of 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.

Ted Gurr, finally, 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 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 dep-

rivation" 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" cumulates 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 cumulation 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.[2] Our purpose in sketching the three arguments and their implementation is to provide a rationale for our own choice of models and indexes representing 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.

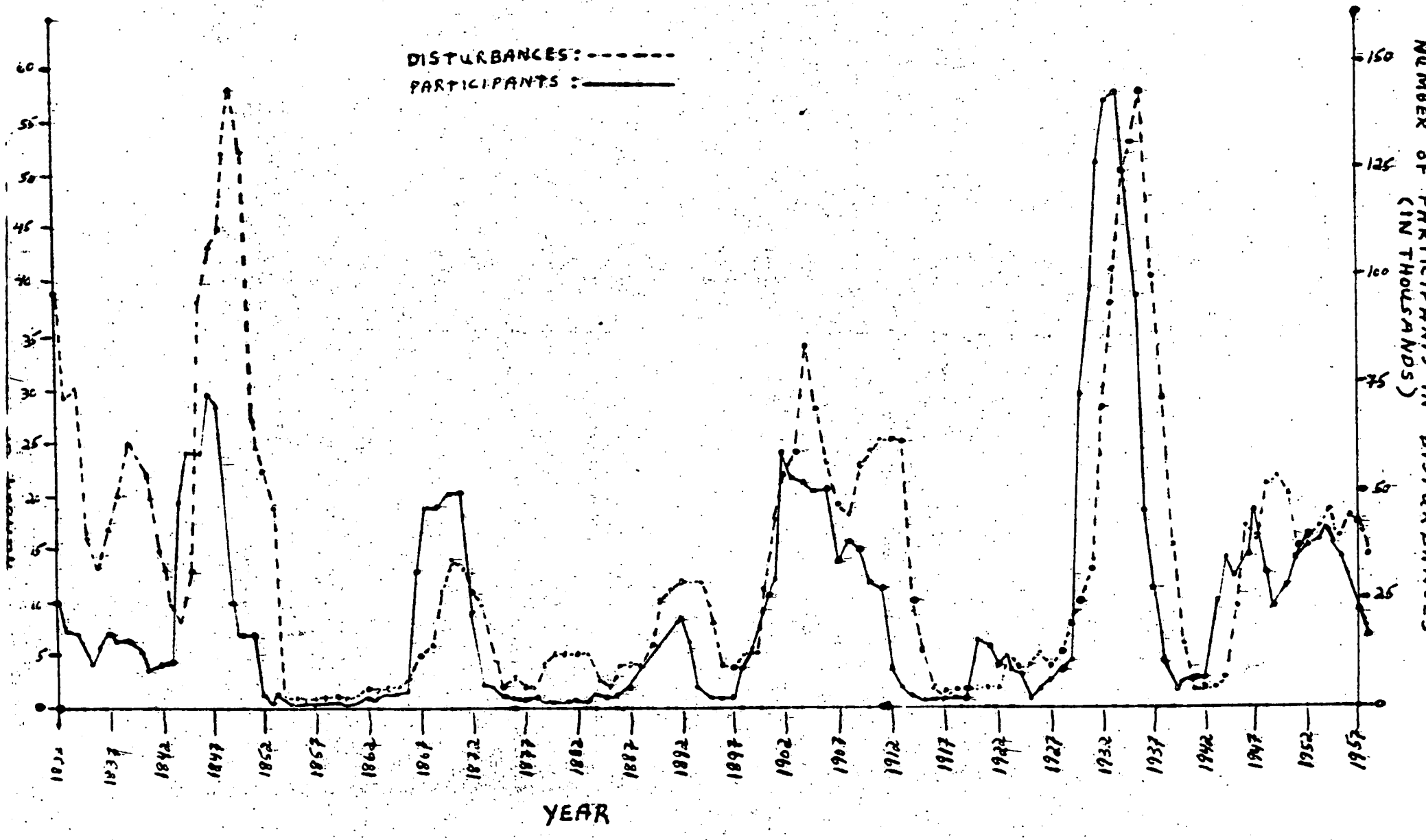
All the data are yearly aggregate measures for France during the period from 1830 through 1960. Our measure of collective violence is the

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 resistence.[3] 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.[4] 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 have 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.[5] 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 93 disturbances and some 90,600 participants in 1851, followed by 2 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 1851-52, the crucial events were Louis Napoleon's coup d'etat, 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.[6] 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 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

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



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 groups 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 <u>set</u> 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 participant in disturbances and the economic indicators -- manifests a trend verified by the non-parametric techniques described in Malinvaud (1966: 390-92). 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 by 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

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

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:

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

Multiple Correlation Coefficient: 0.0270 $F_{3,107} = 0.3539$, $p < 0.55$

Coefficient of Determination: 0.0007

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 0.05 throughout this paper.) Furthermore, the serial correlation of the residuals is quite large (0.5304), 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.

Despite the fact that the multiple regression model does not fit the data, 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 significantly 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.

Table 1 about here

What in fact do the data look like (see Table 1)? 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

TABLE 1

INTERCORRELATIONS OVER TIME OF ECONOMIC DEPRIVATION INDICATORS AND NUMBER OR PARTICIPANTS IN DISTURBANCES

	TIME (LAG/LEAD)	W_t PRICE OF FOOD INDEX	X_t PRICE OF MAN. GOODS INDEX	Y_t INDEX OF INDUS. PRODUCT.
	8-	+.0115	+.0016	+.0967
	7-	+.0008	+.0015	+.0687
	6-	-.0012	+.0005	+.0566
	5-	+.0066	-.0042	+.0292
Z_t	4-	-.0160	+.0526	+.1469
	3-	+.0021	-.0211	-.1159
Number of	2-	-.0377	+.0014	-.1374
Participants in	1-	+.0107	-.0496	+.0757
Disturbances	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 intthe "time" column indicate that the column variables (ΔW_t, ΔX_t, ΔY_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.

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: $r = +.1067$). However, we have already 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: $\Delta Z_t = b_0 + b_1 \Delta Y_{t-2} + u(t)$; However, neither the analysis of variance for the multiple correlation nor the coefficient of the independent variable is significantly different from zero (for both, $p < 0.16$).

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 major means of coercion which are 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. The ability to mobliize extensive resources -- especially manpower and coercive resources -- is almost always prominent among the criteria.

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 a 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 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 to the extent of mobilization, repression and contention for power. We do not think that extent is very 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, as represented in the government's response to collective action by contenders for power. 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. The key relationship is surely the effect of repression on collective action by non-members of the polity, rather than, say, the increased cost of the calculated use of violence against the government.

If the government permits the organization of large public gatherings and demonstrations, all other things being equal, we expect the magnitude of collective violence to relatively high. If the government represses these collective actions, on the other hand, 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 relationship between governmental repression and the magnitude of collective violence.

We propose to test this argument for France with an elementary model encompassing the period from 1830 to 1960. The estimated number of participants in disturbances is again our indicator of the magnitude of collective violence for each year. The ratio of the number arrested in

disturbances to the number participating is our measure of repression, on the following argument: 1) a larger proportion of arrests indicates the presence of more repressive forces -- especially police and troops -- at disturbances; 2) so far as we can tell from our study of accounts of disturbances, it also indicates the earlier presence of repressive forces at the site of collective action, which in turn indicates the government's advance preparation for the event; when police and troops arrive after the fact, fewer arrests appear to occur. Where most forms of association are illegal, the early presence of repressive forces at public gatherings is almost assured. In this sense, the proportion of arrests to participants measures the general repressiveness of the government.

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

$$P_t = b_0 + b_1(Q/P)_t + u(t),$$

where P = the number of participants per year

Q = the number of arrests per year

t = time

u = error or residual term

These letter-variable combinations will be consistent through this section.

One statistical problem arises: Built into the model is the constraint that as P increases, the ratio (Q/P) decreases. However, we can alleviate this problem by including a trend term for both variables (P and Q/P), and then, in effect, seeing whether deviations from the trend in the repression indicator Q/P predict significantly deviations from the trend

in the participants series. To some extent, this procedure will reduce the constraints on Q/P due to the dependent variable P. [7]

Given that each series has a number of points which are distant outliers (1848, 1871, 1934 in the participants series and a number of peaks in the repression indicator) the trends in the two series could not be fitted with simple polynomial expressions in time. Therefore, a square root transformation was performed on each series to reduce the effects of the outliers which prevented fitting a simple trend expression. These transformed variables will be designated P'_t and $(Q/P)'_t$ respectively. Both of these series were then detrended significantly by a second-order polynomial in time ($t^2 + t$: a simple parabolic function of time). Our full model is therefore the following:

$$P'_t = b_0 + b_1(Q/P)'_t + a_1 t + a_2 t^2 + u(t);$$

When the regression and correlations are computed, we find:

$$P'_t = 171.500 - 183.457\,(Q/P)'_t - 0.2882t + 0.0047t^2 + u(t);$$

Multiple correlation coefficient: 0.6396

Coefficient of Determination: 0.4091 $\qquad F_{3.127} = 29.3,\ p < .01 \times 10^{-12}$

Standard Error of Estimate: 88.79

The analysis of variance for the multiple correlation is highly significant ($p < 0.01 \times 10^{-12}$), as is the coefficient of the repression indicator b_1 ($p < 0.02 \times 10^{-12}$). Although the trend terms t and t^2 were significantly different from zero in detrending the variables separately, in this model neither is significant at the .05 level. One indication that this model

fits the data well is that the serial correlation of the residuals is low (0.083), suggesting that this model accounts for most of the systematic variation in the dependent variable. The coefficient of the repression indicator (which was highly significant) is negative, which confirms our expectation of the relationship between violence and repression, noted above.

A priori, we don't know whether or not there is a feedback effect of violence on repression or a lagged relationship between the variables. We cannot test the feedback effect with the limited number of available variables, but one model which does test a single year lag between repression and violence is the following:

$$P'_t = b_0 + b_1(Q/P)'_{t-1} + a_1 t + a_2 t^2 + u(t);$$

When we compute the regression and correlations, we find:

$$P'_t = 148.270 - 79.032\ (Q/P)'_{t-1} - 1.375\ t + 0.014 t^2 + u(t);$$

Multiple correlation coefficient: 0.3800

Coefficient of determination: 0.1444 $F_{3,126} = 7.09$, $p\ (< 0.02 \times 10^{-3})$

Standard error of estimate: 106.77

The analysis of variance for the multiple correlation coefficient is significantly different from zero ($p < 0.02 \times 10^{-3}$), as is the coefficient of the repression indicator ($p < 0.004$). Once again the relationship between repression (lagged one year here) and violence is negative. However, this lagged model does not fit the data so well as the preceding unlagged model, since in the lagged model the coefficient of determination is lower, and the standard error of estimate is higher.

While a comparison of the results of testing the sets of models representing the two theories (relative deprivation and relative ease of collective organization) certainly leads us to reject the former and to consider further the latter, we are still interested in ascertaining the relative effects of all our independent variables in the same model. To remain consistent with our analysis of relative deprivation, we use the first differences method on all the variables, and, to ascertain the relative weights of the independent variables, we compute standardized regression (Beta) coefficients. This model is then of the form:

$$\Delta P_t = B_o + B_1 \Delta W_t + B_2 \Delta X_t + B_3 \Delta Y_t + B_4 (\Delta Q/P)_t + u(t);$$

Where

$$\Delta P_t, \ldots, (\Delta Q/P)_t = P_t - P_{t-1}, \ldots, (Q/P)_t - (Q/P)_{t-1};$$

P = number of participants per year

W = price of food index

X = price of manufactured goods index

Y = index of industrial production

Q = number of arrests per year

t = time

u = error or residual term

We compute the regression and correlations:

$$P_t = 0.034 + 0.044 \Delta W_t + 0.085 \Delta X_t - 0.079 \Delta Y_t - 0.186 (\Delta Q/P)_t + u(t);$$

Multiple correlation coefficient: 0.2246

Coefficient of determination: 0.0504 $F_{4,103} = 1.36$, $p < 0.25$

Standard Error of estimate: 0.923

Tha analysis of variance for the multiple correlation coefficient is not significantly different from zero. Nor are the coefficients of any of the independent variables except that of our repression indicator (p 0.04). Given the first difference method of detrending (which is consistent with the relative deprivation argument but not necessarily with the ease-of-organization argument) the repression indicator explains a smaller proportion of the variance in this model. Nevertheless, the repression indicator is still the only significant predictor in the model; its weight is more than two times as large as that of the largest value for any of the measures of hardship. Once again, the sign of the coefficient of the repression indicator is in the expected negative direction.

We have not, by any means, ruled out all plausible versions of the expectation-achievement explanation of collective violence. 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; 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) testing 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 responsive to

repression and insensitive to hardship. That way of saving the hypothesis would have the virtue of novelty. For our part, however, the results 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 appear to lie in the analysis of struggles for power.

FOOTNOTES

1. 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), 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, and to Priscilla Cheever, Freddi Greenberg and Glen Jones for assistance in assembling the data.

2. 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.

3. 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 nonviolent days of the revolution from consideration.

4. 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.

5. 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.

6. 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: 452-53. 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.

7. Since the correlation coefficient is relatively high, it was possible that the trend terms were accounting for a major portion of the explained variance. So, to separate the possible confounding influence of time, we regressed, first, the participants series and, then, the ratio of arrests to participants on the trend expressions, and correlated the residuals. This correlation was -0.5903, which indicated that the trend terms were not a major influence on the variation explained by the model.

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