COMMENTARIES

Daniel Goldrich, Department of Political Science, University of Oregon

In the treatment of the concept of political legitimacy, Bwy states that people support their political system "not only because 'this is the way it has always been' (traditional legitimacy), and because of gratifications received from the system, but also because they had a hand in making it the way it is. In short, the ability to participate in a system leads directly to the building of positive affect toward it." He goes on to point out that this mechanism is not at work in all systems, for example in semi-authoritarian ones, and therefore perhaps cannot be analytically applied to some of the Latin American countries.

This overlooks an important theoretical and empirical phenomenon, the generation of widespread participation and support within highly controlled political processes. I refer here to the concerns of K. H. Silvert[1] regarding the integrative functions of "mass" parties, and to those of I. L. Horowitz[2] regarding charismatic parties in the third world. The type of participation involved tends in our social science not to be perceived as partaking of "representativeness," and yet the people involved may consider it as such and respond by according a high degree of legitimacy to the system. For example, if one measures legitimacy by Cutright's Political Representativeness Index, of which Bwy makes some use, one focuses on the legislative function and the open and competitive election of the executive. This has certain obvious utilities. But if one is concerned about processes of legitimacy formation and disintegration especially in the third world, and the ways in which these processes are associated with the generation and absorption of violence, then one must also try to get at the wide range of variation in popular participation that may take place under highly controlled conditions. For example, although Cuba has experienced a brief relatively recent period of parliamentary democracy (during the 1940s), its inability to resolve the major problems of the island led to the acquiescence in Batista's seizure of power in 1952. In legitimacy terms, this meant that the constitutional order had either failed to win or to maintain sufficient support to challenge the Batista coup: it was met with no immediate opposition. Castro's government chose not to reconstitute the parliamentary order but to carry through sweeping changes, affecting not only a redistribution of available wealth and status, but also something approaching equalization of opportunity in the society. Participation of the lower strata was generated in support of these changes, and intense support and participation was evidenced in such areas as the militia. Many of these activities were highly structured and certainly did not have to do with competitive policy alternatives, but the participation has apparently been associated with the generation of a high degree of legitimacy. For example, the Free[3] survey done in mid-1960 showed overwhelming, intense support for the Revolution and its institutions. And a survey carried out within the working class in mid-1962 by Zeitlin showed overwhelming (67 per cent) rejection of the proposition that the country should have elections soon, coupled with an even more overwhelming (84 per cent) opinion that workers "have more influence on the government now than before the revolution."[4] The point, then, is that participation may contribute to system legitimacy within a system structured very differently from those of the Western parliamentary sort, and we cannot take as a measure of that legitimacy the dimensions of the Fitzgibbon or Cutright indexes, which are limited to the Western forms.

The further point needs to be made that in such a setting as the Cuban, described above, there may also be found a high degree of organized violence, reflecting both the struggles of the old elite and other dispossessed sectors and the undergrounds sponsored by foreign opponents, despite the high degree of legitimacy of the incumbent government.

I mention this at some length because it seems to me that while Bwy covers a lot of very useful ground, his formulations tend to overlook the revolutionary third world situation, which, while surely not dominant in that sector of the world today, does provide some important cases currently (Cuba, Vietnam), and may predictably provide more in the future.

1. "Parties and the Masses," *The Annals,* Vol. 358 (March, 1965).
2. *Three Worlds of Development* (New York: Oxford, 1966), chapter 8 especially.
3. Lloyd A. Free, *Attitudes of the Cuban People toward the Castro Regime, in the Late Spring of 1960* (Princeton: Institute for International Social Research, 1960).
4. Maurice Zeitlin, *Revolutionary Politics and the Cuban Working Class* (Princeton: Princeton University Press, 1967), p. 38. See also his analysis of the significance of militia participation, p. 39.

Irving Louis Horowitz, Department of Sociology, Washington University

Professor Bwy has produced a sober and serious piece of work. It is marked by a deep appreciation of the drama no less than the deficiencies in traditional views on the subject of political instability in Latin America. His careful exposition of the two main protagonists: those who see revolution as a structure for the expression of discontent, from those who see it as a projection of a structure of expectations, is done with sensibility. His examination of legitimacy, while considerably weaker, relying as it does on a secondary literary view which sees legitimacy as "allegiance" (instead of authority) and illegitimacy as "alienation" (instead of reliance by rulers on power) also represents a serious effort at operationalizing concepts which all too often in the literature of political science appears as vague precisely to the extent that they are in vogue.

Given Bwy's expository power one would be hard pressed to quarrel with his understanding of the problem. Indeed, his reading of the quantitative literature on development done by political scientists and sociologists itself represents a real contribution. Unfortunately, he does not reveal the same level of awareness of the economic literature, particularly the works of Irma Adelman, Jacob Smookler, and Alexander Gerschenkron—and this is unfortunate, since this gap has very real consequences for the sort of analysis Bwy attempts. What has to be examined is Bwy's own contribution. Here we are in effect dealing with his ability to find a niche for himself in the growing macro-quantification literature on development in Latin America, so significantly worked out by Banks, Textor, Russett, Tanter, and most recently, by Soares and Hamblin.

Hopefully, Professor Bwy will allow me to pass over the many fine points in his study and get directly to the critical points in the small amount of space available to me.

The factor analytic technique used in relation to the measures of social revolution seem adequate enough—both in their original form as separate indicators and in their later condensed form. However, I am uncertain about a technique of analysis that does not seem to yield a conceptual variable, or paired variables, that gets beyond the condensations procedure. More specifically, the division of violence into two basic types: organized and anomic, while receives considerable support from both the hard and soft data available, does not, for instance, help us appreciate why strikes are a useful measure of anomic violence. Indeed, in my own experience, strikes, even Latin American General Strikes, are highly organized affairs, reflecting an acute level of trade union and working class organization. In this sense, strike actions are the very opposite of riots, demonstrations, and manifestations. The latter may indeed reflect anomic forms of response to crisis situations. But the former—strikes—whether measured by the degree of organization or by large-scale social consequences, might better be clustered with *organized* violence.

This in turn raises two questions: whether governmental crisis, on the "organized side," and strikes, on the "anomic" side, really belong in the same universe of discourse as measures of violence. The linkages seem to be extremely tenuous; and the condensation procedures used by Bwy tend to obscure rather than to clarify differences in factor weighting. Further, whether the amount of violence is a relatively independent measure from the extent of revolutionary activity is not adequately considered. It might be that organizational structures rather than the quanta of violence best measure revolutionary potential or action.

The z-scores of Table III are highly confusing. Just what is being explained remains in some doubt. Things work out adequately enough with respect to the Cuba loadings, with an anticipated movement from high anomic to high organized forms of violence. But examining the data for Argentina, we simply find high scores on both sorts of violence; while the nation that most resembles the Cuban pattern (Haiti), has so different a political or historical profile between 1955 to 1964 that the factor similarities must be treated with extreme caution. Further, Cuba's sister Caribbean nation, the Dominican Republic, performs so differently in the Tables from Cuba that Professor Bwy is compelled to introduce an *ad hoc* explanatory device that is absent earlier. We are certainly not prepared to learn that "fear of punishment inhibits aggressive actions against a government," since fear of punishment is until this point in the study considered a constant, common to all Latin American nations. It is certainly as high for Cuba, Mexico, or Bolivia—nations which did carry off revolutions—as for those which did not. Further, the "explanation" for Peruvian behavior is in turn neither related to the organized or anomic formulas. Neither is it linked to the psychologistic high fear—low violence formula. Instead, we are told it is linked to "the fortunes of the APRA." Instead of fulfilling the promise of the paper, how factor analysis helps generate theory construction, this section seems tragically weak, creating a different explanatory device for practically every nation with a z-score higher than that of the Dominican Republic.

The third section on "testing the causal model" brings to the fore the host of problems involved in converting an analysis of variance into a test for causality. For even if a high degree of explained variance can be garnered between two or more measures, the sequential ordering of these measures is not demonstrated. Actually, the number of students who have attempted such causal analysis equals the number who have tried and failed. Even Professor Bwy is compelled to speak of the "strange bedfellows" turned up in his curvilinear model between force and anomic violence. Totalitarian nations such as Nicaragua, El Salvador, and Honduras cluster at the low end of the force continuum with democratic nations like Costa Rica and Uruguay and Bolivia. On this plotting, Mexico becomes a "deviant case," in that it is an "open system" (legitimate), tolerating high violence, of an anomic sort. It is said to be the only such case, with Peru representing the opposite pole in htis curvilinear expression, exhibiting high expenditures on the military with relatively low violence.

The problem here is that Bwy does not take seriously the complex nature of a one-party State modeled on an authoritarian system but exhibiting democratic strains. To speak of a deviant type means to resist what the data do show: namely the high degree of Falangism built into the Mexican system. Nor is this resistance to the data occasional. The problem of the deviant case rears itself in Bwy's definition of legitimacy, which like the earlier definitions or organized and anomic violence leaves one in doubt. Legitimacy is nowhere adequately defined; and we are led to accept an equation between "openness" and legitimacy; and presumably closure and illegitimacy. This definition is faulty in general and in detail, as I have tried to show elsewhere in my study on the "Norm of Illegitimacy" (briefly summarized in LARR II:2, item 707).

As is true elsewhere in the paper, subordinate hypotheses get introduced in order to explain deviant national cases. Thus Argentina, which exhibits one of the highest GNP per capita rates in Latin America, and should therefore show a correspondingly low use of force and planned violence according to Bwy's data, shows no such pattern. Bwy explains this by pointing out that "Argentinians have steadily been getting smaller shares of the total goods and services their economy produced before." But this quasi-Marxist hypothesis about higher produciton linked with poorer equity in distribution, while probably true, does not help the cause of showing the importance of anomia in revolutions since far from an increase in force to match an increase in violence, the planned violence of the State in countries like Argentina far exceeds the anomic (or organized) violence of the citizenry. His data do not show whether the "force" of the State or the "violence" of the people has any causal primacy. Indeed, they do not even show that they are strongly related statistically.

The suspicion that the definition of legitimacy used is faulty is given support by the fact that among those nations rated high on legitimacy between 1950 and 1955 is Brazil—now a thoroughly "illegitimate" regime run out of a tight military box. And those nations which once scored "low on legitimacy," such as Cuba and Venezuela, now each exhibit high legitimacy (the Cubans through a Leninist polity of the mass; and the Venezuelans through a constitutionalist system of laws). In short, the predictability of even crude measures of stability is not assured by the use of factor analytic methods, at least not those factors taken into account by this paper.

Professor Bwy concludes that factor analysis was able to reduce nine operational indices to a bi-variate conceptualization of the area in terms of anomic and organized factors in instability. We are told that systematic satisfaction is negatively associated with political stability, which eliminates the likelihood that relative measures of satisfaction, particularly external factors, can be accounted for. The second notion that high and low uses of force correlate with low levels of political instability is only weakly correlated, if we take the events of recent years in the Dominican Republic, Colombia, and Peru seriously. Further, there seems little difference between Argentina and Peru in the use of federal force levels, despite their different factor scores. Perhaps other measures such as urbanization might be looked into by Bwy with greater advantage. Referring to a point made earlier, the expressed lack of correlation between force and organized violence would probably not result in such a low level of correlations were the matrix of organized violence to include rather than exclude strikes and unionization.

The area of legitimacy is too broad to be dealt with fully here. But it is difficult to resist the belief that the reason Bwy found that being high on a political legitimacy scale was just as likely to lead to anomic revolutionary activity as not, should have compelled him to examine the question: Just how normative are conventional forms of legitimacy such as social order in Latin America? Put another way: Doesn't a norm of illegitimacy exist for the goal of redefining—almost in Paretan optimality terms—the basis of political power by redistributing the holders of power? That this is done precisely by those nations desirous of some stability, if not of democratic stability, would seem to require a different sorting of the data than that provided in this paper.

To conclude that as legitimacy formations decrease, organized violent activity increases, loses sight of an alternative formulation of the meaning of democratic attainment. To reach democracy may require precisely high participation in violent activities, and holding in low regard what Bwy calls "legitimacy formations." For where one finds high legitimacy formations (and irrespective of definitional problems, let us take Mexico, Uruguay, and Costa Rica as examples), there one may find a single-party State closer to Falangism than to democracy (Mexico); or a displacement effect, where police activities displace rather than supplement normal internal military repressions (Costa Rica). Further, one must not lose sight of the tenuous relationship that political "openness" seems to have with such crucial indicators of development as industrialization, migration, and urbanization.

All of this is said not to reproach Professor Bwy for his Herculean attempt at formulating a General Theory of Latin America, but rather to have him go after that General Theory in a way that better accounts for the unexplained portions of the variance, which may extend—even in Bwy's work—from 30 to 50 per cent.

Quantitative analysts should remember that their formal measures may exclude important qualitative considerations deeply affecting the nature of their outputs. For example, not simply the fact of revolution but the economic quality of that revolution, *i.e.,* capitalist, socialist, or fascist, must be considered. Professor Bwy has considerable difficulties with "deviant" cases such as Cuba and Argentina because his "normative" standards are not of universal applicability. Notions such as legitimacy, anomie, and even revolution may, or may not, be susceptible to generic treatment. Expressed methodologically, the data input may change sufficiently so that the combination of variables that are amalgamated into factors might require different weights to be assigned to each factor. It is true that a multiple regression analysis isolates the underlying relationships between the dependent and several independent variables. When the sample of independent variables changes, so too, might the results change. Contexts determine significance, and the inability of Bwy to make such adjustments leads to a reductionism that is not necessarily the fault of the multiple regression technique. Factor analysis can, as Professor Bwy well points out, serve as an important vehicle for generating hypotheses concerning the structural prospects of processes; but to ignore basic differences in types of social systems may lead to an encouragement of spurious variables in place of conceptual variables.

Sandra Powell, School of Government and Public Administration, The American University

Professor Bwy has undertaken a difficult task; namely, that of transforming a conceptual scheme into a testable model. Various conceptual schemes (as Easton, Lipset, and Almond and Verba) intuitively have seemed analytically useful but have remained, for the most part, untested. Too often the schemes are used as theoretical explanations when, in fact, they are only *ex post facto* descriptions. (For example, the government fell because it was illegitimate and ineffective. How do you know that it was illegitimate and ineffective? Because the government fell.) Rarely have attempts been made to independently ascertain the impact of legitimacy, effectiveness, or demand load on system stability in a rigorous, non-tautological manner. To actually test and predict the effect of the three variables—legitimacy, satisfaction, and fear of retribution—on system stability Professor Bwy was required to develop empirical indicators at the system level. Limited by the non-availability of survey data, he accomplished this with considerable methodological ingenuity.

I think an interesting question for discussion concerns Professor Bwy's conceptualization of the two factors, Anomic and Organized Violence. It is difficult for me to see the unorganized aspect of demonstrations and strikes. Such activities require prior planning to materialize and to be successful. Riots, however, do seem unorganized. We tend to think of demonstrations as organized and goal-directed while riots are unorganized, violent expressions of frustration without specific goals. However, in the present study specificity of goals and organizational control were not the definitional distinctions made between riots and demonstrations. A demonstration that became violent was considered a riot.[1] Defined in this way, riots do seem similar to demonstrations and strikes but not, in my mind, because they are anomic, unorganized.

Conceivably, riots, demonstrations, and strikes may be distinguished from guerrilla war, revolution, governmental crises, and deaths from domestic violence by *intent*. The latter, falling on the Organized Violence factor, are aimed at the destruction of the system while those on the Anomic Violence factor (riots, demonstrations, strikes) are designed to accomplish certain objectives within the system. They are tactics for influencing government, not tactics for system change. Demonstrations, riots, and strikes may, in Charles Anderson's terms,[2] demonstrate a power capability with no intent to overturn the system. They are legitimate tactics of influence for obtaining concessions from the present government. Such an interpretation would explain the high negative correlation Professor Bwy found between Legitimacy and Organized Violence and the lack of correlation between Legitimacy and Anomic Violence. Anomic violence is unrelated to system legitimacy because system instability is not the goal of demonstrators or strikers. Conversely, legitimacy is associated with guerrilla war, for example, because guerrillas are attempting to change the entire system. Guerrilla war occurs when people do not consider the system legitimate and are willing to turn to violence to replace it, while a demonstration is simply a tactic of power, much like voting, used to acquire certain specific goals from the existing system.

In general this paper represents a real landmark in Latin American research. The theory is clearly presented and rigorously tested by someone obviously skilled in sophisticated techniques. Such analysis demonstrates the possibilities for greatly enhancing the quality of our information concerning Latin American political processes.

NOTES

1. Definition—Riot: "Any violent demonstration or clash of more than 100 citizens, involving the use of physical force," p. 39.
2. Charles W. Anderson, *Politics and Economic Change in Latin America.*

Peter G. Snow, Department of Political Science, University of Iowa

Although I have some reservations about other aspects of this article—such as the means of operationalizing the conceptual variables[1]—my one major criticism is of Mr. Bwy's three general conclusions, which appear to me to be valid only if much of his data is ignored.

The first conclusion is that "systematic satisfaction (as measured by *change* in gross national product per capita) is negatively associated with political instability" and that the stronger correlate of discontent is organized violence. Bwy points out that the correlation between "satisfaction" and 1958–60 Organized Violence is —.63 and for 1958–60 Anomic Violence —.33; however, nowhere does he mention the correlations between satisfaction and violence (either organized or anomic) in the 1955–57 or 1962–64 periods. These correlations, which I have computed from the data presented in Figure II and

https://doi.org/10.1017/S0023879100028806 Published online by Cambridge University Press

Table III, cast some doubt on both the strength and direction of the postulated relationship. In both of these periods anomic violence is more highly correlated with satisfaction than is organized violence (—.106 vs. —.087 in 1955–57, and +.272 vs. —.095 in 1962–64). And the only correlation of any significance is positive rather than negative. Bwy's first conclusion would thus appear to be valid in only one of the three time periods—and the wrong one at that. Since satisfaction (which is supposed to be the causal variable) is measured between 1948 and 1960,[2] it should have its highest correlation with violence in the subsequent time period (1962–64), yet this is not the case at all. In terms of Bwy's causal model, satisfaction between 1948 and 1960 evidently had no effect upon organized violence between 1962 and 1964, while it appears to have "caused" an increase in anomic violence during this period.

With regard to the relationship between force and violence Bwy says "a test of the inhibitive effects of the use of Force (as measured by expenditures on defense as a percent of gross national product) on Anomic violence revealed that the curvilinear model does indeed apply to Latin America. . . . It is the middle range internal force countries which are the ones experiencing most of the anomic breakthroughs" and "correlating the same Force data with Organized violence, on the other hand, yielded no association." These conclusions would appear to be quite valid—but *only* as applied to the 1958–60 period. Once again Bwy neglects to mention the relationship between force and violence in the other two periods. This appears to me to be especially strange in view of the fact that force (allegedly the causal variable) was measured in 1959–60, and thus should be related most strongly to 1962–64 violence. If one plots these variables in this time period, and calculates the correlation coefficients, it becomes apparent that there is a moderate degree of association, *but* the stronger correlation is with organized violence (+.486 vs. +.307 for anomic violence) *and* both relationships are linear not curvilinear. Between 1962 and 1964 it was not the "middle range internal force countries" which experienced most of the anomic breakthroughs. Of the seven countries with the highest anomic violence z-scores, five were among the seven countries "applying" the most force, and *none* were among the six middle range internal force countries."

It is with regard to the effect of legitimacy on the occurrence of violence that Bwy makes his most extravagant claims: "In all cases as political legitimacy decreased, organized violence increased. When the effect of political legitimacy on Anomic violence was tested, however, in every case the association was proved to be very weak or non-existent." Even the incomplete correlation matrix presented in Table IV contradicts these all-inclusive claims. For example, the correlation between "Change in PRI (1960–1955)" and 1962–64 Organized Violence is positive, not negative, thus indicating that when political legitimacy decreased organized violence also decreased. The computation of the correlation coefficients which Bwy omitted from Table IV[3] reveals additional inconsistencies: the relationship between the 1955 Fitzgibbon Index and 1955–57 Organized Violence is positive, as is the correlation between the 1955–1960 change in the Fitzgibbon Indices and 1962–64 Organized Violence; in each instance organized violence decreased after a decrease in legitimacy. It might also be noted that the correlation between the 1950–1955 change in the Fitzgibbon Indices and 1955–57 Anomic Violence is —.609. Such a coefficient (especially in relation to those presented by Bwy in Table IV) is neither "very weak" nor "non-existent."

NOTES

1. For example, I do not understand how political legitimacy can be defined, as "the amount of positive affect toward the political system (and the government) held by the populace," and at the same time be measured by means of North American ratings of some of the components of democracy. Nor do I understand how "budget expenditure allocated to defense as a percentage of GNP" can be used as a measure of the amount of *internal* force actually applied by a country.
2. At the bottom of Figure II is the notation "Satisfaction[101] (Annual Growth of GNP per Capita (1950 to 1959)"; however, if one turns to footnote 101 and the references listed therein, it becomes apparent that "satisfaction" was actually measured in ten different time periods which began between 1948 and 1953 and ended between 1955 and 1960. Only in the case of Colombia was the annual growth of per capita GNP measured between 1950 and 1959.
3. In footnote 100 Bwy says that "although coeffcients were available for all of the data points within the matrix. . . . correlations only appear which test the causal model." Nevertheless, a careful reading of this table will show that not only has Bwy omitted correlations which could have been used to test his causal model, but he has also included correlations which can not be used for this purpose. For example, the table shows a higher correlation between 1958–60 Organized Violence and "Legitimacy (1955)" than between 1955–57 Organized Violence and "Legitimacy (1960)." That these coefficients can *not* be used to test the causal model may be seen by reading footnotes 99 and 100 which explain that "Legitimacy (1955)" is actually the sum of the 1955 and 1960 Fitzgibbon Indices, and "Legitimacy (1960)" the sum of the Fitzgibbon Indices for 1960 and 1965.

Raymond Tanter, Department of Political Science, University of Michigan. (On Leave)

This paper is very well organized, builds cumulatively upon past efforts and makes a profound contribution to knowledge despite certain defects. The organization of the paper may not be apparent because of its length. The first section on Psycho-Social Dissatisfaction and Political Instability places the study in the general literature; the second gives the definition and measures of the behavior to be explained—political instability; the third "tests" (sic) the causal model and the fourth provides a summary and set of propositions.

The paper builds cumulatively on past efforts in an admirable way. The author appears to draw from a range of contributions, e.g., from area studies to general quantitative cross-national works. The transitions between the several authors quoted are quite adequate. The studies provide us with a continuous flow of knowledge from multiple streams of evidence. Indeed, this is one of the most positive features about the paper. That is, when one finds convergence among multiple streams of evidence, one pushes the waters of ignorance back that much. Eventually, one may be able to build a dam of cumulative experiences from which other scholars might tap and also convert to intellectual energy. Douglas Bwy's paper moves us a step closer to this dam of intellectual resources.

The contribution to knowledge may be summed up in the form of four propositions for Latin American countries during the late 1950's:

1. Domestic conflict behavior during 1958–60 may be divided into at least two dimensions: organized and disorganized (anomic) violence.[1]

2. Increases in "satisfaction" (annual changes in gross national product per capita, 1950–1959) are associated with linear decreases in the levels of organized violence, 1958–60.

3. Levels of resource allocation which enable a greater capacity for suppressing violence (expenditure on defense as a percentage of GNP, 1959–60) are associated curvilinearly with levels of disorganized violence but unrelated to organized violence, 1958–60. That is, moderate levels of force are associated with the highest levels of disorganized violence.

4. Increases in legitimacy (changes in democracy and standard of living, 1950–1955) are associated with linear decreases in levels of organized violence, 1958–60.

I re-state these four propositions from Bwy's paper in order to highlight the important contribution to knowledge that they make. For example, there is considerable debate over whether programs that increase aspirations, sense of political efficacy and personal effectiveness of individuals and mobilize groups depress or facilitate violence. Bwy suggests that it depends on the location of the national population on an economic development continuum as to whether increasing their "satisfactions" will increase or decrease violence. Thus, he predicts that increasing satisfaction will decrease violence in Latin America because these countries are largely on the right or more developed side of a continuum of change. This finding accords with the empirical findings of the present writer. We found that increasing satisfaction (1955–60) related to increases in the levels of violence in Middle Eastern and Asian countries experiencing sucessful revolutions during 1955–60, but that increasing satisfaction is related to small decreases in the level of violence in Latin America.[2]

The important contribution to knowledge that the Bwy study makes may be marred somewhat by defects of a theoretical nature, some of his substantive interpretations, methodological problems and technical difficulties. Theoretical defects may be the lack of acknowledgment of a contrary theoretical orientation to the frustration-aggression, aspiration-expectation ideas that he seeks to vindicate. For example, one could argue that the organized violence in Latin America need not be a function of dissatisfaction. Violence could be explained on the basis about the way groups decide to influence politics without reference to psychological states of individuals. That is, consider the fact that a certain level of violence is a "normal" part of the landscape. Now look to the consequences that the violence has on the political system. Could it be the case that violence and the threat of violence used strategically in the pursuit of political ends helps the political system survive by constantly changing the basis upon which values are allocated? Is it necessary to look to the psychological motivation of individuals who are participating in a political process that *includes* violence as a legitimate means of access and influence within the political system? Could one explain violence on the basis of political goals without referring to psychological states?

In addition to failing to look at alternative theoretical orientations, Bwy makes several substantive interpretations of secondary sources that are questionable. Several times he refers to works by Hayward Alker and Bruce Russett and confuses the *levels* of economic development and the *rates* of change (p. 21) as is illustrated by the use of "increasing, decreasing" language when referring to levels. When Bwy wants to prove a point, he goes far out on the limb to stretch the data, moreover. Again as regards Alker and Russett, as well as Betty Nesvold, Bwy cites the fact that a curvilinear test increased the linear correlation from −.43 to −.47 and .62 to .67 respectively. Without pausing to see whether such small increases were in fact significant, Bwy rushes ahead to make an interpretation that these two studies are evidence of a curvilinear relationship between levels of development and violence. (Also on p. 34, he concludes that 3.0% and 2.8% are "considerably more than" 2.2%!).

Methodological problems also detract from the contributions of this study. Bwy slips casually from individual level to group concepts without questioning the allocation of resources which could be used to suppress violence is equiva- concepts such as social violence are definable in terms of individual concepts and that there are in fact laws which connect psychological dispositions to group behavior, Bwy's analyses may be warranted.

Also related to methodological problems of concept formation is the slippage from concept to measure and indicator. He assumes, for instance, that the allocation of resources which could be used to suppress violence is euqivalent to force *and* to punishment. This is in keeping with his frustration/

aggression theorizing. Is the proportion of resources allocated to defense necessarily equal to the employment of force and are such proportions perceived and/or intended as punishment?

Another illustration of slippage is based also on his attempt to make his measures congruent with the concepts in his phychological theory. He begins with a discussion of aggression and then slips into a treatment of political instability. Some of the measures of instability, however, are not aggressive actions against the regime. Are major governmental crises or purges aggressive acts against regimes?

The theoretical, substantive, and methodological questions raised here, however, do not detract from the overall worth of the study. Indeed, it is because his procedures were stated explicitly that a critique is possible!

NOTES

1. The writer prefers to label these Internal War and Turmoil. Bwy found three dimensions but only interpreted two.

2. Raymond Tanter, Manus Midlarsky, "A Theory of Revolution," *Journal of Conflict Resolution* (September, 1967), pp. 264–280.

Anthony Leeds, Department of Anthropology, The University of Texas

Though, in principle, I am all for quantification and am convinced that it provides an ultimate resolution to certain theoretical, methodological, and factual problems in the social sciences, I am very much opposed to mindless quantification of which this paper and, apparently, many of the works it cites, are examples, especially when it is carried out prior to basic structural, qualitative analysis which defines appropriate categories for quantitative treatment.

The quantification—here—is mindless because it occurs in a theoretical, methodological, and empirical vacuum; it consists of statistical operations, gimmicks one might say, in lieu of fundamental thinking. There *are* masses of extant data about social systems, their violences, and what actually happened at given times and places. Such data regarding situations and events can be seen in the entire tissue of contexts which are involved and which are the ultimate source materials for the methodological validation of the use or non-use of some statistic or some statistical procedure, especially one which, on the one hand, abstracts some exceedingly narrow category of enumerable data from the entire range of data relevant to understanding the event and, on the other, compares it to similarly abstracted data from up to scores of cases (countries) without reference to context.

These statistical procedures involve basic methodological problems, even aside from the multiplicity of operations which the statistics themselves involve, each step of which is one further interference with the system observed, or, at least, with the direct knowledge of the system involved and therefore increases the possibility of error or meaninglessness. Among such problems are questions of typology and comparability.

Closely related to the methodological problems, in some cases, are broad-ranging problems of theory common to Bwy's paper, to a number of the papers he cites, as far as one can tell from his quotations, and to a large body of literature being turned out on "revolution" (Johnson: 1964), "internal warfare," "underdeveloped countries" and a multitude of items too numerous to cite here. Two of the main problems are, a) the absence of any theory, as in the present instance, b) the absence of any analysis, sociological or societal, or, more strikingly, *political.*

I shall focus on some notable methodological-theoretical issues in Bwy's paper, noting, in passing, that I think it is high time broad and sharply critical discussion be given to such productions as this, hopefully to stop the flow of nonsense that is taking up so much publication space and reading time. The issues involve fundamental data questions, too, and I shall refer to these as I go along.

One of the most marked problems in Bwy's paper is the inability or unwillingness to distinguish anything from anything; for analytical purposes to distinguish one class of events from another, in short to *define* what is being talked about (Johnson, cited above, has the same problem, as do many others). Defining is no easy mater. It requires a rich acquaintance with a broad range of qualitative and quantitative empirical data. It requires a broad introduction to theoretical conceptions, heuristic models, and the hypotheses derivable from functional, historical, evolutionary, structural, and other types of propositions. It requires a keen methodological awareness of the canons (poorly formulated as they are) of comparability and controls. In other words, the cogency of a definition depends a great deal on the cogency of extant theory, methodology, factual inventory, and so on.

We are, presently, endowed with a tremendous heritage of data and a rich inventory of theories and methods—in short, we are in a position to give at least reasonably precise definitions, even of a term such as "instability" or "revolution." But in Bwy's paper we find no genuine definition at all. Instability comes to equal "revolution;" "revolution" comes to include "palace revolts" (my own 'elite replacements,' see Leeds: 1962), true "revolutions," "rebellions," "riots," "feuds," and every conceivable type of organized or mob violence. Furthermore, "stability," the opposite of instability, arbitrarily comes to be equated with "democratic"; hence "instability" with "non-democratic," presumably autocratic or authoritarian or totalitarian or dictatorial. "Stable" comes to be equated with "open," with "development," with "modernization," with citizen's "participation," with "consensus," with "elections" and "voting," with "media participation," with "opposition within legislatures," as well as with "democracy," all these without reference to their structural and functional variations and natures. In passing, it seems plain that Bwy, like so many other political scientists wrapped up in their own ethnocentrism, consider all this—the "democratic," "developmental," "competitive," etc.—Good, with no inquiry as to a) whether or not it is, in fact, good; b) the grounds on which one desides that it is, indeed, Good; c) whether, in fact, observed *forms* of say "participation" or "elections" are *merely* that—forms—actually covering diametrically opposed actualities, e.g., the recent farce of "elections" in Viet-Nam (Jack: 1967), or, from *my* citizen's point of view, the farce of presidential elections in the United States.

In short, I consider it absolutely essential, before any further attempts of this sort are engaged in, to do some basic definitional work. Not all cases of everything can be included in the statistical analyses. I have, for example (Leeds: 1962) suggested at least a three-fold classification of internal violences, with definitions of each—the revolution, the elite replacement, the cyclical reestablishment—which do not include the riot, the rebellions, and other clearly distinguishable types of organized violence within a polity, and do not include mass jailings which might well be considered violences or instability.[1]

It is to be noted that the definition of the first three, revolution, elite replacement, and cyclical reestablishment, is linked with a typology of societal types with which each type of organized violence corresponds as a functional expression (e.g., one does not find revolutions in archaic states or what I call 'static agrarian societies' (Leeds: 1964, 1321) like medieval Europe, medieval and pre-European India, S.E. Asia, and China, but rather in societies whose "class" structure is undergoing systemic change). Put another way, the substantive situations, carefully examined empirically *in context*, in their connections with a complex of interrelated variables whose variations in state affect each other (Leeds: 1964, 1321–2), are quite different from each other, must be separated analytically and typologically, and referred to by precise definition; a necessary series of steps, prior to quantification (which may, among other things, serve as a test of the typology). All such differences are indiscriminately lumped in the work under review and in many of the references cited by the author. Much of the indecisiveness of result (e.g., p. 28 and the contradictions discussed on pp. 21 ff) result from such crude untheoretical inquiry.

Connected with this problem is the total divorce from consideration of political realities prior to statistical manipulation. For example, Bwy and his sources are troubled by an alleged difference between Afro-Asian countries and Latin American countries as to the occurrence of so-called revolutions in upswings or downswings of the economy. Even assuming that the results regarding such occurrence are correct or meaningful, about which I have doubts, several realities need to be considered; these involve typological considerations, conceptions of differential acculturation, and divergent revolutionary models.

a) A number of Afro-Asian states are states with socio-cultural-political underpinnings going back one to three millenia, e.g. S. and S. E. Asia. Despite European acculturation accruing to the colonial relation, many of these underpinning structures continued and continue today, fundamentally to affect the body politic in ways which may be drastically different from b) or c) below.

b) Most of the rest of the Afro-Asian states are "new" countries, e.g. Nigeria or the Congo, which are almost entirely artificial creations, products of the colonial administrative structure. The problems of polity and economy and their inter-relationships are quite different from those of a) or c).

c) The Latin American countries, with the partial exception of Mexico, Perú, Ecuador, and Bolivia, created new socio-cultural-political systems between 400 and 500 years ago and, with the exception of Brazil and Haití, were ruled by a single colonial country with relatively uniform legal and administrative norms. After so-called independence, the essential colonial relation continued under the predominant sway of a single country, first Great Britain and, now, in a profoundly permeating way—including force —the United States, *for the entire continent.* There is no equivalent power confronting Asia or Africa.

A second set of political considerations needs to be examined but is left out (how can political "science" so consistently fail to examine political systems, political relations, political events, and, above all, power, especially when talking about the most essential expression of power, bloody organized violence?). These are the internal political relations of the unit countries. This problem crops up continually in the statistical treatments in this and other works.

First, let us look at that "stability," so value-laden in the usage accorded it by Bwy and so many others. Stability, as I pointed out above, is equated with democracy, with elections, with modernization, with a broad income distribution, with consensus, with participation, etc. This conception is derived from an ideologized model of what theamericanway type of democracy is supposed to be like, projected on to other societies. The supposed absence of *overt organized violence* on any significant scale in our country (a supposition which grows more dubious in each summer of our discontent and disregards the American mechanisms for externalizing organized violence as in Santo Domingo, Viet Nam, Korea, and, through others, in Bolivia, the Near East, and elsewhere) is contrasted both explicitly and implicitly with overt violence in, say, Latin American countries. *La Violencia* in Colombia, any number of governmental overturns (not distinguished as between coup d'etats or revolutions) and endemic violence in any number of countries, are equated with instability.

What amazes me is the failure to see the context in which such phenomena occur—the failure to analyze the socio-politico-economic, in short, the societal systems. When one examines such events in their functional and/or causal relationships with other variables of the society and ultimately examines the dynamic *system* of variables (with any of the functional or structural models today available), one discovers the remarkable continuity—the stability, in fact —of the system and its patterning. What is notable is that the Latin American countries—which have nearly half a millenium of institutionalizing behind them and a half a millenium of colonial relationship—have stayed consistently within certain frameworks whose parameters have evolved very slowly but quite regularly ("developed," "modernized") over very long periods of time (Leiserson: 1966). In short, I think the societal system can be described as *highly stable*. Among their equilibrium mechanisms are occasional coups d'état, violences, palace revolts, elite replacements, but rarely revolutions. Such mechanisms have regulatory functions such that the systems are orderly and provide a degree of predictability.

If one then looks at systems this way, the whole basis of argument—undefined, untypologized, and untheoretical—falls to the ground. Bwy's categories fall to pieces; his very argumentandum—instability—can be disproved for a large number of his cases, assuming any ordinary definition of the word; his "data" can be shown to include a series of non-comparable cases selected out of context; and his conception of instability can be shown to be imbued with the value position that violence is dysfunctional and undesirable. All social situations are treated as if identical in his methodology.

Let us turn briefly to some questions of theory involved in the preceding. I remarked above that Latin American systems are very stable and that the organized violence included many examples of many things, but only rarely revolutions. Which, by definition and typology, are the revolutions in Latin America? A minimal differentiating definition of revolution, I find useful, designates revolution as a system-wide disturbance characterized by organized fighting growing out of conflict in a social situation where one or more major *classes* (Marx and Engel: 1846[1947], 48) is fundamentally unrepresented in the decision-making and reward-allocating operations of the state. Note that even though non-representation may exist, it is not until organized fighting develops to try to create representation that there is revolution. The United States, for example, up until recently, provides a case in point. Conversely, there may be organized fighting, but by the unrepresented classes (e.g. many of the cases of elite replacements); or even by the unrepresented classes, used as tools by members of represented classes, who fight but not essentially in their own interests (e.g. other elite replacements or cyclical resestablishments).

In short, we describe empirically quite different socio-power situations whose "stage" of evolutionary development is drastically different. Their violence symptoms—despite external physical similarities—are thoroughly different if consideration is given to the social structure, motivations, pressures, military operations, decision-making, etc., generating the killing—all of which are indiscriminately lumped in the procedures of mindless statistics but could be interestingly treated in the meaningful quantification of theoretically ordered variables of different types (also theoretically ordered) of society, informed by extant or to-be-developed theory of society and its evolution.

Taking the view of revolution defined above, Cuba's "revolution" of 1959 is a genuine one, possibly Bolivia's of 1952, and perhaps the major political changes of 1875 in Mexico when Diaz enters into power and initiates the development on a large scale of the industrial bourgeoisie and of foreign capital investment. This period of Mexican history seems to me more significant in the fundamental systemic social and economic changes brought about than the so-called revolution of the 1910's. These are the only cases I would call genuine revolutions.

Before concluding, I wish to take up some specific items.[2]

1. On p. 24, "low income" is equated with "traditional." Does this mean that low income groups like Negroes, Puerto Ricans, and Mexicans in the United States are "traditional" or does it mean that there is an institutional system—in the United States and in Brazil, for example—which maintains low incomes? My answer is that—e.g. in Brasil—it is the latter, that the low incomes relate to a highly "modern" colonialist system whereby major investment, major financing, major markets for Brazil (and for Texas vegetables raised by Texas Mexicans) are located in the United States so that a basically export-economy is maintained by the Brazilian power elites—whose incomes are *not* low. In an export—a dollar—economy, it is of major interest to the powers that be *economically* to keep wages, i.e. most incomes, low. They are not building an internal consumption market which requires improvement of real wages and greater income distribution. Thus "traditional" is, here a function of "modern" and, indeed, a part of a *single* system rather than, as treated in Bwy and others, a separate system. To equate low income with "traditional" and contrast it with the type of society characterized by high incomes as "modern" is nonsense. In fact, we have enough data to know that a great proportion of these "low income" people, e.g. significant segments of the urban "poor" (Mangin, 1967; Leeds and Leeds, 1967) are in most other senses "modern—transistorized, TV'd, mod-dressed, and all.

2. The notion of competition is assumed by Bwy and many others (cf.

his references to Phillips Cutright, Daniel Lerner, and others, p. 20) to be desirable, good and *inherently* connected with "stability" and "modernization"—e.g. "interparty" and "system competition"—Why is competition so desirable? I see no intrinsic good—or bad—in "it." Nor do I see "it" (is "it" unitary?) as performing only certain sorts of functions as is assumed by these writers. In 1964, I argued that a certain form of competition *increases* when what our authors call "development" decreases (Leeds: 1964, 1346–7). Either this is not competition (in their usage), or they are talking only about "Good Competition," or there are varieties of expression of competition under different kinds of circumstances with different functions. I favor the last view since I think it is more tightly tied to demonstrable fact and distorts our data much less than forcibly pushing data into a single view of competition.

3. I think it high time that the term "democratic" be eliminated from social science discourse and be replaced by analysis (*not*, at first, by statistical correlations which may or may not have any signification) of political systems, especially where, as so often in this and other papers, in the popular press, in the mass media, "democratic" is signalized by the presence of elections. Political science, for the most part, has shown a remarkable debility or irresponsibility in analysing the power structure of electoral systems, e.g., in the United States or more recently in Brazil (1965, 1966). Elections have been controlled in one way or another from time immemorial (Blondel: 1967). We also know, in cross-societal (Bwy's title is a misnomer, he does not deal with culture) perspective, many social systems whose popular interest articulation is as broad as or broader than many of those systems with elections using other means although it is not politically popular today to look at such systems (e.g. the *mir* and the soviets in the U.S.S.R.; the mass meetings for mass participant decision-making in Cuba). It is essential in all systems to look at the system of controls, an area in which I feel we have had little help from the majority of political scientists today, e.g. for Brazil, for Mexico (according to the myth, virtually the acme of democracy in Latin America, despite its monolithic centralized control system), the United States, East Germany, Cuba, etc., etc.

4. Finally, I consider pernicious the kinds of implicit and often explicit equations recited on p. 80 above, which can be broadly documented from the political science, the development, the American overseas sociological literatures (historians and anthropologists, on the whole, are not prone to this). Such equations should be eradicated root, stock, and branch with a deliberate, professional attempt to move towards as objective and value-free a set of premises and analytic approaches as possible. The premises cited above are pernicious in their ethnocentrism, their evangelical assurance of righteousness (Our Rightness), their imposition of clearly American norms and values on the rest of

the world. The upward mobility assumptions, for instance, provide the accepted basis for conceiving of the "dissatisfaction" resulting from frustrations with respect to rising expectations and treat as axiomatic that the frustrated want to move into positions conceived by the observer, American, to be desirable (Leeds and Leeds: 1967). One might even, in an unkind moment, call such notions intellectual imperialism.

In conclusion:

a. Bwy's paper and a class of papers like it are striking for their avoidance of useful economic, social, and political theory in defining events and meanings—the grist of their statistical mill. They particularly avoid relevant structural theory of power.

b. Bwy's paper, like so many others, strikes me as being in a hopeless methodological quandary especially in regard to definition and typology and regarding rules of correspondence between some so-called propositions and the raw, contextually-controlled field data, mostly omitted from consideration.

c. The poor methodology is covered by ostensibly good technique, the elaborate statistical operations. I have referred to the effect of numerous operations on distorting data and to the gimmicky nature of many of the so-called measures, e.g. the GINI scale.

d. Many of the statements are contradictory to known data, e.g. most cases of organized violence do not start with the poor, are not led by the poor, the frustrated in expectations, yet these figure predominantly in the discussion of frustrations and dissatisfaction. Historical data seems also at fault, e.g. on p. 20 where a temporal sequence from urbanization to literacy to mass media to voting is asserted. This seems to me largely contradicted by almost all known cases.

e. The value assumptions are so pervasive and so powerful that they give form to the entire undertaking and block of analysis, definition, and understanding of data. They give rise to a whole series of conceptual equations such as those listed on p. 80 which make impossible not only meaningful differential analysis and generalization, but also the objective understanding of any system, *including* our own.

NOTES

1. For example, tens of thousands were jailed after the coup d'état of 1964 in Brazil; their rights, their elective political positions, even their economic means of livelihood were often suppressed. Since very few persons were killed, and, given the "measures" for violence used in Bwy's and contained in the others' papers, this golpe would weigh little. The fact that the *coup* of 1964 generated a number of drastic political, economic, and social changes, depressed the real wages of the poor, and so on, cannot be treated by the highly selective, perhaps irrelevant criteria used by Bwy. The same might be said for the United States. Actually deaths from riots in the last years have been few. Beatings, jailings, and other forms of coercion have

been quite extensive, are symptomatic of the operation of the socio-political system, but do not enter into their analysis. If they did, the curves would most probably change radically. Cf. for example, the citation of Cutright and Lipset on p. 20 which links stability with economic development. Nazi Germany is nowhere discussed in this connection.

2. A number of other points should also be discussed but time and space do not allow enumeration. The strange notion of an "ideal" land distribution and the measurement of departures from this norm as a measure of inequality, as a measure of instability should be discussed. "Unequal" land distributions are universal and one wonders what possible meaning such a notion can have, or what the economic consequences of a strictly equal distribution of land would be: drastic I'll warrant! The equally strange use of "illiteracy" and basic misconceptions about it with regard to data; its necessity in social systems especially modern ones, needs examination. Third, the underlying, implicit assumption of the unilinearity of evolution of political systems—ending up in Ouramericanway of politics should be examined. The built-in upward mobility assumption I have mentioned before; it, too, needs raking over. Finally, for every positive hypothesis presented and "tested" an equivalent negative hypothesis ought to be formulated and tested. I am willing to bet that many of the positive ones will turn out to be insignificant.

BIBLIOGRAPHY

BLONDEL, JEAN

N.D. As Condições da Vida Política no Estado de Paraiba. Getúlio Vargas Foundation. Rio.

JACK, ALEXANDER

1967 The Hidden Doves of Vietnam—A Failure of the U.S. Press. The Register Leader. Boston, Mass.

JOHNSON, CHALMERS

1964 Revolution and the Social System. Hoover Institution of War, Revolution and Peace. Stanford University, California.

LEEDS, ANTHONY

1962 Borderlands and Elite Replacement. Paper presented at the Annual Meeting, American Anthropology Association. Chicago.

1964 Brazilian Careers and Social Structure: An Evolutionary Model and Case History. American Anthropologist, 66:1321–1347.

———— and ELIZABETH LEEDS

1967 Brazil and the Myth of Urban Rurality: Urban Experience, Work, and Values in Squatments of Rio de Janeiro and Lima. Paper presented at Conference on Work and Urbanization in Modernizing Societies, St. Thomas, Virgin Islands.

LEISERSON, ALCIRA

1966 Notes on the Process of Industrialization in Argentina, Chile, and Peru. Institute Of International Studies, University of California, Berkeley.

MANGIN, WILLIAM

1967 Squatter Settlements: Peru's Shantytowns. Scientific American 217:21–29. New York.

MARX, KARL and FRIEDRICH ENGELS

(1846) The German Ideology (1846). New York.
1947

NATIONAL DIRECTORY OF LATIN AMERICANISTS

Second Edition in Preparation

The Hispanic Foundation in the Library of Congress is preparing for publication the second edition of the *National Directory of Latin Americanists.* The volume lists persons in the social sciences and humanities who have specialized knowledge related to Latin America and provides bio-bibliographical information on such individuals. The second edition will include up-dated data on those listed in the 1966 *Directory* and information on additional specialists who now qualify for inclusion.

If you would like to become a candidate for inclusion in the *National Directory,* please write to:

DR. HOWARD F. CLINE
Director
Hispanic Foundation
Library of Congress
Washington, D.C. 20540

Persons seeking additional information are also invited to send their inquiries to the above address.

Copies of the 1966 *Directory* (LC 24.7:10) may be obtained from the Superintendent of Documents, Government Printing Office, Washington, D.C. 20402, for $2.00 in check, money order, or Superintendent of Documents coupons.

https://doi.org/10.1017/S0023879100028806 Published online by Cambridge University Press