



Measuring Political Violence and Land Inequality in Central America

Author(s): Charles D. Brockett

Source: *The American Political Science Review*, Vol. 86, No. 1 (Mar., 1992), pp. 169-176

Published by: [American Political Science Association](#)

Stable URL: <http://www.jstor.org/stable/1964022>

Accessed: 12/01/2011 14:39

Your use of the JSTOR archive indicates your acceptance of JSTOR's Terms and Conditions of Use, available at <http://www.jstor.org/page/info/about/policies/terms.jsp>. JSTOR's Terms and Conditions of Use provides, in part, that unless you have obtained prior permission, you may not download an entire issue of a journal or multiple copies of articles, and you may use content in the JSTOR archive only for your personal, non-commercial use.

Please contact the publisher regarding any further use of this work. Publisher contact information may be obtained at <http://www.jstor.org/action/showPublisher?publisherCode=apsa>.

Each copy of any part of a JSTOR transmission must contain the same copyright notice that appears on the screen or printed page of such transmission.

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.



American Political Science Association is collaborating with JSTOR to digitize, preserve and extend access to *The American Political Science Review*.

<http://www.jstor.org>

MEASURING POLITICAL VIOLENCE AND LAND INEQUALITY IN CENTRAL AMERICA

CHARLES D. BROCKETT *University of the South*

The value of cross-national quantitative studies of the relationship between mass political violence and land inequality is challenged along three lines. First, gross and systematic errors in the political violence data of the World Handbook of Political and Social Indicators (the usual data source for empirical studies) render them worthless for Central America at least and probably much of the Third World as well. Second, conceptualizations of land inequality have been too simplistic to be of much theoretical value. Third, the temporal nature of this relationship has been inadequately considered. Responding to such deficiencies, I elaborate a broader understanding of land inequality and provide a fuller discussion of the temporal nature of its relationship to political violence. Throughout, the five nations of Central America are utilized for appropriate case material.

Land is the most important resource in rural societies, and access to sufficient land is invariably the fundamental desire of most peasants. Accordingly, it has long been thought that the maldistribution of land is the key determinant of rural unrest, particularly unrest in its most dramatic forms, rebellion and revolution. Land maldistribution can have different manifestations. The most common aspects cited are landholding patterns so concentrated that either (1) many peasants are left with *minifundios* too small for family support (the *minifundización hypothesis*) or (2) that many peasants own no land at all (the *landlessness hypothesis*). In the most current literature the first position is argued by Midlarsky (1988) and the second by Prosterman and Riedinger (1987).

However, Muller and Seligson claim that this conventional wisdom is wrong (1987, 433-34). Their cross-national longitudinal data analysis of 85 states finds both that landlessness is irrelevant as an explanation for political violence and that land inequality (as measured by the Gini index) has only weak predictive value. Instead, they discover a stronger relationship for mass political violence with national income inequality (again measured by the Gini index).¹ They conclude, therefore, that agrarian inequality is relevant to mass political violence "only to the extent that it is associated with inequality in the nationwide distribution of income" (p. 443).

This controversy has important policy ramifications, as the various authors point out. Land redistribution (and associated support programs) is often promoted as a prophylactic against rural disorder and instability.² Muller and Seligson's sophisticated analysis appears compelling, thereby questioning the adequacy and wisdom of land redistribution. However, in reality, their study is flawed because of the meaningless data used to measure their dependent variable, mass political violence. The same flaw also negates Midlarsky's analysis, as in fact it does most empirical data analyses on this subject.³ Their shared deficiency is their reliance on the grossly inaccurate

data set provided by the *World Handbook of Political and Social Indicators* (Taylor and Jodice 1983). In addition, I shall also argue that previous studies are inadequate in their conceptualization of the independent variable. Land inequality cannot be adequately captured in a single dimension no matter how sophisticated the measure but must be understood instead as multidimensional in nature.⁴ It is my argument, then, that the empirical relationship between land inequality and political violence remains undetermined.

MEASURING MASS POLITICAL VIOLENCE

February [1977] An army base is set up near San Juan Cotzal [Guatemala]. Since 1976, killings have included 68 cooperative leaders in the Ixcán, 40 community leaders in Chajul, 28 in Cotzal, and 32 in Nebaj. In one community, the presidents of Catholic Action, Caritas and the local cooperative and community development committee, as well as five sacristans and four bilingual teachers, have been killed. (Davis and Hodson 1982, 47)

Guatemalan deaths from political violence, 1976-77, according to the *World Handbook*: 0.

The most complete and most widely used cross-national longitudinal data set on political protest and violence is provided by the *World Handbook of Political and Social Indicators* (Taylor and Jodice 1983). A prodigious undertaking, providing annual events data for 1948-77, it is not good enough. The data are mortally flawed for Central America and (I suspect) for the rest of Latin America and the Third World more generally.

The primary source for the data set is the *New York Times Index*, supplemented for Latin America by *Keesing's Contemporary Archives*. Taylor and Jodice point out (p. 12) that *Keesing's* draws from nine basic sources (all European, with the exception of the *New York Times*), supplemented, for Latin America, with

six additional sources—again, not located in Latin America itself.

Taylor and Jodice give some consideration to the accuracy of their data set, but this attention is minimal.⁵ They do mention the alternative of utilizing national newspapers, which would, of course, be inordinately expensive (1983, 11, 181). Their defense rests largely on a study by Jackman and Boyd (1979) of the costs and benefits of using multiple sources to measure political conflict. Jackman and Boyd's sample is 30 African countries during the 1960–67 period.⁶ Jackman and Boyd conclude that their "results provide good support of [the 1972 *Handbook's*] claim that increasing coverage beyond two sources is not likely to result in substantial changes in the 'overall picture'" (p. 457).

Well, yes and no. It depends on whether *overall picture* refers to the accurate measurement of what happened in African (or Central American) country X or to the score obtained from using additional sources of the type already used for these data sets. Jackman and Boyd's "multiple sources" for Africa are (in addition to the primary source of the *New York Times Index*), *Keesing's*, *Facts on File*, *Africa Diary*, and *African Recorder*. The latter two are claimed as "regional sources," but both are published in New Dehli. None of their "multiple sources" are African.⁷ It is not surprising, therefore, that Jackman and Boyd discover "that the cost-benefit ratio from using multiple sources may be unnecessarily high" (1979, 456).⁸

Obviously, cost factors cannot be ignored; it is certain that to incorporate truly regional—if not national—sources into the *Handbook* would present tremendous, and perhaps insurmountable, difficulties. However, the validity of the data set rests not on financial considerations but on accuracy. In response to criticisms of "imprecision" in the 1972 edition of this data set (as well as in others), Gurr has argued that "the kinds of inferences drawn in quantitative macropolitics are not likely to be affected by anything less than gross and systematic error" (1974, 250). And that is precisely the charge. If valid, this claim has serious implications. As Lichbach has noted, the *World Handbook's* indicator has been settled upon by scholars as the common dependent variable in cross-national statistical studies of political conflict (1989, 451).

On November 23, 1974, National Guard troops with tanks and bazookas attacked a group of peasants in La Cayetana, San Vicente, [El Salvador,] killing six and arresting twenty-six (later thirteen were found to have disappeared).⁹ These peasants had occupied sixty *manzanas* of idle land belonging to an absentee owner, after repeated attempts to rent the land had failed. . . . By some accounts, the military met . . . and decided that to allow the land occupation would be setting a dangerous precedent, and so they decided to attack the area. (Berryman 1984, 110)

New York Times and *Keesing's*: event not reported.

The *Handbook* grossly understates Central American deaths from political violence. For example,

standard estimates for deaths and disappearances from political violence in Guatemala for 1966–68 are 3 thousand to 8 thousand, with another 15 thousand for 1970–75 (Brockett 1988, 106–7); the *Handbook* gives only 207 deaths for the overall period.¹⁰ Similarly, for Nicaragua, it has been estimated that over a thousand peasants were killed during the 33 months of martial law beginning at the end of 1974 (Brockett 1988, 168); the *Handbook* gives only 59 deaths for the encompassing five-year period. Although the death toll in El Salvador was far less for the period under examination than in the first two countries, those acquainted with Salvadoran events will agree that the 22 deaths reported by the *Handbook* for 1973–77 substantially underreports the tragic reality.¹¹ Even allowing for wild exaggerations in the "standard" estimates for these countries, the *Handbook* is clearly in gross error.¹²

Probably the most striking anomaly in the data set is that the *Handbook* reports political deaths in Honduras during the mid-1970s as twice as numerous as in either El Salvador or Guatemala! In reality, Honduras would rank fourth with Guatemala far ahead for all five. The unavoidable conclusion, then, is not just that the *Handbook* scores seriously underreport various countries but, more seriously, that a more accurate reporting would alter both the rankings between countries and the magnitude of the intervals between country scores, thereby substantially altering quantitative analyses utilizing this data set.

There are undoubtedly systematic factors at work producing the gross errors in the data reported by the *Handbook's* sources. Political violence directed at peasants in remote sections of countries without permanently stationed *New York Times* correspondents is much less likely to be reported than urban violence in countries with permanent correspondents. Simply said, remote rural disappearances are harder to tabulate than assassinations in city streets. Other factors are at work, as well. The *Handbook's* Honduran data for 1975 are surprisingly complete.¹³ This was probably a coincidence, however, resulting from a systematic bias. The Honduran "bananagate" scandal of 1975 reached right into Wall Street, generating unparalleled coverage of Honduran affairs in the U.S. press.¹⁴ As a result, the six people killed in June of that year at a peasant training center received mention by the *New York Times*, although the six peasants killed in 1972 in La Tlanquera were missed, their deaths having preceded the scandal.

Finally, there can be discrepancies between the *New York Times Index* and the news story itself:

While a worldwide television audience saw El Salvador's sunny beaches before the "Miss Universe" finals July 19 [1975], off-camera heavily armed troops were called out to halt demonstrations by students protesting the Government's expenditure of \$4-million on the contest. . . .

Then on July 30 about 3,000 students demonstrating in San Salvador against repression of . . . earlier marches were stopped by machine-gun and automatic-rifle fire from soldiers.

According to the military Government, which con-

TABLE 1

Land Inequality and Disruption Potential

MEASURES OF LAND INEQUALITY	COSTA RICA	EL SALVADOR	GUATEMALA	HONDURAS	NICARAGUA
1. Gini index of land concentration	.81	.81	.82	.78	.80
2. Smallholders ^a as % of all operators	43	74	87	64	51
3. Average size of smallholder farm (hectares)	1.7	1.2	1.8	1.9	2.6
4. Minifundización score ^b	25	62	48	34	20
5. % of all land in largest farms ^c	54	64	69	56	54
6. Landless score	20	36	32	32	27
7. Inequality score ^d	99	162	149	122	101
8. % rural population	59	62	66	72	51
9. Relative rural disruption potential ^e	59	100	99	87	52

Sources:

Row 1: Muller and Seligson 1987, Table A-1 for early 1970s except Guatemala (1964) and Nicaragua (1963).

Rows 2–3: Muller, Seligson, and Fu 1989, Table 2 for early 1970s except Guatemala (1964) and Nicaragua (1963); substituting data for Guatemala 1979 and Nicaragua 1971 made little difference.

Row 5: Brockett 1988, Table 4.1; also United Nations, Food and Agriculture Organization 1981, Table 3.3; for further discussion, see my n. 20.

Row 6: Prosterman and Riedinger's data for early 1970s (as reported by Muller and Seligson 1987, Table A-1) does not include Honduras or Nicaragua; Prosterman and Riedinger's later efforts (1987, Table 2) are for the early 1980s (except for El Salvador and Nicaragua, which are for the period just prior to their reforms). The data used here are an estimate for the mid-1970s, using the midpoint of the two data sets (and the mid-point for the 1987 data set where a range was given). Mid-1970 scores for Honduras and Nicaragua were estimated by reducing the score for the later date by the same amount as the average of El Salvador and Guatemala (Costa Rica is less comparable).

Row 8: United Nations 1976, 171–72 for early 1970s.

Notes:

^a Smallholders are those with five hectares or less.

^b Row 2 divided by row 3.

^c Amount of total land held by largest 3.5–4% of farms in early 1970s (1979 for Guatemala). Cut-off sizes in hectares: Costa Rica, 200; El Salvador, 20; Guatemala, 22.4/44.8; Honduras, 50; Nicaragua, 345/690.

^d Sum of rows 4–6.

^e Row 7 times row 8.

tended that the march was part of a "Communist plot," one person was killed, five wounded and 11 arrested. But according to the students, at least 12 persons were killed, 20 wounded and 40 arrested. Witnesses said that about 50 persons, some apparently dead and others bleeding, had been taken away in army ambulances and trucks.¹⁵

The index lists only the one death claimed by the Government in the July 30 shootings, ignoring the higher number reported by witnesses.

Nonetheless, if the Central American data for 1948–77 were to be corrected, the political violence scores of Honduras and El Salvador would probably group together when placed alongside the much higher score of Guatemala. This raises at least two major theoretical questions: (1) What is the relationship, if any, between the political violence rates of the past and the future? (the *Handbook's* 30-year data set ends in 1977—just when violence in El Salvador escalated to the tragic level of Guatemala)¹⁶ and (2) What do these country groupings mean for the purported relationship between land inequality and mass political violence? Was the agrarian structure of El Salvador more similar to that of Honduras than to Guatemala prior to 1977? Did El Salvador's rural inequality then change, precipitating the violence that has now persisted for more than a decade?

MEASURING LAND INEQUALITY

Land inequality is often just measured by the Gini index,¹⁷ which is so simplistic and misleading that it is of limited utility for meaningful cross-national comparisons. Indeed, as Table 1 indicates, the Gini land inequality scores for the Central American countries are virtually identical. For such reasons, Midlarsky (1988, 1989) argues, instead, for a measure of "patterned inequality," by which he means a comparison of the pattern of concentration of land holdings between smallholders and largeholders. In turn, however, Muller, Seligson, and Fu convincingly demonstrate that Midlarsky's measure is "interesting conceptually but so flawed in construction as to be of little utility" (1989, 586).¹⁸ They create instead an "index of bifurcated inequality." This measure is constructed from the proportion of small farms and from the average sizes of small farms compared to large farms.

Both sets of authors are attempting to measure what might be termed *minifundización*, that is, to devise a measure that captures the discontent of the smallholder without enough land to support a family yet confronts other farms that are of vast and "unjust" size. The Muller, Seligson, and Fu approach appears adequate for this purpose on the lower end

of the land distribution scale. However, it would seem that what is most relevant to the smallholder about large estates is not their average size but rather the extent to which they dominate the local land supply. Beyond a certain size, an "unjustly" huge farm is just that—a large farm unjustly dominating the land supply, be it four hundred or four thousand hectares in size. Furthermore, the subdivision of large farms within the elite (by inheritance, sale, or subterfuge to avoid land reform laws) has no effect on land inequality from the perspective of the smallholder but could have a substantial impact on the bifurcation measure if the subdivisions were sufficiently prevalent. Accordingly, the share of total agricultural land dominated by the largest farms would be a better indicator of the role of the upper end of the land distribution scale in creating land inequality and peasant discontent than is the average size of "large" farms.¹⁹

Unlike the Gini index, these measures indicate substantial differences between the Central American countries. As row 2 of the table shows, *minifundios* are much more prevalent in Guatemala and El Salvador—in fact, twice as prevalent in the former than in Costa Rica. Similarly, the countries vary in the average size of the smallholder's land. Row 4 reports a score created by combining these two measures, which demonstrates *minifundización* to be the worst in El Salvador, followed by Guatemala and then Honduras, with Costa Rica and then Nicaragua grouped at the bottom. At the top end of the land distribution scale, essentially the same rankings obtain: Guatemala is closely followed by El Salvador, with the greatest concentration of land in the largest estates, and Nicaragua and Costa Rica with the least.²⁰

Land distribution data are important indicators of rural inequality but are insufficient by themselves because of the increasing prevalence of landlessness among peasants. In fact, the landless of Central America constitute about one-third of the total population (prior to the agrarian reforms of the 1980s), with the exception of Costa Rica. Prosterman and Riedinger (1987, 10–11, 25) define landlessness as the lack of "ownership or ownership-like rights" among "those who cultivate the land." The landless include tenants, since they lack secure use of the land and face landlord extractions, amounts over which they usually have little control. The figures reported in Table 1, however, mask significant differences in the situation of the landless. Tenants vary widely in the security of their tenancy and the income-generating potential of "their" land. It is probable, for example, that the higher landless score of Honduras is less a source of intense discontent than the lower score of Guatemala, where the seasonal migration of Indians from the highlands to work on plantations was the largest migratory-labor-force-as-a-percentage-of-total-population in the world during the 1970s—and under extraordinarily oppressive circumstances (Paige 1975, 361).

These various indicators can be combined into an overall measure of land inequality. Row 7 reports

such an inequality score, which combines, with equal weight, the three dimensions: *minifundización*, land concentration in the largest estates, and landlessness. By this measure, the worst land inequality in the early 1970s in Central America was found in El Salvador and then Guatemala, with a larger gap between them and Honduras, followed (by a large gap, again) with Nicaragua and Costa Rica. If land inequality translates into political action and conflict, it might be argued that the relative size of the rural population could be an important factor. Accordingly, row 9 takes this factor into consideration, giving a score for "the relative rural disruption potential" (with largely the same country rankings but a closing of the gap between the top three countries). Regardless of which score is used, the essential point is that a multidimensional measurement of land inequality demonstrates substantial differences between the five countries of Central America.

Devising adequate measures of land inequality, however, takes us only a short distance, since it is unlikely that there is any direct relationship between land inequality and political violence. A major weakness of statistical analyses of comparative political violence is that they are largely devoid of any theoretical link between macro-economic processes and structures and either individual or collective behavior.²¹ For example, much of the case literature suggests that changes for the worse in economic status are more likely to lead to discontent and violence than would a constant misery.²² It is conceivable, then, that countries and regions with high but stable levels of land inequality might be less prone to rural unrest than would others with lower levels of land inequality but where the economic security of the peasantry is markedly deteriorating.

BRINGING IN SOCIAL COMPLEXITY

To make the point more concretely, imagine two countries with similar land distribution patterns and landlessness rates. In country A, the agrarian system has remained fairly stable for decades; but high population growth rates have steadily driven up the levels of landlessness. Country B also experiences population growth; but, in addition, the commercialization of agriculture has rapidly transformed its agrarian system. Profit-motivated commercial farmers in country B use their superior resources (monetary and coercive) to dispossess peasants of lands to which they had long enjoyed ownershiplike use and to break long-term tenancy relationships. In both countries there will be mass poverty, but deprivation alone seldom leads directly to mass mobilization. In country B, which is the superior approximation of the Central American reality, discontent has a target.²³ It is not so much poverty alone, Prosterman and Riedinger point out, "as it is *blamable* poverty that seems to serve as a predictor of violence" (1987, 9). When the disadvantaged live in dangerous political environments, assertion of their demands is risky. To

overcome this discouraging environment, mass collective action usually requires “not only a sense of misery but also a sense of outrage and injustice” (Huntington and Nelson 1976, 102).²⁴ A good illustration of this point is provided by White’s explanation of peasant mobilization in Honduras:

The brutality of many of these evictions [of semisubsistence farmers by larger landholders] proved to be the catalyst in breaking down the friendly dependence on helpful patrons and developed a profoundly emotional opposition. The evictions were the sudden, sharp deprivation which moved campesinos to risk their lives in organizing to counter rural elites and protest before government authorities. . . . It was the small farmers who became the leaders in the national mobilization of campesinos. The evictions or other means of blocking access to land to which campesinos thought they had a legitimate right also became the basis for the land occupations or recuperations (1977, 181–82).

A final difficulty in assessing the relationship between land inequality and mass political mobilization and violence is the temporal dimension of this relationship. Do changes in the level of rural discontent resulting from growing inequality promptly cause corresponding changes in the level of mobilization and/or violence or might that effect be delayed? More concretely, high levels of peasant mobilization and resulting rural conflict were reached in Honduras in the late 1960s, about half a decade prior to similar mobilization and conflict in Guatemala, itself preceding those of El Salvador and then Nicaragua.²⁵ To what extent do these country rankings correlate with differences in levels of land inequality?

Although critical, the temporal dimension of the relationship between land inequality and political violence has received little theoretical attention. Midlarsky, for example, ignores the question except for a passing reference to the “generally slow” development of antiregime organizations (1986, 498–99).²⁶ If there is a long lag between the generation of inequality-based discontent and of social mobilization (and, therefore, political conflict and violence), then the correlation between inequality and violence is confounded (and many data analyses, as well).²⁷ If we postulate that there is a long lag, that is, that country C and country D have the same level of inequality, but that the inequality is of more recent origin in country D, then it follows that we would expect lower levels of violence in country D. However, why should we even hypothesize a temporal gap between the origins of discontent and of political mobilization and/or violence? And might whatever gap that does exist vary between countries because of the influence of other variables?

To return to the example of Central America and the development of high levels of peasant mobilization in Honduras prior to other countries—in actuality, the postwar agrarian transformation accelerated *last* in Honduras; that is to say, in Honduras there was little delay in time between the generation of discontent based on increasing land inequality and peasant mobilization and political conflict—certainly

less of a gap than in the other countries. In contrast, El Salvador entered the postwar period with the most unequal agrarian system of the five; but its rural mobilization lagged behind that of Honduras, and its political violence rate was far below that of Guatemala until the end of the 1970s.

The countries of Central America, then, provide wide variations in the temporal relationship between land inequality and violence. The critical intervening variable that explains these variations is the political context. Largely ignored in earlier research, the critical conditioning role of political variables has fortunately received growing emphasis in recent studies.²⁸ Although too broad a subject to include here, the inclusion of the political context would explain the confusing relationships between land inequality and political violence found in the Central American cases, such as the very different relationship between the two variables for El Salvador and Honduras, as well as high enough levels of political violence in Nicaragua in the last third of the 1970s to place it with El Salvador and Guatemala, when its land inequality score was more like that of Costa Rica.²⁹

CONCLUSION

This research note has discussed a number of hypotheses concerning the relationship between land inequality and mass political violence. Empirical social science teaches well the importance of rigorous hypothesis testing with the appropriate evidence. Regrettably, it sometimes unintentionally reminds us of the necessity of ensuring the validity of the empirical data utilized for such testing. If the *World Handbook’s* data set provided reasonably accurate measurements of the cross-national levels of mass political violence, it would allow for the meaningful testing of the relationships I have examined. However, the *World Handbook* cannot be used for this purpose. The data for at least Central America are grossly inaccurate. Since the data set is not valid for this region, the burden of proof falls on future users to demonstrate that it is sufficiently accurate for the regions of their study. The appropriateness of this data set for cross-national studies that wish to generalize globally, however, is another matter. Since at least part of the *World Handbook’s* data is the product of gross and systematic error, valid cross-national statistical tests of global hypotheses and theoretical models concerning political violence must await the construction of better data sets.

The compilation of the *World Handbook’s* data set on political protest and violence was an extraordinary undertaking and accomplishment; but by the standard of accuracy, it was an unjustifiable shortcut for Central America and (probably) for the rest of the Third World. The construction of reasonably accurate data sets for these countries will probably have to be done on the basis of a substantial division of labor, with small regions researched by different scholars. Instructive for these future efforts would be Tarrow’s

(1989) use of national newspapers for his study of Italy and Tilly, Tilly, and Tilly's (1975) use of truly multiple sources in their comparative study of France, Italy, and Germany. Of course, as we move from institutionalized democracies to most Third World countries, censorship becomes a major obstacle to the development of accurate data sets.³⁰ A notable attempt to address this problem for a Central American country through the use of both national and international sources is the chronology of political violence in the Guatemalan highlands for 1976–82 compiled by Davis and Hodson (1982). They utilized five Guatemalan and four Mexican newspapers; one Costa Rican newspaper; North American newspapers and magazines; and reports from human rights, governmental, and religious organizations.

Future scholarship also must take social complexity into account better than heretofore. First, land inequality is multidimensional, involving, at a minimum, landlessness, insufficient land, and land concentration. Second, a more general concept of *rural inequality* would add to these dimensions other factors, such as income inequality and the security of tenancy and paid labor arrangements. Third, subjective experience is critical to mobilization. Similar levels of inequality can correlate with various levels of political mobilization and violence because of differences in the perception of the sources of that inequality. The available case material suggests that the most explosive situations arise when peasants believe they have been "unjustly" dispossessed of land. Finally, there is no direct relationship between rural discontent based on the perception of unjust inequality with levels of peasant mobilization, political conflict, and political violence. Instead, this relationship is mediated by the always-changing political context.

Notes

An earlier version of this study was presented at the fifteenth international congress of the Latin American Studies Association, Miami, 1989. I thank Mitchell Seligson for his helpful comments.

1. Midlarsky (1988, 503–4) appropriately objects that the Gini index is an invalid measure of what I have called here the *minifundización* hypothesis, of which his "patterned inequality" hypothesis is an example. Additional objections to the Gini index are provided by Prosterman and Riedinger (1987, 24–25). Midlarsky's debate with Muller and Seligson continued in more recent publications.

2. The underlying arguments vary from the inherent value of stability, to reform as preemptive of more radical movements and changes, to stability as the by-product of doing what is just (i.e., reallocating to peasants what they deserve).

3. These two studies are among the best of those that have examined the relationship between economic inequality and political conflict. For critical reviews of this literature, both highlighting the disappointing lack of theoretical progress, see Zimmerman 1980 and, almost a decade later, Lichbach 1989.

4. Although the focus here will remain on land inequality, a fuller account of the sources of rural violence would include other dimensions of rural inequality, such as income. There is, of course, a thriving literature on the socioeconomic determinants of rural collective action, among them Mc-

Clintock 1984, Migdal 1984, Paige 1975, Popkin 1979, Scott 1976, Tutino 1986, and Wolf 1969.

5. Earlier editions of the *Handbook* were more tentative about the data set's accuracy (Russett et al. 1964, 97–98; Taylor and Hudson 1972, 64–65, 417–423).

6. Jackman (1978) uses this data for a quantitative analysis of the determinants of African coups.

7. The great preponderance of the *African Recorder's* sources are Indian and Western European (particularly British). A quick check of the first four issues of 1962 did find some reports from Africa-based newspapers, all from Nairobi (except one from Dar es Salaam). The first four issues of 1988 were even more European-based. In a sample of six scattered 1961 issues of the *Africa Diary* close to half of the sources were African, but 61% of these were from two newspapers, one in Accra, the other in Nairobi. In addition, with the exception of Ghana and Kenya, coverage ran heavily to official travels and government announcements. For both sources for this analysis, Northern Africa and South Africa were ignored.

8. An additional deficiency of Jackman and Boyd's study is that they test only whether source differences affect hypothesis testing, but, examination at this gross level of aggregation could mask wide variations in country scores between sources. Precisely this problem had been demonstrated earlier by Doran, Pendley, and Antunes (1973); but Jackman and Boyd do not address this aspect of the earlier study. The Jackman and Boyd study neither compares its five sources for variations in their annual scores for a particular country nor, more importantly, does it compare differences between these five sources and other, more authoritative, sources. For example, while the relevant *Handbook* score mirrors the Burundi massacre of 1972, its report of 106 deaths in Burundi for 1963–67 obviously misses the massacre of some 2,500 to 5,000 Hutu in 1965 (Kuper 1981, 63).

9. The names of the murdered and of 10 of the disappeared can be found in the congressional testimony of Fabio Castillo, former president of the National University of El Salvador (U.S. House, Committee on International Relations 1976, 41).

10. A small story reporting the Amnesty International charge of some twenty thousand deaths and disappearances in Guatemala during the prior decade appeared in the *New York Times* on 12 December 1976. Although it is obvious that this kind of data could not be included in the events data set, it is curious that it did not lead to a reexamination of the validity of the Guatemalan data compiled for the *Handbook*.

11. To his credit, Midlarsky (1988) realizes that there are serious problems with the Central American data. His "solution," however, is inadequate. He uses the entire 1948–77 data set, dividing the Latin American countries into quintiles. However, because "El Salvador and Nicaragua . . . are known to have experienced intense violence in this period," they are artificially moved into the top quintile (El Salvador all the way from the bottom). Guatemala, where the most deaths actually occurred, is left in the third quintile (when it belongs in the first); nor are the scores of any of the other countries reconsidered. In response to the excellent critique by Muller, Seligson, and Fu (1989) of his handling of the dependent variable, Midlarsky (1989, 589) refers to the "arguments developed" in note 7 of his 1988 article. These "arguments" however, consist solely of a quick reference to LaFeber's *Inevitable Revolutions* (1983). The cited page in LaFeber makes no reference to El Salvador at all; its discussion of Nicaragua is in terms of the tens of thousands of deaths in the war against Somoza, almost all of which occurred after 1977, that is, after the end date of the data set.

12. If the data are erroneous for one region, similar problems would presumably be found elsewhere as well. Just a quick glance at the annual country scores shows the following examples of underreporting of political deaths: Chile 1973, 537 deaths; Brazil 1968–72, 36; and Uruguay 1973–77, 14.

13. The data for 1976, however, are another matter. The *Handbook* reports 15 Honduran deaths for 1976. The only relevant *New York Times* story (24 July, 1976) lists 14 deaths—but of Salvadoran soldiers killed in a border conflict with Honduran armed forces. The *Keesing's* edition of 30 April,

1976 (p. 27708) does list 15 peasants killed in Honduras; however, the deaths are reported as having occurred on 8 November 1975 (and are already included in the *Handbook* total for 1975). In fact, the process by which *Keesing's* was utilized by the compilers of the *Handbook's* scores is a mystery. For example, the *Handbook* reports 22 deaths for Guatemala in 1973; the *New York Times*, 17; so *Keesing's* (6–12 August, 1973, p. 26025) total of 22 must have been utilized. Yet in the following year *Keesing's* (18–24 March, 1974, p. 26419) mentions 8 Guatemalan deaths but the *Handbook* only lists 1. Similarly, the *Handbook's* total for El Salvador during 1974–75 is only 3 political killings but *Keesing's* (10 September, 1976, p. 27938) lists 26 killings just for November 1974–September 1975.

14. The *New York Times'* first 1975 story on Honduras appeared on 10 April with the U.S. Security and Exchange Commission's charge that United Brands (the successor to United Fruit) had paid a \$1.25-million bribe to the Honduran president to obtain favorable tax treatment on its banana exports. In April and May, the *Times* ran some 24 stories related to Honduras, all involving the scandal and its aftermath. The rest of the year had 17 more stories, including several concerning the escalating rural unrest in the country. During the previous three years the *Times* had run only nine stories on Honduras, none of which involved the considerable unrest in the countryside. Meanwhile in 1975, the rest of Central America was largely ignored in the *Times*. For El Salvador there were but three stories and for Guatemala only one. Costa Rica did have nine articles; but five were scandals related to the United States, and the remainder were short pieces. Similarly, four of Nicaragua's seven stories ran in January following the audacious Christmas party attack by the Sandinistas; and two of the remainder concerned the new U.S. ambassador.

15. "Unrest Growing in El Salvador" *New York Times*, 10 August 1975 (emphasis added). Other sources give a higher death toll—for example, Berryman at least 20 dead (1984, 111); Montgomery at least 37 dead and "several dozen" more disappeared (1982, 89), Webre at least 37 dead and "many more" disappeared (1979, 189).

16. One presumes that it was this juxtaposition that led Midlarsky (1988) to arbitrarily alter El Salvador's place in his ranking of the Latin American countries on his political violence scale.

17. Muller and Seligson, for example, use the Gini index for measuring inequality in the distribution of land, which is then combined with the proportion of the labor force employed in the agricultural sector to yield an "agrarian inequality" score (1987, 435).

18. Indeed, this statement can be aptly applied to the whole study. Most of its weaknesses are identified by Muller, Seligson, and Fu (1989). Although still critical, Lichbach sees more merit in Midlarsky's work, praising it as "the most sophisticated and relevant formal models of the [economic inequality-political conflict] nexus" (1989, 452).

19. Another advantage is that this approach avoids the impossible task of deciding what constitutes a "large" farm. For a discussion of these difficulties, see Muller, Seligson, and Fu 1989, 580–81.

20. The decision of how to operationalize "the largest farms" was an artifact of the data sources. The available land distribution data are reported by size categories, whose number varies between countries but usually around 10–12. The procedure was to find breaks between categories that yielded approximately the same cutoff point for "the largest farms" for each country (which turned out to be between the largest 3½–4% of all farms). This could be done for three of the countries, but the percentage of land held by the largest farms had to be extrapolated for two (Guatemala and Nicaragua). As note c to Table 1 indicates, the cutoff point in hectares varied substantially between countries—another manifestation of the large difference between them in the extent of land concentration.

21. Lichbach discusses this deficiency insightfully (1989, 448–55).

22. Eckstein is a recent example of the argument that "economic relationships, especially changing economic relationships [are] the principal cause of protest and pressure for change" (1989, 4; also 16–17).

23. Midlarsky accounts for *minifundización* almost entirely in terms of population growth (1988, 494–95). He gives virtually no consideration to the other factors discussed for country B, which scholars have identified as crucial to accounting for the political conflict in the region (Brockett 1988; Williams 1986).

24. Similarly, see Gurr 1970, 155–231; Jenkins 1985, 5–6; McAdam 1982, 34; Moore 1978, 459–71; and Scott 1977, 236.

25. These generalizations are not based on a quantitative ranking but, rather, a familiarity with the relevant literature (see Brockett 1988). This ranking disregards state terrorism directed at largely unorganized peasants as part of counter-insurgency campaigns (i.e., Guatemala in the mid-1960s and Nicaragua in the mid-1970s).

26. This hypothesis has no bearing on Midlarsky's empirical test, which completely overlooks the temporal relationship of his variables, thereby confusing the causal connection between them. His dependent variable is the total number of deaths from political violence for 1948–77 (as compiled by the *Political Handbook*). His independent variable is constructed from the earliest data available on land concentration patterns. For Costa Rica, the date of the independent variable data comes after virtually all of the political killings; for Honduras and Nicaragua, it is approximately simultaneous with the date of the midpoint of the killings; while for El Salvador and Guatemala, the date of the independent variable data precedes most of the killings. What, then, is the theoretical relationship between the two variables? Muller and Seligson (1987) are better; their dependent variable is for 1973–77, and they try to get their independent variable data from as close to 1970 as possible.

27. In a commentary on an earlier study by Muller, Weede notes the absence in the literature of any "theoretical defense of lagging or not lagging" (1986, 438). Also see the more general, and still relevant, theoretical critique by Gurr and Duvall (1973, 136).

28. In addition to Muller and Seligson 1987, among the best examples are McClintock 1984 and Midlarsky and Roberts 1985.

29. For an explanation of these differences, see Brockett 1991.

30. Illustrative of the problem is Lindenberg's discovery concerning social discontent coverage in Central American newspapers: "Precisely at the moments when historical sources indicate that there was the most discontent, evidence of the volume of that discontent disappears in countries whose newspapers were censored." (1990, 405).

References

- Berryman, Philip. 1984. *The Religious Roots of Rebellion*. Maryknoll, NY: Orbis.
- Brockett, Charles D. 1988. *Land, Power, and Poverty: Agrarian Transformation and Political Conflict in Central America*. Boston: Allen & Unwin.
- Brockett, Charles D. 1991. "The Structure of Political Opportunities and Peasant Mobilization in Central America." *Comparative Politics* 23:253–74.
- Davis, Shelton H., and Julie Hodson. 1982. *Witnesses to Political Violence in Guatemala: The Suppression of a Rural Development Movement*. Boston: Oxfam America.
- Doran, Charles F., Robert E. Pendley, George E. Antunes. 1973. "A Test of Cross-national Event Reliability: Global Versus Regional Data Sources." *International Studies Quarterly* 17:175–204.
- Eckstein, Susan, ed. 1989. *Power and Popular Protest: Latin American Social Movements*. Berkeley: University of California Press.
- Gurr, Ted R. 1970. *Why Men Rebel*. Princeton: Princeton University Press.
- Gurr, Ted R. 1974. "The Neo-Alexandrians: A Review Essay

- on Data Handbooks in Political Science." *American Political Science Review* 68:243-52.
- Gurr, Ted R., and Raymond Duvall. 1973. "Civil Conflict in the 1960s: A Reciprocal Theoretical System with Parameter Estimates." *Comparative Political Studies* 6:135-70.
- Huntington, Samuel P., and Joan M. Nelson. 1976. *No Easy Choice: Political Participation in Developing Countries*. Cambridge: Harvard University Press.
- Jackman, Robert W. 1978. "The Predictability of Coups d'État: A Model with African Data." *American Political Science Review* 72:1262-75.
- Jackman, Robert W., and William A. Boyd. 1979. "Multiple Sources in the Collection of Data on Political Conflict." *American Journal of Political Science* 23:434-58.
- Jenkins, J. Craig. 1985. *The Politics of Insurgency: The Farm Worker Movement in the 1960s*. New York: Columbia University Press.
- Kuper, Leo. 1981. *Genocide*. New Haven: Yale University Press.
- LaFeber, Walter. 1983. *Inevitable Revolutions: The United States in Central America*. New York: Norton.
- Lichbach, Mark I. 1989. "An Evaluation of 'Does Economic Inequality Breed Political Conflict?' Studies." *World Politics* 41:431-70.
- Lindenberg, Marc. 1990. "World Economic Cycles and Central American Political Instability." *World Politics* 42:397-421.
- McAdam, Douglas. 1982. *Political Process and the Development of Black Insurgency, 1930-1970*. Chicago: University of Chicago Press.
- McClintock, Cynthia. 1984. "Why Peasants Rebel: The Case of Peru's Sendero Luminoso." *World Politics* 37:48-84.
- Midlarsky, Manus I. 1988. "Rulers and the Ruled: Patterned Inequality and the Onset of Mass Political Violence." *American Political Science Review* 82:491-509.
- Midlarsky, Manus I. 1989. "Land Inequality and Political Violence." *American Political Science Review* 83:587-95.
- Midlarsky, Manus I., and Kenneth Roberts. 1985. "Class, State, and Revolution in Central America: Nicaragua and El Salvador Compared." *Journal of Conflict Resolution* 29:163-93.
- Migdal, Joel S. 1984. *Peasants, Politics, and Revolution: Pressures toward Political and Social Change in the Third World*. Princeton: Princeton University Press.
- Montgomery, Tommie Sue. 1982. *Revolution in El Salvador*. Boulder: Westview.
- Moore, Barrington. 1978. *Injustice: The Social Basis of Obedience and Revolt*. White Plains, NY: M. D. Sharpe.
- Muller, Edward N., and Mitchell A. Seligson. 1987. "Inequality and Insurgency." *American Political Science Review* 81:425-52.
- Muller, Edward N., Mitchell A. Seligson, and Hung-der Fu. 1989. "Land Inequality and Political Violence." *American Political Science Review* 83:577-86.
- Paige, Jeffery M. 1975. *Agrarian Revolution: Social Movements and Export Agriculture in the Underdeveloped World*. New York: Free Press.
- Popkin, Samuel L. 1979. *The Rational Peasant*. Berkeley: University of California Press.
- Prosterman, Roy L., and Jeffrey M. Riedinger. 1987. *Land Reform and Democratic Development*. Baltimore: Johns Hopkins University Press.
- Russett, Bruce M., Hayward R. Alker Jr., Karl W. Deutsch, and Harold D. Lasswell. 1964. *World Handbook of Political and Social Indicators*. New Haven: Yale University Press.
- Scott, James C. 1976. *The Moral Economy of the Peasant: Rebellion and Subsistence in Southeast Asia*. New Haven: Yale University Press.
- Scott, James C. 1977. "Peasant Revolution: A Dismal Science." *Comparative Politics* 9:231-48.
- Tarrow, Sidney. 1989. *Democracy and Disorder: Social Conflict, Protest, and Politics in Italy, 1965-1975*. New York: Oxford University Press.
- Taylor, Charles L., and Michael C. Hudson. 1972. *World Handbook of Political and Social Indicators*. 2nd. ed. New Haven: Yale University Press.
- Taylor, Charles L., and David A. Jodice. 1983. *World Handbook of Political and Social Indicators*. 3d ed., Vol. 2, *Political Protest and Government Change*. New Haven: Yale University Press.
- Tilly, Charles, Louise Tilly, and Richard Tilly. 1975. *The Rebellious Century, 1830-1930*. Cambridge: Harvard University Press.
- Tutino, John. 1986. *From Insurrection to Revolution in Mexico: Social Bases of Agrarian Violence, 1750-1940*. Princeton: Princeton University Press.
- United Nations. 1976. *Demographic Yearbook*. New York: UN.
- United Nations. Food and Agriculture Organization. 1981. *1970 World Census of Agriculture: Analysis and International Comparison of the Results*. Rome: FAO.
- U.S. House. Committee on International Relations. 1976. *Human Rights in Nicaragua, Guatemala, and El Salvador: Implications for U.S. Policy*. Hearings before Subcommittee on International Organizations. 94th Cong., 2d sess.
- Webre, Stephen. 1979. *José Napoleón Duarte and the Christian Democratic Party in Salvadoran Politics, 1960-1972*. Baton Rouge: Louisiana State University Press.
- Weede, Erich. 1986. "Income Inequality and Political Violence Reconsidered." *American Sociological Review* 51:438-45.
- White, Robert A. 1977. "Structural Factors in Rural Development: The Church and the Peasant in Honduras." Ph.D. diss., Cornell University.
- Williams, Robert G. 1986. *Export Agriculture and the Crisis in Central America*. Chapel Hill: University of North Carolina.
- Wolf, Eric R. 1969. *Peasant Wars of the Twentieth Century*. New York: Harper & Row.
- Zimmerman, Ekhart. 1980. "Macro-comparative Research on Political Protest." In *Handbook of Political Conflict*, ed. Ted Robert Gurr. New York: Free Press.

Charles D. Brockett is Professor of Political Science, The University of the South, Sewanee, TN 37375.