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INEQUALITY AND INSURGENCY

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Maldistribution of land in agrarian societies is commonly thought to be an important precondition of mass political violence and revolution. Others argue that because of the difficulty of mobilizing rural populations for political protest, land maldistribution is irrelevant except as part of an inegalitarian distribution of income nationwide. These rival inequality hypotheses have significant implications with respect to the kinds of reforms likely to reduce the potential for insurgency in a society. They are tested using the most comprehensive cross-national compilation of data currently available on land inequality, landlessness, and income inequality. Support is found for the argument that attributes the greater causal import to income inequality. Moreover, the effect of income inequality on political violence is found to hold in the context of a causal model that takes into account the repressiveness of the regime, governmental acts of coercion, intensity of separatism, and level of economic development.

Many students of domestic political conflict consider inequality in the distribution of land and/or lack of land ownership (landlessness) to be among the more fundamental economic preconditions of insurgency and revolution (e.g., Huntington 1968; Midlarsky 1981, 1982; Midlarsky and Roberts 1985; Paige 1975; Prosterman 1976; Prosterman and Riedinger 1982; Russett 1964; Tanter and Midlarsky 1967). Huntington (1968, 375), whose writing on the subject has been particularly influential, advanced a strong version of the land maldistribution hypothesis as follows: "Where the conditions of land-ownership are equitable and provide a viable living for the peasant, revolution is unlikely. Where they are inequitable and where the peasant lives in poverty and suffering, revolution is likely, if not inevitable, unless the government takes prompt measures to remedy these conditions." However, because mass revolutions are rare events, it is more plausible to relax the postulate that revolution is an inevitable consequence of land maldistribution and to restate the hypothesis: the greater the maldistribution of land, the greater the probability of mass-based political insurgency and, consequently, the greater the *vulnerability* of a country to revolution from below. This weaker, necessary-but-not-sufficient version of the land-maldistribution-leads-to-revolution hypothesis directs attention to the relationship between land distribution and mass political violence.

The land maldistribution hypothesis is based on the assumption that discontent resulting from a highly concentrated distribution of land and/or lack of land ownership (landlessness) in agrarian societies is an important direct cause of mass political violence. Advocates of what has come to be called the "resource mobilization" approach to the explanation of collective protest and violence (e.g., Gamson 1975; Oberschall 1973; Tilly 1978) reject such discontent hypotheses for the reason that inequality and discontent are more or less always present in virtually all societies and that consequently the most direct and influential explanatory factor must not be discontent per se but rather the *organization* of discontent. Thus Skocpol (1979, 112–57), who is skeptical of discontent theories of revolution, argues that the peasant revolts that were a crucial insurrectionary ingredient in the French, Russian, and Chinese revolutions occurred not because of the maldistribution of landholdings but rather because communities of French, Russian, and Chinese peasants had sufficient autonomy from local landlords to enable them to mobilize collectively. By contrast, Midlarsky (1982, 15–20), a proponent of discontent theory, explains the peasant revolts in each of these cases by the fact that rapid population growth severely exacerbated land inequality until a level of deprivation was reached that no longer could be tolerated.

Two contemporary cases cited by Midlarsky and Roberts (1985) in support of the land maldistribution hypothesis are El Salvador and Nicaragua.[1] Compared with other middle-income developing countries, population growth in El Salvador and Nicaragua was above average during the 1960s and 1970s (see World Bank 1981, tbl. 17). Maldistribution of land also was a serious problem, as the Gini coefficient of land concentration was .80 for Nicaragua and .81 for El Salvador (values well above the global mean of .60) and agricultural households without land (i.e., tenants, sharecroppers, and agricultural laborers) amounted to 40% of the total labor force in El Salvador circa 1970, which was the highest level of landlessness in the world at that time (data are not available for Nicaragua).[2] Each country subsequently experienced a relatively high rate of mass political violence, which in the Nicaraguan case culminated in revolution.

But the seemingly obvious conclusion that land maldistribution must have been a primary cause of political violence in El Salvador and Nicaragua ignores the fact that, during the same period of time, two other Central American states, Costa Rica and Panama, remained quite peaceful despite the presence of exactly the same preconditions supposed to have caused the insurgency in El Salvador and Nicaragua. Costa Rica and Panama experienced above-average population growth (in fact, Costa Rica's 3.4% annual population-growth rate during 1960–70 not only exceeded the 2.9% rate registered by El Salvador and Nicaragua but was also among the highest in the entire world); land was concentrated in the hands of the few to about the same degree in Costa Rica (the Gini coefficient was .82) and Panama (Gini coefficient of .78) as in El Salvador and Nicaragua; and the amount of landlessness in Costa Rica (24%) and in Panama (36.2%) ranked ninth and third highest in the world, respectively. Nevertheless, during 1970–77 Panama registered only a single death from political violence, and there were no instances of deadly political violence in Costa Rica (see Taylor and Jodice 1983, vol. 2, tbl. 2.7).

Comparison of Costa Rica and Panama with El Salvador and Nicaragua thus raises the issue of the general validity of the land maldistribution hypothesis: Are Costa Rica and Panama merely exceptions to the rule, or is maldistribution of land in reality a minor or even irrelevant factor in the process that generates insurgency and revolution? That question is significant not only because inequality is frequently assumed in academic writing to be an important determinant of political instability; it also has profound policy implications because land reform has traditionally been a cornerstone of U.S.

efforts to promote political stability in developing countries.

Inequality, Resource Mobilization, and the Structure of the State

We argue that theories emphasizing land maldistribution as a fundamental precondition of insurgency and revolution are misspecified. They attribute direct causal significance to an inequality variable that plays only a relatively small, indirect part in the generation of mass political violence. We hypothesize that the more important direct cause of variation in rates of political violence cross-nationally is inequality in the distribution of income rather than maldistribution of land. This hypothesis is predicated on the following assumptions:

1. Inequality in the contemporary world generates discontent;
2. Although inequality is present to some degree in all societies, some societies are significantly more inegalitarian than others;
3. Inequality in the distribution of land and inequality in the distribution of income are not *necessarily* tightly connected; in particular, they are sufficiently independent of each other that an effect of one on a response variable such as the rate of political violence does not necessarily imply that the other will have a similar effect;
4. Given the existence of inequality-based discontent, it is more difficult to mobilize peasant communities than urban populations for political protest; peasants normally become the foot soldiers of insurgent movements only if they are effectively organized by a "vanguard" of urban professional revolutionaries.

From these assumptions we derive the following postulates:

1. A high level of income inequality nationwide significantly raises the probability that at least some dissident groups will be able to organize for aggressive collective action. This is because, first, the pool of discontented persons from which members can be drawn will include the more easily mobilized urban areas; and, second, it may be possible for urban revolutionaries to establish cross-cutting alliances with groups in the countryside
2. A high level of agrarian inequality does not necessarily raise the probability that dissident groups will be able to organize for aggressive collective action; this is because the pool of discontented persons from which members can be drawn may be restricted to the countryside, which is difficult to mobilize; consequently, we predict that if income inequality is relatively low, the rate of political violence will tend to be relatively low, even if agrarian inequality is relatively high; wherreas if income inequality is relatively high, the rate of political violence will tend to be relatively high, even if agrarian inequality is relatively low

Our inequality hypothesis, which is based on an integration of discontent (or relative deprivation) arguments (e.g., Gurr 1970) with the resource mobilization approach, can be illustrated by the cases of Costa Rica and Venezuela, where egalitarian redistribution of income occurred despite persisting high agrarian inequality; and the case of Iran, where income inequality worsened, especially in urban areas, despite an egalitarian land reform.

Costa Rica circa 1960 had a relatively inegalitarian distribution of land (the 1963 Gini coefficient was .78) and an extremely inegalitarian distribution of income (the richest 20% of families received 61% of total personal income in 1961). During the decade of the 1960s the distribution of land in Costa Rica became slightly more concentrated (the 1973 Gini coefficient was .82). The distribution of income, however, was substantially altered in an egalitarian direction by democratically elected reformist administrations who pursued welfare-state policies similar to those of European social democratic governments. By 1970 the share of national income accruing to the richest quintile of Costa Rican households had been reduced to 50%.[3] As mentioned above, violent conflict was absent from Costa Rican politics during the 1970s.

Venezuela was a similarly inegalitarian society circa 1960, when a democratic regime was inaugurated. The 1956 Gini index of land concentration was .91—the second highest in the world next to Peru—and the richest quintile of Venezuelan households received 59% of total personal income in 1962. During the 1960s the distribution of land in Venezuela remained highly concentrated (the 1971 Gini coefficient was .91), but the distribution of income became more egalitarian—although not as dramatically so as in Costa Rica—due to a combination of reformist administrations and an expanding petroleum-based economic pie (by 1970 the income share of the richest quintile of households had been reduced to 54%). Deaths from political violence in Venezuela registered a sharp decline over this period (according to Taylor and Jodice 1983, vol. 2, tbl. 2.7, they amounted to 1,392 during the years 1958–62; 155 during 1963–67; 53 during 1968–72; and 9 during 1973–77).

In the early 1960s, the shah of Iran, under pressure from the Kennedy administration, launched his so-called white revolution (it was to be without bloodshed), which accelerated a land reform begun hesitantly in the 1950s. Initially, about 70% of the arable land in Iran was owned by a very small number of landowners. By 1967, according to official sources, land had been redistributed to about 520 thousand families; 3,238 landlords had sold land to 46 thousand families; 153 thousand tenants had received a percentage distribution of land; and 203,049 landlords had written tenancy agreements with over a million peasant families. Thus a relatively large proportion of Iran's rural population benefitted in some way from the land reform, and approximately 15% of peasant families actually became landowners.[4] Although data on land concentration are not available for the end of this period, already by 1960 the Gini coefficient was .63, an intermediate value.

The shah's land reform has been criticized for being superficial in a variety of respects—for instance, the percentage distribution of land received by the 153 thousand former tenants entailed parcels that were below the minimum needed for family subsistence; there was no bureaucracy to ensure that tenancy agreements would be respected; former landowners often were able to keep the best land for themselves; and there was no follow-up program to provide new farmers with assistance in obtaining seed, water, and fertilizer. Nevertheless, there can be no doubt that its massive scope did contribute to some extent to reduction of agrarian inequality in Iran. At the same time, however, the shah's program of "forced modernization" appears to have caused an increased inequality of income among urban households. Data collected by Jabbari (1981) from family expenditure surveys of the Central Bank of Iran and the Iran Statistical Center can be used as a proxy for income. Gini coefficients calculated from these expenditure data by Jabbari (1981, 174) show that while inequality of rural income decreased in Iran during the 1960s and early 1970s, the increasing inequality in urban income offset the rural trend and resulted in an overall trend of steadily increasing inequality in the nationwide distribution of income. Thus the shah's land reforms were overshadowed by macroeconomic policies that produced an inegalitarian upward redistribution of income, which presumably alienated the growing urban middle class. Political violence began to increase in the mid-1970s, culminating in the revolution of 1979.

Of course, income inequality is not the only cause of mass political violence. In Panama, for example, income was distributed very unequally circa 1970, as the richest 20% of households earned 62% of total national income. But by the early 1970s, General Omar Torrijos Herrera, who had led a successful coup d'etat by officers of the national guard in 1968, had crushed all opposition, established firm censorship of the media, and taken control of the judiciary. Ratings of political rights and civil liberties in Panama during the mid-1970s on a scale of one to seven (most free to least free) averaged 6.5.[5] During this period (1973–77) the Torrijos regime in Panama was the most repressive in the Western Hemisphere next to Cuba, where the rating of political rights and civil liberties averaged 6.9. Inequality-induced discontent presumably existed in Panama, and it probably was relatively widespread, but there was little or no opportunity to organize it.

By contrast, Panama's next-door neighbor to the northwest, Costa Rica, enjoyed the distinction in the mid-1970s of being the oldest democracy in Latin America. Since 1949 Costa Rica had held regularly scheduled free and fair elections, the media were uncensored, unions were free to organize, the judiciary was independent of the executive and legislative branches of government, and citizens were not subject to arbitrary arrest. Costa Rica's political and civil rights ratings averaged a maximum score of 1.0 during 1973–77.

The "open" and "closed" political systems of Costa Rica and Panama exemplify polar extremes of regime repressiveness. Differences in regime structure are relevant to the explanation of cross-national variation in mass political violence because they can be assumed to affect three important variables emphasized in some versions of resource mobilization theory (e.g., McAdam 1982): (1) the extent to which dissident groups are able to develop strong organizations, (2) their belief in the likelihood of success of collective action, and (3) the range of political opportunities available to them for achieving their goals.

In the context of an extremely repressive regime, dissident groups are severely restricted in their ability to organize; their belief in the likelihood of success of collective action will probably be low; and opportunities to engage in collective action of any kind will be quite limited. Consequently, under the condition of a high level of regime repressiveness, rational actors most likely will attach a relatively low utility to violent collective action, and the rate of mass political violence therefore should be relatively low.

In the context of a nonrepressive or "democratic" regime, dissident groups will not face significant restrictions on their ability to organize for collective action, and their belief in the likelihood of achieving at least some success from collective action will probably be relatively high. Moreover, a democratic regime structure will afford a variety of opportunities for dissident groups to participate legally and peacefully in the political process. Because the costs of peaceful collective action will be lower than those of violent collective action and because the likelihood of success of peaceful collective action will be reasonably high, rational actors under the condition of a nonrepressive regime structure presumably will usually attach a much higher utility to peaceful as opposed to violent collective action, and, therefore, the rate of mass political violence here too should be relatively low.

In the context of a semirepressive

regime, it is possible for dissident groups to develop relatively strong organizations. However, opportunities to engage in nonviolent forms of collective action that effectively exert influence on the political process are limited. Semirepressive regimes allow only for, in Green's (1984, 154) apt terminology, *"pseudoparticipation* . . . an elaborate charade of the participatory process." Polities with pseudoparticipation typically have elections that are not free and fair, legislatures that are little more than debating societies, and a judiciary that is not independent of the will of the executive; the media are subject to censorship at the whim of the executive; and citizens are subject to arbitrary arrest and detention by security forces, which are under the exclusive control of the executive. In short, semirepressive regimes erect a facade of participatory institutions but do not permit popular input to significantly influence governmental output. Because opportunities for genuine participation are restricted, many politically activated citizens may come to perceive civil disobedience and violence as being more efficacious than legal means of pseudoparticipation; and since the expected costs of insurgency may not be perceived to be prohibitive, rational actors may well attach a relatively high utility to aggressive political behavior. Therefore, it is plausible to expect that the rate of mass political violence cross-nationally will be highest under semirepressive authoritarian regimes.

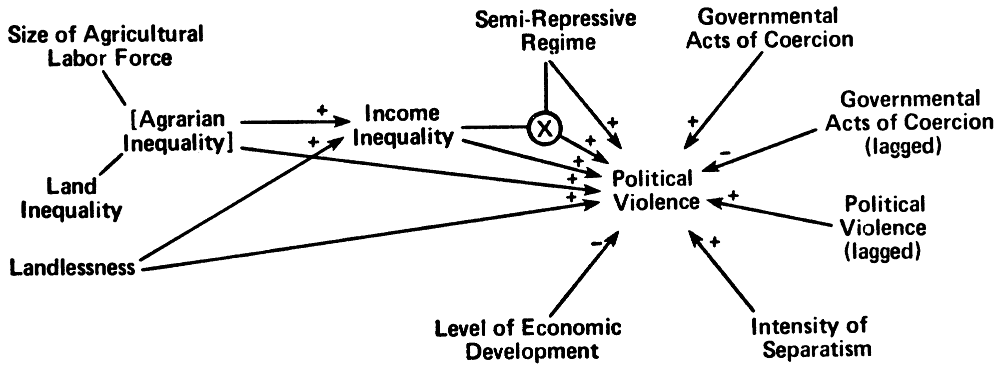
The analysis of the causes of the Iranian revolution by Green (1982, 1984) documents in detail how the shah vacillated between fully restricting mass participation and allowing pseudoparticipation and concludes that "the effects of such tactics served to increase popular hostility among those socially mobilized Iranians eager to have a measure of influence over the manner in which their society was ruled" (Green, 1984, 155). Green's case study description is corroborated by global comparative measures of regime repressiveness, which show that Iran in the late 1950s was classified as having a "semi-competitive" regime (Coleman 1960); was scored for 1960 and 1965 as intermediate (34.9 and 45.0, respectively) on a 0–100 scale of extent of political democracy (Bollen 1980); was ranked circa 1969 at an intermediate level on a scale of opportunity for political opposition (Dahl 1971); received a mean rating of 5.7 on political and civil rights for 1973–77; and had shifted in 1978 to a mean rating of 5.0 on political and civil rights. Thus, while pursuing a strategy of economic development that had the short-term consequence of increasing inequality in the distribution of income, the Pahlavi government would appear to have added fuel to the fire by following a semirepressive political development strategy that allowed opposition groups to organize but did not enable them to participate effectively.

If one takes income inequality and the repressiveness of the regime into account simultaneously, it might be argued that each variable could have an independent causal impact on the likelihood of mass political violence. An equally plausible specification of the joint relationship is that discontent resulting from income inequality will affect political violence only (or most strongly) in countries with semirepressive regime structures; whereas in countries with nonrepressive regime structures, inequality-induced discontent will tend to be channeled into peaceful participation; and in countries with repressive regime structures, it will be borne apathetically or else perhaps lead to various kinds of nonpolitical deviant behavior.

A Causal Model

The diagram of Figure 1 includes the hypotheses elaborated above in the con-

Figure 1. Inequality and Insurgency: A Multivariate Causal Model



text of a more comprehensive causal model. The set of inequality hypotheses is shown to the left of the dependent variable, mass political violence. Reading clockwise, hypotheses about the structure of the regime and the behavior of the incumbent political authorities are shown above the dependent variable; hypotheses referring specifically to time-lagged effects are to the right of it; and hypotheses about socioeconomic factors other than inequality are below it.

The argument that land inequality in agrarian societies is a major precondition of violence is represented by the arrow from agrarian inequality (defined as land inequality weighted by the size of the agricultural labor force) to political violence. It is important to weight land inequality by size of agricultural labor force because, as Huntington (1968, 381) observes, "in countries which have reached a high level of economic development, agriculture has a relatively minor role, and consequently even highly inequitable patterns of land ownership do not pose substantial problems of social equality and political stability." Thus, for example, it is implausible to expect that the relatively high Gini coefficients of land concentration in industrialized societies such as Australia and the United States, where the agricultural labor force is less than 10% of the population, could generate significant discontent (a point noted by Prosterman 1976, 350). The issue of land reform will be salient only if land inequality is high *and* the proportion of the labor force employed in agriculture is large. Therefore, the appropriate specification of the land-inequality hypothesis is an interaction between land inequality and the size of the agricultural labor force.

The model also includes the possibility that the proportion of the labor force that does not own or possess ownershiplike tenure of land (landlessness) is an alternative indicator of land maldistribution that may be correlated with land inequality but also could have an independent effect on political violence. Indeed, Prosterman (1976) argues that land inequality is a much less important cause of political violence and revolution than landlessness, and he regards landlessness as the single most salient cause of mass political violence in the twentieth century and a powerful predictive tool for anticipating which societies will have the greatest potential for revolution.

Our rival hypothesis, that agrarian inequality affects mass political violence only indirectly, depending on whether or not it contributes to an inegalitarian nationwide distribution of personal income, is represented by the arrows linking agrarian inequality to income inequality and income inequality to political violence. Similarly, we expect that landlessness will not have a significant direct effect on mass political violence and will be related to political violence only indirectly via the mediation of income inequality.

The expected relationship between political violence and the repressiveness of the regime can be expressed in two forms. If regime repressiveness is defined operationally as an ordinal typology consisting of the three properties, *nonrepressive*, *semirepressive*, and *very repressive*, then the hypothesis is simply that semirepressive regimes will be more likely to have high rates of political violence than either nonrepressive or very repressive regimes. This specification of the hypothesis, which facilitates the testing of an interaction between regime repressiveness and income inequality, is shown in Figure 1. If regime repressiveness is measured as a continuous variable, however, then an alternative specification is that violence will vary as an inverted U-curve of regime repressiveness (see Muller 1985).

In addition to the inequality and regime-repressiveness hypotheses, we include a hypothesis based on the distinction between the repressiveness of regime structures and acts of coercion undertaken by the political authorities that entail the use of negative sanctions to suppress the activity of dissident groups. On the one hand, extremely repressive regimes may deter insurgency by the threat of severe penalties, and the actual use of negative sanctions often may be relatively low. On the other hand, the political authorities in democratic regimes may on occasion engage in acts of coercion against dissidents who are considered to be a threat to internal security, and if such use of negative sanctions is limited in time and scope, the structure of the regime may remain nonrepressive. Therefore, it seems plausible to postulate that the coercive *behavior* of governments may vary more or less independently of the *structural* repressiveness of the regime. Experimental studies have shown that "aversive stimulation" in the form of insults and physical assault is the most potent elicitor of aggressive response in the laboratory (see Bandura 1973; Buss 1961). Tilly (1969) concludes from a historical survey of collective violence in Europe that coercive responses by political authorities to illegal but nonviolent action on the part of dissident groups frequently caused the level of protest to escalate from civil disobedience to violence. In addition, the seminal cross-national study of determinants of mass political violence by Hibbs (1973) found that the variable with by far the strongest direct—and positive—causal impact on variation in internal war (the sum of armed attacks, deaths from political violence, and assassinations) was governmental acts of coercion (called "negative sanctions"). Consequently, we hypothesize that regardless of the structural repressiveness of the regime, coercive actions taken by political authorities to suppress dissident groups will tend to instigate a violent response. This hypothesis is denoted by the arrow specifying a direct positive effect of governmental acts of coercion on political violence.

Hibbs's final model of causes of internal war cross-nationally for the 1948–67 period included a *negative* lagged effect of 1948–57 negative sanctions on 1958–67 internal war in addition to the positive instantaneous effect. Our model, which will be tested for the 1968–77 period, incorporates the possibility of a delayed deterrence effect, expressed by the negative sign attached to the arrow linking lagged governmental acts of coercion to political violence.

Hibbs also took lagged values of political violence into account. Inclusion of lagged values of the dependent variable in an explanatory model means that the results apply to change in that variable rather than to current level. We expect that our hypotheses about inequality and regime repressiveness will apply both to level of political violence and to change in political violence. Consequently, the model will be tested with and without a lagged political violence term—where lagged values refer to the first five-year interval of the period (1968–72) and current values refer to the second five-year interval (1973–77).

Political violence often occurs as a result of attempts by groups or regions to gain greater autonomy or to secede from the state. Relatively high rates of political violence during the 1970s resulting at least in part from the presence of intense separatist sentiment include such instances as the Catholics in Northern Ireland, the Muslims in the Philippines, blacks in South Africa and Rhodesia (now Zimbabwe), South Sudan in Sudan, and Baluchistan in Pakistan. We hypothesize that the intensity of separatist sentiment is a cause of political violence that can operate independently of the other variables in the model. Support for this hypothesis has been reported for earlier years by Hibbs (1973), where political separatism was found to be one of the four significant determinants of 1958–67 internal war in the final model.[6]

Level of economic development also is included in our model because Hardy (1979) and Weede (1981) have argued that political violence will vary cross-nationally as an inverse function of economic development and that income inequality has no effect on political violence independent of a country's development level. Consequently, one should control for level of economic development when testing inequality hypotheses in a multivariate context.

A Cross-National Test of the Causal Model

There have been no studies reported to date that compare the causal importance of land maldistribution versus income inequality as determinants of mass political violence cross-nationally.[7] Until the 1970s, reasonably reliable information on the distribution of land and income was available for only a limited number of countries. Thus in Hibbs's (1973) comprehensive cross-national study of determinants of mass political violence during the 1948–67 period, inequality variables had to be excluded because of insufficient data. We now have been able to compile a relatively comprehensive data set on inequality circa 1970 (see Appendix). Information on land inequality is available for approximately three-quarters of the population of independent political units in 1970, while information on landlessness and income inequality is available for approximately one-half of the population. Regionally, these data are quite comprehensive for Europe and the Americas. In regard to landlessness and, especially, income distribution, coverage is poor for states in the Middle East and North Africa, and it is somewhat limited for the states of sub-Saharan Africa. Since it is unlikely that much new data on inequality circa 1970 will emerge in the future, results using the current data set can probably be regarded as being about as definitive as possible for this time period.

Measurement of the Dependent Variable

Political violence is measured by the natural logarithm of the death rate from domestic conflict per one million population.[8] Annual death counts are from Table 2.7 of Taylor and Jodice (1983, vol. 2). Current political violence is the logged sum of annual deaths from domestic political conflict during 1973-77 divided by midinterval population; lagged political violence is the logged sum of annual deaths from domestic political conflict during 1968-72 divided by midinterval population. Countries where domestic political conflict overlaps with major interstate wars are excluded: Kampuchea, Laos, and South Vietnam for the 1968-77 period; and Pakistan for the 1968-72 period (where an extremely high death rate reflects the conflict between India and Pakistan in 1971 over the secession of Bangladesh). Ireland also is excluded for the 1973-77 period because the relatively high death rate there reflects a spillover from the Northern Ireland conflict.

In the vast majority of countries, the death rate from political violence per one million population is less than 50. A few countries register very extreme scores, however; for example, Zimbabwe's 1973-77 death rate from political violence was 544 per million and Argentina's death rate was 177 per million. Even after logging, countries with political violence death rates of 50 or more almost always show up as outliers in regression equations (i.e., they usually have extremely high standardized residuals). Consequently, in order to reduce the problem of extreme scores on the dependent variable, it is desirable to set a ceiling on the death rate. The upper limit that we have selected is 50 deaths per million. The adjusted death rate variables thus range from a minimum value of 0 to a maximum value of 50 or more; and the range of the logged death rate variables is from 0 to 3.93.

Measurement of the Independent Variables

The data on land inequality circa 1970 encompass 85 states in which agriculture was not collectivized. Land inequality is measured by the Gini coefficient of land concentration. A weighted index of land inequality is the geometric mean of the Gini coefficient (expressed as a percentage) and the percentage of the labor force employed in agriculture in 1970 (see Taylor and Jodice 1983, vol. 1). Apart from measurement of the extent to which land is concentrated in the hands of the few, we also take into account a second aspect of land maldistribution, landlessness, as measured by agricultural households without land as a proportion of the total labor force. These data are derived from estimates by Prosterman and Riedinger (1982) of the proportion in 64 countries of agricultural households without land.

Income inequality is measured by the size of the share of personal income accruing to the richest quintile of recipients, based on information about the nationwide distribution of income in 63 countries compiled principally from publications of the World Bank. Although some previous studies have used Gini coefficients of income concentration, this measure tends to be unduly sensitive to inequality in the middle of the distribution, whereas inequality in reference to the top of the distribution probably is more relevant to political violence. In any event, income shares also have a more direct meaning than Gini coefficients and are currently more frequently used in research on income inequality.

Regime repressiveness is measured by a country's 1973-77 average annual combined rating on 7-point rank-order scales of political rights and civil liberties that have been reported by Raymond D. Gastil since 1973 (the data are from Taylor and Jodice 1983). A semirepressive regime structure is defined operationally as a mean political rights and civil liberties rank in the range of 2.6-5.5. These cutpoints are identical to those used by Gastil for classifying political systems as "free" (1.0-2.5), "partly free" (2.6-5.5), and "not free" (5.6-7.0).

The indicator of governmental acts of

coercion is the negative sanctions variable (imposition of sanctions) from Taylor and Jodice 1983 (vol. 2, tbl. 3.1). Current negative sanctions is the frequency of negative sanctions summed over the years 1973–77 and divided by midinterval total populatioan in millions; lagged negative sanctions is the 1968–72 frequency per one million midinterval population. The negative sanctions variables are expressed as natural logarithms (after adding an increment of one).

The indicator of intensity of separatism is an ordinal scale developed by Ted and Erika Gurr. The data for circa 1975 are from Taylor and Jodice 1983, 55–57 and tbl. 2.5. We express intensity of separatism as a dummy variable, scored 1 (i.e., high intensity) if groups or regions actively advocating greater autonomy were forcibly incorporated into the state (codes 3 and 4) and 0 (i.e., low intensity) otherwise (codes 0, 1, and 2).[9]

Level of economic development is measured by energy consumption per capita in 1970 (from Taylor and Jodice 1983, vol. 1). Values of this variable are expressed as natural logarithms.

Land Maldistribution, Income Inequality, and Political Violence

According to what is generally considered to be the most appropriate specification of the land inequality hypothesis (e.g., Huntington 1968; Nagel 1976; Prosterman 1976), the strongest effect on political violence should be observed when inequality in the distribution of land is weighted by the proportion of the labor force employed in the agricultural sector of the economy. This specification implies a multiplicative interaction between land inequality and the size of the agricultural labor force, which we call *agrarian inequality*, defined operationally as the geometric mean of Gini land concentration and the percentage of the labor force employed in agriculture (i.e., the square root of the product of these variables). When the 1973–77 death rate from political violence (denoted *lnDPM75*) is regressed on agrarian inequality for 83 cases,[10] the result is

$$\mathit{lnDPM75} = 0.29 + \underset{(2.27)}{0.017}(\text{AGINEQUAL}) \qquad (1)$$

$$R_a^2 = .05$$

Although the size of the t-ratio (in parentheses) indicates that the parameter estimate for agrarian inequality is statistically significant at the .05 level, the magnitude of R_a^2 (the adjusted accuracy of prediction —an estimate of R^2 for the population) is trivial. Indeed, the correlation (r) of .24 between the political-violence death rate and agrarian inequality is weaker than that for unweighted Gini land concentration, which is .27. Thus, weighting Gini land concentration by size of agricultural labor force, a procedure that theoretically should enhance the correlation, in fact reduces accuracy of prediction.[11]

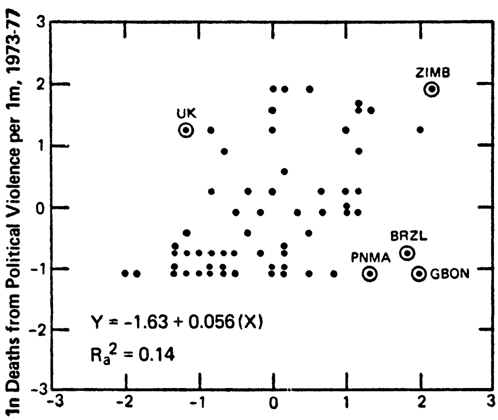
If the hypothesis of an effect of landlessness on the 1973–77 death rate from political violence is taken into account in addition to Gini land concentration, the regression equation estimated across 60 countries is

$$\mathit{lnDPM75} = 0.46 + \underset{(0.71)}{0.62}(\text{GINILAND}) + \underset{(1.51)}{0.023}(\text{LANDLESS}) \qquad (2)$$

$$R_a^2 = .03$$

The t-ratios show that for countries with information on both of the land maldistribution variables, the parameter estimate for landlessness is of borderline significance (between the .05 and .10 levels for a one-tailed test), while that for Gini land concentration is not significant. This finding supports the argument of Prosterman (1976) that the proportion of the labor force without land is a more relevant determinant of political violence than the extent to which landholdings are unequally distributed. When Equation 1 is

Figure 2. Plot of Standardized Death Rates from Political Violence by Standardized Income Inequality Scores



reestimated with the nonsignificant Gini land-concentration variable deleted, the t-ratio for the landlessness parameter estimate rises to 1.87, which is significant at less than .05 for a one-tailed test. R_a^2 remains trivial, however, increasing only to .04. The strength of the correlational support for Prosterman's argument thus is quite weak in cross-national perspective.[12]

If inequality in the distribution of income—defined operationally by the share of personal income accruing to the upper quintile of recipients—is taken into account in addition to landlessness, the regression equation for 47 countries with information on both variables is

$$lnDPM75 = -1.40 + \underset{(0.62)}{0.011(\text{LANDLESS})} + \underset{(2.03)}{0.048(\text{U20INCOME})} \quad (3)$$

$$R_a^2 = .12$$

In this equation the effect of landlessness is reduced to nonsignificance while the effect of income inequality is estimated to be significant at the .05 level. Thus, on controlling for income inequality, landlessness becomes an irrelevant variable.

The correlation between the 1973–77 death rate from political violence and income inequality is .39 for 62 cases—a moderate degree of fit to a linear trend.[13] As can be seen from the scatterplot shown in Figure 2, most countries with relatively low income inequality (an upper 20% share that is one-half of a standard deviation or more below the mean) have relatively low death rates from political violence (in the range of 0 to −1 standard deviation), whereas most countries with relatively high income inequality (an upper 20% share that is one standard deviation or more above the mean) have relatively high death rates (equal to or above the mean). The relationship is subject to heteroskedasticity, however, because Brazil, Gabon, and Panama have very low death rates from political violence despite income-inequality scores greater than one standard deviation above the mean. These countries comprise three of the five most influential cases in the bivariate regression (circled in Figure 2), and, because they are deviant cases, their presence reduces accuracy of prediction.[14]

The United Kingdom also is an influential deviant case because it has a relatively high death rate from political violence despite an income inequality score that is more than one standard deviation below the mean. In the following multivariate regressions, we want to analyze the extent to which these deviant cases can be explained by taking into account the set of variables postulated to have additional effects on political violence independent of income inequality.

The Multivariate Model

We turn now to the question of whether the relationship between income inequality and mass political violence holds cross-nationally in the context of a multivariate equation that takes into account the explanatory variables shown to the right of income inequality in the model of Figure 1. Table 1 reports the parameter estimates[15] obtained from the regression of the 1973–77 death rate from political violence on income inequality and the other explanatory variables, including the lagged political-violence death rate (Equations 1.1 and 1.2) and excluding it (Equations 1.3 and 1.4).[16]

The hypothesis of a direct effect of income inequality on change in political violence is confirmed in Equation 1.1. Otherwise, the effects are as predicted in the model of Figure 1 except for that of energy consumption (the indicator of level of economic development), which is estimated to have a positive instead of a negative effect on change in political violence. The expected negative effect of lagged negative sanctions (the indicator of governmental acts of coercion) is not significant at the .05 level, however.

Table 1. Regressions of Death Rate from Political Violence on Income Inequality and Other Explanatory Variables

	ln Deaths from Political Violence per 1m, 1973–77			
	Change		Level	
Explanatory Variables	1.1	1.2	1.3	1.4
Intercept	-4.02	-3.81	-2.55	-1.56
Upper 20% income share, ca. 1970	0.046*	0.045*	0.038*	0.034*
	(2.95)	(2.83)	(2.29)	(2.31)
	.32	.31	.27	.24
ln energy consumption per capita, 1970	0.25	0.22	0.11	
	(2.32)	(2.07)	(1.03)	
	.29	.25	.13	
Intensity of separatism, 1975	0.87*	0.86*	1.23*	1.24*
	(2.51)	(2.44)	(3.42)	(3.60)
	.24	.23	.35	.35
Semirepressive regime, 1973–77	0.86*	0.83*	1.23*	0.82*
	(2.93)	(2.81)	(3.08)	(2.96)
	.33	.32	.37	.31
ln negative sanctions per 1m, 1973–77	0.54*	0.39*	0.33	0.46*
	(2.51)	(2.09)	(1.47)	(2.44)
	.28	.20	.17	.23
ln negative sanctions per 1m, 1968–72	-0.38		0.13	
	(-1.37)		(0.50)	
	- .17		.06	
ln deaths from political violence, 1968–72	0.48*	0.38*		
	(3.54)	(3.27)		
	.42	.34		
R_a^2	.55	.54	.46	.46
Number of cases	60	60	61	62

Note: t-ratio in parentheses; standardized regression coefficient underscored.
*p < .05, one-tailed.

When the nonsignificant lagged negative-sanctions variable is removed, the results of Equation 1.2 are quite similar to those of 1.1. (Energy consumption is retained in 1.2 because it is significant in 1.1 at the .05 level for a two-tailed test.)

The regressions for explaining variation in level of political violence are reported in Equations 1.3 and 1.4. The hypothesis of a direct effect of income inequality on level of political violence is confirmed in Equation 1.3. The unexpected positive effect of energy consumption is weak (and not significant at the .05 level for a two-tailed test) in 1.3, while the effect of lagged negative sanctions changes from negative to positive but is very weak and nonsignificant. Otherwise, the effects are as predicted both in Equation 1.3 and in Equation 1.4, where nonsignificant variables are removed.

The unexpected and implausible positive relationship between economic development and political violence is thus observed only in regard to change in the political-violence death rate. The positive income inequality effect, as well as the positive effects of high intensity of separatism, the presence of a semirepressive regime structure, and the rate of negative sanctions,[17] hold in regard to explanation of variation in change in and in level of the political-violence death rate.

The equations of Table 1 specify an additive effect of income inequality on political violence. Is this the correct specification? Is the magnitude of the inequality effect more or less the same across all countries, regardless of their regime structure?

A strong version of the alternative inequality-repressiveness–interaction hypothesis entails two predictions: (1) that the product of income inequality and the semirepressive-regime dummy variable will have a significant direct effect on political violence and (2) that income inequality alone will not have a significant direct effect on political violence. A weaker version of the inequality-repressiveness–interaction hypothesis entails only the first prediction. Regressions that test the hypothesis of an interaction between income inequality and a semirepressive regime are reported in Table 2. The possibility of an independent effect of a semirepressive regime cannot be evaluated because of multicollinearity: this variable correlates with the product of itself and income inequality at $r = .99$ for the 60 countries and at $r = .98$ for the 62 countries used to estimate the equations for explaining change in and level of political violence, respectively.

Equation 2.1 includes the inequality × semirepressiveness product term in addition to the significant variables from Equation 1.2. Equation 2.2 excludes the possibility of an additive effect of income inequality. The additive effect is significant in Equation 2.1. Exclusion of it in 2.2 reduces R_a^2. Support thus is observed for the specification of an additive effect of income inequality on change in political violence. The product term is also significant in Equation 2.1. This finding indicates that the presence of a semirepressive regime may *enhance* the strength of the income-inequality effect. R_a^2 for 2.1 is identical, however, to that for 1.2. Therefore, the product term is a superfluous variable. The results of Equations 2.3 and 2.4 (re level of political violence) are similar to those of 2.1 and 2.2 but slightly less clearcut. On the one hand, the strength of the additive effect of income inequality in 2.3 is weaker than in 2.1; on the other hand, inclusion of the product term produces only a trivial increase in R_a^2 (from .46 in Equation 1.3 to .47 in Equation 2.3). In sum, (1) there is no evidence to indicate that the additive specification is incorrect, but (2) there is also a possibility that the effect of income inequality on political violence may be greater in semirepressive regimes than in nonrepressive or in extremely repressive regimes.

Another important issue is the question of the robustness of the income inequality effect. Two tests of robustness will be performed. First, we will assess the extent to which the income-inequality parameter estimates from Equations 1.2 and 1.4 are sensitive to the presence of influential cases. Then we will determine whether the inequality effects hold across a more restrictively defined sample.

According to Cook's D,[18] the most influential case in Equation 1.2 is Sudan, which registers a value of .18 (the next highest values are .14 and .13, registered by Indonesia and Panama, respectively). Equation 3.1 of Table 3 shows that when Sudan is deleted, the unstandardized and standardized regression coefficients for income inequality remain virtually unchanged (.46 and .33) in comparison with the full sample estimates (.045 and .31) from Equation 1.2. In Equation 1.4 two cases are most influential: Spain, with a Cook's D of .24, and Indonesia, with a Cook's D of .23 (the next highest values are .15 and .13, registered by Portugal and Sudan, respectively). Equation 3.3 shows that when Spain and Indonesia are

Table 2. Regressions of Political Violence and Income Inequality with Tests for Interaction with the Presence of a Semirepressive Regime

	ln Deaths from Political Violence per 1m, 1973–77			
	Change		Level	
Explanatory Variables	2.1	2.2	2.3	2.4
Intercept	-3.41	-1.00	-1.21	0.028
Upper 20% income share, ca. 1970	0.038* (2.31) .26		0.027* (1.73) .19	
Upper 20% income share × semirepressive regime	0.016* (2.93) .34	0.020* (3.94) .43	0.016* (3.07) .34	0.021* (4.57) .44
ln energy consumption per capita, 1970	0.20 (1.97) .23	0.11 (1.13) .13		
Intensity of separatism, 1975	0.83* (2.39) .23	0.80* (2.20) .22	1.24* (3.62) .35	1.20* (3.45) .34
ln negative sanctions per 1m, 1973–77	0.42* (2.30) .21	0.43* (2.26) .22	0.48* (2.54) .24	0.47* (2.47) .24
ln deaths from political violence, 1968–72	0.39* (3.34) .34	0.37* (3.06) .33		
R_a^2	.54	.51	.47	.45
Number of cases	60	60	62	62

Note: t-ratio in parentheses; standardized regression coefficient underscored.
*p < .05, one-tailed.

deleted, the unstandardized and standardized regression coefficients for income inequality are smaller (.027 and .19) than the full-sample estimates (.034 and .24) from Equation 1.4, but the income-inequality effect in 3.3 nevertheless remains significant at the .05 level. Thus the effect of income inequality on political violence is always statistically significant, regardless of influential cases, but its estimated causal weight in regard to explanation of variation in the level of political violence is enhanced somewhat by the presence of influential cases.[19]

The second test of robustness is severe because it entails eliminating almost one-third of the sample. Our primary source of information on income distribution is the *World Development Report*, published annually by the World Bank since 1978. These data have been supplemented with information from other sources—

Table 3. Regressions of Political Violence and Income Inequality with Tests of the Robustness of the Inequality Effect

	ln Deaths from Political Violence per 1m, 1973–77			
	Change		Level	
Explanatory Variables	3.1[a]	3.2[b]	3.3[a]	3.4[b]
Intercept	-4.13	-3.69	-1.26	-1.42
Upper 20% income share, ca. 1970	0.046* (2.97) .33	0.040* (1.76) .27	0.027* (1.96) .19	0.035* (1.67) .24
ln energy consumption per capita, 1970	0.25 (2.43) .30	0.22 (1.73) .29		
Intensity of separatism, 1975	0.68* (1.93) .18	1.05* (2.40) .30	1.69* (4.98) .44	1.38* (3.10) .39
Semirepressive regime, 1973–77	1.01* (3.34) .39	0.84* (2.34) .36	0.97* (3.76) .37	0.62* (1.79) .25
ln negative sanctions per 1m, 1973–77	0.39* (2.16) .21	0.40* (1.91) .23	0.50* (2.62) .23	0.40* (1.81) .23
ln deaths from political violence, 1968–72	0.32* (2.70) .28	0.44* (3.07) .40		
R_a^2	.54	.48	.56	.37
Number of cases	59	43	60	44

Note: t-ratio in parentheses; standardized regression coefficient underscored.

*$p < .05$, one-tailed.

[a]Most influential case(s) deleted.

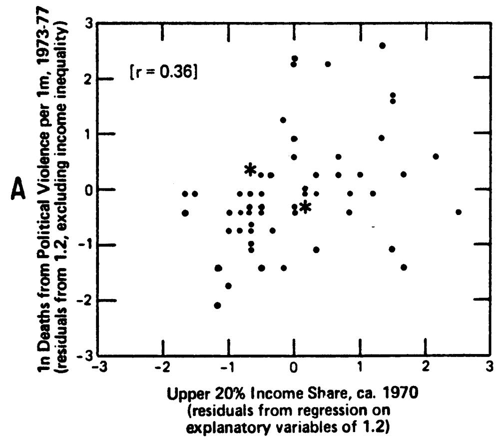
[b]Excluding countries with data on income distribution exclusively from sources other than the World Bank's *World Development Report*.

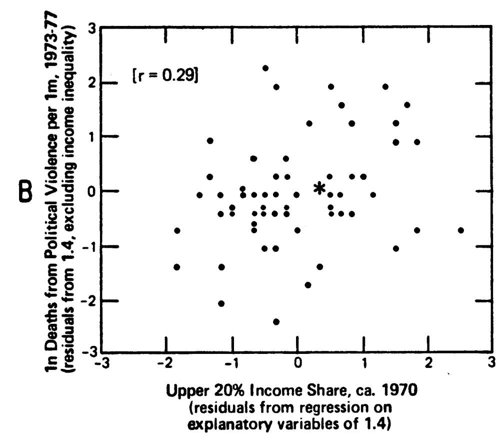
including previous World Bank compilations—that seem to be reliable. From Table A-1 one may observe that 18 countries have data on income distribution exclusively from sources other than the *World Development Report* (denoted *W*). We have included them in the interest of maximizing sample size. Let us see if excluding them makes a difference.[20]

Equation 3.2 includes the same variables as 1.2 but is estimated across 43 instead of 60 cases.[21] Although the t-ratio for the income-inequality parameter estimate is substantially reduced, it still indicates the presence of a significant effect at the .05 level. More important, the unstandardized and standardized regression coefficients for income inequality in Equation 3.2 are only slightly smaller than the full-sample estimates. Equation 3.4 includes the same variables as 1.4 but is estimated across 44 instead of 62 cases. Again, although the t-ratio for the income-inequality effect is substantially reduced, the null hypothesis still can be rejected with 95% confidence. And in this equation the unstandardized and standardized regression coefficients for income inequality are virtually identical with those of the full sample.

A final question that should be considered is whether heteroskedasticity remains a problem. We observed from the scatterplot of Figure 2 that heteroskedasticity was present in the bivariate relationship between income inequality and the death rate from political violence. Plot A of Figure 3[22] shows a standardized partial plot of the political violence residuals by the income-inequality residuals, when both variables are regressed on the other explanatory variables from Equation 1.2. The correlation between these residuals (r = .36) is equal to the partial correlation between income inequality and the 1973–77 death rate from political violence, controlling for the other explanatory variables. If heteroskedasticity has been removed by

Figure 3. Standardized Partial Plots of Political Violence Death Rate Residuals from Equations 1.2 and 1.4 (Excluding Income Inequality) by Income Inequality Residuals (Income Inequality Regressed on the Other Explanatory Variables)



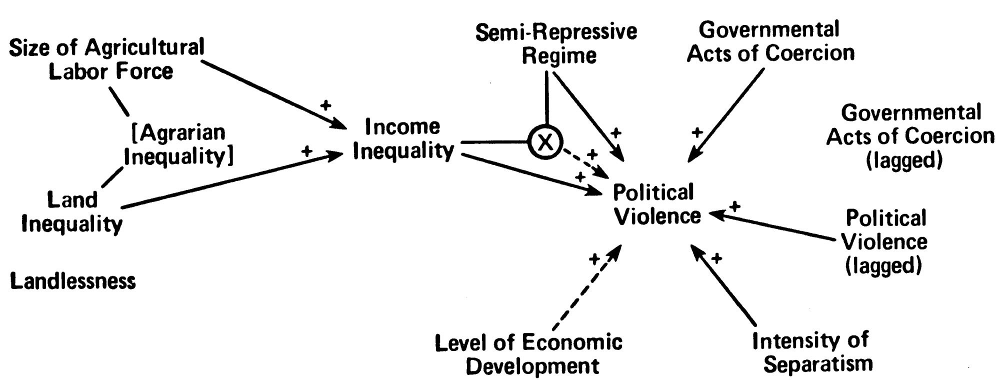


inclusion of additional variables, the standardized partial residual plot should be approximately linear; whereas, a fan-shaped joint distribution would indicate that heteroskedasticity had not been removed. The observed joint distribution of scores clearly is linear instead of fan-shaped, so we conclude that the heteroskedasticity in the relationship between income inequality and political-violence death rate has been substantially reduced by taking the other explanatory variables into account.[23]

Plot B of Figure 3 shows a standardized partial plot of the political-violence residuals by the income-inequality residuals, when both variables are regressed on the other explanatory variables from Equation 1.4. The distribution of scores in plot B also conforms to a linear trend rather than being fan-shaped, so it appears that, in the multivariate context, heteroskedasticity has again been more or less eliminated.

The results of testing the inequality hypotheses in the context of a multivariate model of determinants of political violence are summarized in Figure 4. All of the evidence that we have considered points to the presence of a robust, positive monotonic (positively accelerated) relationship between income inequality and political violence that is independent of the other variables in the model. The effect of income inequality on political violence may be enhanced by the presence of a semirepressive regime, but the evidence is not conclusive in that regard, so we represent the possibility of an interaction between income inequality and semirepressiveness by dashed arrows. The other solid arrows linking explanatory variables to political violence also denote relationships that hold for change as well as level of violence and seem to be robust. We have tested the regime-repressiveness hypothesis with a dummy variable in this study (in order to take into account the possibility of an interaction with income inequality). It should be noted, however, that the same kind of effect appears if regime repressiveness is expressed as a continuous quantitative variable—that is, if the semirepressive-regime dummy variable is replaced by regime repressiveness and its square, a statistically significant nonmonotonic-inverted-U-curve relationship between regime repressiveness and political violence is consistently observed in multivariate equations that include income inequality and the other explanatory variables. We have not tested for the possibility of an instantaneous reciprocal relationship between political violence and governmental acts of coercion (see Hibbs 1973) because that is a complex topic requiring a separate paper. From preliminary work, however, we are confident that it is valid to infer the pres-

Figure 4. Observed Causal Paths in the Multivariate Causal Model



ence of a positive effect of current governmental acts of coercion on current political violence.

The causal paths linking the maldistribution of land variables to income inequality are inferred from the following regression equations:

$$U20INCOME = 33.60 + \underset{(5.09)}{0.33}(AGINEQUAL) + \underset{(0.28)}{0.028}(LANDLESS) \quad (4)$$
$$R_a^2 = .55 \qquad N = 46$$

$$U20INCOME = 33.02 + \underset{(8.10)}{0.36}(AGINEQUAL) \quad (5)$$
$$R_a^2 = .55 \qquad N = 54$$

$$U20INCOME = 26.56 + \underset{(5.85)}{23.96}(GINILAND) + \underset{(7.40)}{0.21}(\%AGLABOR) \quad (6)$$
$$R_a^2 = .60 \qquad N = 54$$

In Equation 4, which compares the effects on income inequality of agrarian inequality and landlessness, landlessness is an irrelevant variable (the t-ratio is virtually zero). Equations 5 and 6 compare the multiplicative index of agrarian inequality with its components (the size of the agricultural labor force is denoted *%AGLABOR*) as determinants of income inequality. Since the R_a^2 for the multiplicative model (5) is lower than that for the additive model (6), we infer that land inequality and size of agricultural labor force have independent additive effects on income inequality instead of a multiplicative-interaction effect.

The only completely irrelevant variable in the model is landlessness, a finding that runs counter to the strong claim of causal importance for this variable made by Prosterman (1976). Moreover, at least as a general determinant of mass political violence, the condition of high agrarian inequality also fails to warrant the strong causal claims made for it by many scholars. The components of agrarian inequality, land inequality and size of the agricultural labor force, affect income inequality and, therefore, are indirectly relevant to political violence, but neither the weighted index of agrarian inequality nor land inequality per se has any direct effect on political violence.

Discussion

The finding that agrarian inequality is relevant only to the extent that it is associated with inequality in the nationwide distribution of income has important policy implications. Land reform in third world countries all too often is considered to be a panacea for problems of inequality. However, as Huntington (1968, 385) points out, redistribution of land is the most difficult of reforms for modernizing governments because it almost always entails some degree of outright confiscation. And our study indicates that land redistribution is also not necessarily the most meaningful of reforms. If land redistribution is carried through to the point of actually effecting an egalitarian redistribution of income, as seems to have been the case in countries as diverse as Taiwan and Egypt, and/or if other economic development policies do not exacerbate income inequality, then land reform can make a contribution to the promotion of political stability. However, there are cases such as Bolivia and Mexico in which land reform has not been associated with egalitarian income redistribution. Land reform without income redistribution is probably at best merely a temporary palliative; and at worst, as the case of Iran demonstrates, it can be quite counterproductive by alienating powerful conservative groups such as the nobility and the clergy. Indeed, by simultaneously encouraging both land reform and a policy of rapid economic growth that ignored inegalitarian distributional consequences, U.S. advisors to the shah would appear unwittingly to have exacerbated the economic preconditions of revolution in Iran.

If the effect of income inequality on change in political violence and its level, observed for 60 and 62 cases, is reliable and more or less generalizable across time in the contemporary world (at least for nontraditional societies where modern values like equality can be assumed to have become salient), it follows that redistribution of income must be ranked as one of the more meaningful reforms that a modernizing government can undertake in the interest of achieving political stability. Unfortunately, redistribution of income may conflict not only with the class interests of many third world governments but also with their predilection for rapid industrialization. The shah's great dream of surpassing Sweden by the year 2000 was dashed in part by his single-minded concern with economic growth and the raising of per capita income. As Green (1982, 70–71) points out, "the premise of the Pahlavi development ethos rested on the assumption that economic development was more important than political rights or justice." Iran in the years immediately preceding the revolution indeed registered an extraordinary growth of per capita gross national product, which averaged an increase of 13.3% annually during 1970–78, the highest rate of growth of GNP per capita in the world (see Taylor and Jodice 1983, vol. 1, tbl. 3.6); but at the same time that per capita income was increasing phenomenally, the distribution of that income was apparently becoming more concentrated at the top, presumably heightening perceptions of economic injustice. It is important to emphasize, however, that there is no necessary trade-off between rapid economic growth and income inequality. Taiwan's average annual growth of GNP per capita during 1960–78 was 6.6% (see World Bank 1980, tbl. 1), a rate that, although surpassed by Iran (the world leader excluding Romania), was nevertheless almost twice as high as the average rate (3.7%) for all middle-income countries. At the same time (1964–78), the income share of the richest 20% of households in Taiwan declined from 41.1% to 37.2% (see Tsiang 1984, tbl. 9). Thus, by following a different set of economic policies than the shah, the government of Taiwan achieved growth with equity. And the death rate from political violence in Taiwan during 1973–77 was .06, as compared with Iran's rate of .91.

Political rights also are a relevant source of stability and instability, not only as was the case in Iran but, as it appears from our cross-national results, globally. The problem faced by modernizing governments in this regard is that political rights must either be granted fully, in which case the government is allowing for the real possibility of being voted out of power, or not be granted at all, in which case the government must enforce a degree of totalitarian control over the populace that is costly to maintain and is probably inherently at odds in the long run with a capitalist economic system.

Another problem faced by all governments is that the use of coercion against dissidents—closing down their newspapers, preventing them from assembling in public, and imprisoning them or killing them if they disobey—seems in the short run to provoke violence rather than deter it. If the political system is open and liberal, however, the rate of coercion-provoked violence will usually not reach regime-threatening proportions because the presence of meaningful nonviolent possibilities of influencing the political process will inhibit the ability of revolutionary-minded dissidents to mobilize large followings. Also, if the political system is so repressive that dissidents have little or no opportunity to organize, then coercion-provoked violence probably will not become regime threatening. The worst strategy for preventing regime-threatening rates of mass political violence would seem to be that of attempting to suppress opposition by acts of coercion while at the same time maintaining a semirepressive regime structure that permits some organization and expression of discontent but does not give dissident groups genuine opportunities to participate effectively in political decision making. Were this strategy to be pursued by the political leadership in countries with high levels of income inequality and/or a high potential for separatism, the results of our cross-national analysis indicate that the likelihood of a high level of political violence and protracted civil war would become very strong.

Appendix

Data on land inequality, landlessness, and income inequality in independent states are listed by region in Table A-1.

Table A-1. Distribution of Land and Income Circa 1970

	Land Concentration		Agricultural Households without Land as % of Total	Income Share of Upper 20%	
	Year	Gini	Labor Force	Source and Year	%
Europe & North America					
Austria	1970	.70	3.6		
Belgium	1970	.59	0.4	*W* 1974–75	39.8
Canada	1971	.51	1.6	*W* 1969, 1977	41.5
Denmark	1970	.43	2.0	*W* 1976	37.5
Finland	1970	.25	1.3	*W* 1977	36.8
France	1971	.53	2.9	*W* 1970, 1975	46.4
Germany, East	collectivized			*A* 1970	30.7
Germany, West	1971	.51	0.6	*S* 1970; *W* 1973, 1974	45.5
Greece	1971	.48	4.1		
Hungary	collectivized			*A* 1969	33.4
Ireland	1970	.49	4.5	*W* 1973	39.4
Italy	1970	.75	4.0	*W* 1969, 1977	45.2
Luxembourg	1970	.46			
Malta	1969	.53			
Netherlands	1970	.47	0.4	*W* 1967, 1975, 1977	39.0
Norway	1969	.30	0.7	*W* 1970	37.3
Poland	1970	.46	5.1		
Portugal	1968	.81	7.3	*W* 1973–74	49.1
Spain	1960	.80	9.6	*W* 1974	42.2
Sweden	1971	.23	0.9	*W* 1972	37.0
Switzerland	1969	.51	2.0	*B* 1968	45.9
United Kingdom	1970	.69	0.8	*S* 1972; *W* 1973	39.1
United States	1969	.72	1.3	*W* 1972	42.8
Yugoslavia	1969	.56	3.5	*A* 1968; *W* 1973	40.8
Central & South America					
Argentina	1960	.87	8.2	*W* 1970	50.3
Barbados				*J* 1969–70	44.0
Bolivia			12.8	*P* 1968	61.0
Brazil	1970	.84	29.2	*W* 1972	66.6
Chile				*W* 1968	51.4
Colombia	1971	.86	13.6	*A* 1970	59.4
Costa Rica	1973	.82	24.5	*T* 1971, 1974, 1977	52.1
Dominican Republic	1971	.82	31.2		

TABLE A-1 (continued)

	Land Concentration		Agricultural Households without Land as % of Total	Income Share of Upper 20%	
	Year	Gini	Labor Force	Source and Year	%
Ecuador	1974	.82			
El Salvador	1971	.81	39.8	*J* 1965–67, 1969; *W* 1976–77	52.1
Guatemala	1964	.82	17.7		
Haiti	1971	.47			
Honduras	1974	.78		*W* 1967	67.8
Jamaica	1969	.80	8.9	*A* 1958; *US* 1982	58.3
Mexico	1970	.93	16.7	*A* 1969; *W* 1977	59.2
Nicaragua	1963	.80		*L* 1977	60.0
Panama	1971	.78	36.2	*W* 1970	61.8
Peru	1972	.91	10.8	*W* 1972	61.0
Trinidad & Tobago	1963	.69		*W* 1975–76	50.0
Uruguay	1970	.82	8.4	*A* 1967	47.4
Venezuela	1971	.91	5.9	*W* 1970	54.0
Middle East & North Africa					
Algeria	1973	.65	0		
Egypt	1964	.67	16.3	*W* 1974	48.0
Iran	1960	.63			
Iraq	1971	.61	20.0		
Israel	1971	.75			
Jordan			21.0		
Kuwait	1970	.75			
Lebanon	1970	.77			
Libya	1960	.70			
Saudi Arabia	1974	.78	17.2		
Syria	1971	.67			
Tunisia				*A* 1970	55.0
Turkey	1963	.59	24.4	*W* 1973	56.5
Sub-Saharan Africa					
Botswana	1969	.47			
Cameroon	1973	.42			
Central African Republic	1974	.34	6.4		
Chad	1973	.34	3.6		
Congo	1973	.27	2.9		
Gabon	1975	.41		*A* 1968	67.5
Ghana	1970	.55	14.6	*R* 1968	47.8
Ivory Coast	1975	.42		*A* 1970	57.2
Kenya	1974	.67	12.3	*W* 1976	60.4
Lesotho	1970	.36	18.8		
Liberia	1971	.73			
Malawi	1969	.34	8.9	*W* 1967–68	50.6
Mali	1960	.48			
Nigeria			6.2		
Sierra Leone	1971	.43	17.2	*W* 1967–69	52.5
South Africa	1960	.70	14.2	*J* 1965	62.0
Sudan				*W* 1967–68	49.8
Tanzania	1972	.45		*W* 1969	50.4
Togo	1970	.52			
Zaire	1970	.57	12.7		
Zambia	1971	.76	8.0	*W* 1976	61.1
Zimbabwe				*J* 1968	69.1

TABLE A-1 (continued)

	Land Concentration		Agricultural Households without Land as % of Total	Income Share of Upper 20%	
	Year	Gini	Labor Force	Source and Year	%
Asia					
Australia	1971	.82	3.2	*W* 1966–67, 1975–76	43.0
India	1971	.62	38.1	*J* 1968; *W* 1975–76	51.3
Indonesia	1973	.56	22.5	*J* 1971; *W* 1976	50.7
Japan	1970	.42	1.4	*W* 1969	41.0
Korea, South	1970	.31	10.2	*J* 1971; *W* 1976	44.4
Malaysia	1960	.47	12.2	*W* 1970, 1973	56.4
Nepal	1972	.56		*W* 1976–77	59.2
New Zealand	1972	.73	4.8	*S* 1966	41.4
Pakistan	1972	.51	32.4	*J* 1971	41.5
Philippines	1971	.51	35.6	*W* 1970–71	54.0
Singapore	1973	.30			
Sri Lanka	1973	.67		*W* 1969–70	43.4
Taiwan	1960	.46	5.0	*W* 1971	39.2
Thailand	1963	.46	20.0	*W* 1975–76	49.8
Vietnam, South	1960	.59			

Concentration of Land

Data for 1966–75 are from the Food and Agriculture Organization (FAO) of the United Nations (1981). The Gini index of concentration is calculated using total areas of farms (in hectares) and standardized size categories. Data prior to 1966 are considered to be reasonable estimates of 1966–75. The 1964 Gini score for Guatemala is from Seligson et al. 1983; other pre-1966 data are from Table 4.14 of Taylor and Hudson (1972).

Landlessness

These data, circa 1970, are derived from calculations by Prosterman and Riedinger (1982, tbl. 17) of the proportion of agricultural families that are owner-operators and collective or state farmers based on information from the 1960 and 1970 rounds of the FAO World Census of Agriculture and collateral estimates. Countries with 50% or more collective or state farmers are excluded. Agricultural households without land as a proportion of the total labor force are estimated by multiplying the proportion of agricultural families that are not owner-operators or state farmers by the percentage of the labor force employed in agriculture circa 1970 (from the data file of the *World Handbook of Political and Social Indicators*—see Taylor and Jodice 1983).

Income Share of the Richest Quintile

The sources of the data are *A*, Ahluwalia (1976); *B*, Bornschier and Heintz (1979); *F*, Fei, Ranis, and Kuo (1979); *J*, Jain (1975); *L*, Leal (1983); *P*, Paukert (1973); *R*, Roberti (1974); *S*, Sawyer (1976); *T*, Trejos (1983); *US*, United States Agency for International Development (1983); *We*, Webb (1976); *W*, World Bank (1979–85). If more than one source is listed, the income share is the average value. Most data are for households, but in the interest of maximizing coverage, data for individuals are used in the cases of Barbados, Bolivia, Colombia, Gabon, Ghana, Hungary, Ivory Coast, South

Africa, Switzerland, Tunisia, and Zimbabwe. Data are not used if they pertain to economically active males only, to workers only, or otherwise are not comprehensive (e.g., based on tax statistics). The data for developed countries are posttax; data for less-developed countries are not adjusted for taxation but can be regarded as comparable to posttax data in developed countries (World Bank 1979, 185–86). Note that Iran is not included. The expenditure data analyzed by Jabbari (1981) and discussed in the text are not strictly comparable with the data in Table A-1; they probably underestimate the extent of income inequality that existed in Iran under the shah.

Notes

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1. Midlarsky and Roberts distinguish between these cases in regard to the dynamics of coalition formation leading to different kinds of revolutionary movements. Although both countries had inegalitarian distributions of land, creating a potential for insurgency in each case, the revolutionary movement in El Salvador was more narrowly class-based than in Nicaragua, due to differences in population density that produced greater land scarcity in El Salvador than in Nicaragua. This difference is thought to have enhanced the likelihood of a successful revolution in Nicaragua.

2. Unless otherwise noted, data on land and income distribution referred to in the text are either from Table A-1 or, for years other than those in Table A-1, from the sources cited therein.

3. Based on a study reported by Cespedes (1979). Trejos (1983) reports the income share of the richest 20% of households in Costa Rica as 51.1% in 1971, 52.1% in 1974, and 53% in 1977.

4. These data are from Keddie 1968; see also Bharier 1971.

5. These and all subsequent data on civil and political liberties referred to in the text are calculated from the data file of the *World Handbook of Political and Social Indicators*. For a description of the ratings, see Taylor and Jodice 1983 (1:58–65).

6. The other three significant determinants of internal war, apart from lagged internal war, were current and lagged negative sanctions and a dummy variable for presence versus of a communist regime. The inhibitory effect of a communist regime is subsumed under our more general regime-repressiveness hypothesis.

7. The only previous research on this topic is reported in Midlarsky 1981, where income distribution is measured by an index of intersectoral inequality. As Sigelman and Simpson (1977, 111) have pointed out, however, this index "is at best a second-rate measurement proxy for personal income, lacking theoretical interest of its own."

8. Deaths from political violence are an attribute of political-protest events like riots, armed attacks, and assassinations. Deaths are thus a summary measure of the intensity of political-protest events. Deaths are used in preference to a composite index for the following reasons: (1) a single-variable indicator is more easily interpretable than a composite measure; (2) deaths will necessarily correlate very strongly with a composite measure such as that constructed by Hibbs (1973), which includes deaths, armed attacks, and assassinations; and (3) there is probably less reporting bias for deaths than for indicators such as armed attacks (see Weede 1981). Death *rate* is preferred over raw counts because the former is an indicator of the extent to which the regime is threatened by insurgency, which depends not on the absolute frequency of political violence but rather on its frequency relative to size of population (for further discussion of this issue see Linehan 1976; Muller 1985; and Weede 1981). The logarithmic transformation is theoretically appropriate because death rate from political violence is expected to vary as a positively accelerated function of inequality; it is also necessary because of the presence of extreme values—although the problem of extreme values still exists after logging. An increment of one is added to each death score before logging because the log of zero is undefined.

9. In testing the multivariate model across 62 cases, the following countries are missing data on intensity of separatism: Barbados, Gabon, Honduras, Ivory Coast, Malawi, Nepal, Sierra Leone, and Trinidad and Tobago. Based on country descriptions from Banks 1976, these countries were scored zero on intensity of separatism.

10. The number of cases is 83 because Ireland and South Vietnam are coded as missing on political violence for the 1973–77 period.

11. An alternate multiplicative model, which includes Gini land concentration, the size of the agricultural labor force, and the product of these variables, yields an R_a^2 of .06, a value that is identical to the R_a^2 for Gini land concentration alone.

12. An alternative operationalization of the landlessness concept is to express landlessness as a proportion of the agricultural labor force instead of as a proportion of the total labor force. Regression of the 1973–77 political-violence death rate on landlessness as a proportion of the agricultural labor force yields

an R_a^2 of .04, which is identical to that for landlessness as a proportion of the total labor force.

13. Because death rates are logged, the underlying model is monotonic but curvilinear, that is, unlogged death rate would plot as a positively accelerated function of income inequality.

14. Gabon, Brazil, Panama, and the United Kingdom are instances of deviant cases that depress the strength of the relationship between income inequality and political violence, while Zimbabwe is an instance of a case that enhances its strength because of an extreme score on the independent variable. The most influential case, as determined by Cook's D, is Gabon, followed by Zimbabwe, Brazil, the United Kingdom, and Panama. If Gabon is deleted, the regression coefficient for income inequality rises to .064, R_a^2 rises to .18, and the remaining most influential case is Brazil; if Brazil is then deleted, the income inequality coefficient rises to .072, R_a^2 rises to .22, and the remaining most influential case is Panama; if Panama is then deleted, the income inequality coefficient rises of .078, R_a^2 rises to .25, and the remaining most influential case is the United Kingdom; if the United Kingdom is then deleted, the income inequality coefficient rises to .084, R_a^2 rises to .29, and the remaining most influential case is now Pakistan (the sixth most influential case in the initial regression); if Pakistan is then deleted, the income inequality coefficient rises to .088, R_a^2 rises to .33, and the remaining most influential case is Zimbabwe; if Zimbabwe is then deleted, the income inequality coefficient declines to .082, R_a^2 declines to .28, and no further cases are influential.

15. One-tailed t-tests are used because the expected relationships are specified a priori in the theoretical model depicted in Figure 1.

16. There are 63 cases with information on income distribution. Of these, Ireland is coded as missing on political violence for both the 1968–72 and 1973–77 period, while Pakistan is coded as missing for the 1968–72 period. Information on energy consumption per capita is not available for Taiwan. Thus, depending on whether lagged violence and/or energy consumption per capita are included in a given equation, the N will vary between 60 and 62.

17. As predicted, the rate of negative sanctions is relatively independent of the structural repressiveness of the regime. The regime repressiveness variable correlates with the 1968–72 and 1973–77 negative sanctions variables at .26 and .21, respectively.

18. There are essentially two kinds of influential cases that could distort regression results: outliers (or extreme errors of prediction) and cases with extreme scores on the independent variables. Outliers can be diagnosed from the residuals, while Mahalanobis's distance is an index of the extent to which a particular case in a multivariate equation has unusual values of the independent variables. Cook's D is a useful summary measure of the extent to which a data point is influential due to being an outlier and/or having an extreme combination of scores on the independent variables. For a good nontechnical discussion of these diagnostics see Norusis 1986 (sec. B 207–13).

19. Weede (1986) has shown that Zimbabwe is an influential case in regard to the strength of the income-inequality effect across a smaller sample analyzed previously (see Muller 1985). Since Zimbabwe was influential principally because of an extremely high death rate from political violence, it was possible to take corrective action by setting a ceiling on the death rate, and Zimbabwe was no longer influential once this was done (Muller 1986). Zimbabwe also is not influential for this sample. It is not among the 10 highest values of Cook's D in regard either to Equation 1.2 or to 1.4; and when Zimbabwe is deleted from 1.2, the income-inequality parameter estimates do not change, while deletion of Zimbabwe from 1.4 also makes virtually no difference (the unstandardized regression coefficient is reduced from .034 to .032 and the standardized coefficient is reduced from .24 to .22).

20. We have not recalculated the upper 20% income shares for countries with average values derived from other sources in addition to the *World Development Report* because in every instance the other sources are either previous World Bank compilations (denoted *A* and *J*) or an Organization for Economic Development and Cooperation (OECD) compilation (denoted *S*) used in part as a source for data in the *World Development Report*.

21. The number of cases is 43 because Pakistan was not included in Equation 1.2 and also is one of the 18 deleted cases.

22. An asterisk denotes the presence of three or more cases.

23. Brazil and Panama are still deviant cases, however (Panama probably because of a high level of acts of governmental coercion, Brazil probably because of the presence of a semirepressive regime). They are the two data points in the lower-right-hand corner of the graph with income-inequality residuals slightly less than 2 and political violence residuals approximately equal to −1. Because other countries with low inequality residuals also have even lower political violence residuals than Brazil and Panama, heteroskedasticity is less of a problem.

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