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REPRESSION OF HUMAN RIGHTS TO PERSONAL INTEGRITY IN THE 1980S: A GLOBAL ANALYSIS

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T*his crossnational study seeks to explain variations in governmental repression of human rights to personal integrity (state terrorism) in a 153-country sample during the eighties. We outline theoretical perspectives on this topic and subject them to empirical tests using a technique appropriate for our pooled cross-sectional time-series design, namely, ordinary least squares with robust standard errors and a lagged dependent variable. We find democracy and participation in civil or international war to have substantively important and statistically significant effects on repression. The effects of economic development and population size are more modest. The hypothesis linking leftist regime types to abuse of personal integrity rights receives some support. We find no reliable evidence that population growth, British cultural influence, military control, or economic growth affect levels of repression. We conclude by considering the implications of our findings for scholars and practitioners concerned with the prevention of personal integrity abuse.*

In the past two decades there has been a burgeoning of information on governmental terror and the abuse of internationally recognized human rights in countries around the globe (see Cain, Claude, and Jabine 1992).¹ While the development of theories to explain why, and to predict when, such crimes will be committed would seem to be a vital undertaking, social science scholars have only begun to use the newly developed information toward this end. To date only a few studies have attempted to construct and test theories seeking to explain variations in the levels of repression found in countries around the world (Henderson 1991, 1993; McKinlay and Cohan 1975, Mitchell and McCormick 1988; Park 1987).

We seek to build upon the strengths of existing empirical studies on this question, while improving upon them in several respects. We therefore construct a model of the most dramatic form of repression—repression of personal integrity rights—that tests several hypotheses suggested by previous studies, but goes on to test hypotheses suggested by theories not considered in previous empirical work on repression. The data set we employ clearly represents the most comprehensive yet analyzed in a global study of national human rights practices in terms of the number of countries and years it covers, as it includes relevant characteristics and behaviors for a pooled cross-sectional time-series sample of 153 countries for the eight years 1980–87.

RESEARCH ON HUMAN RIGHTS AND REPRESSION

Recent years have seen the publication of several empirical studies relating to human rights, but the tendency has been to focus on human rights practices as an independent variable, specifically as a determinant of foreign policy outputs.² The first studies that

sought to explain variations in human-rights-related phenomena apparently were conducted by McKinlay and Cohan (1975, 1976), who analyzed the general policy performance of military, as compared to civilian, regimes. For the period 1951–70, McKinlay and Cohan (1975) compare the performance of their two types of regimes with respect to a number of “political variables” relevant to human rights—the proportion of regime-years in which constitutions were declared not in full force and the proportions in which assemblies, political parties in general, or the Communist party were banned. Their statistical analyses demonstrated that poor regimes were more likely than richer ones—and military regimes more likely than civilian ones—to ban constitutions, assemblies, and parties.

McKinlay and Cohan’s work was accomplished before Amnesty International, the U.S. Department of State, or Freedom House had begun to publish their assessments of human rights performance annually and with worldwide coverage. More recently, spurred by increasing public and scholarly interest in human rights issues, a few studies have appeared that seek to explain the variations in these more general assessments of regime performance in human rights. In a brief article, Park (1987) reports statistically significant positive relationships between political rights, evidently measured by inverting the Freedom House civil political rights index, and a physical quality of life index, urbanism, welfare expenditures, ethnic diversity, and percent Christian population. He reports statistically significant negative relationships for political rights with military expenditures, education expenditures, and percent Muslim population and no relationship for inequality and political rights.

Ambiguities in Park’s discussion of measurement procedures and the bivariate and essentially atheoretical nature of his statistical analyses make us wary of the relationships that he found. Nevertheless,

Park's research is potentially useful as a source of implied hypotheses to be tested in a more rigorous framework.

More recently Mitchell and McCormick (1988) went well beyond Park's study by framing several hypotheses to explain national violations of the integrity of the person through the use of imprisonment and of torture, assessed through their ratings of the reports issued by Amnesty International in 1984. Their analyses indicated that economic conditions, as measured by per capita gross national product (GNP), were negatively, if moderately, related to both types of repression and that experience with British colonial rule and authoritarian (as opposed to totalitarian) form of government, were significantly negatively related to the taking of political prisoners but not to the use of torture. Capitalist trade and investment (dependency) and length of independence, on the other hand, were not significantly related to either aspect of repression.

The strength of Mitchell and McCormick's analysis lies in its delineation of hypotheses that theoretically might explain, in part, variations in repression. Its major weakness, as the authors acknowledge, is that its analyses of these theoretically important relationships are effectively bivariate.

The most recent additions to the literature seeking to explain variations in repression are a pair of studies by Henderson (1991, 1993). Henderson hypothesized that democracy, economic growth, and economic development would reduce—and "socioeconomic needs" and inequality would increase—levels of repression (1991, 123–27). His multiple regression model for repressive behaviors, using a measure created from the State Department's *Country Reports on Human Rights Practices for 1985*, confirmed his hypotheses concerning the effects of democracy (negative), inequality (positive), and economic growth (negative) on repression but found no significant relationship for level of economic development.

Henderson (1993) focuses on the impact of population variables on his governmental repression measure for 1985. Henderson theorizes that two aspects of population—density and rate of growth—have an effect on propensities to repress. He reports finding "a meaningful relationship" between population growth and political repression but fails to turn up much evidence of a similar relationship with population density.

Though suggestive, the research we have just summarized has some limits that impede our ability to formulate an understanding of the societal determinants of respect for personal integrity and the practice of state terror. With one exception, the studies are cross-national, cross-sectional sample designs that take no account of the change that can occur within a given country.³ With two exceptions (Henderson 1991, 1993), the studies use little more than bivariate statistical methods to test hypotheses that require multivariate formulation. Also, only one study uses a more or less comprehensive sample of countries (Henderson 1993).

A final difficulty for those who wish to draw inferences from this literature is that the analysts employ four different dependent variables to measure respect for human rights or governmental coercion: formal measures of the effectiveness of representative institutions (McKinlay and Cohan 1975, 1976), a Freedom House rating (Park 1987), a rating derived from an Amnesty International report (Mitchell and McCormick 1988), and a rating derived from a State Department report (Henderson 1991, 1993).

We shall construct a model to test hypotheses suggested by contending theories of *state terrorism* and *human rights abuse*. A first necessary step toward that end is to define what we mean by these terms and to operationalize them using available measurement techniques.

DEFINING AND MEASURING HUMAN RIGHTS AND STATE TERRORISM

The problem of measuring human-rights-related concepts has received much attention in recent years. The problem of finding a consensual measure of any such concepts is likely an impossible one, due to inevitable disagreements regarding values and definitions (e.g., Van Dyke 1973). We believe, however, that researchers should carefully define the terms and describe the operationalizations adopted, in order to minimize confusion and enhance replicability.

We shall focus on the subset of human rights categorized as dealing with the "integrity of the person" (see Cingranelli and Pasquarello 1985; Henderson 1991, 1993; Mitchell and McCormick 1988; Stohl and Carleton 1985). We also refer to violations of these rights as instances of *state terrorism*, which, consistent with Gurr (1986), we consider to be a category of coercive activities on the part of the government designed to induce compliance in others. Examples of such activities include murder, torture, forced disappearance, and imprisonment of persons for their political views.

Our adoption of these definitions is not meant to suggest that components of broader definitions of human rights—including rights economic, political, or social in nature—are unfounded or that they are unimportant. We simply focus our current efforts on the integrity of the person because we believe governments abusing this right are committing the most egregious and severe crimes against humanity and that these violations are of the sort that can usually be avoided.⁴ Further, limiting the term to this category of rights allows us to separate the concept of human rights from related concepts (e.g., democracy, economic standing) that may be, or have been, linked theoretically with national propensities to respect human rights (e.g., Henderson 1991, 1993; Mitchell and McCormick 1988).

Even after we specify the subset of human rights with which we are dealing, difficulties with measure-

ment of the concept remain to be solved. The evolving consensus among researchers is that the development of better data sources would be helpful but that sufficient data have been developed to begin the theory building and falsification enterprise, through the use of empirical tests.⁵

We have chosen to adopt the standards-based approach, as opposed to the events data approach described by Stohl and his colleagues (Lopez and Stohl 1992; Stohl et al. 1986).⁶ Fortunately, relatively recent developments in the collection of data on human rights offer us measurement alternatives that fit our purposes well. Two groups of researchers have applied similar sets of standards, each placing countries on five-point ordinal human rights scales, according to their reading of human rights information sources. Mitchell and McCormick (1988) created different variables to represent what they considered to be the two conceptually distinct dimensions of torture/killing and imprisonment, according to their reading of the Amnesty International Reports. Stohl and his colleagues (n.d.) have opted to include torture, imprisonment, and political killings and executions on the same ordinal scale and to create ratings by analyzing the reports of both Amnesty International and the U.S. Department of State.

While either classification would be acceptable, we have chosen to use the classificatory system employed by Stohl and his colleagues (Carleton and Stohl 1987; Stohl and Carleton 1985; Stohl et al. n.d.). We believe that it can be persuasively argued that the two dimensions postulated by Mitchell and McCormick stem, in reality, from the one dimension that Stohl and his colleagues tap—that both torture/killing and imprisonment are rooted in a regime's willingness to repress its citizens when they are considered a threat. To operationalize repression for this study, we used standards-based data (Poe and Sirirangsi 1993, 1994), as well as data generously provided by Stohl and his colleagues that appears in their unpublished work (Stohl et al. n.d.) and have been used in their previous studies (e.g., Carleton and Stohl 1987; Gibney and Stohl 1988; Stohl and Carleton 1985; Stohl, Carleton, and Johnson 1984). Following them, our data includes two five-point ordinal human rights indices generated through analyses of the contents of both the State Department and Amnesty International reports.⁷ These five-point scales of human rights abuse range from 1 for a country with a healthy record of respect for personal integrity (Canada, the United States) to 5 for a human rights disaster (Iran during several years in the 1980s). The scale and further information regarding the coding of cases are presented in the Appendix.

In coding the cases from the profiles available in the Amnesty International and State Department reports, it became evident that the samples of countries covered by those two sources were somewhat different. Amnesty International was not as comprehensive in its coverage as the State Department: Amnesty provided an average of 132 national profiles per year with sufficient information to code, while the

State Department covered an average of 151. The simplest way to deal with this problem would be to conduct analyses on the cases that are available for each measure. But there is good evidence that this would bias the findings, since Amnesty tends not to produce profiles for countries that have respectable human rights records.⁸ Because of the rather high correlation between the two measures in our sample (zero-order correlation = .83), we instead chose to substitute the value coded for the State Department scale when profile information was unavailable on a country in the Amnesty International reports and vice versa (in the few cases where it was necessary) as the best available approximation of those scores.

Following the practice in several of the Stohl studies, parallel analyses are conducted with the two indicators. This provides us with a check against nonvalid findings that might arise due to biases in either indicator, an advantage not found in previous studies explaining variations in human rights behavior. Where the results gained with the two indicators are similar, we can be more confident that our findings are not due to biases in the measures but are, in fact, "real".⁹

BUILDING A MODEL OF HUMAN RIGHTS ABUSE

Now that we have defined and operationalized the dependent phenomenon, we shall outline the theoretical perspectives on repression from which we draw the hypotheses that we shall test. Having outlined the theoretical justification for testing an hypothesis, we then specify how each of the key concepts is operationalized in our model.

Democracy

That democracy ought to decrease governmental resort to terrorism is strongly argued by Henderson: "The democratic process, with its emphasis on bargaining and compromise, offers a meaningful alternative for handling conflict if leaders choose to use it. Democracy should not be viewed as an idealistic process, but as a realistic way to accommodate demands with a minimum of conflict. . . . With a large measure of democracy, conflict should not grow so sharp as to invite repression"¹¹ (1991, 123–24). Henderson goes on to note that democracy "cannot be based on pseudoparticipation. There must be legitimate channels, such as political parties and elections, that can carry interests forcefully into government" (p. 124).¹⁰

The dampening effect of democracy on conflict surely does not represent the only way in which it inhibits repression. Effective democracy also provides citizens (at least those with political resources) the tools to oust potentially abusive leaders from office before they are able to become a serious threat. In addition, the freedoms that are essential to proce-

dural democracy may make it easier for citizens and opposition leaders to publicize attempts at repression, thereby bringing down on would-be abusive leaders the weight of majority or world opinion.

While the arguments relating democracy to human rights are strong, there can be a problem of tautology when one tries to put democracy and human rights abuse into an independent-dependent variable relationship. Certain minority and procedural rights are normally regarded as elemental features of a democratic form of government. If the rights that must be respected in a democracy are defined very broadly, they may well merge imperceptibly into the respect for human dignity that is, by definition, antithetical to the use of state terrorism.¹¹ If democracy is to function as an independent explanation for state terrorism and abuse of personal integrity, it must be defined in terms of procedures and rights that do not themselves preclude repression, even though they may represent considerable respect for human rights other than those most closely related to the integrity of the person. In addition, if we are empirically to examine the relationship between democracy and repression, democracy must be defined in terms that allow independent operationalization of the concept for the sample of nations we analyze.

For a theoretical definition of democracy, we turn to the work of Bollen, who, after surveying the definitions of a number of leading democratic theorists, defines *political democracy* as "the extent to which the political power of the elite is minimized and that of the nonelite is maximized" (1980, 372). Since *power* is no more easily operationalized than *democracy*, Bollen follows the lead of other theorists in noting the crucial role of political liberties and in seeing elections as mechanisms that "may increase the power of the nonelite." If they are to increase the power of the nonelite, elections must be fair, allow choice, be based on a universal franchise, and have results that are "binding on all parties": "Political liberties refer to the rights of all individuals and groups to protest or support—freely—government policies and decisions." These "provide additional political power for the nonelite, allowing them to organize opposition to the elites and their policies" and include free speech, free press, and freedom of opposition (p. 372).¹²

Two measurement alternatives suggest themselves as useful indicators for the concept of democracy as it has been defined. These are the measure of democracy used by Vanhanen (1990) in his studies of democratization and the Freedom House indicator of political freedom (see Gastil 1988; 1990). A third alternative, the index of "institutionalized democracy" in Gurr's Polity II data would have been an ideal candidate for use in our models (1990, 37–38); but it is not available for the year 1987, the final year covered by our data set.¹³

Vanhanen's index concentrates exclusively on the electoral components of democracy: it is the percentage of the population actually voting in a given election times the difference between the percentage of the

votes won by the largest party and 100%. The index is zero for nations whose governments were not elected. Since it is multiplicative, it can also be zero or near zero for a nation that lacks party competition or has an extremely low turnout percentage. Though noninstitutional, a strength of Vanhanen's index is that, like the Gurr index, it does not depend upon the extent to which a nation respects the dignity of the person. And unlike the Polity II measure, Vanhanen's index is available for the whole period of our analysis and for 142 of our maximum of 153 countries with full data on our dependent and other independent variables. (Pooled N using the Vanhanen indicator = 1,136.) We were, however, somewhat uncomfortable with the exclusively electoral focus of Vanhanen's index.

Thus the second measure of democracy that we use is the Freedom House index of political rights.¹⁴ Our examination of the criteria used to create the Freedom House political rights indicator convinced us that it can serve appropriately as an indicator of democracy, even if others have used *Freedom in the World* indicators as alternative measures of the state of human rights (Park 1987; Wesson 1987, pref.).¹⁵ According to a recent volume of *Freedom in the World*, the Freedom House index of political rights assesses

whether the people have "the right to vote and compete for public office";
whether elected representatives "have a decisive vote on public policies";
whether "the people have a choice in determining the nature of the system and of its leaders";
whether chief executives and legislative representatives "are elected through free and fair elections";
whether "there are fair electoral laws, equal campaigning opportunities, fair polling and honest tabulation of ballots";
whether "voters are able to endow their leaders with real power, or whether unelected elements reduce or supersede this power";
whether the system allows "the people to organize in different political parties or other competitive political groupings of their choice" and is "open to the rise and fall of these groups";
whether citizens are free "from domination by the military, foreign powers, totalitarian parties, religious hierarchies, economic oligarchies or any other powerful group"; and
whether "cultural, ethnic, religious and other minority groups have reasonable self-determination, self-government, autonomy or participation through informal consensus in the decision-making process" (McColm 1990, 19–20).¹⁶

This measure is by no means ideal for our purposes. The political liberties ratings supplied by Freedom House do not include specific assessments of the freedoms of speech and press that are included in Bollen's index of democracy.¹⁷ The indication that the political rights measure includes "freedom from domination" by unelected groups also raises the possibility that it might slightly overlap with our

measures of abuse of personal integrity.¹⁸ Finally, the Freedom House indicators have been criticized as "highly impressionistic, being no more than an estimate by a person who has collected a lot of seemingly relevant information on all the countries of the world" (McCamant 1981, 132.)

Despite these criticisms, we use the Freedom House political rights index as one of our two primary measures of democracy in the analyses that follow, for several reasons. First, precision in the statement of the Freedom House measurement criteria has improved over the years so that criticisms based on measurements for the 1970s are less persuasive for the 1980s. Second, the Freedom House indicators have been used successfully in a number of studies of freedom-related concepts (see Poe 1991, 1992; Rummell 1983; Stohl, Carleton, and Johnson 1984). Third, the Freedom House indicator of political rights is available for the largest number of countries (153) for which we have human rights measures for the years 1980–87, thus allowing us to conduct the most extensive possible analysis. Fourth, recent research by Bollen has found the Freedom House political rights indicator to have the highest validity rating among the several he analyzed, suggesting that if one is to use a single indicator, the political rights measure is certainly a good one (1993, 1225).¹⁹ Finally, and most important, our extensive statistical analyses of the three available measures of democracy indicate that they are so highly correlated as to be virtually substitutable and that there are few substantively interesting differences in the pooled cross-sectional time-series models of repression one might compute using the three indices of democracy. The seven-point ordinal scale is inverted in our analyses, so that more democratic countries achieve larger scores.

In sum, we believe the Freedom House indicator is a useful measure of democracy. Since we are employing this measure with the Vanhanen index, in parallel analyses, it is unlikely that any of its weaknesses will lead us to invalid findings. Democracy as we have now defined it is, in concept, substantially independent of human rights abuse. Furthermore, the results of preliminary analyses indicate that the two concepts are relatively independent empirically.²⁰

Population Size and Growth

Henderson develops a detailed and persuasive argument that population size and growth increase regime tendencies to use repression: "Growth in numbers of people can create scarcity—a short-fall between what people need and want and what they have. Under this pressure governments may be pushed in an authoritarian direction. . . . What is worse, government may resort to repression as a coping mechanism" (1993, 8).

A large population may increase the occurrence of state terrorism in at least two ways. First, a large number of people increases the number of occasions on which such coercive acts can occur. As a matter of simple probability, such an increase should lead to

the occurrence of more instances of coercion. Second, a large population places stress on national resources and bring the threat of environmental deterioration, further reducing available resources (*ibid.*).

Arguments made by Henderson indicate that rapid population growth may also promote resource stress, perhaps even more than population size: "The extent of scarcity varies from country to country, but in the more hard-pressed countries, burgeoning demands will keep governments off-balance and will incline them to resort to repression. Growing populations absorb any economic growth rate that may occur, thus frustrating governments' efforts" (1993, 4). Rapid population growth also increases the proportion of the population falling into the youngest age categories that require the constant creation of new jobs, new housing, and many other government services, as well as posing the greatest tendency to engage in criminal activities and other threats to public order (*ibid.* and the sources cited therein).

To operationalize the effects of population on the abuse of personal integrity through state terrorism, we employ two variables: the natural logarithm²¹ of total national population and the average percent increase in national population from one year to the next, over the eight-year period of the study.²²

Level of Development and Economic Growth

Despite the use of different indicators for both independent and dependent variables, the research we have summarized uniformly found a negative relationship between level of economic development or wealth and repression (Henderson 1991; McKinlay and Cohan 1975, 1976; Mitchell and McCormick 1988; Park 1987). The logic underlying these results appears straightforward: "The poorest countries, with substantial social and political tensions created by economic scarcity, could be most unstable and thus most apt to use repression in order to maintain control" (Mitchell and McCormick 1988, 478). Again, "It is only logical to think that, with a higher level of development, people will be more satisfied and, hence, less repression will be needed by the elites" (Henderson 1991, 1226). The only caveat to these musings comes from Mitchell and McCormick's interpretation of Huntington's argument concerning political instability suggesting that the use of coercive measures might rather be expected to be most serious in states in the middle, transitional levels of development, an argument for which they find little empirical evidence (Mitchell and McCormick 1988, 479, 488–90).

The presumed effects of economic growth are more problematic. On the one hand, it is logical to assume that rapid economic growth, since it expands the resource base, should reduce the economic and social stresses that lead governments to use terrorism as a policy tool. But there has also been a strong argument that rapid economic growth is most likely to be a destabilizing force that will, in fact, increase instability and a regime's temptation to resort to coercive

means to maintain control. (Olson 1963 is the seminal work.) The destabilizing effect of rapid growth may occur because it can rarely be rapid enough to outstrip the growth in expectations that is simultaneously occurring (Gurr 1970, 1986); because it increases the number of declass  individuals and groups most prone to promote instability (Olson 1963); or because rapid growth inevitably occurs unevenly, possibly even creating growth by decreasing the well-being of the nonelite. In any case, frequently sharpening class differences within the population cause the elite to promote repression to keep the nonelite quiet (Henderson 1991, 126 and the sources cited therein).

We thus expect abuse of the right to personal integrity to be decreased by level of economic development but increased by economic growth. When we turn to operationalizing economic development and growth, we find some controversy over how these variables ought to be measured. Dissatisfaction with the defects of the most traditional of indicators—gross national product (GNP) per capita—has led to the development of several alternative measures of economic standing.²³ The major problem with these alternatives is that they do not exist—and cannot easily be created—for most of the years, or for a significant number of the countries, of our study. As a result we follow Mitchell and McCormick (1988) and McKinlay and Cohan (1975, 1976) in using GNP per capita and percentage growth in GNP per capita as our measures of economic development and economic growth. Despite the deficiencies in these measures, we have little reason to believe they produce inaccurate or misleading results compared to those achievable with other currently unavailable alternatives.

Leftist Regimes

The argument of former U.S. ambassador to the United Nations Jeane Kirkpatrick (1979) that “totalitarian” (i.e., noncompetitive, Marxist or Marxist-Leninist) regimes were the world’s most repressive is reviewed and analyzed by Mitchell and McCormick (1988, 480–81, 493–95). They find that such regimes are, in fact, more repressive on at least one of their dimensions of repression than are “authoritarian” regimes of a non-Marxist nature. Such a finding is not surprising if one takes seriously the tenets of Marxist-Leninist theory about the need for a dictatorship of the proletariat, and some other communist doctrine.

Even so, critics of U.S. foreign policy have taken Kirkpatrick and the State Department to task for what the critics see as their efforts unfairly to paint all socialist regimes with the tar of repression. We test the hypothesis that leftist regimes will be more coercive than other regimes, where leftist regimes are defined quite precisely as those governed by a socialist party or coalition that does not allow effective electoral competition with nonsocialist opposition.

Military Regimes

That we expect military regimes to be more coercive than others probably surprises no one, whether they are familiar with McKinlay and Cohan’s findings or not. Military juntas are based on force, and force is the key to coercion. Yet, in many of the nations in which soldiers forcibly take power, they do so alleging that the leaders they are replacing were themselves violating the constitution and, possibly, engaging in repression of the rights of the citizens. In addition, since military rule is by definition antithetical to democracy, it might be that any apparent relationship between military rule and state terrorism is spurious, a result of a failure to control for the democratic/nondemocratic nature of the regime.

For our operationalization of military-controlled regimes, we use a classification created under our direction by Madani (1992) from the *Europa Yearbook*. Following McKinlay and Cohan, the classification defined military regimes as those which had come to power “as a consequence of a successful coup d’etat, led by the army, navy or air force, that remained in power with a military person as the chief executive, for at least six months in a given year” (p. 61; see McKinlay and Cohen 1975, 1). The category *military regimes* also included a small number of mixed regimes “with either a civilian as the chief executive and several military persons in the cabinet or a military head of government who nominated a civilian as the head of government and himself worked behind the scenes” (Madani 1992, 61). All other governments were considered civilian regimes.²⁴

British Cultural Influence

Mitchell and McCormick argue that the political cultures of nations may have an important impact on the extent to which their governments engage in repression and that “one important factor that is thought to have shaped political culture for most states is the colonial experience” (1988, 479–80). Specifically, they note: “British colonial rule . . . is commonly thought to be strongly associated with the postcolonial development of democracy. The British legacy may be a relatively greater respect for human rights. By contrast, other colonial experiences (Spanish, for instance) are generally assumed to have introduced a greater degree of hierarchy and authoritarianism. The legacy here may well involve higher levels of human rights violations” (p. 480).²⁵

This is certainly not the place to review the efficacy of political cultural explanations of national political behavior. At their best, they connote that certain attitudes inculcated by the culture, but not directly measured, are partially responsible for differences in the dependent behaviors of interest. That is the direct implication of Mitchell and McCormick’s argument, and, following their lead, we include “British cultural influence” in our models of state terrorism. British influence is therefore represented in our model by a variable coded 1 for countries that had been territo-

ries of Great Britain at some point during their histories, which all other countries coded 0.

International War Experience

In their study of economic summity, Putnam and Bayne (1987) draw on game theory to illustrate that when regime leaders ascend to power, they face the prospect of playing simultaneously in two distinct but nevertheless interrelated games: one is played in the domestic political arena, with the primary purpose is to keep power, whether through election or the use of terror; the other takes place in the international realm, with other major players being the leaders of other countries, their representatives, and relevant international governmental and nongovernmental organizations. We believe that this analogy is also useful to researchers interested in explaining human rights abuse. The fact that these two games are intimately intertwined suggests that leaders' actions in the domestic political realm will likely be affected when their nations are a direct participant in an international crisis situation. In fact, studies of what Stohl (1980) has called the "nexus of civil and international conflict" abound, and the results of some systematic empirical studies (e.g., Stohl 1975, 1976) tend to point to the conclusion that there is a positive linkage between participation in international war and the levels of domestic political violence in participant countries. In the only study we know that deals directly with the question of whether participation in international war affects political repression, Rasler (1986) focused on the twentieth-century United States and found evidence that administrations did indeed increase levels of repression during wars.

We hypothesize that such a relationship applies generally in our worldwide sample. As a guide to our coding of the concept *war* during the 1980s we used as a guideline the criteria developed by Small and Singer (1982). These researchers coded a country as having been a participant in an interstate war when (1) there was a total of a thousand or more battle deaths suffered by all of the participants in the conflict, (2) the particular country suffered at least a hundred fatalities or had a thousand or more personnel taking part in the hostilities (pp. 50, 55). We ignored a further criterion employed by the Small and Singer study—that a participant country should be a member of the "international system" (p. 51)—because it did not fit well with the purposes of this cross-national study, which includes countries regardless of whether they are members of the international system as defined by Small and Singer.

Civil War Experience

Just as governments may employ repression when threatened on the international front, it is also a tool commonly used by governments that are faced with internal problems (see Nieburg 1969; Skocpol 1979; Tilly 1978). The most serious of threats in the domes-

tic arena is posed by a condition of civil war, in which the authority of the regime in power is being challenged by an armed and organized resistance controlled by a shadow government. Therefore we shall propose a hypothesis that to our surprise had not yet been tested in quantitative studies of human rights and state terrorism—that regimes are more coercive when they are involved in civil conflict.²⁶

In order to operationalize the concept of civil war we again look to the criteria developed in conjunction with the Small and Singer (1982) study for guidance. First, the government, as the central authority in a country, must be involved as a direct participant in the war. Second, there must be an effective resistance, that is, either both sides must be "organized for violent conflict" or "the weaker side, although initially unprepared [must be] able to inflict upon the stronger opponents at least five percent of the number of fatalities it sustains" (p. 215).²⁷ Thus genocides and massacres are not considered to be civil wars, and this concept is kept distinct from our dependent variable.

TESTING A MODEL OF ABUSE OF PERSONAL INTEGRITY RIGHTS

In recent years studies employing the pooled cross-sectional time-series (PCT) design have appeared much more frequently in political science journals. The PCT design is especially appealing because it enables researchers to test theories over both space and time simultaneously and thus to witness the interplay of two dimensions usually viewed separately in either cross-sectional or time-series studies (Clarke 1992; Sayrs 1989; Stimson 1985). With this notable advantage, however, comes some statistical difficulties. Specifically, autocorrelation and heteroscedasticity often complicate efforts to apply ordinary least squares (OLS) regression, probably the discipline's most widely used and understood statistical technique, on PCT data. Both autocorrelation (also known as serial correlation) and heteroscedasticity may lead to inaccurate estimates of the standard errors of parameter estimates, thus calling into question the results of significance tests (Ostrom 1990; Stimson 1985).

Many PCT studies have used what Beck and Katz (1994) call the feasible generalized least squares method to deal with these problems. This is essentially generalized least squares (GLS) as it is usually practiced, where an estimate of the error process is generated to evade or overlook the assumption underlying GLS, that the error process is known. But Monte Carlo trials undertaken by Beck and Katz show that there is good reason to be concerned about violating this assumption, as FGLS is shown consistently to underestimate standard errors (1994, 15). As a result this method sometimes yields extremely optimistic estimates of statistical significance. We are therefore persuaded by the recommendations of Beck

and his associates (Beck and Katz 1994; Beck et al. 1993) who favor the use of OLS regression, in tandem with a variation on White's (1980) robust-standard-errors technique that Beck and Katz developed for use on panel data, to control for heteroscedasticity.²⁸ The White technique provides a consistent or "robust" estimate of the standard error by estimating a parameter covariance matrix that is consistent in the presence of heteroscedasticity. In order to control the effects of autocorrelation that we found to be a problem in our preliminary analyses, we shall employ a lagged dependent variable in the model, along with the substantive variables already outlined.²⁹

To review, our general model is as follows:

Personal Integrity Abuse_{ij} = a

$$\begin{aligned}
 &+ \beta_1 \text{Personal Integrity Abuse}_{(t-1)j} + \beta_2 \text{Democracy}_{ij} \\
 &+ \beta_3 \text{Population Size}_{ij} + \beta_4 \text{Population Change}_{ij} \\
 &+ \beta_5 \text{Economic Standing}_{ij} + \beta_6 \text{Economic Growth}_{ij} \\
 &+ \beta_7 \text{Leftist Government}_{ij} + \beta_8 \text{Military Control}_{ij} \\
 &+ \beta_9 \text{British Cultural Influence}_{ij} \\
 &+ \beta_{10} \text{International War}_{ij} + \beta_{11} \text{Civil War}_{ij} + e_{ij}.
 \end{aligned}$$

Data generated from Amnesty International and U.S. State Department human rights profiles will be used to construct two different measures of the level of personal integrity abuse, and parallel analyses will be conducted on these measures. Further, we shall conduct tests with the Vanhanen and the Freedom House political rights measures of democracy with both of these dependent variables, so that in all, four models will be tested. Though we gathered data for the 1980–87 period, the addition of the lagged dependent variable meant that we had to discard 1980 from our final analyses.³⁰

Modeling Personal Integrity Ratings Derived from Amnesty International Reports

Our first set of analyses seeks to explain variations in the repression scale generated from the Amnesty International reports, using the statistical techniques described. Under model 1 and model 2 of Table 1 we present the coefficients and standard errors obtained in these tests. The statistics for model 1 are those yielded when the Freedom House political rights rating was used to operationalize the concept of democracy. Those for model 2 are the results when we substituted the Vanhanen democracy measure for that of Freedom House.

The strongest predictors of the repression scores operationalized from the Amnesty International reports are, as might be expected, the lagged dependent variables that achieved large, statistically significant coefficients in both analyses. The inclusion of the lagged dependent variable on the right side of the equation represents our means of correcting for the serial correlation present in our time-series data.

Nevertheless, the substantive meanings of the strong coefficients for the lagged endogenous variable are worth considering. Such coefficients mean that our individual country scores on state terrorism are strongly seated characteristics of their political systems that do not change easily or rapidly. In addition, these coefficients provide a mechanism through which the effects of changes in the independent variables at a given time (time_t) continue to influence the dependent variables beyond the time of the occurrence of the change. We shall use this characteristic to illustrate the dynamic effects of our independent variables on repression of personal integrity rights.

Once the effects of autocorrelation are controlled by incorporating these lagged variables,³¹ several others are shown to be important determinants of levels of state terrorism. Our results indicate that level of democracy has a strong and statistically significant impact on governments' respect for personal integrity. Both the Freedom House and Vanhanen measures of democracy prove to be statistically significant determinants of national propensities to use repression. The smaller coefficient of the Vanhanen variable should not, however, be interpreted as being evidence that this variable has a smaller effect, since that variable is measured on a scale ranging from 0 to 46.4, as compared to the Freedom House variable which ranges from 1 to 7. In fact, when one considers the different scales of these two indicators, the coefficient of the Vanhanen scale is somewhat stronger than that of the Freedom House variable. We find that if a country were to achieve the least democratic score on the Vanhanen measure after having achieved the most democratic score in the previous time period, this would make a difference of about .4 in the dependent variable at time_t. While the initial impact is moderate, we can see after performing some simple calculations, considering the dynamic effect mediated by the lagged dependent variable, that the impact of a loss of democracy increases substantially over time.³² Figure 1 shows that if the abandonment of the democratic process in our example country were to continue, other factors held equal, the human rights index would have increased by about 1.3 (on the five-point state repression scale) after five years, (i.e., at time_t + 5) as a result of the lagged effect, stabilizing at 1.4 points greater than the original score a few lags thereafter. The movement from most democratic to least democratic on the Freedom House measure is associated with a .26 increase in the Amnesty index, at time_t, stabilizing at about a one-point rise in that index after several lags.

Relating these effects back to the scale of the dependent variable, if a democratic country with a near perfect human rights record were suddenly to abandon the democratic process, we would expect that the country would, at the very least, begin to hold some political prisoners, and that political brutality, executions, and murders might become a common feature of life, other relevant factors being equal. Is such a complete abandonment of democracy an

TABLE 1

Pooled Cross-sectional Tests of Explanations of Abuse of Personal Integrity Rights

INDEPENDENT VARIABLES	AMNESTY INTERNATIONAL		STATE DEPARTMENT	
	DEMOCRACY MEASURE: FREEDOM HOUSE (MODEL 1)	VANHANEN (MODEL 2)	DEMOCRACY MEASURE: FREEDOM HOUSE (MODEL 3)	VANHANEN (MODEL 4)
Constant	-.02 (.14)	.07 (.12)	.11 (.16)	.23 (.18)
Personal integrity abuse _{t-1}	.73** (.08)	.73** (.08)	.63** (.10)	.64** (.10)
Democracy	-.05** (.02)	-.009** (.003)	-.06** (.02)	-.01** (.003)
Population size	.05** (.02)	.04** (.02)	.06** (.02)	.04** (.02)
Population change	.008 (.009)	-.004 (.01)	.01 (.02)	-.002 (.01)
Economic standing	-.008* (.004)	-.007 (.004)	-.02** (.004)	-.01** (.004)
Economic growth	-.0009 (.001)	-.0005 (.001)	.0003 (.001)	.00007 (.001)
Leftist government	-.04 (.07)	-.03 (.07)	.08* (.04)	.11** (.04)
Military control	.05 (.05)	.05 (.06)	.04 (.05)	.06 (.06)
British cultural influence	-.03 (.05)	-.05 (.06)	-.01 (.04)	-.04 (.04)
International war	.21** (.07)	.22** (.07)	.34** (.09)	.35** (.01)
Civil war	.33** (.11)	.33** (.11)	.49** (.12)	.49** (.12)
R ²	.77	.77	.75	.75
N	1071	994	1071	994
Average contemporaneous Correlation of Errors	.43	.43	.45	.43

Note: Main entries are unstandardized OLS coefficients, generated using RATS 386 version 4.02. A lagged dependent variable is included in each model to control the effects of autocorrelation. The robust standard errors, which were used to control heteroscedasticity (Beck et al. 1993; White 1980), are in parentheses.

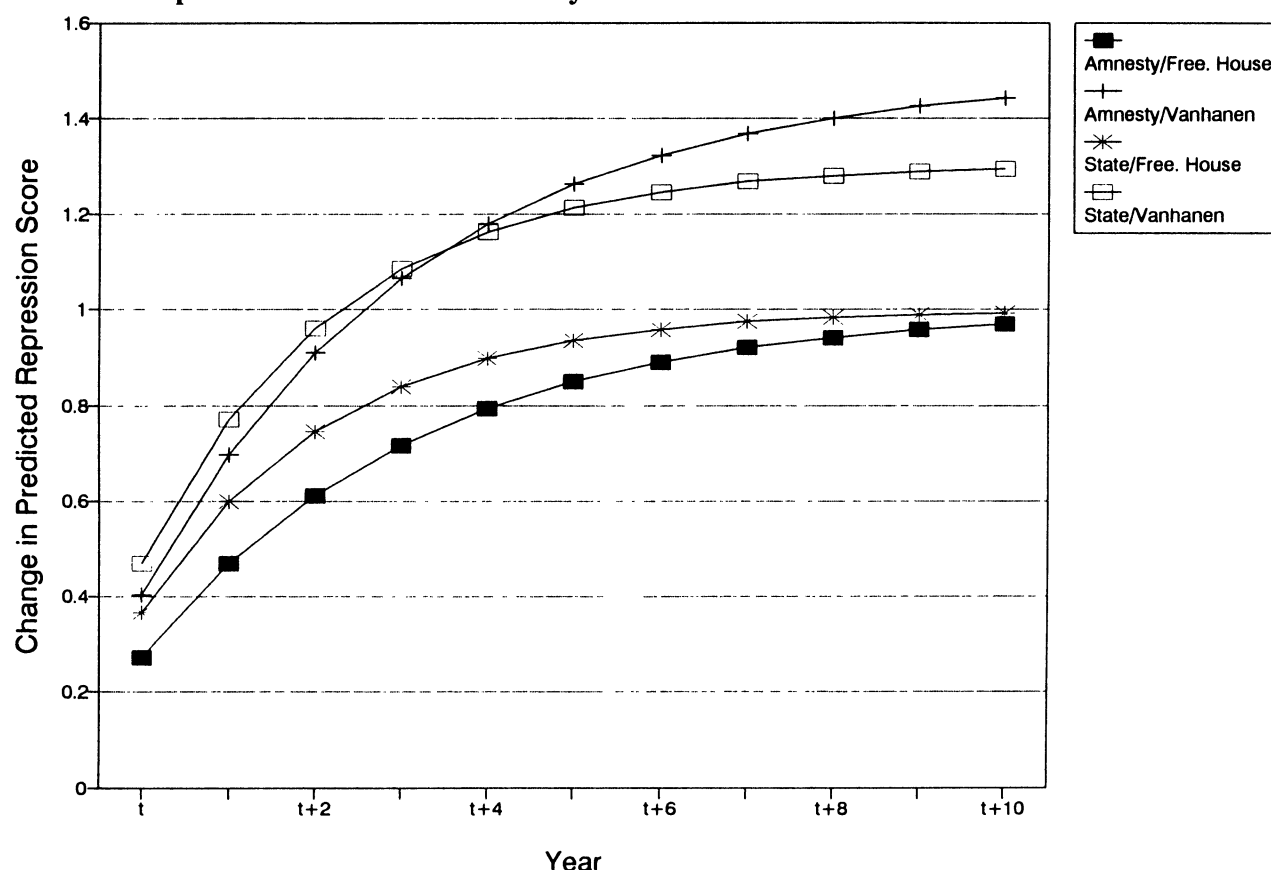
* $p < .05$ (one-tailed test).

** $p < .01$ (one-tailed test).

unlikely scenario? Perhaps, but there are precedents for such quick and drastic changes in governmental institutions. Though it falls outside the period of our analysis, the example of the downfall of Chilean democracy with the overthrow of Salvador Allende and the installation of the extremely authoritarian Pinochet regime represents exactly this phenomenon. In 1973, Chile achieved the maximum democracy score on the Freedom House measure of democracy, while in 1975, the next year for which the Freedom House measure is available, it achieved the minimum democratic score!³³ The collapse of Weimar democracy with its catastrophic aftermath might well be another concrete example of such a drastic change.

With regard to population-related determinants of human rights abuse, the findings are mixed. In both sets of statistical analyses shown in the Table 1, the relation between population size (logged) and the

repression measure derived from Amnesty International reports indicates that the larger a country's population, *ceteris paribus*, the greater the state's tendency to violate the integrity of its citizens through the use of repression and terror. Judgments regarding the substantive importance of the size of the coefficient would, however, vary depending on one's viewpoint. These findings are at least moderately important when viewed from the perspective of theory. One who emigrated from a country with a tiny population of about 200,000 (e.g., the Bahamas during the eighties) to one the size of the People's Republic of China would likely find, *ceteris paribus*, that levels of repression were much higher, since the lagged effects of population become asymptotic at around 1.4 and 1.7 in the analyses with the Freedom House and Vanhanen measures of democracy, respectively.³⁴

FIGURE 1
Increase in Repression Due to Loss of Democracy


Note: Change in repression score in any year shows the direct effect of absence of democracy in that year and the cumulative lagged effects of loss of democracy on repression in the previous years, for a country that in year t changed from the most to least democratic score on the respective (Freedom House or Vanhanen) democracy index. Based on unrounded coefficients.

In contrast, the sizes of the coefficients are probably rather unimportant from the perspective of those seeking to conduct policy-relevant research, who would probably focus on changes in the dependent variable that result from achievable changes in the values of manipulable independent variables. When we examine the differences in repression that result from the addition or subtraction of population from countries of various sizes, the impacts seem rather small. For example, the impact of adding one million people to a population the size of China's in year t would result in an increase of less than .0002 in the repression index at time $t + 10$, in both sets of analyses. In the unlikely event that one million people were added to a country about the size of the Bahamas, *ceteris paribus*, the increase in the repression index would only be around .3 at time $t + 10$, regardless of which measure of democracy we might use.

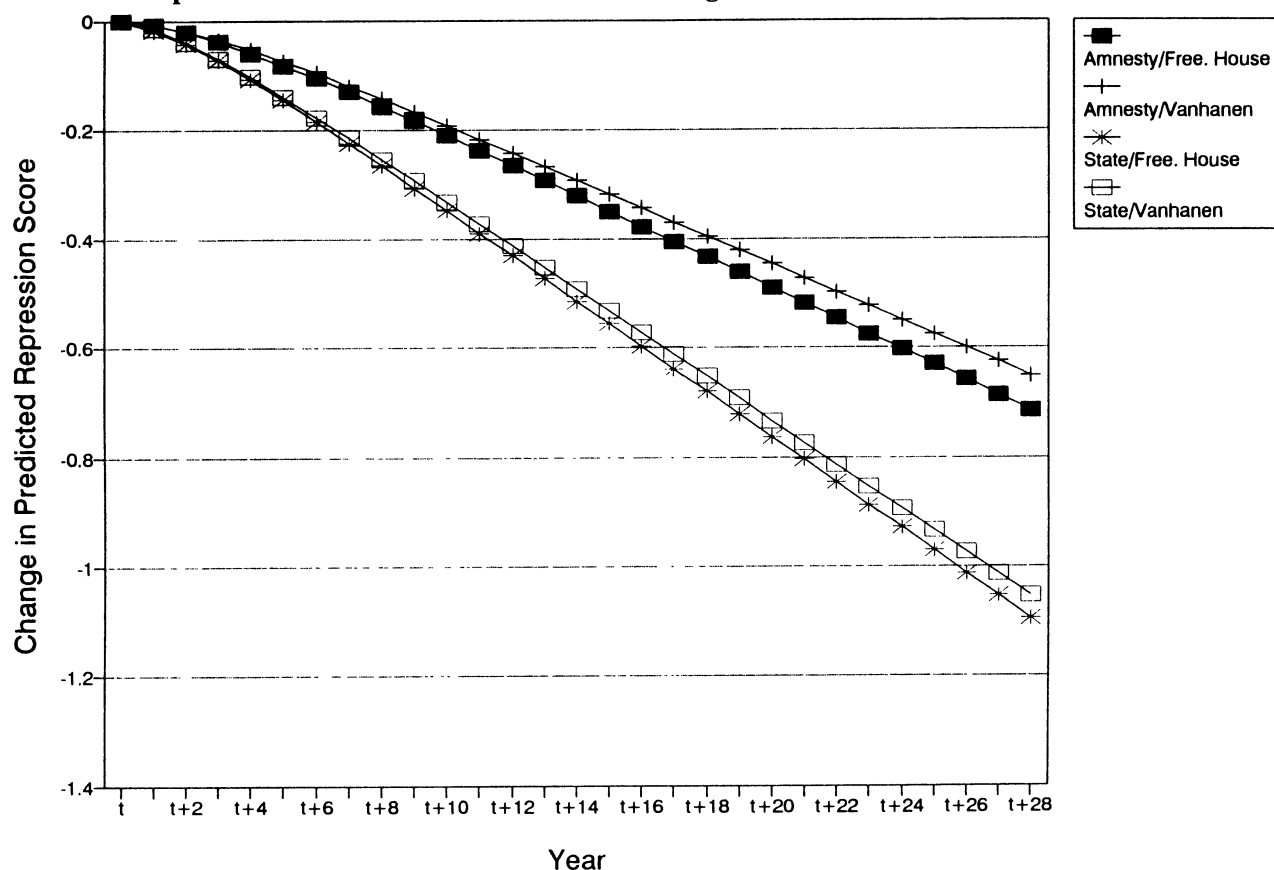
We find no support for the conclusion reached by Henderson (1993) that rapid rates of increase in population lead to political repression. In the set of analyses utilizing the Freedom House democracy variable (model 1), a weak and statistically insignificant positive coefficient is the result, while in the

second set of analyses, using the Vanhanen democracy variable (model 2), the coefficient is weak, statistically insignificant, and negative.

The results yielded by these analyses are also mixed with regard to the effects of economic variables on national propensities toward the violation of personal integrity rights. The hypothesis linking economic growth rates to repression is not supported by the results of either model 1 or model 2. Our findings do indicate that economic standing, as measured by per capita GNP, has a statistically significant effect on such violations in model 1 and falls just short of statistical significance in model 2. But here an examination of the magnitude of the coefficient leads us to the conclusion that the effect is rather weak, since the coefficients indicate that a relatively unlikely increase of ten thousand dollars in a country's per capita GNP from one year to the next would translate into a rather small decrease in the tendency for abuse of personal integrity of between .07 and .08 on the five-point human rights abuse index, *ceteris paribus*. If that country's per capita GNP remained stable at ten thousand dollars above the original value, this

FIGURE 2

Decrease in Repression Due to Increase in Economic Standing



Note: Change in repression score in any year shows the direct effect of the level of GNP/Capita in that year and the cumulative lagged effects of gains of \$1,000 each year on repression in the previous years, for a country that began in year t at the poorest GNP/Capita level. Based on unrounded coefficients.

would translate to a modest decrease of .24 or .22 in the repression scale at time $t + 5$.

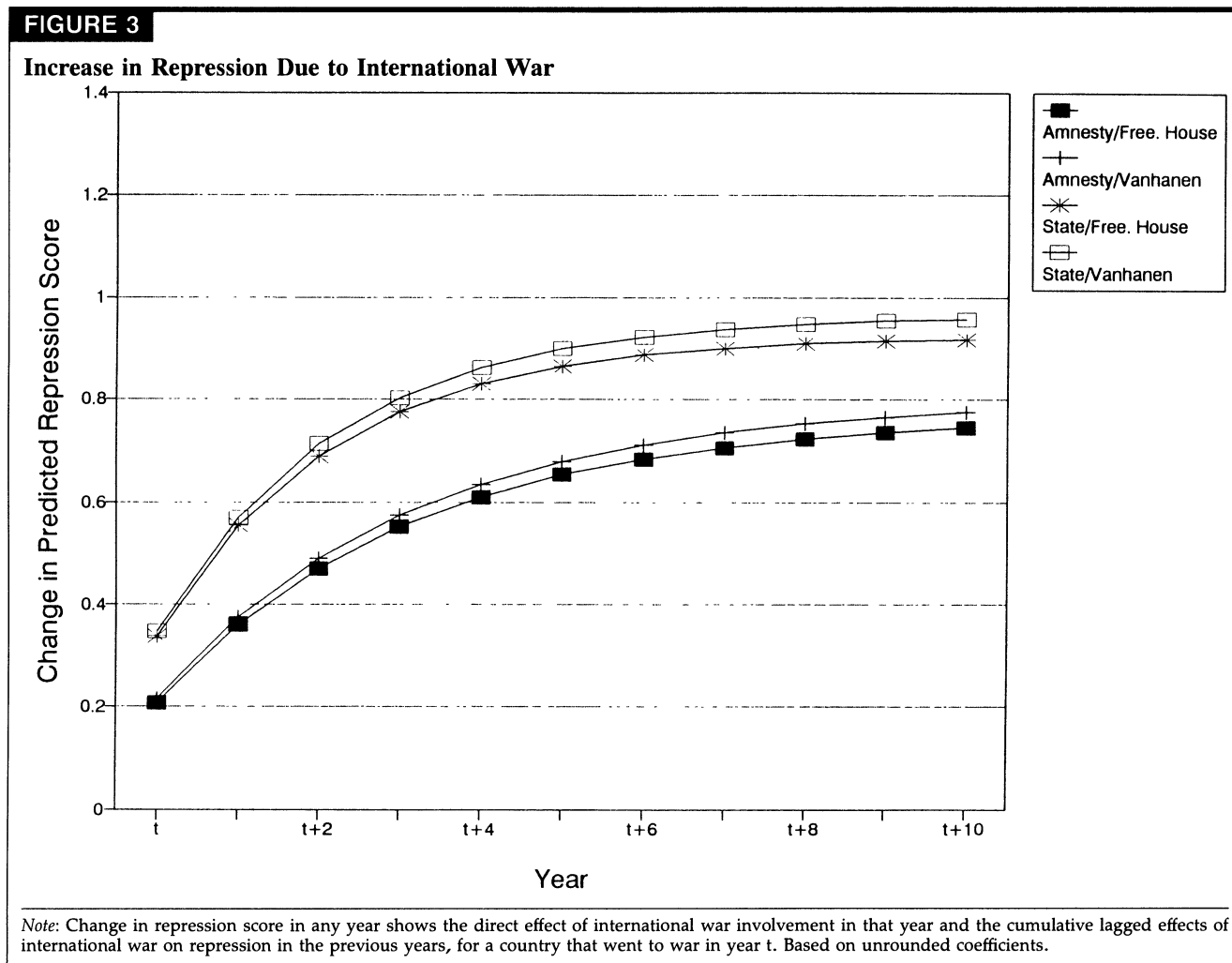
If one optimistically assumes for the world's poorest country at time t a GNP per capita increase of one thousand dollars per year for a period of time long enough to make the income of that poorest country equal that of the richest country at time t , the projections are those represented in Figure 2. They show that after the 28 years this change would require, the decrease in repression due to such an impressive advance in economic standing would be expected to range from about .7 or .8, depending upon the measure of democracy used, with the personal integrity abuse index derived from Amnesty International reports. These truly long-term effects would represent important decreases in repression, in the unlikely event that a nation could sustain such increases in economic development. Nevertheless, they are much less impressive when compared with the short-term effects of the loss of democracy or the onset of civil or international war.

The variables identifying particular types of regimes thought to have an important impact on levels of repression do not fare too well in these analyses.

While the military control variable has coefficients in the expected direction, they are statistically insignificant. And the variable identifying leftist regimes achieves a statistically insignificant coefficient in an unexpected direction.

In contrast, the dummy variables identifying levels of external and internal threat, in the form of international and civil wars, do achieve substantively important and statistically significant coefficients, indicating that as threat increases, so does a country's propensity to use repression. The variable identifying participants in ongoing international wars achieves statistically significant coefficients of .21 and .22 in models 1 and 2, respectively. These coefficients indicate that if a country were to be involved in international war for six consecutive years, we would expect an increase of about .7 in the human rights index, other factors held constant, in the sixth year (see Figure 3).

The measure of national involvement in civil war achieves a coefficient of .33, statistically significant at the .01 level, in each analysis. Once again assuming a country's ongoing involvement in civil war, its repression scale score would have increased by just



over one point in the sixth year of that war, with the impact stabilizing at about 1.2 a few lags thereafter (see Figure 4). Thus the results we obtained when using data derived from Amnesty International reports to operationalize human rights abuse strongly support the conclusion that regimes repress political opponents when faced with threats, domestically and abroad, in the form of civil and international wars.

The R-squared of .77 yielded by both models 1 and 2 shows that the theoretical model achieves a quite respectable level of explanatory power. Thus while the possibility of misleading results emerging as a result of specification error is not precluded, we believe there is good reason for confidence in these findings.

A Second Model: Ratings Derived from State Department Reports

In order to check the reliability of the results obtained in our first set of analyses, we next conducted tests of similar multivariate models with an index measuring the abuse of personal integrity derived from the U.S. State Department human rights profiles. The results

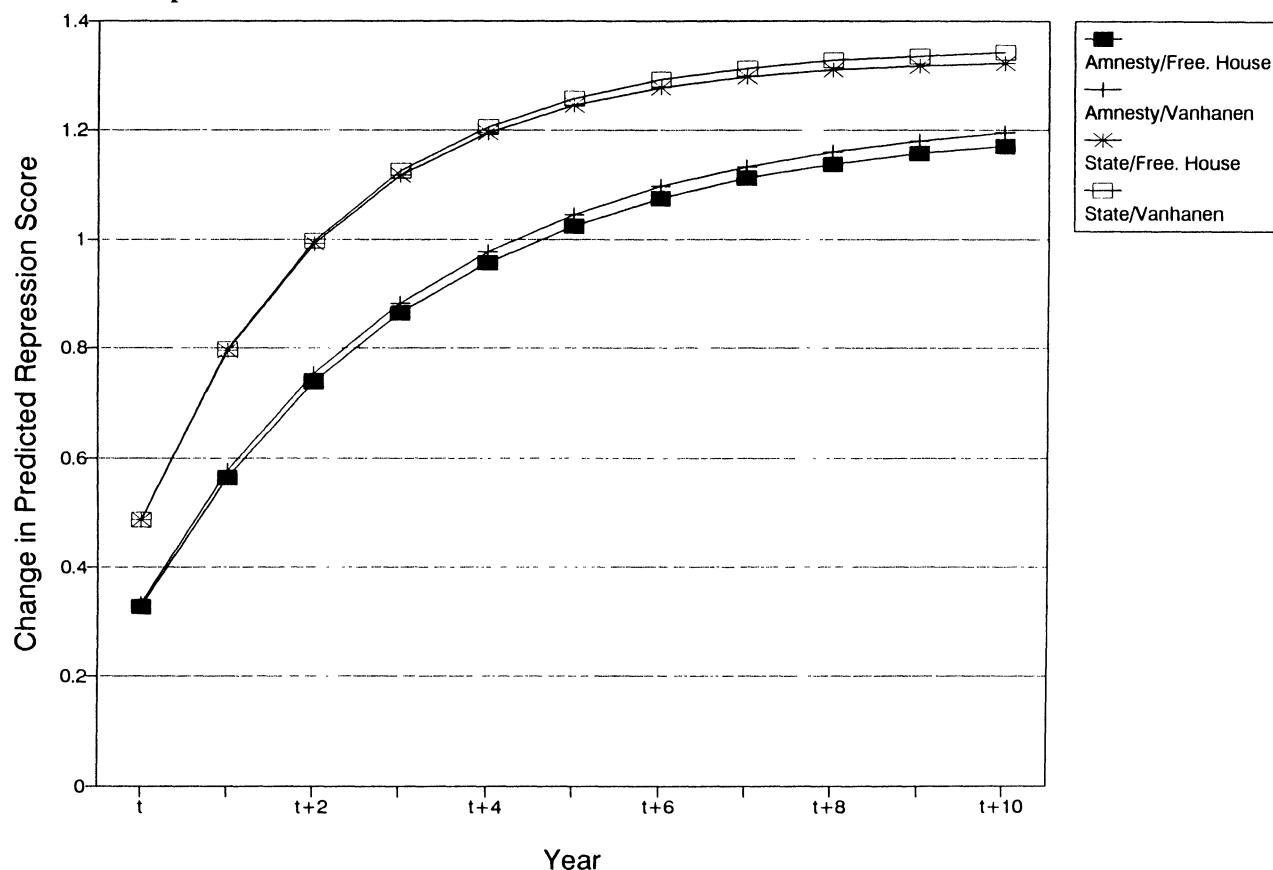
of these analyses are presented in Table 1: Model 3 uses the Freedom House measure of democracy, and model 4 uses Vanhanen's operationalization of that concept. The R-squared of .75 again indicate that the models achieve a respectable level of explanatory power.

The results in models 3 and 4 are very similar to those yielded when the focus was on the dependent variable generated from Amnesty International reports. The most serious divergence between this and the first set of results is that relating to the leftist government variable, which achieves strong, statistically significant coefficients in the expected direction. In our previous analyses, focusing on an index derived from Amnesty International reports, the coefficients had been extremely weak, with a negative sign. These results are precisely what one would expect if indeed the State Department's profiles are biased against leftist governments (or, alternatively, if Amnesty International's ratings are biased *in favor* of leftist regimes and movements).

The list of variables exhibiting statistically significant effects is otherwise about the same as in our tests of models 1 and 2. The variables identifying British

FIGURE 4

Increase in Repression Due to Civil War



Note: Change in repression score in any year shows the direct effect of civil war in that year and the cumulative lagged effects of civil war on repression in the previous years, for a country that began a civil war in year t . Based on unrounded coefficients.

cultural influence, and military regimes again have weak and statistically insignificant coefficients. The same can be said of the population and economic growth variables, which also failed to exhibit important effects in our initial tests. But the impacts of many of the variables found to be important in models 1 and 2 appear to be somewhat larger in this set of analyses. Economic standing, democracy, international war, and civil war each have somewhat larger effects in these models, while these of the population variable were larger in one of the two sets of analyses, and identical in the other.

A Comparative Look at Dynamic Effects

Now that we have conducted four sets of analyses, we are in a better position to assess the importance of the variables we hypothesized would affect levels of repression. With regard to the variables found to have statistically significant effects in each of the four sets of analyses, it is evident that population size, economic standing or development, international war, civil war, and democracy affect levels of repression. In Figure 1 we show the impact that the abandonment of democracy would have on personal in-

tegrity abuse, assuming a change from the most democratic score at time $t-1$ to the least democratic score at time t , stabilizing at the least democratic score throughout the period. While the estimates provided by the Freedom House variable are somewhat more cautious, it is clear, nevertheless, that the loss of democracy increases the abuse of personal integrity, especially when one considers its lagged effect.

The same can be said of international and civil wars, whose effects over time are plotted in Figures 3 and 4. Put in terms of the scale of the dependent variable, the results plotted in these figures show that we should expect that a country under the rule of law at the time of initiation of either an international or civil war would likely have limited political imprisonment by the end of the period, other factors held equal. A regime that already had limited political imprisonment but few more serious violations, would be expected, *ceteris paribus*, to hold a more extensive number of political prisoners and perhaps to torture, execute, or murder its political opponents if those wars continue.

By way of contrast, even the strongest plausible projections based on the effects of economic standing and population size seem weak when compared to

the effects of the democracy and international and civil war. Generously plausible assumptions about changes in population size yield at best only modest increases in predicted abuse of personal integrity right, *ceteris paribus*, when projected over time. Similarly, the most optimistic estimate of economic standing's effect is that an increase of ten thousand dollars in per capita GNP, held constant for several years thereafter, years would decrease state terrorism by .15 at time_t, by .39, at time_{t + 5}, stabilizing at .42 a few lags thereafter. So even though these analyses do show that economic development makes a statistically significant difference in propensities to use state terror, the size of that difference is rather small. On the other hand, given the long research tradition that has found strong correlations between economic development and democracy,³⁵ it is notable that both economic development and democracy have statistically significant effects on repression of personal integrity rights, regardless of the magnitudes of their impacts.

SUMMARY AND CONCLUSIONS

In this study we sought to explain variations in cross-national respect for the subset of human rights known as personal integrity rights, through the use of sophisticated multivariate methods, on a comprehensive pooled cross-sectional data set covering the years 1980–87. A model explaining variations in national respect for these rights was constructed to test several hypotheses drawn from a variety of theoretical perspectives on why such human rights violations might occur. Two distinct dependent variables, derived from information on human rights conditions provided by Amnesty International and the U.S. Department, and two indicators of the key concept of democracy were used in four sets of analyses. Because the tenor of the findings yielded by the four models is very similar, we can conclude with some confidence that the effects we isolated are in fact an accurate representation of reality.

Several of our hypotheses were supported in each of the four sets of analyses that we presented. As expected, democracy was shown to be associated with a decreased incidence in repression, regardless of which of the two indicators of that concept—and which of the two dependent variables—was employed. These results are important because they substantially extend the findings of Henderson (1991, 1993) with different measurements of democracy. As a result of the cumulation of findings on this linkage, then, it would now seem difficult to deny that democratization decreases governments' use of coercion to abuse the human rights of private citizens.

Following Henderson (1993), two population variable were employed in our multivariate model of human rights abuse. With regard to the logged population size variable, the results were stable and quite clear, and we can therefore conclude that population size does have a positive impact on human rights

abuse, with more populated countries having a greater propensity to abuse personal integrity rights, *ceteris paribus*.

Our findings regarding several other variables thought to be related to state terror were negative. None of our results supported the conclusion that military control or British cultural influence affects levels of repression.³⁶ Results for the effects of population growth were stable across all of the analyses that were conducted, indicating no statistically significant or substantively important impact on repression. This finding contradicts the conclusions of Henderson (1993), which he based on his analysis of a cross-sectional data set covering the year of 1985. However, here we should note that we dealt with only a seven-year time frame and that the population increase variable utilized in this study was an average of the population gain over this seven-year period, so that unlike other variables in the model (with the exception of British cultural influence), it did not vary across time. A study using accurate yearly estimates (if such estimates are possible) conducted over a longer time frame might yield more positive results.

Rather mixed results were yielded by the variable identifying leftist governments. Leftist regimes appeared to violate personal integrity rights more seriously than others when such rights were operationalized using information gathered from U.S. State Department sources. When our analysis focused on the measure derived from Amnesty International information, however, no such relationship appeared. Here we are inclined to give less weight to the results obtained with the index derived from State Department reports, because our findings are consistent with those that would be expected if allegations regarding the biases of the State Department reports against (or of Amnesty International in favor of) some leftist regimes are valid (e.g., Innes 1992). Thus we cannot conclude that, *ceteris paribus*, doctrinaire socialism is a cause of coercion that abuses human rights; but the results do perhaps provide limited empirical evidence of the different "pictures" of human rights realities painted by these two sources of human rights behavior during the 1980s.

Our study does provide very firm support for the hypotheses linking national experience of international and domestic threats, in the form of international and civil wars, to an increased tendency to abuse personal integrity rights. Variables identifying countries that were participants in both kinds of wars were found to have statistically significant and substantively important impacts on national respect for the personal integrity of citizens in each of the four sets of analyses, with civil war participation having a somewhat larger impact than participation in international war. Thus this study is the first to document the impact of involvement in violent conflicts, in both the international and domestic arenas, on levels of repression around the world.

Finally, with regard to economic explanations of human rights abuse, we found in each of our four sets of analyses that economic standing is negatively,

but only rather weakly, related to regimes' propensities to abuse of personal integrity rights.

Having now nearly finished this investigation of the conditions that lead to the most serious forms of human rights abuse, we believe we would be remiss if we failed to consider the vitally important question of what we have learned that might be helpful to scholars and practitioners hoping to decrease abuses of personal integrity worldwide. While our primary focus in this study has been on theory, our findings do speak to issues of interest to practitioners. If governments, international organizations, and sub-national actors are interested in improving respect for personal integrity around the world, one way for these actors to make headway toward this goal is, not surprisingly, to promote democracy. Promoting democracy is clearly not the entire answer, however. Our results indicate that if a nondemocratic country known to be a very serious violator of personal integrity rights were to change to a democratic form of government, human rights abuse would probably not disappear completely, other factors being equal. Neither should economic development be viewed as the panacea. Efforts to improve economic conditions within a country through programs like foreign aid might at times successfully promote human rights, defined more broadly, by leading to the provision for basic human needs, but our study indicates these conditions have, at best, a moderate impact on respect for personal integrity. Neither would population control efforts be likely to have a substantively important impact on repression of these rights.

Our findings do, however, reveal another course toward greater respect of personal integrity, one that would have an impact of about the same magnitude as converting autocratic regimes to democratic ones. We have shown that these basic rights can be enhanced by actors who would encourage countries to solve their political conflicts short of war, and use whatever means are at their disposal to assist them in doing so.

APPENDIX

In order to add to the already extensive data set gathered by Stohl and his colleagues (n.d.), Poe and Sirirangsi (1993, 1994) analyzed the content of the State Department and Amnesty International reports for the years 1980–87 and assigned values on a five-point ordinal scale according to the following coding rules, originally set forth in Gastil (1980):

1. Countries [are] under a secure rule of law, people are not imprisoned for their views, and torture is rare or exceptional. . . . Political murders are extremely rare.
2. There is a limited amount of imprisonment for nonviolent political activity. However, few persons are affected, torture and beating are exceptional. . . . Political murder is rare.
3. There is extensive political imprisonment, or a

recent history of such imprisonment. Execution or other political murders and brutality may be common. Unlimited detention, with or without trial, for political views is accepted.

4. The practices of [level 3] are expanded to larger numbers. Murders, disappearances are a common part of life. . . . In spite of its generality, on this level terror affects primarily those who interest themselves in politics or ideas.
5. The terrors of [level 4] have been expanded to the whole population. . . . The leaders of these societies place no limits on the means or thoroughness with which they pursue personal or ideological goals. (Gastil 1980, quoted in Stohl and Carleton 1985)

Following Stohl and his colleagues (n.d.), two coders read and analyzed the content of each source and assigned a number between 1 and 5 to each country. After all coding was completed, the scores assigned by the two coders were compared. It was found that intercoder agreement was 85.6%, a percentage a bit lower than that achieved in one similar human rights data-gathering project (McCormick and Mitchell 1988, 1989) but still quite respectable. This percentage and the gamma statistics for the two variables indicate high intercoder reliability. For the measure derived from the Amnesty International reports, the gamma was .94; for that derived from the State Department profiles, it was .98.

After compiling the scores, the coders found that disagreements often occurred on three kinds of cases. First, there were cases that were categorized as a 2 by one coder and as a 3 by the other. These tended to be countries in which there was limited political imprisonment but considerable alleged torture or in which there was more torture than would ordinarily be indicated by the wording for the 2 category. A second kind of disagreement occurred on cases in which prisoners were taken after alleged demonstrations, riots, or revolution attempts but with uncertainty over whether their political activity was violent or not. Since a major concern was to make our data comparable to those gathered for use in the Stohl study, we consulted those researchers to see how they had dealt with these cases. A telephone conversation with Mark Gibney (February 1991) confirmed that those researchers gave the benefit of the doubt to the government, following the dictate that they should be "innocent until proven guilty." To remain consistent with the data provided by these researchers, we, too, followed this decision rule.

A third type of case on which coders tended to disagree involved countries that imprisoned conscientious objectors identified by Amnesty International as "prisoners of conscience." In such cases, according to Gibney, countries with large numbers of imprisoned conscientious objectors were coded as being in the 2 category as long as other human rights difficulties did not exist, but those that had only one or two such prisoners were coded as being in the 1 category. Again, following the practice of these researchers,

most of these disagreements were resolved through discussion and reconsideration of the facts of the particular cases. But if no resolution was in sight, again following the practice described by Gibney, a third coder was brought in to cast the deciding judgment.

Notes

We could not have written this article without the help of our colleagues and students. Comments and advice from Harold Clarke, Neal Beck, Jim Meernik, and Motoshi Suzuki kept us on track in our analysis of the pooled cross-sectional data set. The editorial suggestions of Vernon Van Dyke added grace and clarity to our sometimes inelegant language. Mark Gibney, Michael Stohl, David Carleton, and their coresearchers at Purdue University generously shared the human rights data they had coded from the Amnesty International and State Department reports for 1980 to 1987 and graciously answered inquiries about the coding of these data. The Interuniversity Consortium for Political and Social Research provided machine-readable versions of the data series contained in the World Bank's *World Tables*, in several volumes of Ruth Leger Sivard's *World Military and Social Expenditures*, and in the Freedom House *Freedom in the World* series, all of which provided portions of the data used in the analysis. These data are available to interested parties via GOPHER (gopher.unt.edu, port 70): look under "UNT Information & Resources/Departments, Schools & Colleges Information/College of Arts and Sciences/Political Science/Repression." They are also available via anonymous FTP (ftp.unt.edu.)

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1. The focus of our analysis will be the abuse of internationally recognized human rights, specifically those having to do with integrity of the person (Cingranelli and Pasquarello 1985; Henderson 1991, 1993; Mitchell and McCormick 1988; Stohl and Carleton 1985), a choice that will be discussed and justified later. We will, however, also refer to the dependent phenomenon as *state terrorism*. Our use of this term is compatible with that of Gurr, who states: "Regimes do many coercive things to induce compliance. They threaten, arrest and jail, fine and confiscate as well as murder. It is plausible, both analytically and psychologically to limit the concept of state terrorism to coercion that takes or grossly endangers the lives of its targets. Thus it includes imprisonment in conditions where many are worked or starved to death, and other denials of means of life, as well as outright killing. . . . Violence by regimes is terroristic only if it is 'instrumental', which means designed to have a wider effect on some audience" (1986, 46). We choose to employ the terms interchangeably because the two phenomena are inextricably linked: abuse of individuals is a human tragedy directly resulting from terrorist policies employed by nation-states.

2. For the linkage of U.S. foreign aid allocation to human rights issues, see Carleton and Stohl; Cingranelli and Pasquarello 1985; McCormick and Mitchell 1988, 1989; Poe 1990, 1991, 1992; Schoultz 1980, 1981; Stohl and Carleton 1985; and Stohl, Carleton, and Johnson 1984. For respect for human rights in relation to U.S. refugee policies, see Gibney, Dalton, and Vockell 1992; Gibney and Stohl 1988. For a linkage between democratic practices and national tendencies to go to war, see Rummell 1983.

3. McKinlay and Cohan analyze a set of regimes and regime-years across a 20-year time span, but they do not explicitly build time or within-unit change into their work.

4. Violations of personal integrity are usually perpetrated

directly by government officials and thus are more easily dealt with by a change in government policy, in direct contrast to violations of economic and social rights, which are often less amenable to change. It is unfortunately doubtful that economic rights (e.g., a right to food, a job, and a home) will be guaranteed in many African countries in the near future even if the government's intentions are exemplary, since the causes of these problems are oftentimes not in the government's control. Respect for social rights (e.g., social equality for women and minorities) are often based on centuries of cultural practice. For a regime to attempt a sudden change in practice before the time is right might lead to a coup, a revolution, or civil war. In such circumstances even reform-minded regimes will likely be slow to change, for to change too quickly could result in a serious setback for the causes they are attempted to promote. By focusing our study on rights associated with the integrity of the person we are purposely directing our inquiry toward policies and crimes we feel can more easily be altered by national governments in the short term, in an effort to be policy-relevant. Perhaps in our future work we will deal with other definitions and dimensions of human rights.

5. For arguments that better data are needed, see Jabine and Claude 1992, esp. chapters by Banks, Bollen, and Lopez and Stohl. However, statements in some of these chapters and empirical work by some of these same researchers (e.g., Stohl and Carleton 1985; Stohl, Carleton, and Johnson 1984) support the argument that current data are sufficiently good to be used in meaningful empirical tests.

6. The measurement alternatives currently available to researchers of human rights phenomena are outlined and discussed in Lopez and Stohl 1992 and Stohl et al. 1986. Events data measures, like those reported in the *World Handbook of Political and Social Indicators* (Taylor and Jodice 1983), depend on newspaper accounts of different human-rights-related phenomena and use counts of state coercive behaviors (e.g., the imposition or lifting of government "sanctions" and political executions) to generate measures of human rights abuse. Research using events data to operationalize various kinds of repression has been reported by a number of scholars (Alfatooni and Allen 1991; Davenport 1992; Davis and Ward 1990; Hibbs 1973; Muller 1985; Ziegenhagen 1986).

Stohl and his colleagues (1986) cite major problems with this approach. First, the different repressive behaviors counted are often substitutable means used to reach the same end—the repression of those who are thought to pose an obstacle to the policies or continued power of the incumbent regime. For example, a decrease in reports of politically motivated arrests by a government might not be an accurate indicator of improving human rights if that decrease takes place soon after several hundred opponents of the government were executed. Second, since reports of human rights abuses are routinely suppressed by closed regimes to an extent that would not be possible in more open societies, events data measures sometimes lead to counterintuitive and invalid findings, like that reported by Stohl and colleagues indicating that the United States was a more repressive society in the years 1948–50 than the Soviet Union under Stalin. We therefore are in strong agreement with the pleas of Stohl and colleagues, who argue that "we must bring political knowledge to bear in shaping our collection of information and measurement schemes" (p. 598). So while events data may provide a useful supplement to the approach we take in this research, we choose not to use it primarily for the reasons just rehearsed. In addition, the events data we would need to operationalize our dependent variables are simply not currently available and our comprehensive international sample for our time period.

Accordingly, we see a standard-based approach as the best available measurement alternative at this time. An advantage of this approach is that it allows for a greater degree of political knowledge to be used in the development and application of standards to sources of human rights information specified by the researchers. Further, such measurement

techniques allow us to focus clearly on the integrity-of-the-person rights in which we are most interested in this study.

7. The ratings were derived from country profiles included in the Amnesty International Reports for 1981–88 and the State Department's *Country Reports on Human Rights Practices* for 1981–88.

8. In coding the Amnesty International profiles, the least repressive coding of 1 was assigned in only 71 cases for 1980–87, compared to 376 cases for the State Department reports. But there was little difference between the two in the number of cases their reports led us to place in the four higher categories. Further, of the 302 cases that were missing in Amnesty International, we gave 220 codes of 1 in our ratings derived from the State Department reports. Therefore, while Amnesty insists that the "omission of a country entry should not be interpreted as indicating that no human rights violations took place in that country" (Amnesty International 1987, 2), in practice, those countries that were omitted from the Amnesty International reports tended to be (but were not always) countries that appeared to adhere to the rule of law.

9. An issue of special concern to us are allegations of bias against the State Department reports (e.g., Carleton and Stohl 1987 and the sources cited therein). However, it is also important to note that some argue that the consistency of the U.S. State Department reports improved during the 1980s and therefore may be more valid in that period than they had been previously (e.g., Innes 1992). Furthermore, if some have argued that the State Department has been biased against leftist regimes, others have suggested that Amnesty International may have been more likely to note abuses by governments against armed leftist oppositions than by those oppositions against government and its supporters.

10. Closely related to these arguments are those of Howard and Donnelly (tested in the Mitchell and McCormick study) that "internationally recognized human rights require a liberal regime" because such regimes give the individual priority over the state (1986, 802). Since liberal regimes clearly fall into the subclass *democracy* as that term is conventionally defined, we see no reason to represent them separately in our analysis.

11. For example, the preface to a recent "worldwide survey" of democracy, notes that "humanitarian concern for human rights is inseparable from . . . democracy or the lack of it. Human rights are part, a good part, of democracy, and they can be assured only by legal, responsible, that is, democratic governments—at the same time affirming that "the question of democracy is broader" than the question of the status of human rights as typically assessed by Amnesty International, the State Department, and Freedom House (Wesson 1987, vii).

12. Bollen (1980) went on to create a variable based on his definitions, covering the years 1960 and 1965. Unfortunately, the measure has not been replicated on an annual basis and thus cannot be used in this study. For further discussion, see Bollen 1990, 1993.

13. The Polity II operationalization of institutional democracy omits any consideration of whether a regime respects the dignity of the person, which makes it an ideal measure for our purposes. Unfortunately, the Polity II measure is available only through the year 1986 and for only 127 of the maximum of 153 nations for which we have measures of state terrorism and our other independent variables. Since we lacked the time and resources to expand the Polity II democracy data to cover 1987 for all nations and 1980–86 for 26 additional nations, using Gurr's democracy index would have substantially reduced the N for our pooled analysis, so we explored other alternatives. However, analyses conducted with the Polity II measure on this reduced sample led us to conclusions similar to those reached in the analyses we shall present. The former are not presented in more detail due to space limitations.

14. A summary collection of values for this variable is reported in *Freedom in the World* (McColm 1990).

15. We do not wish to argue that the Freedom House indicators cannot be used to measure human rights, very broadly considered. But, as we have explained, the indicators derived from the Amnesty International and State Depart-

ment reports, which we analyze, focus much more narrowly on those infringements of the dignity of the person that we and others label "repression."

16. The criteria quoted from the 1989–90 edition of *Freedom in the World* (McColm 1990), are more elaborate than those in earlier editions. Nevertheless, the criteria, if not the clarity with which they have been expressed, appear to have been consistent over the years. Compare, for example, the following from the 1980 edition of *Freedom in the World*: "Political rights . . . allow people to participate freely and effectively in choosing their leaders or in voting directly on legislation. . . . The rating a nation receives for political freedom is determined by factors such as the existence of two or more competing political parties or the independence of opposition candidates from government control. . . . Elections and legislatures have to demonstrate a significant opposition, and those elected have to be given real power" (Gastil 1980, 4–5; emphasis original).

17. These freedoms and others are a part of the Freedom House civil liberties rating. However, the civil liberties rating is not a viable measure of democracy for our use because it specifically includes "protection from unjustified political terror, imprisonment, exile or torture," which overlaps clearly with the indicators derived from the Amnesty International and State Department reports, which are our dependent variables, as well as a number of other rights that are not essential to democracy as envisioned by Bollen (1990, 20–21). Perhaps because the Freedom House civil rights indicator includes basic civil liberties that are a part of many definitions of democracy, the overall correlation between the political and civil rights ratings in our data set is very high, .935. Nevertheless, the conceptual differences between the two indicators and the explicit overlap of the civil liberties indicator with repression convinces us to consider only the political liberties rating as our measure of democracy.

18. This was also noted by Gurr. In contrasting the Polity II indicators with the Freedom House measures, Gurr noted that "the problem with the latter is that Gastil assigned annual scores partly on the basis of repression" (personal communication, 15 September 1992).

19. Bollen's (1993) article appeared too late for us to take full account of his findings and his measurement suggestions in our analysis. But we should note that despite his conclusion about the superior validity of the Freedom House measure, Bollen does not endorse the use of a single indicator. Instead, he suggests using structural equation models to construct indicators that maximize validity while minimizing systematic and random measurement error. Unfortunately, he also notes that techniques appropriate for dealing with "pooled cross-sectional and time series data for nations" are "underdeveloped in latent variable models" (p. 1224). As a secondary strategy, he suggests using factor scores from several indicators as a composite measure of democracy. We might have adopted this approach but did not because (1) the data required to construct the composite index ultimately recommended by Bollen were not available to us in appropriate time series form prior to our completion of our analyses and (2) the suspicion with which many analysts regarded the Freedom House measure mandated that we analyze it separately in order to contrast it with an indicator that represented a different and nonjudgmental approach to measuring democracy, the Vanhanen index.

20. The zero-order correlations between the Freedom House measure of democracy and indicators of human rights abuse are only $-.47$ for ratings derived from the Amnesty International reports and $-.54$ for those derived from the State Department profiles in the 1980–87 period. With the Vanhanen measure of democracy, the correlations were $-.50$ and $-.52$, for those two measures of human rights, respectively. With the Polity II measure, which limits the analysis to a smaller and temporally different number of cases the correlations were $-.55$ and $-.52$, respectively.

21. The skewed distribution of total population made it desirable to log this variable to meet the statistical assumptions of our methods.

22. Clearly, the ideal would have been to employ estimates that accurately reflect changes from year to year. When efforts were made to obtain such estimates, however, the results were disappointing. The numbers generated for many data points were simply unrealistic, due to sudden changes (sometimes unexplainable decreases) in gross population estimates for Third World nations and for countries with small populations. We therefore employed a variable indicating the average population change of a country over the eight-year period of the study as the measurement alternative that was better in practice. We did not expect—and saw no—evidence of faulty estimates causing any difficulties in estimating the gross population variable, as the few difficulties were not of a difference of magnitude.

23. For example, Henderson used energy consumption and its growth as his measures of economic growth and development, and Park used the physical-quality-of-life index created by Morris (1979) as his measure of “economic basic needs rights.” In addition, Henderson used an international human suffering index to measure “socioeconomic needs,” which turned out to be very highly correlated with his energy consumption measure of economic development (Population Crisis Committee 1987). The continuing dissatisfaction with GNP per capita has also led to the creation and reporting annually of a new human development index by the United Nations Development Programme (1990, 1991).

24. Although this classification was created by a single coder, Hamed Madani, it has been subjected to a variety of tests designed to document its validity and reliability. Cross-verification against the coup data set of O’Kane (1987) and against the “military control” classifications of Sivard (1989) found very few instances in which Madani’s data disagreed with O’Kane’s as to whether a coup had occurred in a given country and relatively few instances when Sivard coded a nation as military-controlled and Madani did not. In the latter case, such disparities appeared to result more from the use of a broader and vaguer classification scheme by Sivard than from any error on Madani’s part. In the former case, O’Kane’s data usually appeared to be in error. As a final test, Madani’s classification was compared to two independent classifications prepared by two undergraduate research assistants, Jessie Hill and Michael Prior. Again, there proved to be few instances of disagreement, and, when there was disagreement, it was most often because the undergraduates coded a regime as civilian due to a failure to realize that it had originally come to power through a coup.

25. Bollen and Jackman (1985, 1988) provide strong empirical evidence for the effect of British colonial influence on democracy, as operationalized by Bollen’s democracy index for 1965. However, readers should remember that like Mitchell and McCormick, we take pains to distinguish democracy, one of our independent variables, from repression of personal integrity, our dependent variable.

26. Using events data in a crossnational research design, Hibbs does show a positive relationship between “internal war” and “negative sanctions” by government (1973, 182).

27. In coding international and civil wars we used as a guide information from Sivard 1991 and Brogan 1990. In cases where there were doubts as to whether the criteria were met, we sought information from other sources dealing with the situation in the particular country.

28. The White technique was adapted to the PCT design by Beck and his colleagues (1993). According to them, “if X is the variance-covariance matrix, the robust variance-covariance matrix of the errors is estimated by”

$$(X'X)^{-1}X' \frac{(E'E)}{T} \otimes I_T X(X'X)^{-1},$$

where E is the $T \times N$ matrix of residuals, T is the number of time points, N is the number of nations, I is the $T \times T$ identity matrix, and \otimes is the Kronecker product (p. 946). The equation

$$\frac{(E'E)}{T} = \hat{\Omega}$$

represents the estimated covariance matrix of the country errors used in feasible generalized least squares analyses of pooled cross-sectional time-series data sets, (*ibid.*). This procedure does not affect the OLS coefficients, but it does estimate more consistent standard errors. The RATS procedures that allowed us to use the robust-standard-errors approach on PCT data were developed and used in Beck et al. 1993 and Beck and Katz 1994 and were generously provided by Nathaniel Beck, via his FTP server.

29. To test for the possibility of autocorrelation difficulties, we computed the basic model for both the dependent variables derived from the Amnesty International and State Department reports with both democracy variables. An inspection of the residuals gained from OLS regression turned up few examples of sign changes in the residuals resulting for particular countries, a pattern indicating that positive autocorrelation is a difficulty (Ostrom 1990). The Durbin-Watson statistics ranged from .44 to .86, in the four preliminary tests of the basic model, thus also indicating that positive autocorrelation likely affects OLS results. For an argument advocating the use of lagged dependent variables to deal with autocorrelation difficulties, see Beck and Katz 1994.

30. We were reluctant to discard data from this year because it decreased our sample size by one-eighth. We therefore experimented with an estimation method that allowed us to retain the first time point while dealing with autocorrelation and heteroscedasticity—the Prais-Winsten estimation technique (Ostrom 1990, 31–41), used in a first-order autoregressive (AR(1)) model for which we calculated White’s robust standard errors, using RATS 386, version 4.02. There were some divergences between the results yielded by this approach and those we rely on herein. Specifically, it appeared that military control and British cultural influence had much stronger and usually statistically significant effects on repression when the first case was retained in an autoregressive model with robust standard errors. However, when we inspected the estimates calculated for the first case for each of our variables, we became skeptical. The values of the estimates usually appeared quite atypical of their series, especially for the dichotomous variables. Also, when tests were conducted with a similar AR(1) model, with the first year discarded to ascertain the effects of these estimates, the coefficients for these variables decreased in both size and statistical significance. We took this as an indication that the strength of these variables in our AR(1) models with the Prais-Winsten estimates was likely due to faulty estimations of the first time point. In the text, therefore, we choose the more cautious path and present the analyses with the first case discarded, due to our use of an lagged endogenous variable—analyses that do not include significant effects for British cultural influence or military control.

31. Since the Durbin-Watson statistic is biased when a lagged dependent variable is entered on the right side of the equation, the autocorrelation functions were examined to determine whether the autocorrelation difficulties were solved. The autocorrelation functions for lag 1 in the four sets of analyses ranged from $-.16$ to $-.18$, with standard errors of about .03. Most of the autocorrelation functions for the remaining lags were at, or below, .10, the largest being just .18. Thus we can say with some confidence that autocorrelation does not pose a problem in our interpretation of these results.

32. This can be done quite simply. To find the effect of the loss of democracy at time_{*t*+1}, we simply multiply the effect of this loss at time_{*t*} by the coefficient of the lagged dependent variable and add the direct effect of democracy at time_{*t*+1}. Repeating this process for each successive lag will, after several lags, fall into an asymptotic pattern if the loss of democracy continues.

33. The Vanhanen democracy index is not available for the 1970s.

34. We do not present graphs of these effects here, but we will gladly provide them on request.

35. This literature is far too voluminous to cite fully. Relevant early research includes Cutright 1963; Lerner 1958;

Lipset 1959; and the studies cited in Tate 1972. More recent work is cited and discussed in Bollen 1990, 1993; Bollen and Jackman 1985, 1988; and others.

36. However, in a set of supplementary analyses for 1981–87 using an AR(1) model to control autocorrelation and White's robust-standard-errors approach to control for heteroscedasticity, the British influence variable did exhibit a statistically significant coefficient of moderate strength (.2 to .3) in analyses using the Vanhanen measure of democracy. So perhaps this hypothesis should not be dismissed completely at this point. Other results yielded by these analyses were very similar in terms of the variables that reached statistical significance, with the exception of the international war variable, which failed to reach conventional levels of statistical significance in analyses with the dependent variable derived from Amnesty International profiles, but did achieve statistically significant coefficients of around .5 in the analyses based on ratings generated from the State Department reports. These analyses are not presented in more detail because of space limitations and because the findings are generally similar. We decided to present the OLS analyses with the lagged dependent variable because it is a more widely understood method, because of the ease in calculating the lagged effects of independent variables, and because the results were more consistent from one set of analyses to another. The results of the AR(1) models described here are available upon request from the authors.

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