Democracies Really Are More Pacific (in General)

REEXAMINING REGIME TYPE AND WAR INVOLVEMENT

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Current consensus in the field of democratic peace research holds that democratic states go to war in general no less than nondemocratic states. The author challenges this consensus by reevaluating the main empirical studies on which it rests, using information that previous studies ignored and statistical techniques unused or even unknown at the time. The results indicate that from 1960 to 1980, democratic nations were less involved in military conflict than other regime types. Estimates of this relationship are robust to different operational definitions of both war and democracy, to the addition of control variables for other possible correlates of war, and to the application of different statistical techniques. This indicates that lack of previous significant findings have less to do with the data than with the methods used to analyze them.

DEMOCRATIC PACIFISM VERSUS DEMOCRATIC PEACE

Democratic states, according to current scholarly consensus, are involved in war as often as states with other regime types. The lack of a general relative pacifism of democracies has been established in numerous empirical studies (Small and Singer 1976; Chan 1984; Weede 1984; Maoz and Abdolali 1989; Morgan 1991) and reinforced through theoretical and philosophical argument (Doyle 1986; Lake 1992; Morgan 1991). A second consensus, albeit one subject increasingly to challenge, focuses on the peace between democratic states. Because investigations about whether democracies have fought fewer wars in general have yielded no significant results, researchers have turned their attention to the difficult-to-refute finding that democracies do not fight wars with each other (Babst 1972; Zinnes 1980; Rummel 1983; Levy 1988; Morgan 1991; Lake 1992). Scholars have given the second issue, referred to as the "democratic peace," nearly all their attention in the recent literature. Yet this emphasis is rather recent, because attention was first focused on distinguishing states

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by regime type and analyzing their relative propensity to be involved in wars (see Morgan 1993). By and large, these efforts were consistently unable to establish a statistically significant pattern, and this led to the refutation of what I shall term the *democratic pacifism proposition.* Democratic pacifism relates to single states and their foreign policy behavior toward all other nations, whereas *democratic peace* refers to the peaceful foreign policy behavior of democratic states toward other democratic states.[1]

In this analysis, I return attention to the democratic pacifism proposition. I come not to praise it so much as to dig it up, in the expectation that the exhumation will reveal that its burial was premature. Renewed attention on the monadic relationship is warranted for several reasons. First, the substantial normative reasons for linking regime type with certain types of foreign policy behavior—reasons that motivated such studies in the late 1970s and 1980s and continue to motivate the democratic peace research—are still quite powerful. If democracies are less war prone, then arguments for peace become additional arguments to support democracy. In other words, if successful transitions to democracy can reduce global conflict, then support for democratization becomes "as much an issue of national security as of national conscience" (Gershman 1989, 20). Moreover, the stakes in this debate have risen as greater numbers of nations experiment with liberal forms of government. Of 186 countries surveyed in 1992 by Freedom House (Taylor 1985), 75 nations were judged to be genuine democracies, 19 more than in 1980. This is more than 40%, compared to only 20% in 1976 (Huntington 1991, 25).[2]

Second, interest in democratic pacifism continues despite the failure of past empirical studies to establish that democracies are less prone to conflict because there was a loose expectation—although perhaps little more than a hope based on cold war propaganda—that democracies really were more pacific. As T. Clifton Morgan and Sally Campbell (1991, 188) explain, this is because "evidence [to the contrary] flies in the face of a theoretical argument that has been advanced, persuasively, by numerous politicians, social scientists, historians, and philosophers." This position holds that democracies have structural and ideological reasons to act with less hostility toward other nations. In a republican regime, it is argued, decision making is diffused, and those bearing many of the burdens of costly wars are in a position to avoid unpopular involvement in foreign conflicts. The constitutional design of representative government is such, according to Kant (1970, 100), that citizens who face "calling down upon themselves all the miseries of war . . . will have a great deal of hesitation in embarking on so dangerous an enterprise." Civilian control of the military, for example, leaves decisions for war in the hands of officials selected by and accountable to a presumably risk-averse public. Furthermore, democracy is thought to foster powerful norms against the use of violence as a means of conflict resolution. A basic tenet of democratic theory is that disputes can be resolved through institutionalized channels without

1. This distinction, except for the characterization of democracy as embodied in freedom, is similar to Rummel's (1983) freedom proposition and joint-freedom proposition.

2. See also Gershman (1989). Starr (1991) statistically establishes the recent global movement toward democratic forms of government.

resorting to force. Lethal violence is considered illegitimate and even unnecessary, a norm that is believed to hold between, as well as within, democratic societies.

Democratic theory thus predicts that support for war between democracies will be difficult to secure: "insofar as the other states' demands are considered ipso facto reasonable according to a view of one's own system that extends to theirs, popular sentiment for war or resistance to compromise is undermined" (Russett 1990, 113). Open relations between liberal peoples lead to the formation of civilian bonds that make it counterproductive to abandon peaceful relations. The intercourse and interdependence of financial, political, educational, and social relations create "cross-cutting transnational ties that serve as lobbies for mutual accommodation" (Doyle 1986, 1161). Such ties are developed and maintained only under conditions of peaceful relations.

Recent interpretations of these normative and structural incentives for democracies to behave peacefully, however, have suggested that these forces work for peace only between democratic states. The structures and ideologies motivating peace between democracies, according to this view, may make war just as easily or even more likely between liberal and illiberal states (Doyle 1986; or see Russett 1993, chap. 2 for a review). Such an explanation offers a means to explain why democracies are peaceful toward each other but not toward other states in general. Yet the empirical discrepancy between the democratic peace and democratic pacifism results remains puzzling, because presumably at least some of the structural constraints that discourage involvement in costly military conflicts would operate irrespective of the opponent state's regime type. The reluctance to incur casualties in contemporary peacekeeping missions seems to support this notion. And when democracies do fight wars, they have been shown to win more often than nondemocratic states (Lake 1992), indicating perhaps that they are relatively judicious in deciding which conflicts to join.

A final justification for a reexamination of the democratic pacifism proposition lies in the recent advances in quantitative political science. The empirical studies of the 1980s, which refuted the proposition, used clumsy statistical methods to which better alternatives are now available in political science. Thus researchers who continue to examine the linkages between war and regime type hope that the incongruity between theory and evidence simply reflects a failure to use data that are adequately precise and comprehensive or modeling procedures that are sufficiently powerful.

In this article, I systematically review the main empirical pieces establishing that democracies fight no fewer wars in general than nations with other regime types, and I develop a new model to test whether their conclusions were correct. To make the results comparable, and to underscore the point that the consensus that democratic monads are not pacific may rest more on methodological than substantive grounds, I use data and basic approaches borrowed from or directly comparable to those found in the original studies. To justify my restructuring of the original research designs, I will discuss war event modelling in some detail, which may be of general interest beyond the democratic peace literature. My conclusions, based on the application of this model, suggest that democracies were indeed more pacific during the 1960s and 1970s than nations with less free regimes.

A CONSENSUS IN NEED OF REVISION

The empirical evidence on which the lack of a significant relationship between regime type and war involvement rests has deep and variegated roots.[3] One of the first direct studies of this question was Small and Singer's (1976) analysis of the regime types of nations from their sample of 93 wars fought between 1816 and 1965. They grouped the warring nations according to regime type and used difference of means statistical tests to estimate whether democracies were on average less frequently involved in wars from their sample. The results failed to show a statistically significant difference and led Small and Singer to conclude that democracies were no more peace prone than other states.

Yet despite its wide influence, problems in the Small and Singer (1976) research design make it a poor foundation on which to begin build a consensus. Not only were Small and Singer's statistical tests simplistic (difference between means) and their measure of democracy very rough (a dichotomous indicator), but their analysis also suffered from a fatal design flaw that makes its conclusions irrelevant to the democratic pacifism issue. Their sample selected wars rather than regimes with the potential for war as the democratic pacifism proposition would suggest. In other words, the probability of war, given regime type, is central to the democratic pacifism question, whereas Small and Singer instead estimated the probability of regime type given war involvement. The two are statistically nonequivalent, a problem which Small and Singer themselves plainly acknowledged (Small and Singer 1976).[4]

The next major empirical test came from Rummel (1983).[5] He examined the association between regime type and conflict from 1976 to 1980 and discovered evidence that freer regimes did indeed commit less acts of "official violence." Although it stands as a lonely voice of dissent, Rummel's study did not establish the democratic pacifism proposition because it suffered from a narrow time period and an idiosyncratic operationalization of its main variables. Rummel's measures of both conflict and democracy were nonstandard, relying on media reports he collected of the first and a measure that included "economic freedom" for the second. His conflict scale also has been contested at length (Vincent 1987a, 1987b). But even if Rummel's methods and measures are accepted, his study examined a period of only 4 years, which other research has found to have been atypical (Weede 1984; Chan 1984).

The two subsequent nails in the coffin for the democratic pacifism hypothesis came from Chan (1984) and Weede (1984). Published in the same issue of the *Journal of Conflict Resolution*, the studies both examined war involvement rates using regimes

3. Reviews of this literature can be found in Morgan (1993), Levy (1988), Rummel (1985), and Zinnes (1980).

4. The problem is illustrated by Bayes's theorem: $P(A|B) = \frac{P(B|A)P(A)}{P(B)}$. The term $P(A)/P(B)$ is the missing factor that makes Small and Singer's conditional probability nonequivalent.

5. Rummel (1985, 1995) has systematically summarized and critiqued the monadic-level regime type and conflict literature, claiming evidence for democratic pacifism and the tendency of democracies to fight less severe wars than other regimes, although these are not systematic empirical tests on the level of the others discussed here.

or regime years as the units of analysis. Both found when looking at regime monads that although estimates seemed to indicate less war involvement by democracies, none of the estimates was statistically significant. Both Weede and Chan concluded that the democratic pacifism proposition could not be upheld.

Chan's (1984) study relied on the correlates of war (COW) data set, using dichotomous measures of both democracy and war from 1816 to 1980 and using nation years as the units of analysis. His conclusions of no relationship are based on chi-squared (χ^2) statistics from 2×2 contingency tables grouping the two dichotomous measures. Weede's (1984) study used a more sophisticated measure of democracy, yet it also relied on χ^2 tests from contingency tables and even simple bivariate correlations. Weede sampled the counts of war involvement for three different war measures from 1960 to 1980 and used Kenneth Bollen's (1980) POLDEM scores of political democracy, yet he threw away this additional information when he reverted to the same association tests used by Chan.

Subsequent research has moved away from the level of the monadic nation-state, focusing instead on dyads and turning attention toward explaining the democratic peace phenomenon of the absence of wars between democracies. Some of these dyadic studies also have dealt with the democratic pacifism proposition but have tended to look at levels of conflict below war (Maoz and Abdolali 1989) to sample disputes and not nation years (Morgan and Scwhebach 1992), or they have used dichotomous measures and low-power statistics to draw these conclusions (Bremer 1992; Farber and Gowa 1995).[6] Although most of these studies have used or at least have included more sophisticated statistical tests, none has applied them to the original monadic-level issue of estimating the influence of regime type on a nation's rate of war involvement.

The shortcomings found variously in previous approaches to the question of democratic pacifism can be summarized as follows:

1. An indirect research focus has led to conclusions not related to the basic substantive question: Are democratic states less likely to be involved in wars than nondemocratic states? When the research has directly targeted this question, the results have focused narrowly on statistical significance and p values instead of on estimates of the causal relationship and its variability.
2. Measures of key variables, notably the democracy variable, have used scales that are information poor or have collapsed scales with more gradations (e.g., Gurr, Jaggers, and Moore 1989) into measures with two or, at best, three categories.
3. Statistical models have been based on false assumptions about the underlying stochastic processes generating the data, most notably those concerning the nature of war events. Although the conventional wisdom is that numerous studies have found no relationship, it would be more accurate to state that these studies have found no statistically significant relationship. This is because nearly all studies have found that democracies do go to war less on average than other states, but then they reject the conclusion based on p values

6. Bremer (1992) also applies a difference of proportions test to the dyadic data set and finds some (but inconclusive) evidence that the presence of at least one democracy in a dyad reduced the probability of war in a dyad year, with a .064 p value. Interestingly, the Farber and Gowa (1995) tests found a nearly identical result: democracies were less involved in wars on average, but the χ^2 statistic was significant only at the .063 level. Not only is this very close to the conventional .05 significance level, but it is also sufficient to reject a one-sided null hypothesis, which is really what substantive theory should lead us to test.

from statistical tests that incorporate none of the specific theoretical information we possess about war events and regime type.

An improved research design should address all three concerns: modelling the causal relationship of regime type on war involvement directly, taking into account the nature of the process by which war events are generated, and using as much information in the data as feasible, including the possibility of including control variables. In the sections that follow, I provide a theoretical development and then an application to data of such an agenda.

A MODEL FOR TESTING DEMOCRATIC PACIFISM

The basic question, as formulated in the pre-1985 scholarship on democratic pacifism, was this: does regime type influence war involvement? More specifically, are democratic states less likely to become involved in wars than nondemocratic states? Resolving this issue requires a directional model that will permit the causal influence of democraticness on rates of war involvement to be estimated. Because it is my argument that previous research has found no relationship for largely methodological reasons, this section goes into considerable detail on the stochastic modelling of war events. For reasons that will soon be explained, this study measures war involvement based on observations from a discrete time period. For each nation, the analysis is structured to estimate how much varying degrees of democraticness, measured once for the time period, affected the number of wars in which that nation was involved.

The principal concern of any model is the following: does it sufficiently conform to the process that generated the data that are observed? To the extent that all models are in a sense wrong, this fit will never be perfect. Some models, however, are clearly more wrong than others. *Event counts* form one class of data in which some process produces discrete and countable outcomes randomly during a fixed period. At the end of this period, only the total number of these events is observed. This process imposes several constraints. First, the number of events can never be less than zero, and second, counts take only integer values.

For these reasons, a model based on ordinary least-squares (OLS) regression is likely to provide a poor approximation to most event count processes. The error terms cannot be normally distributed, for instance, because the observations are nonnegative and discrete. Nor can the expected value of the observed variable be a linear function of covariates, because linear functions permit negative values. These problems are greatest when the observed event counts are small, resulting in parameter estimates and standard errors that are not meaningful because the predictions of the model for event occurrences are no longer sensible.

A common solution is to model event counts as Poisson random variables.[7] When Y is a random event count and only y_i is observed at the end of each observation period

7. Bremer (1992) has applied a Poisson model to test war frequencies among dyads, although here I discuss the issue of model selection in greater detail.

i, then the data conform to a Poisson process having a rate λ, where $\lambda > 0$, provided they conform to a few additional assumptions (for a definition, see Ross 1983). Zero events must have occurred at the start of the period, and more than one event cannot occur at the same time. Furthermore, events occur in time intervals that, in addition to being disjointed, are both *independent* and *stationary*. Independent increments require that the numbers of events that occur in disjoint time intervals be independent. In other words, the number of events at time t is independent of the number of events between times t and $t + s$, for all s, $t > 0$. Stationary increments, on the other hand, mean that the distribution of the number of events that occur in any time interval depends only on the length of the time interval. This means that the probability of event occurrence is uniform across the observed time interval and constant with respect to other time intervals.

Together, these assumptions imply that the Poisson process is *memoryless*. That is, at any point in time, the process probabilistically starts over, regardless of what has occurred in any previous time interval. The process from any point on is independent of all that has previously occurred (by independent increments) and also has the same distribution as the original process (by stationary increments) (Ross 1983).

How well does a typical process that generates war events conform to the Poisson assumptions outlined earlier? The first assumption is generally a technical requirement but one to which war events do not quite adhere. In any data set with a discrete time period, wars could be underway at the start of the observation period. In addition, it is possible and even likely for a nation to be involved in more than one war event at the same time. This would mean that the increments were not disjointed, but it implies an even more serious violation of the Poisson assumptions: The war events are not independent.

Given the nature of international conflict, the assumption that the increments between wars are independent is particularly tenuous. Foreign policy decisions are not made in a vacuum, and those who plan and execute foreign policy have memories that function very well. In sum, one conflict event can be expected to influence future conflict involvement and be influenced itself by past conflict involvement. This condition is known as *contagion* and means that the expected number of events at one time is dependent on the realized number of events at some previous time. Second, and by implication, the counting process cannot really be described as homogeneous because there are likely to be time intervals within the observation period where the probability of conflict events is higher than others. The assumption of stationary increments, therefore, does not hold because wars do not occur at constant rates. International conditions, bilateral relationships, economic circumstances, and a country's national leadership may all change during the period from which the count of wars is observed.[8]

The effect of violating these assumptions is that the stochastic process is overdispersed. Overdispersion occurs when the variance of the expected events is greater than

8. In practice, a process that generates events at rates that differ among increments can still be modelled as a Poisson random variable, as long as the nonconstant rate of event generation is identically distributed among observations (Ross 1983). A Poisson process with nonstationary increments is known as a *nonhomogeneous Poisson process*.

that under the independence assumptions of the Poisson model, which requires that $V(Y_i) = E(Y_i)$. Overdispersion, resulting from the conditions just described, implies instead that $V(Y_i) \geq E(Y_i)$. As a result, although the Poisson regression model may therefore provide a better approximation to the data than a linear normal model, its assumptions about the variance are unlikely to hold as a model of international conflict events.

Alternately, we may select a model that allows a greater variance than the Poisson distribution, ideally one in which the degree of overdispersion itself can be estimated as a parameter from the data. The stochastic model I used for this purpose is referred to as the *negative binomial distribution*, first derived by Greenwood and Yule (1920). It is an example of what is known in the stochastic processes literature as a *compound* or *generalized* Poisson process.[9]

In this model, the Poisson parameter λ_i is modeled as a random variable that follows a gamma distribution. This adds an additional parameter σ^2 to the event count model that acts as a scaling factor, letting $V(Y_i) \geq E(Y_i)$. Here, I use the parametrization and likelihood form derived by King (1989), which after some substitution yields the following probability density function for the negative binomial:

$$f_{nb}(y_i \mid \lambda_i, \sigma^2) = \frac{\Gamma\left(\frac{\lambda_i}{\sigma^2 - 1} + y_i\right)}{y_i!\,\Gamma\left(\frac{\lambda_i}{\sigma^2 - 1}\right)} \left(\frac{\sigma^2 - 1}{\sigma^2}\right)^{y_i} (\sigma^2)^{\frac{-\lambda_i}{\sigma^2 - 1}} \tag{1}$$

where $\lambda_i > 0$ and $\sigma^2 > 1$ and $\Gamma(\bullet)$ is the gamma function. This expression resembles the Poisson distribution but includes a variance parameter (σ^2). As σ^2 approaches 1, the negative binomial distribution approximates the Poisson.

Functionally, the systematic relationship between $E(Y_i)$ and the covariate matrix X is identical to the exponential form normally used in the Poisson regression model: $E(Y_i) \equiv \lambda_i = \exp(X_i\beta)$, where $\lambda_i > 0$. The overdispersion is estimated by the parameter σ^2, where $V(Y_i) = \sigma^2 E(Y_i)$, for $\sigma^2 > 1$.[10] The resulting model can be expressed in log-likelihood form as:

9. General treatment can be found in Daley and Vere-Jones (1988) and Consul (1989). For additional treatment of the negative binomial model, see Plackett (1981) and King (1989).

10. Although σ^2 can be expressed in many possible ways, it is expressed here as a scalar that is constant for each i. One problem with this assumption may be that the variances differ among observations: σ^2 may not be constant for all i. In fact, this condition is not implausible, given the fact that countries of different sizes and foreign policy histories are grouped in the sample (the United States, Switzerland, and Costa Rica, for instance). In other words, even if one assumes that the observations are independent, they may not be identically distributed. This is not, unfortunately, a problem that the addition of a dispersion parameter can address. It does, however, take a first step, at least letting some degree of variance be freed from the mean assumption of the Poisson distribution. In practice, this is nearly always a problem, certainly not one unique to modelling event counts. In least-squares regression, the problem of a nonscalar σ^2 is known as heteroscedasticity and is nearly always present in some form. One solution is to use heteroscedasticity-consistent standard errors for the coefficients.

$$\ln L(\beta, \sigma^2 | y_i) = \sum_{i=1}^{n} \left\{ \ln\Gamma\left(\frac{\lambda_i}{\sigma^2 - 1} + y_i\right) - \ln\Gamma\left(\frac{\lambda_i}{\sigma^2 - 1}\right) + y_i \ln(\sigma^2 - 1) - \ln(\sigma^2)\left(\frac{\lambda_i}{\sigma^2 - 1} + y_i\right) \right\}, \quad (2)$$

where $y_i = 0, 1, \ldots, \sigma^2 > 1$, and $\lambda_i = \exp(x_i\beta)$.

Using iterative methods,[11] this log likelihood can be maximized to produce consistent and efficient parameter estimates when the independence assumptions of the Poisson model are violated. In the next section, this model is applied to the analysis of war and democracy in the manner of the early democratic pacifism scholars. The estimates of the democracy and war involvement relationship that it yields lead to a startling reassessment of previous analyses of the relationship between war and regime type.

REESTIMATING THE DEMOCRACY-WAR RELATIONSHIP

DEFINITIONS AND MEASUREMENTS

My data and measurements are based on those used by Weede (1984). The decision to use this data set was made for several reasons. First, the monadic-level design and measurement of wars as event counts permits the application of a stochastic model tailored specifically to war events, taking into account the process by which such data are generated. Second, using Weede's data set represents a particular challenge, because unlike subsequent studies that have found weak evidence that democracies may be less war prone (Bremer 1992), Weede's conclusions clearly indicated a rejection of the democratic pacifism proposition. If the application of new methods to the same data can in fact show support for democratic pacifism, then my main argument will be directly supported. Third, the immediate importance of demonstrating that the democratic nonpacifism consensus may be unfounded because of methodological flaws overrides the goal of selecting the perfect data set. My approach in this piece suffers from no fewer flaws than the study on which it is based, and the demonstration of its point through a replication-based design outweighs whatever gains might come from selecting a new data set. It is absolutely correct to concern ourselves with such data desiderata as definitions of democracy that change over time, longer time periods, and more discriminating definitions of war and conflict. Yet if the present study can demonstrate with a more modest and previously examined data set that *methodology* also matters greatly in the outcome, then it will have made a contribution to future, more comprehensive studies using superior data sets.

Weede (1984) examined correlations between wars from 1960 to 1980 and several measures of political democracy. The study was a test of Rummel's (1983) tentative finding that freer states are less prone to violence. Weede's (1884, 649) results were congruent with previous (and subsequent) research on this issue that "democracy and war involvement are not consistently and significantly correlated with each

11. Parameters using this model can be (and were) estimated using Gauss and the COUNT library routine negbin().

TABLE 1
Summary Statistics of War and Democracy Measures

	Butterworth (1960-1974)	*Small-Singer (1960-1980)*	*POLDEM 1965*	*Freedom House 1973*
Mean	1.15	0.45	53.99	7.12
Minimum	0	0	5.20	2.00
Maximum	7	3	100.00	14.00
Standard deviation	1.63	0.81	4.17	4.17
N	101	101	100	101

other. . . . Whether a nation enjoys democratic rule or suffers from dictatorship, the risk of getting involved in a war is the same." Weede's statistical analysis indicated, however, that the estimates of the democracy-conflict relationship are negative as predicted, although not statistically significant. From this it was concluded that regime type and war are unrelated. Weede's analysis provides a good point of return to the democratic pacifism issue, because his basic approach was sound and because his data contained a considerable amount of extra information that can be applied using a new model. Weede's study is also one of the pillars on which the democratic nonpacifism consensus stands, and contradicting Weede's conclusions would therefore significantly undermine this consensus.

Weede's (1984) study involved three measures of war. The first came from the COW project (Small and Singer 1982). Only military conflicts with at least 1,000 battle deaths can qualify for the COW data set. Small and Singer also distinguished between interstate wars and extrasystemic wars, although my analysis combines the two.[12]

Weede's (1984) second measure of war involvement was taken from Butterworth (1976). Butterworth's compilation covered military conflict events from 1960 to 1974, from which Weede selected only events with at least 100 casualties. The final measure is from Kende (1982), which I have dropped because of data problems and inconsistencies with his war criteria.[13] Except for the time periods, the indices differ mainly with regard to casualty thresholds. Because Small and Singer (1982) adopted the most restrictive criteria, their wars are the least frequent, with a maximum of three for any country during the period and an average of .45 (see Table 1). The Butterworth war

12. Extrasystemic wars are defined as wars that met the 1,000 battle death threshold but in which a nation did not fight with another member of the international system: imperial, colonial, or internationalized civil wars (Small and Singer 1982). Weede (1984) included extrasystemic wars in his results, although he also tested interstate wars separately. Only six extrasystemic wars occur in the data, and excluding them causes the results to differ only in degree. In the negative binomial regressions for only the Small-Singer interstate wars, presented in Table 2 (for the POLDEM and Freedom House measures, respectively), the coefficients (and standard errors) were –0.0079 (0.0066) and –0.0687 (0.0526), yielding p values of .10 and .11. These slightly weaker results seem to be caused by the extrasystemic war states being highly nondemocratic, and excluding them eliminates five nations from the list of countries that had any war events, making a sparse data set even sparser.

13. Kende (1982) recorded military conflicts from 1960 to 1980, excluding domestic civil wars. The Kende wars correlate only .45 with the Small-Singer wars, for example. None of either my tests or Weede's showed the results to be consistent with the Small-Singer or Butterworth results. I thank Bruce Russett for the suggestion to omit the Kende data from my replication.

measure correlates highly (0.65) with the Small-Singer combined wars, even though the Butterworth measure was collected only through 1974. I agree with Weede that a robust finding of a correlation between democracy and war involvement should hold by using different operational definitions of conflict and democracy.

Weede's (1984) measures of democracy concentrated on civil and political liberties, with more democratic states receiving higher scores. The first democracy measure was the index of political democracy (POLDEM) developed by Kenneth Bollen (1980). The POLDEM scores range from 0 to 100 and rank countries according to press freedom, toleration of opposition, absence of government coercion, fairness of elections, and democratic executive and legislative structures. The second measure of democracy comes from the Freedom House (Taylor 1985) rankings of states according to liberties. Each nation is ranked from 1 to 7 on both civil and political rights—a 1 indicating a completely free nation and a 7 indicating completely nonfree. Like Weede, I added and inverted these scores to form a scale from 2 to 14, where 14 is completely free.[14]

Table 1 presents summary statistics for the two war measures. The correlation between POLDEM 1965 and the Freedom House 1973 score is 0.72, and other correlations indicate that, generally, democracy was stable over this period (Weede 1984).[15]

Following Weede's (1984) practice, I focused on POLDEM for 1965 and on the Freedom House 1973 combined score as indicators of democracy for the 1960 to 1980 period. Finally, for inclusion in the 101-nation sample, a country must have been independent in 1960 and have a population of at least one million.

APPLYING THE REGRESSION MODEL

The influence of regime type on war involvement can be estimated directly by using the negative binomial regression model. Regression analysis is familiar to most political scientists, even if no previous efforts to test the monad-level relationship between war and democracy have made use of this class of models. Weede's (1984) analysis, for instance, relies on correlation coefficients (product-moment correlations) for inferential tests. His second table reports Pearson's r and associated one-tailed p values for correlations between the democracy measures and war counts from Butterworth (1976), Small and Singer (1982), and Kende (1982).

Unfortunately, the inefficiency of the OLS model and all its problems regarding event data apply to Weede's (1984) analysis because of the linearity built into correlation coefficients. Calculating Pearson's r is tantamount to estimating the slope in a bivariate OLS regression model in which both X and Y have been normalized.[16]

14. These scores are for 1973 and are taken from the Taylor (1985) data set.

15. The obvious objections to measuring democracy as a single point in time are that regime types change, and these changes may be linked to the initiation of international conflict. The event count model used in this article, however, requires single measures of democracy, and Weede's original framework used the same single-year measures. My experimentation showed that averaging democracy scores over a range of years and changing measure years did not significantly affect the results of the analysis.

16. Pearson's r is defined as $r_{xy} = \text{Cov}_{\wedge}(X, Y)/s_x s_y$, where s_x and s_y are standard deviations of X and Y. The equivalency follows from the definition of $\beta_1 = \text{Cov}(X, Y)/V(X)$ in the bivariate OLS model $\hat{Y} = \beta_0 + \beta_1 X$.

TABLE 2
Regression Models: Correlations between Democracy and War Involvement

	OLS		*Poisson*		*Negative Binomial*	
	POLDEM 1965	*Freedom House 1973*	*POLDEM 1965*	*Freedom House 1973*	*POLDEM 1965*	*Freedom House 1973*
Butterworth Wars (1960-1974)						
Constant	1.6026	1.4953	0.5167	0.4405	0.6079	0.5679
	(0.3241)	(0.3205)	(0.1729)	(0.1708)	(0.2503)	(0.2482)
Individual variation	–0.0082	–0.0487	–0.0073	–0.0448	–0.0090	–0.0652
	(0.0052)	(0.0389)	(0.0031)	(0.0238)	(0.0045)	(0.0351)
γ	—	—	—	—	0.3906	0.4280
	—	—	—	—	(0.3345)	(0.3303)
σ^2	1.62	1.623	1	1	2.48	2.53
Log likelihood	†0.020	†0.016	–0.9592	–0.9711	–0.7568	–0.7598
N	100	101	100	101	100	101
Small-Singer Wars (1960-1980)						
Constant	0.6489	0.6801	–0.3736	–0.2757	–0.2413	–0.2416
	(0.1610)	(0.1576)	(0.2749)	(0.2802)	(0.3291)	(0.3351)
Individual variation	–0.0037	–0.0329	–0.0085	–0.0825	–0.0115	–0.0884
	(0.0026)	(0.0191)	(0.0050)	(0.0406)	(0.0062)	(0.0484)
γ	—	—	—	—	–0.5128	–0.5928
	—	—	—	—	(0.5655)	(0.5975)
σ^2	0.81	.798	1	1	1.60	1.55
Log likelihood	†0.025	†0.029	–0.7944	–0.7833	–0.7568	–0.7473
N	100	101	100	101	100	101

NOTE: Standard errors appear in parentheses. † indicates R^2 instead of log likelihoods for the OLS models. The parameter γ is a reparametrization of σ^2 where $\sigma^2 = 1 + \exp(\gamma)$. It is used to prevent division by zero in the iterative solution to the maximum likelihood.

For many of the same reasons that OLS is inappropriate for dealing with event count data, using Pearson's *r* for inferential tests of noninterval, nonnormal data also is discouraged.

To demonstrate this problem as well as its remedy, I estimate the democracy-war relationship using first the OLS and Poisson models and then the negative binomial regression model. Table 2 presents the results for regressions of both the Butterworth and the Small-Singer war counts on each of the democracy scores. The first column reports the OLS estimates (without normalizing the variables—a procedure that has no theoretical justification) for comparison with Weede's (1984) figures and to underscore the poor fit of the OLS model in this context.[17]

The results indicate that democracies were less involved in war during the sample period, using both war counts and both measures of democracy. Most notable is the

17. Regressions of the Kende (1982) war measure are not reported for reasons of space and because the democracy-war correlations based on this war measure were uniformly weak, unlike those involving the Butterworth and Small-Singer measures. I can offer no reason for this but note that Weede (1984) also obtained the poorest model fit using the Kende war measure.

TABLE 3
Fitted Values: Bivariate Negative Binomial Model

	Expected War Count			*Expected War Count*	
POLDEM 1965	*Butterworth*	*Small-Singer*	*Freedom House 1973*	*Butterworth*	*Small-Singer*
0	1.84	0.79	2	1.55	0.66
20	1.53	0.62	4	1.36	0.55
55	1.10	0.42	7	1.12	0.42
85	0.84	0.30	12	0.81	0.27
100	0.73	0.25	14	0.71	0.23
Mean *SE*	(0.27)	(0.14)		(0.23)	(0.11)

substantial gain in precision of the coefficient estimates using the improved models, indicated by the standard errors. In both the Poisson and the negative binomial models, in fact, all coefficients on the democracy variables are statistically significant at the conventional .05 level for a one-sided test. That this should hold true for the Poisson indicates that the overdispersion may not be as serious a problem in these data as expected. The estimates of σ^2 are consistently around 2.5 for the Butterworth wars and around 1.5 for the Small-Singer wars. This indicates that the fundamental variability for the two war counts, or $V(Y_i)$, was roughly two-and-a-half and one-and-a-half times greater than $E(Y_i)$, respectively.

Table 3 directly interprets the substantive meaning of the coefficient estimates by using fitted values. It indicates the expected number of wars for a nation at different levels of democracy. The least democratic countries, as measured by POLDEM, fought Butterworth wars at an average rate of more than two times greater than nations completely democratic. As measured by the Small-Singer wars, this rate was three to four times greater. Very similar rates are obtained by comparing the average war involvement at different levels of the Freedom House democracy scores, lending additional confidence to these results. Regardless of the measures of democracy or war involvement one uses, the analysis indicates that fully free countries fought approximately one fewer of each type of war than nonfree states. Regime type means an average difference of one war for individual countries, with even greater implications for large groups of countries. This is a major repudiation of the previous consensus that democratic states are no less war prone than states with other regime types. The expectation of one less war over a 20-year period is both a substantively and statistically significant result.

Interpreting the variance of the predictions from this model is also substantively interesting. With the differences among nations in the sample, the numerous idiosyncrasies in factors leading to war involvement, and the fact that regime type is only one cause of war, we should expect substantial variation to underlie the average relationship predicted by the model. Figure 1 plots the negative binomial regression line, surrounded by the dashed line indicating one standard error, through the observations in each regression from Table 2. The total variance in expected rates of war involvement—taking account the fundamental variability represented by $\sigma^2\lambda$—is large rela-

TABLE 4
Negative Binomial Regression Models: Democracy and War Involvement with Control Variables—Small-Singer Wars (1960-1980)

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Constant	–5.0857	–3.9468	–5.6005	–5.1551	–4.1268	–5.6690
	(1.3901)	(1.7709)	(1.6992)	(1.3654)	(1.7820)	(1.7779)
Freedom House 1973	—	—	—	–0.0687	–0.0692	–0.0141
	—	—	—	(0.0458)	(0.0466)	(0.0533)
POLDEM 1965	–0.0078	–0.0073	0.0004	—	—	—
	(0.0058)	(0.0059)	(0.0063)	—	—	—
Log (population)	0.8343	0.5977	1.1967	0.8632	0.6616	1.1915
	(0.2749)	(0.3381)	(0.3402)	(0.2710)	(0.3387)	(0.3538)
Log (mil size pc)	1.0818	1.3099	2.4487	1.0346	1.2541	2.3556
	(0.5088)	(0.5147)	(0.5753)	(0.5089)	(0.5170)	(0.6056)
Econ interdepend	—	–1.1893	–0.0578	—	–1.0617	–0.0840
	—	(1.2096)	(1.1204)	—	(1.1998)	(1.1326)
Log (enconsmptpc)	—	—	–1.0806	—	—	–0.9919
	—	—	(0.3007)	—	—	(0.3079)
γ	–1.0130	–1.2420	–2.1556	–1.0848	–1.3152	–1.9389
	(0.7988)	(0.9572)	(1.8489)	(0.8391)	(1.0071)	(1.5254)
σ^2	1.36	1.29	1.12	1.34	1.27	1.14
Log likelihood	–0.6657	–0.6564	–0.5912	–0.6607	–0.6518	–0.5984
N	97	96	96	98	97	97

NOTE: Standard errors appear in parentheses. Population = population in 1,000s; mil size pc = military manpower per 1,000 working-age population; econ interdepend = (imports + exports)/GNP per capita in U.S. dollars; enconsmptpc = energy consumption per capita (kg. equivalents of coal). All logarithms are $\log_{10}$.

tive to the data observed, indicating that deviations often occur in the relationship between regime and war involvement. The estimates of the σ^2 parameter and the effects of its inclusion on the estimates of other parameters also show the leverage gained with the negative binomial model. The parameter σ^2 is greater than 1, as expected, although it is very close to 1 and not statistically significant (as determined by the standard errors of γ). Because the values of the dependent variable are numerically so close to 1 themselves, however, we should not expect to observe a large σ^2. Indeed, for the more numerous Butterworth wars, the σ^2 estimate is correspondingly larger. Relative to the Poisson results, both the coefficient magnitudes and their standard errors increased, indicating that when the stochastic nature of war events is better incorporated into the model, a greater focus on estimating the independent variable's precise impact ensues. Both theoretically, and in practice, there are compelling reasons to use the negative binomial over the Poisson or OLS models.

To test the robustness of this relationship, I added a number of control variables to these regressions to produce the estimates in Table 4. The other possible covariates of war, which I added as control variables, are the size of a country, the size of a country's military, its involvement and dependence on world trade, and a measure of its socioeconomic development. Size is measured by population and military size by

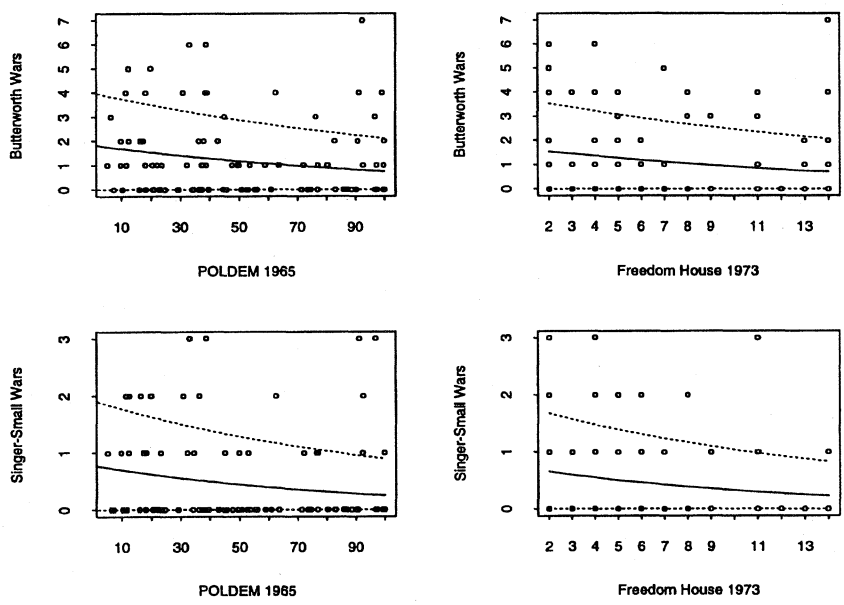

Figure 1: War Involvement as a Function of Regime Type
NOTE: Each point represents an observation. The solid line indicates the negative binomial regression line from Table 2. The dashed line bounds the region of one standard error.

active-duty personnel per capita. Economic interdependence is measured by adding imports and exports and dividing by per capita gross national product. Finally, energy consumption per capita is used as an indicator of socioeconomic development. Data for the control variables come from Charles Taylor's (1985) *World Handbook of Political and Social Indicators III, 1948-1982* for the year 1970, except for the measure of economic interdependence, which was taken from Arthur Banks's (1976) *Cross-National Time Series* data set. I used decimal logarithms to reduce skewness in all variables except economic interdependence.

Precise scales are provided in the text accompanying Table 4, although the purpose here is not to test alternate theories of war but merely to investigate the robustness of the democracy-war relationship. It is satisfying to note that the coefficients for the control variables behave generally as expected: larger countries and countries with larger militaries were more involved in more wars, and more economically interdependent countries fought fewer wars.

The results show that the democracy coefficients estimated in the bivariate models are indeed robust to adding control variables. Table 4 presents regressions of only the Small-Singer wars, but analysis of the Butterworth measures shows nearly identical results. Neither the estimates nor the standard errors change significantly when the control variables are added, with the exception of the energy consumption per capita

variable. When this variable is included (models 3 and 6), the coefficient estimates for democracy become much closer to zero. Additional tests (not shown) reveal that including gross domestic product per capita as a control has the same effect. Does this imply that development, and not democracy, is the cause of less war involvement?

Unfortunately, these data cannot definitively answer that question. Higher levels of development may lead to lower levels of war involvement, and democracy may be associated negatively with war only because it is correlated highly and positively with development. Yet there are a number of reasons why this may not be the case.

Statistically, the changes in the estimates and their standard errors when the development variable is added can be explained by examining the correlations among variables. Energy consumption per capita (logged) and democracy correlate 0.48 and 0.45 for POLDEM 1965 and Freedom House 1973, respectively. Furthermore, regressions of democracy on energy consumption (not shown here) reveal that the two show a large, positive, and highly significant relationship. This is hardly a surprise: Researchers have known for years that democracies tend to be more developed economically. A large and inconclusive literature, in fact, surrounds the linkages between development and democracy. The main finding from this research is that the two exhibit a mutually reinforcing relationship: development is a prerequisite for viable democracy, but democracy in turn may improve economic performance (Helliwell 1994).

Theoretically, although there are substantial reasons why regime type is hypothesized to affect war involvement, there is no similarly coherent theory as to why higher levels of development should lead to more pacific international behavior (it might in fact lead to just the opposite). Furthermore, the estimates of democracy's effect on war involvement by using different measurements and various other control variables have shown a remarkable stability and consistency. There is also a substantial theoretical basis for attributing the variation in conflict to democracy rather than development. Together these reasons point to the tentative conclusion that regime type, rather than development, better explains war involvement.

Finally, even if development does have a causal effect on war involvement, democracy exerts an indirect influence on conflict to the extent that it promotes development. At the least, regime type remains a viable *predictor* of war involvement in the data examined here. If the question of interest is whether democracies, on average, fight fewer wars, then the precise mechanism by which this pacific influence occurs may be of secondary concern.[18]

The questions raised in models (3) and (6) should be considered important grounds for future theoretical and empirical investigation. One means of further testing, however, is to consider the democracy-war involvement relationship from a different angle.

18. The source of consensus about the democratic peace proposition, for instance, is the overwhelming empirical observation that democracies seldom, if ever, go to war with each other. No similar consensus, however, exists for the reasons behind this peace. When it comes to conflict between democracies and other regimes in general, by contrast, even a consensus on what actually happened has yet to be established, much less the reasons why.

CONFIRMATION WITH CATEGORICAL MODELS

Weede's (1984) second type of analysis dichotomized the democracy and war measures and tabulated them to apply association tests. This is also the procedure followed by Chan (1984), who used dichotomous definitions of both variables in association tests. By replicating and improving these results as well as Weede's (implied) regression models, I demonstrate the consequences of dropping information by collapsing measures into too few categories. Although the data come from Weede and not from Chan, they replicate and improve the statistical tests used by the latter. Hence successful results from these models will repudiate Chan's findings as well if it can be demonstrated that the methodological choices in his study were responsible for his lack of a significant finding.

In the analysis that follows, I have used Weede's (1984) dichotomous measure of democracy for consistency[19] in addition to a new classification that ranks democracy according to three categories. This measure follows the Freedom House practice of ranking states as nonfree, partly free, and free. A state was considered nonfree if its POLDEM 1965 score was less than 40 and its Freedom House 1973 combined (inverted) score was less than 5. Partly free states ranged from 40 to 80 on POLDEM 1965 and from 5.5 to 9 on Freedom House 1973. Finally, free states required a POLDEM 1965 of at least 80 and Freedom House 1973 of at least 9. This resulted in 34 nonfree states, 43 partly free states, and 24 free states for the 1960 to 1980 period. Both war counts were collapsed into a three-category variable, indicating zero, one, or more than one war.

The Jonckheere-Terpstra test (Hollander and Wolfe 1973, 120-23) is a test for row and column independence in an $I \times J$ contingency table in which both the column and row categories are naturally ordered. It is more appropriate than the linear-by-linear association test that assumes interval-spaced scores and an underlying continuous distribution that is approximately bivariate normal (Agresti 1990). The values reported in Table 5 for the Jonckheere-Terpstra are one-sided asymptotic p values. This measure was used instead of Weede's (1984) likelihood ratio test because the latter assumes only nominal-level categories.

The Goodman-Kruskal gamma (γ) is a version of Yule's Q (used in Weede's Table 3) for $I \times J$ tables. The measure γ ranges from $-1 \leq \gamma \leq 1$ and tests ordinal association by comparing concordant and discordant category pairs. It has an asymptotic normal distribution, so dividing the estimates by their standard errors indicates one-tailed significance at the .05 level if the result exceeds 1.65. One-tailed tests, as with the regression models, are appropriate because theory and previous evidence indicate that democracy should exhibit a negative relationship with war involvement, and it is this unidirectional hypothesis that we wish to examine.

The results are strikingly different from the nonsignificant associations presented in Weede's (1984) analysis. When democracy is made into categories corresponding

19. A nation is coded as democratic if and only if its POLDEM scores exceed 80 for both the 1960 and 1965 measures and if the sum of its (uninverted) Freedom House political and civil rights scores in 1973 and 1979 are less than 5.5. This resulted in 22 democracies.

TABLE 5
Categorical Models: Democracy and War Associations

	Dichotomous Democracy	*Trichotomous Democracy*
Butterworth (1974-1980)	–.20	–.29
	(.22)	(.14)
	.1753	.0188
Small-Singer (1960-1980)	–.31	–.33
	(.26)	(.16)
	.1231	.0231

NOTE: First- and second-cell entries are Goodman and Kruskal gammas followed by the asymptotic standard errors in parentheses. The third cell is the asymptotic p value for a one-sided test of the Jonckheere-Terpstra test.

to nonfree, partly free, and free, the results indicate a significant association between this scale of democracy and both war measures. Nations tended to be less involved in war at higher levels of democracy, and these results hold for both the Butterworth and the Small-Singer war measures. In addition, the p values for the Jonckheere-Terpstra statistic indicate that when democracy is classified by three categories, the null hypothesis of row and column independence can be rejected at the .05 level.[20]

Table 5 underscores the substantial consequences of information loss when democracy is collapsed into two categories rather than three. In both categorical tests, this made the statistics considerably more inefficient—enough to reject each test for both war measures—even though the estimates themselves of the γ and Jonckheere-Terpstra statistics did not differ much between the dichotomous and trichotomous tabulations. Methodologically, this implies that analyses that collapse democracy, essentially a continuous concept, into binary categories may yield doubtful results. It may explain why previous studies such as Chan (1984) and Small and Singer (1976) found negative but statistically insignificant associations between democracy and conflict. The information available from the Bollen (1980) and Freedom House (Taylor 1985) indexes is sufficiently rich to withstand collapsing into three categories without imparting a false degree of precision, and using this information in the analysis makes a difference. Substantively, the results indicate a difference in rates of war involvement between regimes that are nonfree and regimes that are partly free, because most of the expansion into three classifications came from further dividing the "nondemocracy" category. This differential relationship between states at lower levels of freedom suggests interesting possibilities for future research in the foreign policy behavior of partly authoritarian regimes.

20. Note that the sparseness of some cells may indicate that the asymptotic p values are slightly inaccurate. I also obtained p values for the Jonckheere-Terpstra using exact tests or, for the Butterworth 3×3 table, Monte Carlo simulations by using the software StatXact. In no instance did the exact p values differ significantly from the asymptotic results.

CONCLUSION

The reanalysis of regime type and conflict has demonstrated that democracies were significantly less likely, on average, to be involved in international wars during the 1960s and 1970s than less-free states. This relationship is remarkably robust and stable when estimated using different combinations of the Butterworth, Small-Singer, POLDEM, and Freedom House indicators of war and democracy. The addition of control variables, on the whole, did not change the estimates of the democracy-war relationship. Furthermore, reorganizing the same data to test the relationship using categorical models also confirms a significant and negative association between the level of democracy and higher levels of levels of war involvement. The conclusion from these analyses is that regime type does indeed explain variation in international conflict, and the conventional wisdom regarding the absence of monadic-level democratic pacifism deserves to be reconsidered.

In addition to the substantive result arising from the event count model in this study, the discussion and implementation of the model itself should be of interest to analysts studying discrete international phenomena. Understanding the stochastic process giving rise to the data we observe is crucial to providing realistic and meaningful estimates of the causal processes we wish to examine. The statistical theory employed in this study is not new, but its application provides, I hope, an encouraging example of how more appropriate models may be used to test propositions in international relations.

Specifically, it should impel researchers examining (or reexamining) the democratic peace to pay more attention to their statistical tests. The complications inherent in the quantitative study of international politics may call for mathematically complicated models. In other words, our quantitative analyses should rise to the occasion instead of reverting to the simplest statistical tests as an answer to the challenges of international studies data. The fact that most previous analyses have found coefficients supporting democratic pacifism but a lack of statistical significance is good evidence that further progress will require improving our models, no matter how good our data sets become. Studies that base their conclusions on tests from linear models or binary classifications of democracy simply ignore too much important information about the nature of regime types and war events. The findings most often cited as proof that there is no link between regime type and conflict proneness generally have suffered both shortcomings. Consequently, the absence of a finding that democracies fight fewer wars is probably less attributable to a lack of real patterns in the foreign policies of different regime types than it is to the result of poor model choice.

Several caveats also emerge from this research. First, the findings indicate only that democracies fought fewer wars *on average* than less-free regimes during the period observed. Rates of war involvement vary substantially among nations, even when regime type is taken into account. This variance is to be expected, however, in any inquiry into an issue so resistant to steadfast generalization as the causes of war. We should be humble in our expectations from the data in our search to test either the democratic pacifism or democratic peace propositions, because war events combine

two banes of data analysts: idiosyncrasy and infrequency. Our models should incorporate and, whenever possible, estimate the randomness in the data while identifying the underlying pattern we seek. Few scholars believe in invariant determinism with regard to the causes of war. So then why do so many of our models in this area reflect such a belief? Whether democracies go to war less on average is a simple question applied to a very complex issue. This is all the more reason, if we insist that quantitative analysis offers a means to settle the question, that we address the issue with models whose complexity matches that of the underlying processes rather than the deliberate simplicity of the research hypotheses we apply to those issues.

The results of this study point also to some specific topics for future research. The first is the relationship between democracy and war involvement across different time periods, using theoretically guided models and data that contain sufficient information about regime type. Research of this type is now feasible with the availability of time-series, cross-national data sets containing detailed information about the characteristics of political regimes. Future theoretical research also should devote more attention to the reasons *why* democracies go to war less, including the relationship between democracy and closely correlated indicators such as socioeconomic development. If development rather than democratization is the stronger engine for peace, then some foreign policy priorities may warrant reassessment. Another area deals with the difference in war rates between regimes at low levels of democracy. The categorical models in this article indicate that nondemocratic regimes fight fewer wars than regimes at middling levels of democracy, an interesting, nonlinear pattern that deserves future exploration. The question of whether democratic regimes are more pacific is, after all, simply a rephrasing of the proposition that authoritarian regimes are more warlike. Extensions of this research may well find interesting linkages between regime type and international conflict by focusing on authoritarian rather than on democratic political characteristics.

Finally, a note of caution should be placed on any conclusions about the foreign policy behavior of democracies when democracies comprise only 20% to 30% of the states in the international system. The behavior of states in the international system is intrinsically contextual. From 1960 to 1980, nondemocracies outnumbered democracies nearly two to one. A real test of the proposition that a more democratic world is a more peaceful world would require an examination of conflict proneness when the international system contains a majority of democracies. Until historical developments bring about such conditions, however, research of the type presented here offers a first step.

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