

# Resource Unpredictability, Mistrust, and War

## A CROSS-CULTURAL STUDY

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The results of this cross-cultural study suggest that war may be caused mostly by a fear of nature and a partially resultant fear of others. A history of unpredictable natural disasters strongly predicts more war, as does socialization for mistrust (but less strongly). It seems that people, particularly in nonstate societies, may try to protect themselves against future disasters by going to war to take resources from enemies.

War is a nearly universal fact of life in the ethnographic (anthropological) record, judging by our examination of 186 mostly preindustrial societies. The vast majority of those societies had warfare when they were first described, unless they had been pacified. To discover the predictors of war, therefore, we cannot compare societies with and without war to see how else they might differ. But we can ask why warfare varies in frequency from one society to another. Answers to this question might suggest why people go to war in the first place.

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In the cross-cultural study described here, our main focus is on the possibility that ecological conditions may at least partially explain variation in the frequency of war. In addition, we also tested a number of other (social and psychological) theories.[1] Multivariate analysis suggests that one kind of ecological problem—a history of unpredictable natural disasters—strongly predicts higher frequencies of war. By unpredictable natural disasters we mean events that destroy food resources, such as droughts, floods, storms, killing frosts, and locust infestations. Multivariate analysis also suggests another independent, but weaker, predictor of higher warfare frequencies, namely, socialization for mistrust. We suggest that both of these factors create fear—fear of nature and fear of others—which may lead people (particularly in nonstate societies) to try to protect themselves against future unpredictable disasters by going to war to take resources from enemies. We conclude by discussing implications of the results for state-level and industrialized societies.

## THEORY AND PREVIOUS RESEARCH

### RESOURCE SCARCITY AND WAR

The idea that a shortage of resources or an excess of population lead to war is not new, but heretofore it has not been tested systematically on a worldwide sample of societies (cf. Singer 1980). In anthropology, the possible link between war and resource problems has been discussed mostly by ecological anthropologists such as Vayda (1967, 1976; but see 1989) and Rappaport (1967); see also Harris (1974, 61-107; 1984) and Gross (1975). All of the above think that war may be a functional or adaptive solution to resource problems, but not necessarily in all types of societies (for example, not in the postnuclear world). Pressure on resources is viewed as increasing the likelihood of competition within and between political units (Vayda 1967, 88; Rappaport 1967, 114ff.). War may also play a role in regulating inequities in access to resources (animals, land, and people); when people do not have enough of a resource, they take it from those who have (Vayda 1967, 86-87).

1. See C.R. Ember and N. Ember (n.d.-c) for the bivariate test results. We tested all the theories we knew that strongly imply why a society might have more rather than less war. So, for example, we did not test "fraternal interest group" theory (Otterbein and Otterbein 1965) because we failed to see how it would predict more versus less warfare in general. That theory deals with only one type of warfare, that is, internal or within-the-society (language group) war; but warfare could also be external, that is, between different societies or parts thereof. In another paper (C.R. Ember, M. Ember, and Russett 1992), we present results on predictors of variation in the frequency of internal war.

Warfare is viewed as largely adaptive, not only because some people get resources from others, but also because high mortality during warfare may reduce population pressure on resources. (Durham 1976 phrases the adaptational argument in terms of individual rather than group selection.)

Vayda (1967) and Rappaport (1967) discuss population pressure largely in the episodic sense of a population exceeding its carrying capacity only at times. Thus, if warfare reduces pressure on resources, the resource scarcity that triggers warfare should not be chronic. This suggests that we need to distinguish between periodic (or aperiodic) resource scarcity and chronic resource scarcity. Moreover, if warfare is a response to resource scarcity, there is still the question of when people decide to go to war—during, after, or before scarcity. Finally, if warfare is indeed a response to scarcity, we should find that the victors generally take land or other resources from the defeated.

Another line of reasoning leads to the prediction that chronic scarcity by itself might increase the likelihood of war. If chronic scarcity induces a chronically high level of irritability, it may in turn increase the likelihood of all kinds of aggression, including warfare (LeVine [1961] 1980; Bolton 1973; Bolton and Vadheim 1973).

In addition to these theoretical reasons for expecting warfare to be linked to resource problems (in one or more senses), there are some empirical reasons to expect such a linkage. One is M. Ember's (1982) statistical analysis of Sillitoe's (1977) comparative data on 26 New Guinea societies. Contrary to what Sillitoe had concluded from an "eyeball" inspection of the data, M. Ember's statistical analysis reveals that warfare for land is very strongly and significantly related to land shortage (as indicated by population density) in New Guinea. (We should note that we do not think that population density is a good worldwide measure of population pressure, because we do not believe that denser populations are generally more likely to suffer from population pressure. Only in an area like New Guinea, where the food-getting technology used to be relatively uniform, might population density be a valid measure of degree of population pressure.) M. Ember (1982) also described a worldwide test of the possible relationship between severity of food shortages (data from C. R. Ember 1978) and frequency of warfare. Even though the test sample was small, the relationship was statistically significant: Societies with severe food shortages did tend to have more frequent warfare.

## THE PSYCHOLOGY OF WAR

The idea that psychological factors may lead to war is also not new. The general theory here is that personality characteristics presumably produced by certain childrearing customs may result in more aggressiveness in general and more warfare in particular. The prevailing theoretical expectation in the cross-cultural research literature is that *frustrating* socialization may increase the likelihood and frequency of violence and war (Russell 1972; Eckhardt 1973; Prescott 1975; Ross 1986): If infants and children are subject to frustration and they are therefore likely to exhibit aggression, then they may be disposed, when they grow up, to react similarly to frustration.

On the basis of a factor analysis of various socialization practices and how they relate to various measures of conflict and violence, Ross (1986) distinguishes two types of frustrating socialization—punishment and low need satisfaction. In our own analysis of many of the same codes for socialization that Ross looked at, we find little or no evidence for a possible relationship between punishing socialization and more war. But there is some evidence for relationships between indicators of low need satisfaction and more war; the indicator most strongly predicting more war is socialization for mistrust. (For details of the various bivariate test results, see C.R. Ember and M. Ember n.d.-c.)

Theoretically, people who grow up to be mistrustful of others, and who therefore fear others, may be more likely to go to war than to negotiate or seek conciliation with "enemies" (Jervis 1976).[2] As we shall see later, there is some evidence suggesting that fear of others may be at least partially a result of fear of resource scarcity; if people have a history of resource problems, their fear of scarcity may spill over into fear of others. In any case, mistrustful adults may be more likely to respond aggressively to the arousal of any fears, and therefore socialization for mistrust may lead to more war.

Finally, there is the idea that socialization for aggression may lead to more war. Our results do not support this idea. Rather, the evidence strongly suggests that socialization for aggression is more likely to be a consequence than a cause of war. The major finding consistent with this scenario is that socialization for aggression seems to decrease after warfare ceases because of pacification (see C.R. Ember and M. Ember n.d.-c).

2. For how mistrust might be attributed to others by the psychological defense mechanism of projection, see Erikson (1963, 248-49). If mistrust is attributed to others, fearful people might not believe that others could be sympathetic and helpful; therefore mistrustful people might be more likely to fight than negotiate in the face of, or in anticipation of, a crisis (see Jervis 1976, 44-45, 120, 347, 355). For an instance of mistrust possibly being a factor in deciding to go to war, see Lebow's (1981, 142) discussion of Kaiser Wilhelm's apparent state of mind on July 30, 1914.

## DESIGN OF THE RESEARCH

### RESEARCH STRATEGY

Cross-cultural research differs from cross-national research in a number of ways. First, there is a different unit of analysis. Cross-national research compares countries, each of which is politically unified, at least in some formal respects. Cross-cultural research compares societies, each of which is a population that more or less contiguously inhabits a geographic area and speaks a language not normally understood by people in neighboring societies. Thus, in the modern world there are many states containing more than one ethnic group or society in the anthropological sense. Nigeria is one such; it contains scores of societies or language groups (e.g., Hausa, Ibo, Yoruba). Yugoslavia and the Soviet Union were other examples. A second difference between cross-cultural and cross-national research is implied by the first. A cross-cultural comparison involves many types of societies that vary considerably in degree of political complexity. One aspect of that variation is the fact that about 50% of the societies known to anthropology (as of the times they were first described) had no political organization beyond the local community (percentage calculated from Murdock 1967); indeed, most of these societies had no full-time political officials at all (unpublished data from M. Ember). A third difference between cross-national and cross-cultural research is that nations generally have historical documents covering lengthy periods of time, whereas societies known to anthropology generally have extensive descriptions (ethnographic materials) for only one or a few points in time.

The fundamental assumption of a cross-cultural study is that if a theory or hypothesis has merit, the presumed causes and effect should be significantly and strongly associated synchronically (Whiting 1954). A synchronic association involves variables that are measured for each sample case for more or less the same time period, as if we were examining ethnographic "snapshots," each capturing a culture as of a particular time (and usually a particular locality). Regardless of the different time foci for the sample cases, a significant result should be obtained if there is a systematic relationship between or among the measured variables. Thus the cross-cultural research strategy provides a way to falsify hypotheses that have no predictive, and hence presumably no causal, value.

### SAMPLE

The sample for the study (Murdock and White 1969) includes 186 societies that are presumed to represent the known and well described

cultural "provinces" in the ethnographic record. (The sample usually includes one culture per province, a geographic area of similar, usually historically related, cultures.) We used the Murdock and White sample because of the availability of previous investigators' published codes for various aspects of socialization in the sample cases.

## CODING PROCEDURE

We instructed our coders to rate a case for a 25-year time period around the pinpointed "ethnographic present" listed in Murdock and White (1969), that is, from 15 years before to 10 years after the ethnographic present. The coders were also instructed to rate a case for its place focus (village, locality) if one was indicated in Murdock and White (1969). However, if a politically unified society wages war on behalf of the whole society against another society, the coders were instructed to rate warfare frequency for the society as a whole. As of the times of description specified by Murdock and White (1969), which usually vary from some time in the 19th century to some time in the 20th, few of the sample societies were nation-states. Now, of course, the vast majority of the surviving cases have been incorporated into larger nation-states.

Four other points about the coding procedure should be mentioned. First, one set of coders rated the dependent variables (about warfare and aggression) and another set rated the hypothesized independent variables. Second, at least two assistants independently coded each variable; we used a third coder when the first two could not resolve their disagreement. Third, the coders did not know the hypotheses to be tested. Fourth, although we originally intended to use resolved ratings if two coders disagreed, we found that the more coders disagreed initially, the weaker the results. We suspect this is because more disagreement reflects more ambiguity in the ethnographic information and hence more random error in the resolved ratings. To minimize random error in the measurements, therefore, we do not generally use a resolved rating if the initial ratings are not the same or close.[3]

3. Operationally, when we say that the initial ratings of warfare frequency (by two or occasionally three different coders) were close, we are referring to one of three situations. First, the initial ratings did not disagree by more than 1 point on a 5-point ordinal scale. Second, if the initial ratings disagreed by more than 1 point, they did not straddle the boundary between low and high frequency of war; the boundary for us, which was predictive of various things in past studies (M. Ember and C.R. Ember 1971; C.R. Ember 1975, 1978), is warfare at least once every 2 years (high) versus less often (low). And third, one of the first two coders said "don't know" and the third coder's rating was close (as defined above) to the other initial coder's numerical rating. For the coding of resource problems, which were measured on 4-point scales, close ratings are essentially the same as for warfare, with the following changes. First, the boundary was between 1 (no problem) and 2 or more (some problem or more serious problems). Second,

Most of the information needed to code warfare and resource problems was retrieved from the indexed collection of ethnographic texts known as the Human Relations Area Files (HRAF). For the coded data on resource scarcity and other supposedly independent variables, see C.R. Ember and M. Ember (n.d.-a); for the coded data on warfare frequency and other supposedly dependent variables, see C.R. Ember and M. Ember (n.d.-b).

## DEFINITION AND MEASUREMENT OF WARFARE FREQUENCY

Just as the political organization of most societies known to anthropology is very different from that of modern nation-states, so is their warfare very different in organization and scope. Most societies (before pacification) had frequent armed combat between communities or larger units, but they lacked specialized fighting forces and formal military leaders (Otterbein 1989). Even though armed combat may often have been conducted against members of other societies, it was usually not organized on behalf of the entire society or even a major section thereof.[4] That is, warfare in the ethnographic record did not usually involve politically unified societies.

Because of these differences between modern international warfare and the warfare of preindustrial societies, it is necessary to define warfare somewhat differently for cross-cultural (as contrasted with cross-national) research. So, for the purpose of this study, we define warfare as socially organized armed combat between members of different territorial units (communities or aggregates of communities); see the Appendix for how the coders rated warfare frequency according to a 5-point ordinal scale ranging from less often than once in 10 years to "constant" or occurring at any time of the year.

Unless we note otherwise, *pacified* societies and societies that have a clearly diminished warfare frequency because of pacification attempts are excluded from the analyses. To test theories about warfare frequency, we want to distinguish truly more peaceful societies from societies that had had

because we think the boundary here may be more important than the difference between ratings of 2 and 3 or between 3 and 4, we decided that if two coders disagreed by only 1 point, but the different ratings were on opposite sides of the boundary, we did not consider the ratings close.

4. Episodes of warfare in many ethnographically described societies result in relatively few individuals killed. Thus, in comparison with international war, conventionally defined as 1,000 deaths, the ethnographic record often exhibits a very low threshold of warfare. However, just because the absolute numbers of people killed may not be large does not mean that the warfare in simpler societies is inconsequential. In the few cases where we have detailed information on the number of people killed over time, it seems that "primitive" warfare might have been even more lethal *proportionately* than modern warfare. For example, the Mae Enga of the New Guinea highlands lost about 25% of their males because of intervillage warfare, and other societies in New Guinea and Amazonia lost between 19.5% and 30% (Meggitt 1977, 201).

peace imposed on them, completely or not so completely, by colonial powers. Partly or completely pacified societies might still have conditions predisposing to warfare, which might obscure the results.

### MEASURING RESOURCE PROBLEMS AND OUTCOMES OF WAR

We constructed three measures of resource problems, each of which is a 4-point ordinal scale. Two of the measures tap serious but unpredictable fluctuations (over 25 years) in the supply of food: threat of famines and threat of natural (weather or pest) disasters that destroy food supplies. Our third measure reflects the degree of chronic or regularly recurring scarcity. (See the Appendix for details of these scales.)

The statistic (Spearman's rho) we used to test for bivariate associations assumes that the two sets of ranks are linearly related. With two of our measures of resource problems (famines and natural disasters), this assumption is not warranted. Rather, it appears that the scales for famines and disasters both show the same kind of threshold effect: Warfare frequency is likely to be high if there is *any* level of threat of famines or disasters, and this is particularly so in politically simpler societies. Because of this apparent threshold effect, the multivariate analyses treat both the famine and disaster scales as dummy variables, contrasting no problem (scale score of 1) versus threat or actual occurrence of problems (scale scores of 2 through 4) during the 25-year measurement period.

We have two scales to measure the taking of resources as an outcome of war; the first deals with land, the second with nonland resources such as animals, food, and captives. (See the Appendix for details of these scales.)

## RESULTS

### BIVARIATE TESTS

The results of the bivariate tests dictated which particular predictors we would use in the multivariate analyses described in the next section. The relevant bivariate results were as follows.

The dichotomized scale for threat of natural disasters significantly predicts warfare frequency; the result is particularly striking in nonstate societies (rho = .71).[5] The other measure of nonchronic resource problems, the dichoto-

5. Nonstate societies are those coded by Murdock and Provost (1973) as other than 3 or 4 on their Scale 9; in such cases the local community is politically autonomous or there is just one level of administration above the community.

mized measure of threat of famines, is significant only in nonstate societies (rho = .39).

The threat of natural disasters and the threat of famines both tap nonchronic resource problems, and indeed they are moderately correlated. So why doesn't the famine measure predict as strongly as the disaster measure? We believe the answer may lie in the fact that the famine measure picks up only extremely serious resource problems. Disasters occur more often than famines, and therefore a society may have had a disaster during the 25-year test period, but no famine.

What about chronic scarcity? It is significantly, but not strongly, related to warfare frequency in nonstate societies (rho = .35, $n = 38$, $p < .05$, one tail); but like the famine measure it is not significantly related to warfare frequency in the total sample. Multiple regression analysis suggests that chronic scarcity has no independent effect on warfare frequency;[6] but threat of natural disasters does (as we shall see in the next section).

Thus, the results described so far suggest that resource problems, particularly nonchronic resource problems created by natural disasters, predict more war. On the surface, these findings seem consistent with the Vayda (1967) and Rappaport (1967) model previously discussed. But societies with only the *threat* (not the actuality) of scarcity fight almost constantly. This finding, in conjunction with the finding that chronic scarcity does not independently predict war, suggests to us that *fear* of unpredictable scarcity may be more of a motive to go to war than known or expected scarcity.

What about the related hypothesis that people may go to war in an effort to protect themselves against unpredictable scarcity by taking resources from the defeated? Consistent with the idea that warfare has economic consequences, if not motivations, the victors in war sometimes drive the defeated from their land (and at least sometimes use such land) in 73% of the rated cases ($n = 49$) that have warfare at least once every 2 years; and in 90% of the cases rated with regard to other outcomes of war ($n = 62$), the victors usually take nonland resources. In short, it seems that people who fight frequently almost always take some kind of resources (if they win) from their enemies. And this is true *even in nonstate* societies: The victors in 77% of those cases ($n = 30$), with warfare at least once every 2 years, drive the defeated from their land at least some times; and in 85% of the cases rated with regard to other outcomes of war ($n = 39$), the victors usually take nonland resources.

6. When we regressed threat of natural disasters and chronic scarcity on frequency of war in nonpacified societies, the resultant standardized coefficient for chronic scarcity is close to zero (–.07), whereas the standardized coefficient for threat of natural disasters is .61. In nonstate societies, the standardized coefficient for chronic scarcity becomes slightly more negative (–.11); it is not significant ($p = .536$) and the effect if anything is opposite to the effect of natural disasters (standardized coefficient = .87).

We tested one other ecological interpretation of warfare, namely, the idea that in simpler (band and village) societies women may be a scarce resource because of female infanticide, and therefore men may go to war to obtain access to more women than they might otherwise have access to (Divale and Harris 1976; see also Harris 1984). Female infanticide is supposedly practiced in order to maximize the rearing of fighting men, but note that this explanation of warfare is circular: it begins by assuming that war is frequent; it presumes therefore that people favor males and hence practice female infanticide; finally, because females are in short supply, it presumes that men go to war to get more women.

We can test one link in this argument, that warfare should be more frequent if women are in short supply. Divale and Harris (1976) reported that societies with warfare, in contrast to those that were previously pacified, on the average had sex ratios favoring males among children 14 or younger. But age-specific sex ratios are very suspect in the absence of clear birthdates; females in many societies marry shortly after puberty and may therefore be classified (by the anthropologist or other foreign observer) as older than they are chronologically (Fjellman 1979, 194).[7] To eliminate this possible bias, we use the overall male/female sex ratio. If Divale and Harris (1976) are right that young ages are very skewed in favor of boys in warring societies, whereas the adult sex ratio is more even because of male deaths in war, the overall sex ratio should still favor males in warring societies. To test this expectation, we compare the sex ratios of three groups of societies whose autonomous political units are no larger than a local community such as a band or village:[8] Those that are pacified or have no or rare warfare (less than once every 10 years); those that have little warfare (frequency less than every 2 years but more than every 10); and those that have warfare frequencies greater than once every 2 years. Because sex ratios are not reported that frequently, and because a fine-grained rating of warfare frequency is not necessary for this test, we relaxed the criterion that the ratings of war frequency by the different coders had to be close to be usable. Instead, to maximize the *n*s in each compared group, we use all the resolved ratings (i.e., all the ratings that, even if very different to begin with, were resolved by the coders after discussion).

Based on the sex-ratio information we were able to retrieve, our data do not appear to support the Divale and Harris (1976) argument; if anything,

7. Because of various methodological problems in the Divale and Harris (1976) study, there are other reasons to doubt their finding that unpacified societies have a higher sex ratio (more males than females) than societies pacified 26 or more years before. See M. Ember (1985) for further discussion and references to other critiques of the Divale and Harris (1976) methodology.

8. The cases coded by Murdock and Provost (1973) as 0 or 1 on Scale 9: Level of Political Integration.

there is a nonsignificant tendency (Kruskal-Wallis nonparametric analysis of variance, $p = .49$) in the direction *opposite* to what Divale and Harris would predict. The highest sex ratios (i.e., proportionately the most males) occur in the pacified or rare warfare cases; the middle or low warfare group is intermediate; the frequent warfare group has the lowest sex ratios.[9] It does not appear that warfare in politically simpler societies could be a response to a shortage of women.

Finally, the bivariate test results are consistent with previous findings (Russell 1972; Eckhardt 1973; Prescott 1975; Ross 1986) suggesting a link between war and low need satisfaction in socialization. Of 13 variables presumably reflecting such socialization, 5 are significant and moderate predictors of more war (rhos range from .31 to .37). The strongest of them, as mentioned above, is socialization for mistrust (data from Barry et al. 1976; rho = .37; $n = 65$); and the prediction is stronger (rho = .47) in nonstate societies.[10]

## MULTIVARIATE ANALYSES

Which of the bivariate predictors of more war predict independently? Which predict strongest? And how strongly do the predictors work together? To answer these questions, we computed a series of multiple regression analyses. Although all of the measures are ordinal, we choose to treat them as interval measures in the analyses to be described below. Treating them as interval measures allows us to use more powerful and versatile multivariate techniques (see Labovitz [1967; 1970, 388] for why this strategy can be considered justifiable). In particular, we use multiple regression to evaluate the additive and independent effects of the significant predictors that survived the bivariate tests.

In a multiple regression analysis it is preferable to use all theoretically relevant variables, regardless of whether they were significant predictors in the bivariate tests. However, that strategy is not possible in the typical cross-cultural study, because information is often missing for most of the variables measured. Accordingly, we adopted the following two-pronged strategy: (1) the multiple regression analyses include only those variables that are plausible causal factors and that are significant predictors of more

9. A $t$ test comparing the two extreme groups (pacified and rare warfare cases compared with high warfare cases) is similarly nonsignificant ($p = .533$), and the direction of difference between the means is also opposite to what the Divale and Harris (1976) argument would imply.

10. Another type of presumably frustrating socialization—sexual restrictiveness—does not appear to predict more war. Societies that restrict their children sexually (codes from Barry et al. 1976) do not appear more warlike, nor does premarital sexual restrictiveness (codes from Broude and Green 1976) predict more war.

war in the bivariate tests; and (2) because we sometimes have two or more predictors that are presumably alternative measures of the same theoretical variable, we select the strongest significant predictor (of warfare frequency) in each set to use in the multiple regression analyses.

In the multiple regression analyses, we omit socialization for aggression in boys because, as previously discussed, there are empirical reasons to think that it is more likely to be a consequence than a cause of war. Accordingly, the analyses include just the threat of natural disasters scale (as the measure of unpredictable scarcity) and socialization for mistrust (as the measure of low need satisfaction in socialization). As mentioned above, we treat the disasters scale as a dummy variable because almost all of the cases coded 2, 3, and 4 on the scale have very high warfare frequencies.

Column 1 of Table 1 displays the multiple regression analysis for all the sample cases. Threat of natural disasters and socialization for mistrust are both significant independent predictors of warfare frequency, yielding a multiple $R$ of .71. Threat of natural disasters is the stronger predictor (the standardized coefficient is .591, $p < .001$, one tail; the standardized coefficient for socialization for mistrust is .296, $p < .025$, one tail). Column 2 of Table 1 shows the multiple regression analysis for nonstate societies; in this analysis the multiple $R$ is .82 (compared with .71, not controlling on political complexity). The Systat program (Wilkinson 1988) indicates that there is one outlier in this second analysis.[11] It is important to recompute a multiple regression analysis, omitting any outliers, to see if the outliers might be responsible for the significance of the original result. In the present case (see column 3 of Table 1), omitting the outlier strengthens the overall result (the multiple $R$ is now .88); and the effect of threat of natural disasters is stronger than before (the standardized coefficient is now .778, $p < .001$, one tail), whereas the effect of socialization for mistrust is weaker than before (the standardized coefficient is now .216, $p = .05$, one tail).

Thus multiple regression analysis suggests that threat of disasters and socialization for mistrust are independent and significant predictors of warfare frequency. Of course, the analysis cannot by itself tell us that the two predictors are causes of warfare. But in the case of threat of natural disasters, we can be fairly sure that warfare cannot cause natural disasters, which leaves the reverse causality much more likely. Therefore, the strong relationship between threat of natural disasters and war is strong evidence (but of course not proof) that such threat is a cause of war.

What about socialization for mistrust? The fact that it predicts war does not, of course, show that it is a cause of war. But there are three empirical

11. The outlier is the Warrau of the Orinoco delta. We plan to discuss the Warrau and other cases in the sample in a future publication.

TABLE 1
Predictors of Warfare Frequency (standardized coefficients)

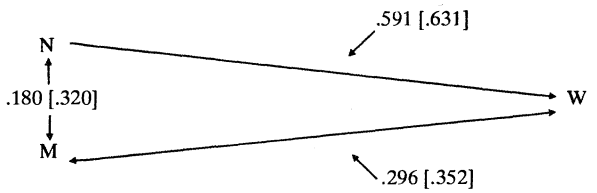
| | *1* *Overall* | *2* *In Nonstate Societies* | *3* *Column 2 Omitting Outlier* |
|---|---|---|---|
| Constant | .000 | .000 | .000 |
| Natural disasters | .591*** | .631*** | .778*** |
| Mistrust | .296** | .352** | .216* |
| *N* | 30 | 20 | 19 |
| *R* | .71 | .82 | .88 |
| $R^2$ | .50 | .67 | .78 |

*$p < .05$, one tail; **$p < .025$, one tail; ***$p < .001$, one tail.

reasons for suspecting that socialization for mistrust is more likely a cause than a consequence of war. First, the correlation between mistrust and warfare frequency goes down when we put the pacified societies back into the computation. This is consistent with the idea that socialization for mistrust is a cause of warfare; if it is, the inclusion of the pacified cases should add "exceptions" to the sample and therefore reduce the correlations. There would be "exceptions" because some of the cases high on mistrust would be low on warfare frequency for an extraneous reason (i.e., pacification), which is exactly what we find. A second empirical reason for suspecting that socialization for mistrust is more likely a cause of war, rather than a consequence, is that if we compare the pacified and nonpacified societies in our sample, there is no significant difference between them on socialization for mistrust. If there had been a significant difference, we might suspect that socialization for mistrust is a consequence of warfare; that is, the pacified societies would have changed their socialization because they no longer needed to produce mistrustful adults. But that is not what we find.

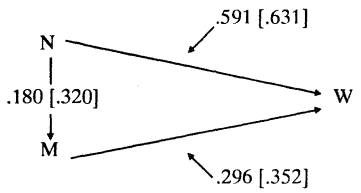
A third empirical reason to suspect that mistrust is a cause of war is suggested by path analysis, which allows us possibly to discriminate between mistrust as a cause and mistrust as a consequence of warfare. Assuming that threat of natural disasters has to come first in the causal sequence, and assuming that such threat is likely to be a stronger cause than mistrust (which is suggested by the standardized coefficients in the multiple regression analysis), there are three possible causal models we can examine by path analysis (see Figure 1). The first two models have mistrust as a cause of warfare in addition to threat of natural disasters. Model 1 suggests that both natural disasters and mistrust (which are assumed to be causally unrelated) may directly increase warfare frequency. Model 2 suggests that disasters may

Model 1



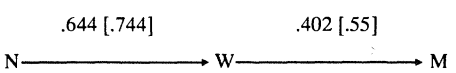

| Predicted *r* | Actual *r* | Discrepancy |
|---|---|---|
| NW = .591 + (.180)(.296) = .644 [.744] | .644 [.744] | 0.00 [0.00] |
| NM = .180 [.320] | .180 [.320] | 0.00 [0.00] |
| MW = .296 + (.180)(.591) = .402 [.554] | .402 [.554] | 0.00 [0.00] |

Model 2




The predicted and actual *r*s (and therefore the total discrepancy) are the same as for Model 1.

Model 3




| Predicted *r* | Actual *r* | Discrepancy |
|---|---|---|
| NW = .644 [.744] | .644 [.744] | 0.00 [0.00] |
| WM = .402 [.555] | .402 [.555] | 0.00 [0.00] |
| NM = (.644)(.402) = .259 [.412] | .180 [.320] | 0.08 [0.09] |

**Figure 1: A Comparison of Three Causal Models of War**
NOTE: Each diagram shows the path coefficients; the ones in brackets apply to nonstate societies. N = threat of natural disasters; M = socialization for mistrust; W = frequency of war. The predicted and actual *r*s are Pearson's.

directly *and* indirectly motivate people to go to war (indirectly because threat of natural disasters at least partially causes mistrust). Model 3 suggests that threat of natural disasters is a cause of war and mistrust is a consequence of war.

The conventional way to evaluate alternative causal models by path analysis is to compare the predicted and actual correlations for each model. The larger the total discrepancy, the less satisfactory the model. Both of the

models that have mistrust as a cause of war have no discrepancy; the model that has mistrust as a consequence of war has some discrepancy between the predicted and actual correlations (.08 overall, .09 for nonstate). Thus path analysis (in conjunction with the two reasons discussed above) suggests that mistrust is more likely to be a cause than a consequence of warfare. But we cannot tell, on the basis of the path analysis, whether threat of natural disasters may have an indirect (through mistrust) as well as a direct effect on warfare frequency.

In sum, the multivariate analyses described in this section suggest that there are at least two significant and independent predictors (we think possibly causes) of warfare – threat of natural disasters and socialization for mistrust. In the next section, we discuss how we think these findings can be explained theoretically, and why the results are stronger for nonstate societies.

## CONCLUSIONS AND IMPLICATIONS

Fear appears to be a common thread in the two obtained predictors of war – fear of nature and fear of others. Fear of others (as indicated by socialization for mistrust) is fairly obvious, but what about fear of nature? We think it is appropriate to infer fear of nature from our measure of threat of natural disasters because the second point on the scale (after "little or no problem") is literally "threat of disasters." Our assistants were instructed to code "threat" when the ethnographer reports that the people are always faced with the possibility of a disaster, but no actual disaster occurred within the 25-year measurement period. Higher scores on the scale refer to one actual disaster (scale score 3) or more than one (scale score 4) within the time period. Because ethnographers rarely stay longer than a few years, the people themselves must have conveyed their worry about disasters to the ethnographers; otherwise the coders would not have used the "threat of disasters" scale score as often as they did.

Further, the various findings about how resource problems relate to warfare strongly suggest that fear of future economic problems (rather than current problems) is the major motive for going to war. Recall that these findings include (1) the relationship between the scale of threat of disasters and the scale of warfare frequency is not linear – scale score 2 ("threat of disasters") appears to predict warfare frequency just as well as the actual occurrence of disasters during the measured time period; (2) chronic scarcity, which is known or predictable, has no independent effect on warfare frequency in the multiple regression analysis; (3) almost all of our cases with some threat of natural disasters fight more or less constantly (not just when

disasters occur); and (4) when they win, victors almost always take resources from the defeated, even when they have no current problem.

Thus it looks like the people who go to war more or less constantly may be trying not to cover present or regularly recurring shortages, but to protect themselves against future disasters that they *cannot* predict; they seem to be trying to protect themselves ahead of time by taking resources from enemies. It seems to us then that the main motive for going to war is the fear of future loss, not current deprivation.

If we are right that fear of future loss is the main motive for going to war, it makes sense that chronic scarcity does not predict war. First, chronic scarcity (annual shortages) may not be as harmful or lethal as natural disasters that destroy food supplies. Second, chronic scarcity may be psychologically easier to deal with than threat of natural disasters because chronic scarcity is predictable. If you know there will be some "hungry" months, you can prepare yourself emotionally. But the threat of natural disasters, which occur rarely and unpredictably, may be so frightening a possibility that people might attempt to protect themselves against it (by going to war) even though they or loved ones could die in the attempt.

After experiencing a run of favorable outcomes (e.g., a number of years of satisfactory resources), people may believe that the opposite kind of outcome (e.g., a natural disaster that destroys resources) is "due" just by chance. Research suggests that most people may believe that chance is a self-correcting process, in the sense that deviations in one direction eventually induce deviations in the opposite direction to restore some imagined equilibrium. This tendency, which has been called the "gambler's fallacy," is apparently found in experienced research psychologists as well as naive subjects (Tversky and Kahneman 1974).

One implication of this propensity is that the longer the run of a favorable outcome, the more the opposite kind of outcome might be expected. Thus, the fear of unpredictable disasters could be greater the longer it has been since the last disaster. This expectation is consistent with our finding that chronic scarcity does not predict higher frequencies of war, but a history of unpredictable disasters (even just the threat of them, in the absence of any in 25 years) does predict more war.

If fear of unpredictable disasters is the main motive for going to war, we should in the future be able to measure the existence of that fear by examining projective materials. For example, a recent cross-cultural study (Cohen 1990) suggests that fear of natural disasters may be exhibited, albeit in disguised form, in folktales. Cohen found that unprovoked or capricious aggression is likely to appear in the folktales of societies that are subject to unpredictable food shortages (he used our threat of disasters measure). Why? One possi-

bility is that the folktales reflect reality; after all, unpredictable disastrous droughts may seem capricious, not possibly provoked by any human activity, brought on therefore by the gods or nature "out of the blue." Curiously, societies with a history of unpredictable food shortages hardly mention natural disasters in their folktales, perhaps because (as we have suggested) the threat of them is too frightening (and therefore dealt with by the psychological defense mechanism of denial). If Cohen is right, the capriciousness of unpredictable disasters seems to be transformed into the capricious aggression of characters in folktales.

What about the other fear—the fear of others? We think that a history of unpredictable natural disasters may also partially account for why people are less trusting and therefore will raise children who are also mistrustful. Part of our reasoning comes from the positive path coefficient linking natural disasters to mistrust in Model 2. But the linkage is also suggested by Cohen's folktale study (Cohen 1990). If societies with unpredictable disasters have folktales abounding with characters who are capriciously aggressive, an image may be created that people cannot be trusted; after all, capriciousness connotes that motives are hidden or not understood. We have argued that mistrustful adults are more likely to respond aggressively to the arousal of fear. Thus, the tentative theory of war we are suggesting here is that war is mostly caused by a fear of unpredictable natural disasters and a partially resultant fear of others.

It appears that this theory applies particularly strongly to nonstate societies; the multiple *R*s (with and without the outliers) are higher when we exclude state societies. But why should this be? There may be at least two reasons. First, in addition to disasters that destroy food supplies, state societies may face threats to other necessary resources. If these additional threats motivate people to go to war, the natural disasters predictor should work less well by itself. Second, state societies are more likely to have redistributional mechanisms that could mitigate the effects of disasters; surpluses could be moved from disaster-free areas to the affected areas and therefore a threat of natural disasters might not predict warfare so strongly in state societies.

Finally, what about the complex societies of the modern world? In particular, what about industrialized societies (which are hardly represented in the Murdock and White 1969 sample)? Is warfare in and between them explainable in much the same way that preindustrial warfare may be explainable? If the answer in future research turns out to be yes, it will certainly be a modified yes, because our conception of the threat of disasters must be expanded to fit the realities of industrialized societies. In the modern world,

with its complex economic and political dependencies, we may not be worried only about weather or pest disasters that could curtail food supplies; possible curtailments of other resources (e.g., oil) may also scare us into going to war.

## APPENDIX: Details of the Measures

Unless otherwise noted, all the measures pertain to the 25-year interval around the ethnographic present (15 years before until 10 years after).

*Warfare frequency.* Scale scores: (1) warfare seems to be absent or rare (absence is *not* inferred from the absence of ethnographic mention; to be rated as absent or rare, the ethnographer had to report no or rare warfare or he or she had to describe intercommunity and intersocietal relationships without mentioning hostilities); (2) warfare seems to occur every 3-10 years; (3) warfare seems to occur at least once every 2 years; (4) warfare seems to occur every year, usually during a particular season; (5) warfare seems to occur almost constantly, at any time of the year. If a society was pacified during the 25-year period, such that there was warfare in the beginning of the period, but none thereafter, the coders were instructed to rate warfare frequency for the nonpacified portion of the period.

*Threat of famine.* The scale for threat of famines refers to the frequency of episodes of famine (time of starvation when many deaths occur because of food shortage or if it is reported that a substantial segment of the society has to move because of food shortage). Even if the ethnographer did not indicate how many people died or moved because of food shortages, coders were instructed to consider famine to be present if the ethnographer reported a famine. Coders were not to consider statements reporting hunger as famine unless there was substantial starvation resulting in many deaths. The 4 points on the threat of famines scale are: (1) low — food is reported to be ample or adequate with no report of famine, or famine only occurred in the past (not in the measured time period), or occasional periods of food shortage are reported but the scarce foods are reported to be replaced by other available foods, or there may be chronic hunger in the absence of the conditions described in scale points 2 through 4; (2) moderate — there was no reported famine during the measured time period, but the ethnographer states that there is an everpresent threat of famine; (3) moderately high — one famine occurred during the 25-year time period; (4) high — more than one famine occurred during the 25-year time period.

*Threat of natural disasters.* The scale for threat of natural disasters refers to the frequency of severe weather problems (droughts, floods, storms, killing frosts, etc.) or pest problems (e.g., locust infestations) that destroy food resources. The 4 scale points on the threat of natural disasters scale are: (1) low — food is reported to be ample or adequate with no indication that there had been severe natural disruptions of food supplies (coders were also instructed to use a rating of 1 if such serious disruptions occurred only in the past or future but not in the measured time period, and if there

were some disruptions of food supplies but they did not seem to be serious because there was plenty of other food available or made available); (2) moderate – there was no reported serious natural disruption of food during the measured time period, but the ethnographer states that there is an everpresent threat of such disruption; (3) moderately high – one serious natural disruption of food occurred during the 25-year time period; (4) high – more than one serious natural disruption occurred during the 25-year time period.

*Chronic resource scarcity.* Chronic resource scarcity, or the frequency of hunger, was rated as follows: (1) low or rare – coders were instructed not to go by the absence of information on hunger, but to code as 1 only if food is reported to be adequate or abundant for the population, with no report of any of the problems outlined in scale points 2 through 4 (coders were also instructed to use a rating of 1, even if the ethnographer does not specifically say that food is abundant, so long as the ethnographer spends a great deal of time describing available foods and does not mention any problems); (1.5) coders were instructed to use this rating if there was some problem or some problem with scarcity, but how often, to whom, or to how many was not clear; (2) there are some "hungry times" during the year when the people complain that they do not have enough food or enough of a particular food; (3) *some* members of the population usually do not have enough to eat; (4) *most* members of the population usually do not have enough to eat – they are chronically undernourished. (Coders were warned that ethnographers sometimes describe hunter-gatherers as living precariously, with not enough food, because they do not have food stored; but the coders were told to be careful to distinguish between people who always seem to get food, even though it appears uncertain to an outsider, and people who end up hungry.)

*Taking of resources.* We coded the taking of land and nonland resources for internal and external warfare separately. For the present study, the scale scores were combined to reflect the taking of resources, according to the following decision rules: if both external and internal warfare were present, the society was given the higher scale score; if there was only one type of war, the scale score equals the score for the one type; if the coders had "don't know" for one type of warfare, but the other type had a resolved score in the high category (e.g., 3-5 on scale 1; or 3-4 on scale 2), the overall score reflected the one score. (If there was only one scale score and it was in the low category, the case was listed as uncodable on the overall score.)

Scale 1 (land): (1) the defeated are *never* driven from their territory; (2) the defeated are *sometimes* driven from their territory, but the victorious *rarely* use the land of the defeated; (3) the defeated are *sometimes* driven from their territory and the victorious *sometimes* use the land of the defeated; and (5) the defeated are *usually* driven from their territory and the victorious *usually* use the land of the defeated.

Scale 2 (nonland resources): (1) nonland resources (e.g., animals, food, tools, transport, captives) are *never* taken from the defeated; (2) nonland resources are *sometimes* taken from the defeated; (3) nonland resources are *usually* taken from the defeated; and (4) nonland resources are *always* taken from the defeated.

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