

CONSTRUCTING MULTIVARIATE ANALYSES
(OF DANGEROUS DYADS)

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This is a revised version of a paper initially prepared for a mini-conference in honor of
Stuart Bremer, at the annual meeting of the Peace Science Society, Ann Arbor, Michigan,

November 13, 2003

ABSTRACT

Bremer's (1992) "Dangerous Dyads" was seminal in part because it analyzed dyads over a prolonged period of time, and relied on multivariate models. Its two almost identical multivariate models produced strikingly different results. Bremer (1993) added one more variable to the same multivariate model, once again changing the results rather dramatically.

Multivariate models of MIDs and wars since 1992 produce inconsistent results. Whether or not states allied to each other are more likely to become involved in conflict is not clear. Political stability has a potentially important impact on the relationship between democracy and peace, but appears as a control variable in virtually no recent analyses. Some studies show no significant relationship between democracy and peace. Wealth might confound the relationship between democracy and peace, but it appears as a control variable in few recently published analyses.

Analyses of data provided by Gartzke and Li (2003) show allied states are more likely to become involved in conflict. However, if geographic proximity is controlled for, alliance ties are negatively related to MID onset. A positive, possibly spurious bivariate relationship between trade and MID involvement persists even with controls for contiguity and distance. Interdependence relates negatively to conflict only with a control for trade share. This result is obscured in the analyses reported by Gartzke and Li (2003), arguably because they include too many control variables.

INTRODUCTION

Rummel (1983) did a rather extensive study of the relationship between regime type and interstate conflict based on 62,404 observations of dyad-years. But his most ambitious analysis in that paper only covered the years from 1976 to 1980. (He compared the results of that analysis with another focusing on contiguous states only.)¹ Maoz and Abdolali (1989) analyzed pairs of states over a much longer time period than previous analysts, from 1816 to 1976. They reported on the relationship between the regime types of pairs of states, and interstate disputes as well as wars.² In “the first book since my volume 4 of *Understanding Conflict and War* (1979) to explicitly test whether democracies don’t make war on each other” (Rummel, 1997, 36), Bueno de Mesquita and Lalman (1992) analyzed a randomly selected set of 238 European dyads observed in the years from 1816 to 1970. Accepting the categorization of those dyads provided by Doyle (1986), Bueno de Mesquita and Lalman find that no jointly democratic dyads became involved in a war with each other.

Bremer’s path breaking (1992) paper built on and surpassed these precursors in three most important respects. (1) It focused on dyads, (2) over a prolonged period of time, and (3) constructed a *multivariate* model of interstate conflict. Bremer analyzed not only the impact of regime type on interstate war, but also the relationship between war and geographical proximity, power ratios, power status, alliance ties, development, and militarization. His paper is obviously deserving of its place in *Classics of International Relations* (Vasquez, 1996).

In this paper I will discuss Bremer’s (1992) findings in some detail. I will then review models utilized by analysts of dangerous dyads since 1992, and make some

general comments about what has been learned. The paper will conclude with some suggestions about how multivariate models might be made more transparent and interpretable; those suggestions will be accompanied by two examples of multivariate analyses whose simplicity, I will argue, helps to produce results that are more useful and informative than results typically produced by more complex models.

BREMER'S ORIGINAL MULTIVARIATE MODELS

I have noted previously (Ray, 2003) that multivariate models in the subfield of international politics tend to fall into two categories. One type is aimed at providing the best possible, comprehensive explanation of the outcome phenomenon. The other type, considerably more common in recent years, evaluates the impact of one key factor.

Bremer's (1992) paper is often cited as an important source of evidence in support of the democratic peace. However, in fact Bremer (1992) focuses on the more general question of which among several explanatory factors are the more potent.³ In one important respect, Bremer sets a valuable precedent deserving of consistent emulation. He provides a baseline in the form of bivariate analyses to which the results of the multivariate analyses can be compared. Relying on these initial bivariate analyses, Bremer rank orders the explanatory factors according to the strength of their relationships to the onset of interstate war. Those rankings appear in Table 1.

(Place Table 1 about here.)

Also included in that table are rank orders of the factors according to their impacts in two multivariate models on which Bremer reports. He included the second model in response to his surprise at finding originally that militarization had no apparent impact on

war onset. So, he substituted an interaction term reflecting the combination of militarization and alliance ties in the revised model.

One striking feature of the data provided in Table 1 is that the rank orderings of the explanatory factors are quite different in the bivariate and the multivariate analyses. In one respect, that is fortunate and intriguing. If multivariate analyses provide only information that can be gleaned from bivariate analyses, then they would be of little value. But ultimately, the main argument here will be that some multivariate analyses provide little information of lasting value because of the way they are constructed.

Note that in the bivariate analyses in Bremer (1992), joint democracy is ranked fifth among the 7 factors in terms of substantive effect. However, in the initial multivariate analysis in Bremer (1992), democracy is ranked second only to proximity. Then, the removal of militarization and the addition of the interaction term of militarization and alliances bring about yet again substantial changes in the rank ordering of the causal factors. In Bremer's (1992) initial multivariate model, democracy, to repeat, was ranked second. In the modified model, it is ranked fourth. In the original multivariate model, power status is ranked fourth, while in the modified model it is ranked fifth. Alliance ties are ranked sixth in the original multivariate model, but second in the modified model.

The ranking of development remains the same in both multivariate models. However, in the original, bivariate analyses, development is found to be *positively* related to the probability of war onset, while in both multivariate models in Bremer (1992), that same relationship is negative. Why? Exemplifying the main point of this paper, the model is too complex to allow us to answer that question.

In a follow-up paper, Bremer (1993) does focus on the hypothesized impact of democracy. He also includes analyses of militarized disputes as well as war onset. The bivariate analyses suggest that “dyads without joint democracy are almost fifty ($e^{3.905}$) times more likely to originate wars than dyads with joint democracy” (Bremer 1993: 241). The multivariate model in Bremer (1993) is identical to Bremer’s (1992) first multivariate model, with one exception. In the latter paper, Bremer includes the presence of a world leader or hegemon as an explanatory factor. He relies on Thompson’s (1988) coding of polarity to operationalize this concept.⁴

One finding in Bremer (1993) important to keep in mind in evaluations of related work is that many explanatory factors have different impacts on involvement in militarized disputes, on the one hand, and on involvement in interstate wars, on the other. For example, joint militarization has a significant and negative effect on dispute involvement, but a positive, insignificant effect on war involvement. Alliance ties significantly reduce the probability of dispute involvement, but have no impact (in the multivariate analyses in Bremer, 1993) on the probability of war involvement.

Even more important is that including hegemony as an explanatory factor (surprise!) substantially changes the rank orderings of the impacts of the other independent variables. With hegemonic presence included, democracy becomes by far the most important explanatory factor in the model addressed to war involvement. In this model, its effect is even more powerful than that of geographic proximity.⁵ So, one important result of including the presence of a hegemon as an explanatory factor is to raise democracy to the number one position (and to demote proximity to the second or possibly even third position). Recall that in Bremer (1992), in the culminating

multivariate model, democracy was ranked fourth in importance. In the model in the earlier paper, alliance ties are ranked second in importance of impact on war involvement. In the model with hegemonic presence included, alliance ties have no statistically significant impact at all. In Bremer's (1992) multivariate analysis, the presence of one major power has a positive, significant bearing on war onset. In the 1993 analysis, the presence of a major power has no significant effect. *Again, the addition of one explanatory factor to a multivariate model originally constructed by Bremer (1992) has several substantial consequences for the rank orderings of the explanatory variables by their "substantive" importance. What was originally fourth becomes first, what was first is now second or third, what was significant is no longer significant, and so on.*

So, the work by Bremer (1992, 1993) was important and path breaking, especially in its development of multivariate analysis of war onsets, as well as involvement in militarized disputes. But the results of Bremer's multivariate analyses also raise several questions about the strength and character of the effects of what have become standard explanatory factors in multivariate models of interstate conflict. These articles were published over a decade ago. Let us now take at least a preliminary look at how research in subsequent years has clarified or resolved issues raised by Bremer (1992, 1993).

BUILDING ON THE BREMERIAN BASE?

In at least one way, the multivariate analyses in Maoz and Russett (1992) are more informative than most of the analyses to follow in the ensuing decade. In the first place, the primary analytic technique was contingency tables.⁶ It is also commendable that the paper begins with bivariate analyses. Most important and admirable of all is that fact that Maoz and Russett (1992) investigate the impact of a set of possibly confounding

variables one at a time. In other words, they do *not* put the explanatory factors of alliances, contiguity, wealth, and political stability all into the same model with regime type for the purposes of evaluating the impact of those third variables on the relationship of key interest between regime type and conflict.

Less fortunate is the fact that when Maoz and Russett (1992) analyze the impact of control variables on the relationship between regime type and conflict, the dependent variable is dispute involvement. Since we know that what predicts dispute involvement may not predict war involvement, and vice versa, we cannot be certain of the implications of these results for explanations of interstate war.

Nevertheless, two of their findings are of special interest. We can assume that richer countries are more likely to be democratic than are poor countries, and Maoz and Russett (1992) show in their analyses that wealthier countries are less likely than poor countries to get involved in disputes. (See Table 4 on p. 256). This raises the possibility that if wealth has a positive impact on democracy and a negative impact on conflict, then a negative relationship between democracy and conflict might be brought about by the prior, third, confounding impact of wealth on both regime type and conflict. However, that is not what Maoz and Russett (1992) report. Whether analyzing pairs of rich states, pairs of developing states (with GNPs between 10 and 29% that of the US), or poor states (with GNPs less than 10% that of the US), Maoz and Russett (1992) find that democratic states are distinctly *less* likely to get involved in disputes than mixed, or jointly autocratic pairs of states. And, of special interest for an issue to be taken up below, Maoz and Russett (1992) also report that while in general wealthier pairs of states are less likely to become involved in militarized disputes than are poorer pairs of states, among

democratic states, wealthier pairs are *more* likely than less wealthy pairs of states to become involved in disputes (Maoz and Russett, 1992, 256-257).

The other finding of greatest interest in Maoz and Russett (1992, 262) is that “stable states are far less likely to fight one another than expected, regardless of their regime type.” Such a finding suggests (without proving) that stability confounds the relationship between democracy and peace. Accordingly, Maoz and Russett (1992, 246) conclude that, “*we cannot, therefore, dismiss the hypothesis that democracies do not fight one another because of their political structure.*”

In the decade after 1992, various authors published a significant number of multivariate analyses of “dangerous dyads.” Several of these are summarized in Table 2. (These represent a kind of “purposive” sample, admittedly based on my idiosyncratic preferences, intended to be at the same time representative, but also noteworthy.) They are marked, in the aggregate, by the following characteristics. (1) They aim at generalizations regarding international conflict, but they focus more on militarized disputes than on interstate war. (2) They tend to rely on rather large numbers of explanatory factors, quite typically all incorporated into their models simultaneously. (3) Finally, these different authors rely on an ever-shifting list of predictor variables. All of these characteristics complicate comparisons of the results, and coming to cumulative conclusions about what we know about the relationship between democracy and conflict, as well as the relationships between conflict and these additional factors, commonly used as “control variables,” but also quite commonly deserving of attention as causes of war in their own right. Let us elaborate now on these points, relying on Table 2 as a point of reference.

(Place Table 2 about here)

Many of these works provide estimates of the “substantive” significance of the factors they analyze in terms of how much change in the dependent variable is associated with a change in the independent variable in question, controlling for all the other explanatory factors included in the various models. For reasons that I hope are abundantly clear by this point in our discussion, all of this information is of limited utility. Changing one variable even in a rather lengthy list of variables can notably change the estimates of the “substantive” effects of all other variables in a model. Even a brief perusal of the different sets of explanatory variables suggests quite clearly that there is no theoretically based agreement on which ought to be included in multivariate analyses. Therefore, all of these estimates of “substantive impact” are basically arbitrary and meaningless.

Another problem created by the multivariate analyses included in Table 2 involves the relationship between alliance ties and conflict onset. Recall that Bueno de Mesquita (1981: 74, 160) argues that “nations can have strong incentives for war with their closest allies, under some circumstances, even stronger than with their enemies.” His data that show that “wars between allies are about three times more likely than one would expect...” This is a truly novel, counter-intuitive hypothesis.⁷ Maoz and Russett (1992, 260-261) provide a rather stirring defense of that hypothesis.

However, Maoz and Russett (1993, 632) report that “while the bivariate relationship between alliance and conflict is positive, after controlling for other relevant variables, allied parties are less likely to fight each other than would be expected by chance alone.” Maoz and Russett (1993) control for five other “relevant” variables.

Which of those changes the relationship between alliances and conflict involvement from positive to negative? There is no way of knowing. And Table 2 shows that while some studies report a significant negative relationship between alliances and conflict (Oneal et al., 1996; Oneal and Russett, 1997; Russett, Oneal, and Davis, 1998; Reed, 2000; Russett and Oneal, 2001; Henderson 2002), several report that the relationship is not statistically significant (Barbieri 1996, 2002; Bennett and Stam, 2000; Oneal and Russett 2001). It is certainly possible to guess at explanations of these differences, but it is fair to say that each of the multivariate models taken alone, and certainly in the aggregate, are sufficiently complex to preclude confident conclusions about the impact of alliance ties on conflict. It is also impossible to know which factor, if controlled for, changes the relationship between alliance ties and conflict from positive to negative.

Maoz and Russett (1992), to repeat, found that political stability might account for the fact that democratic states do not fight one another.⁸ Although Maoz and Russett (1993) include political stability as a control variable in analyses of the relationship between democracy and peace, it is treated as a measure of the strength of political norms, rather than as a potentially confounding variable. More recently, political stability seems to have virtually disappeared from analyses of dangerous dyads. Its inclusion might change the results of many of the analyses shown in Table 2.

A common thread running through virtually all of the results shown in Table 2 is an emphasis on the relationship between democracy and peace. Most show that the democracy does have a significant relationship with peace. Reed (2000) is an exception. But he does report, in a stage of the analysis not shown in Table 2, that democratic states are unlikely to get involved in militarized disputes with each other. In the second stage of

his analysis, he includes several controls: capability ratios, joint satisfaction, alliance ties, GDP growth, and interdependence. Because every one of these might be construed as a variable intervening in the process leading from democracy to peace, his report that democracy is not significantly related to escalation to war is not entirely persuasive.

Henderson (2002) argues forcefully that democracy is not related to the onset of militarized disputes. However, he also includes many potentially intervening variables in his models. Admittedly, many of these same intervening variables are included by democratic peace theorists or proponents, so he has precedents in his favor. In my view, all those precedents are unfortunate, even though most of them did not, it turns out, reduce the relationship between democracy and peace to insignificance. Furthermore, one reputedly confounding variable relied on by Henderson is political similarity, which is related to regime type by definition, rather than empirically or causally. This eliminates it *a priori* from consideration as a potentially confounding variable (Ray, 2003, 15-19). A potentially confounding variable must have a causal impact on both the independent and dependent variables in the original hypothesis. If X and Y are related to each other by definition, then it would obviously be fallacious to argue that X *causes* Y.

In addition, Barbieri (2002) finds none of the control variables of significance in her analysis, except interdependence. She also includes many intervening variables. Perhaps more important, in this case, she focuses on disputes only, trying to account for which disputes escalate to war. As Reed (2000), among others, demonstrates, selection effects may produce misleading results in such cases.

Finally, it is crucial to note that Bremer (1992) found a positive bivariate correlation between wealth and conflict, which turned negative only when wealth was

embedded in a model containing several additional control variables. Which control variables in his multivariate models change the sign of the wealth-conflict relationship is impossible to tell from the information provided by Bremer. In contrast, although focusing on a much shorter time period, and on dispute involvement rather than war involvement, Maoz and Russett (1992) show that in general wealthy pairs of states are less likely than statistically expected to get involved in disputes, while poor pairs of states are more likely than would be expected by chance to be involved in conflict. In Table 2, one can see that Maoz and Russett (1993) report a negative relationship between wealth and conflict. However, this relationship is embedded in an analysis controlling for so many other factors that the results regarding the impact of wealth on conflict are difficult to interpret with any confidence. This problem is compounded by the fact that in this article, Maoz and Russett's (1993) measure of wealth reflects not only the degree of wealth in a pair of states, but also the *differences* in the levels of wealth in dyads, thus creating an index that reflects the interaction of wealth, and differences in wealth in such a way as to make it impossible to disentangle the relationships between wealth, differences in wealth, and conflict. It is also impossible to say from this analysis what the impact of wealth might be on the relationship between democracy and peace.

Oneal, Oneal, Maoz and Russett (1996) exclude wealth as a control variable because "it never proved significant when INTERDEP [that is, trade-based interdependence]⁹ was in the equation" (Oneal et al., 1996). Since my general point here is that analysts typically include too many control variables, it may seem incongruous here for me to criticize the exclusion of wealth. But I am not suggesting adding wealth to the list of control variables already included in their models. I am suggesting that it

might have been a mistake to exclude wealth from all further analyses only because it has no significant relationship with conflict in an analysis with several other predictor variables.

Allow me to elaborate on this point by pointing out that high-powered medical research based on the worldwide web (check out http://www.chestpainperspectives.com/general/risk_fact.html, and <http://www.torrancememorial.org/carrisk.htm>, for example) reveals that among the more important factors predicting to or accounting for the probability of heart attacks are heredity, or family history, cigarette smoking, high blood cholesterol, high blood pressure, and obesity. A plausible description of the relationships among all these factors is presented in Figure 1.

A multivariate analysis of the effect of hereditary influences, smoking, cholesterol levels, high blood pressure, obesity on the probability of heart attack among some representative sample of the population might find that the hereditary factors, as indicated say by family history, would have no impact at all. This might happen because smoking, cholesterol levels, high blood pressure, and obesity are all factors that intervene in the process leading from hereditary background to heart attacks. In other words, chances are that respondents with parents who are/were smokers, and had high cholesterol levels, high blood pressure, and a tendency toward obesity, were considerably more likely than other respondents to be smokers, have high cholesterol, high blood pressure, and a tendency toward obesity themselves. (I understand that environmental as well as genetic influences could be at work here.) If so, controlling for all these intervening variables might result in a coefficient pertaining to hereditary influences

indistinguishable from 0. It would be a fundamental mistake, of course, to conclude from such a result that genetic influences, or family history, have no important impact on the probability of heart attacks. If Figure 1 is an accurate depiction of the relationships among all the causal factors being considered, then hereditary factors are in fact the most profoundly important explanatory factor of all.

(Place Figure 1 about here.)

Similarly, if the relationship between wealth and conflict is not significant in a model including, for example, interdependence and joint democracy as explanatory variables, it might be a mistake conclude that wealth has no important impact on conflict propensity. It is also impossible to tell in the analyses presented by O Neal et al. (1996) whether or not wealth is a confounding variable creating spurious negative relationships between joint democracy and interdependence, on the one hand, and the probability of conflict, on the other. Such might be the case if wealthy states are (1) more likely to be democratic, and (2) trade a lot with each other¹⁰, and (3) less likely to engage in interstate conflict.

When interdependence is included in the model presented by O Neal and his colleagues (1996), the relationship between democracy and peace becomes *almost* insignificant. When they add a variable reflecting trends in interdependence, there is no significant relationship between democracy and peace. They apparently need to resort to a dichotomous measure of joint democracy in order to preserve the “significance” of the relationship between democracy and peace in the model presented in their paper. This is a sign, perhaps, of troubles to come.

Barbieri (1996) uses different trade data, and looks at a different time period, and uses different measures of interdependence, so there are many possible reasons why she finds, in contrast to the results reported by Oneal and Russett in several places, that pairs of states that trade a lot are *more* likely to get into serious conflicts with each other. Barbieri, too, excludes wealth as a control variable, probably because of the lack of GNP data for much of the time period beginning in 1870 that she analyzes. She also reports that alliances do not decrease the probability of conflict.

In short, since wealth washed out in one of the earlier multivariate analyses of dangerous dyads, it has tended to be absent from most subsequent work. There is one important exception. Mousseau (2000) argues quite persuasively that it is only among relatively *wealthy* states that democracy has a substantial pacifying impact. There are several reasons to doubt that claim. One is that Maoz and Russett (1992) report directly contradictory results. Another is that the interaction term of democracy*development correlates with the indicator of democracy by itself at the .99 level, thus creating some uncertainty as to the possibility of distinguishing the impacts of each. Furthermore, though Mousseau is virtually alone among the analysts included in Table 2 to show sensitivity to the problem of including intervening variables in models aimed at evaluating the impact of key factors,¹¹ he still arguably includes variables in his most important model that might intervene in the process leading from democracy to peace. Thus, his estimate of the impact that democracy in particular has on the probability of conflict, as opposed to the impact of democracy combined with economic wealth, might be faulty.

WHAT IS TO BE DONE?

Basically, my argument here is that multivariate models of interstate conflict should be simplified. The basics of this argument have been succinctly stated recently by Achen (2002: 446), who asserts that “a statistical specification with more than three explanatory variables is meaningless.” He asserts that evaluating models with only two explanatory factors is relatively easy, but that the “curse of dimensionality sets in quickly,” and that “collinearity among explanatory factors plagues social science and multiplies the pains of data analysis rapidly as the number of factors rises.” He concludes that “using four [explanatory factors] is so hard and so time-intensive that it is almost never done astutely and thoroughly.”

Achen stresses the methodological difficulties of dealing with more than three explanatory factors in a single multivariate model. Perhaps even more daunting are the theoretical complexities. Generally speaking, our theoretical notions focus on two factors simultaneously (simply stated, a cause and an effect); occasionally, theoretical notions are developed about the possible impact of a third factor on the original relationship of interest. This is not to say that theoretical frameworks typically focus on only two explanatory factors. But in their specific implications regarding causal linkages, generally theories and theorists deal with only two variables at a time.

This suggests in turn that in general, multivariate models of interstate conflict with more than two or three explanatory factors, to be tractable, should be structured in the manner exemplified in Figure 2. In short, as long as none of the independent variables has any impact on or statistical association with another, then our theoretical notions can deal with combining them into a single multivariate model. However, if there are inter-connections between any of them (or all of them), then analysts would be

well-advised to refrain from putting more than three of them into the same model unless they provide a fully specified model that justifies their inclusion. That is, they should specify which independent variables will have an impact on what other independent variables, and how the inter-relationships of those independent variables will affect their association with the dependent variable. In addition, the theoretical basis for such models should support the argument that no important causal factor has been overlooked. Such theoretical preconditions for the construction of multivariate models with more than two or at most three explanatory factors are virtually never met, in my experience.

(Place Figure 2 about here.)

Multivariate analyses should, in most circumstances, be utilized when there are specific theoretical ideas about how third, (and, occasionally, fourth or fifth) variables might affect the relationship between the key explanatory variable and the phenomenon to be predicted. Most fundamentally, and most commonly, perhaps, multivariate models should address the issue of whether or not any bivariate relationship of interest is spurious. This means that analyses should provide bivariate analyses as a baseline for comparison, in most circumstances. Then additional variables should be added in incremental steps.

Let us discuss a couple of multivariate analyses that because of their simplicity, address in a useful way issues left unclear by prior multivariate analyses. For both, we will rely on a data set made available by Gartzke and Li (2003), who in turn rely on data sets originally assembled by Oneal and Russett (1997, 1999). The data focus on all pairs of states in the international system in the years from 1950 to 1992.

In two tables, Gartzke and Li (2003: 566) report that there is no significant relationship between alliance ties and the probability of MID onset, thus adding to a series of inconsistent results mentioned above. Table 3 shows that pairs of allied states are more likely than are unallied pairs to become involved in militarized disputes. (See Model 1.) But when contiguity is controlled for, the relationship between alliance ties and the probability of conflict is negative, and *almost* significantly so. (See Model 2.) A control for distance alone, even though it is a more sensitive, interval level indicator of the geographic relationship between two states, does not by itself reveal a negative relationship between the presence of alliance ties and the onset of MIDs. It takes controls for both contiguity and distance simultaneously¹² to bring out or reveal a significant negative relationship between the presence of alliance ties between a pair of states and the likelihood that the pair will become involved in a militarized interstate dispute. (See Model 4) Recall, too, that Gartzke and Li (2003) report that there is no significant relationship between alliance ties and MID onset. Our analyses here show that this is an arguably misleading result, apparently brought about by what are from our current viewpoint extraneous control variables. In short, their model is too complex to allow clear conclusions about the relationship between alliances and conflict, controlling for geographic proximity.

(Place Table 3 about here.)

One response to that assertion might be that Gartzke and Li (2003) are not particularly concerned in their paper about the relationship between alliances and conflict, and so the fact that the results of their analyses are misleading in their implications about that relationship is at best a minor problem. However, in the case of

Gartzke and Li (2003), and in all of the papers utilizing similar models, the results can be misleading even about relationships of central interest.

(Place Table 4 about here.)

Relying on the data set from Gartzke and Li (2003), Table 4 addresses the relationship between trade interdependence, (defined as the amount of trade between two states as a proportion of the GNP of the state for which this proportion is lower) and the onset of MIDs. In light of all the attention that this relationship has received in the last 10 to 15 years, it should be clearly understood by now (but is it?) that states that trade extensively with each other are also more likely to become involved in serious disputes with each other. Table 4 shows a strongly positive bivariate relationship between trade interdependence and the onset of MIDs.

However, one might expect the bivariate relationship between trade and conflict to be positive because both trade and conflict are affected so fundamentally by the geographic relationship between states. In other words, states that are close together will be more likely to be involved in conflict, *and* more likely to trade a lot with each other, just because they are close together. A control for contiguity will probably be necessary to uncover any pacifying effect that trade might have on relationships between states. Somewhat surprisingly, however, Model 2 in Table 4 shows that even with a control for contiguity, the relationship between trade and conflict for all pairs of states in the system from 1950 to 1992 is *positive*, and significantly so. That relationship remains, as in Model 3 in Table 4, even if *both* contiguity and distance are controlled for. It is only when, as in Model 4, a control for trade *dependence*, (defined as the amount of trade between two states as a proportion of the total trade engaged in by the state for which this

proportion is lower), that the relationship between trade *interdependence* and conflict turns significantly negative.

Gartzke and Li (2003) present their results in such a fashion as to indicate that the positive relationship between trade dependence and conflict supports arguments by Barbieri (2002), while the negative relationship between trade interdependence and conflict supports arguments made by Oneal and Russett (1997, 1999a) This is arguably a misleading interpretation of the data they analyze, made viable or plausible only by the overly complex character of the multivariate analyses in their paper. (This is, I hasten to add, the type of multivariate analysis appearing in virtually every one of the previous analyses of dangerous dyads.) In other words, I do not believe that theoretical notions of Oneal and Russett (1997, 1999a) suggest that trade interdependence has a pacifying impact if and only if trade *dependence* is controlled for. Analyses of the data provided by Gartzke and Li (2003) show that the positive relationship between trade share (defined here as dependence) and conflict exists in bivariate analyses, and with controls for geographic proximity. Such results do support theoretical arguments by Barbieri (2002). The implication by Gartzke and Li (2003) that their findings support arguments by Oneal and Russett (1997, 1999a), *as well as* those by Barbieri (2002) seems plausible, to repeat, only because the multivariate analyses on which they rely are sufficiently complex that they obscure the fact that even with contiguity and distance controlled, the relationship between interdependence and conflict is positive. This is not, it is important to reiterate, a criticism of Gartzke and Li (2003). Rather, it is an example of how the results of analyses with more than two or three explanatory factors can be misleading. It is also a

gentle, virtually implicit criticism of all such analyses that have been published since Bremer (1992) published his original paper on “Dangerous Dyads.”

CONCLUSION

Bremer’s (1992,1993) analyses of dangerous dyads enlightened us in important ways about the impact of regime type, and several other important factors, on interstate conflict within pairs of states. Research since then has added substantially to our knowledge about the factors and processes leading to MIDs and wars. But multivariate models of interstate conflict in recent work have also produced inconsistent findings. Many of these inconsistencies, and the confusion they produce, are both products to an important extent of multivariate analyses that are too complex to be interpreted in a confident, consistent fashion. Multivariate analyses should always be preceded by bivariate analyses, models should contain more than three explanatory factors only rarely, and variables should be added to them only in an incremental fashion that makes it obvious which third factors have what kind of impacts on relationships of special interest.

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NOTES

¹ Interestingly, Rummel also analyzed all pairs of states for the duration of their existence between 1945 and 1965, and reports that none of the 276 jointly democratic states during that time period even *threatened* to use force against each other.

² Maoz and Abdolali (1989: 11) assigned scores for regime type to each of the pairs of states in every year they observed, but they were required to impute indirectly the vast majority of those scores because, as they explained at the time. “the current version of the polity data is based on scores assigned to each of these characteristics [i.e., method of executive selection, type of political competition and opposition, the characteristics and independence of executive policymaking and its decision-making latitude, the distribution of authority, the type of political participation, and the scope of governmental functions] at two points in time: the date of polity onset and its termination date.”

³ It does, however, conclude that “core components of the Wilsonian recipe for a more peaceful world were: establish collective security alliances, spread democracy, promote economic progress, and reduce armament levels. All save the last have been found to reduce strongly the likelihood of war...” (Bremer 1992: 338).

⁴ “I assume...that a hegemon was present in the system if, according to Thompson, the major power system was unipolar or near unipolar and assume that it was absent if the major power system was bipolar or multipolar” (Bremer 1993: 239).

⁵ “[D]yads without joint democracy are about forty times more likely to originate a war than those with joint democracy. This effect is...very strong and easily the most potent explanatory variable included in Table 5” (Bremer 1993: 245). Geographic proximity is one of the variables included in Table 5.

⁶ Maoz and Russett (1992: 251) assert that “more sophisticated analyses such as log-linear models might yield misleading results for reasons that will become obvious.” They never became obvious to this reader.

⁷ Furthermore, it is one for which this writer has in the past uncovered some modest supporting evidence, in Ray (1990).

⁸ Actually, even though the analyses in Maoz and Russett (1992) are simpler and more straightforward than analyses of dangerous dyads in general, they are not sufficiently straightforward to support the conclusion

that political stability is a confounding variable with respect to the relationship between democracy and peace. What their data show, as they claim, is that stable states are relatively peaceful, regardless of regime type. What they would have had to show, in order to demonstrate that the relationship between democracy and peace is spurious, is that democratic states are no less peaceful than undemocratic states, if political stability is controlled for. What they have demonstrated is that regime type is not an intervening variable that accounts entirely for any pacifying impact that political stability might have. This would necessarily be the case if political stability is a confounding variable, but that finding is not sufficient to warrant the conclusion that the correlation between democracy and peace is spurious.

⁹ Let me suggest here in a footnote to which such futile gestures are rightly relegated that the use of such acronyms should be banned from the body of, and perhaps even the tables in, all professional publications in the field. They are fit only for communications between consenting adults and their computers.

¹⁰ Oneal et al. [1996: 18], acknowledge that “economically advanced countries have been among the most interdependent nations in the postwar period”

¹¹ For example, at one point Mousseau (2000: 488) says that “alliance links are intervening rather than confounding factors in any examination of market democracy on international behavior. Statistical control for this variable is thus not appropriate...”

¹² Such controls for “independent” variables that are in fact related to each other by definition is not a step in general to be recommended. (See Ray 2003).

TABLE 1

**Rank Order of the Impact of Explanatory Factors on
Interstate War, 1816-1965, Bremer (1992)**

<i>BIVARIATE ANALYSES</i>	<i>MULTIVARIATE ANALYSIS</i>	<i>REVISED MULTIVARIATE ANALYSIS</i>
1. Proximity (+)	1. Proximity (+)	1. Proximity (+)
2. Power Status (+)	2. Democracy (-)	2. Alliance Ties (-)
3. Alliance Ties (+)	3. Development (-)	3. Development (-)
4. Militarization (+)	4. Power Status (+)	4. Democracy (-)
5. Democracy (-)	5. Power Diff. (-)*	5. Power Diff. (-)
6. Development (+)	6. Alliance Ties (NS)	6. Power Status (+)
7. Power Diff. (-)	7. Militarization (NS))	7. Mil.*All. (+)

* My reading of Bremer (1992) leaves me uncertain as to nature of the relationship between power ratios and war onset in these multivariate analyses. The tables show a *positive* relationship between “no large power difference” and war onset. This would seem to indicate that the *presence* of a large power difference is negatively related to the probability of war onset. However, Bremer (1992, 334) in the text asserts that “the presence of a[n] overwhelming preponderance in a dyad...[has] a...positive impact on the likelihood of war.” In his conclusion, however, Bremer (1992, 338) categorizes the “absence of overwhelming preponderance” as characterizing “dangerous dyads,” which leads me ultimately to conclude that he means to indicate the power differences are negatively related to the probability of war onset.

TABLE 2

Predictor Variables Included in Multivariate Models of Interstate Conflict from a Selected Sample of Analysis of “Dangerous Dyads” #

Maoz and Russett (1993)	Oneal, Oneal, Maoz and Russett, (1996)	Barbieri (1996)
Democracy (-)	Democracy (-)	Saliency (-)
Wealth(-)	Growth (-)	Symmetry(-)
Growth (-)	Alliance (-)	Interdependence(+)
Alliance (-)	Contiguity (+)	Contiguity(+)
Contiguity (+)	Capability ratio (-)	Democracy(-)
Capability ratio (-)	Interdependence (-)	Alliance (NS)#
	Change,interdep. (-)	Capability Ratio(-)

Oneal and Russett (1997)	Russett, Oneal, and Davis, 1998	Reed, 2000¹²
Democracy, lo (-)	IGOs, (-)	Capability ratio (+)
Democracy, hi (+)	Democracy, lo (-)	Democracy (NS)
Economic growth (+)	Democracy, hi (+)	Joint Satisfaction (NS)
Alliance (-)	Interdependence (NS)	Alliance (-)
Contiguity(+)	Interdep. Trend (-)	Growth(-)
Capability ratio(-)	Growth (NS)	Interdependence(NS)
Interdependence, lo (-)	Capability ratio (NS)	
Change in inter., hi(-)	Alliance (-)	
Dem., lo*contig.(NS)	Contiguity(+)	
Ec. Growth* Contig. (-)		

TABLE 2 (Cont.)

Reed, 2000	Mousseau, 2000	Bennett and Stam, 2000
Capability ratio (+)	Democracy, lo (-)	Interdependence, lo (-)
Democracy (NS)	Development, hi (-)	Capability ratio (-)
Joint Satisfaction(NS)	Development, lo (+)	Democracy, lo (-)
Alliance(-)	Devel.*Democ. (-)	BdM, IIG, equil., NS)
Growth(-)	Interdependence (-)	IGOs, (NS)
Interdependence(NS)	Capability ratio (-)	Alliance (NS)
	Power status (+)	Contiguity (-)
	Contiguity (+)	Distance (NS)
	Distance (-)	Both minor powers, (-)
	Hegemonic power (-)	
	Hegemonic status (+)	

Russett and Oneal, 2001	Henderson, 2002	Barbieri, 2002
Capability ratio (-)	Democracy (NS)	Saliience (NS)
Alliance (-)	Growth (-)	Symmetry (NS)
Dem,lo – Avg. Dem, (-)	Alliance (-)	Interdependence (+)
Trade, lo – Avg. trade(-)	Contiguity (+)	Contiguity (NS)
IGO – Avg. IGO (-)	Capability ratio (-)	Democracy (NS)
Avg. Dem (-)	Interdependence, lo (-)	Alliance (NS)
Avg. Trade (-)	Political Similarity (-)	Capability Ratio (NS)
Avg. IGO (NS)		
Hegemonic Power (NS)		
Joint Satisfaction (NS)		
Contiguity (+)		
Distance(-)		
Both minor powers (-)		

#”NS” = “Not significant”

TABLE 3

Logit Analysis of the Relationship Between Alliance Ties and the Probability of MID Onset, All Dyads, 1950-1992, with Controls for Contiguity, and Distance Introduced Sequentially and Simultaneously*

Dep. variable MID onset	Model 1# (N=366,782)	Model 2 (N=366,782)	Model 3 (N=366,782)	Model 4 (N=366,782)
Alliance ties	1.17 8.56 0.00	-.180 -1.47 0.14	-.024 -0.15 0.88	-.322 -2.57 0.01
Contiguity		3.88 28.45 0.00		3.83 17.79 0.00
Distance			-1.09 -14.97 0.00	-.287 -4.52 0.00

Cells show coefficients
z-scores
probability

**Peace years and splines not shown*

#In this and all other models, standard errors are adjusted for clustering on dyad

TABLE 4

Logit Analysis of the Relationship Between Trade Interdependence and the Probability of MID Onset, All Dyads, 1950-1992, With Controls for Contiguity, and Distance, and Contiguity, Distance and Trade Dependence Introduced Sequentially.*

Dep. variable Mid onset	Model 1# (N=270,994)	Model 2 (N=270,994)	Model 3 (N=270,994)	Model 4 (N=270,994)
Interdependence	.130 9.78 0.00	.024 2.00 0.05	.024 2.17 0.03	-.600 -6.59 0.00
Contiguity		3.90 21.27 0.00	3.39 15.26 0.00	3.22 13.86 0.00
Distance			-.277 -3.83 0.00	-.352 -4.50 0.00
Dependence				.567 6.77 0.00

Cells show coefficients
z-scores
probability

**Peace years and splines not shown*

#In this and all other models, standard errors are adjusted for clustering on dyad

FIGURE 1
PLAUSIBLE INTER-RELATIONSHIPS AMONG FACTORS
LEADING TO HEART ATTACKS

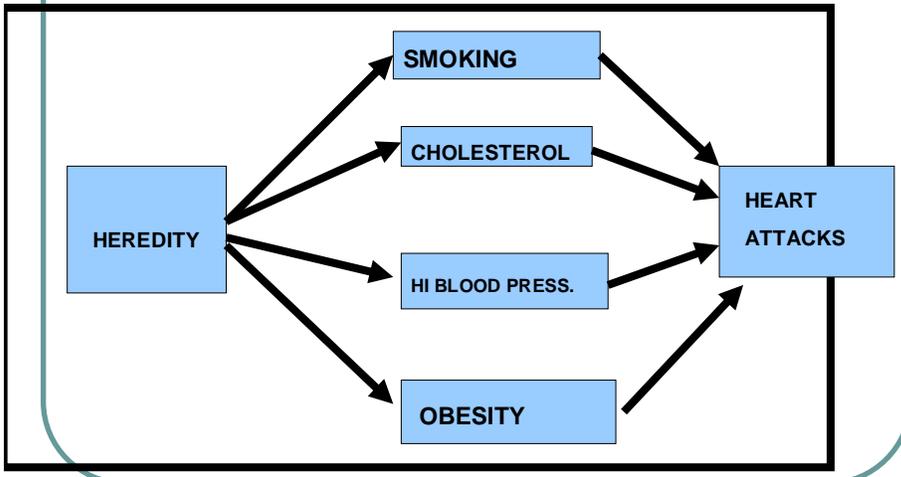


FIGURE 2
“IDEAL” MULTIVARIATE MODEL

