Conflict Management and Peace Science, 22:277–292, 2005
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ISSN: 0738-8942 print / 1549-9219 online
DOI: 10.1080/07388940500339175

Routledge
Taylor & Francis Group

Constructing Multivariate Analyses (of Dangerous Dyads)

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Most multivariate models aimed at evaluating the impact of democracy on interstate conflict contain a set of control variables sufficiently large to have a confusing impact. Partly for that reason, the potentially confounding impacts of such variables as wealth, political stability, and political similarity on the relationship of democracy to conflict have still not been evaluated in a definitive manner. In other cases, multivariate models contain intervening variables that are likely to produce misleading results. Multivariate analyses aimed specifically at uncovering spurious relationships in a more straightforward and incremental manner are better able to produce clear and informative results.

Keywords democracy, peace, multivariate models, control variables, spurious relationships

Research in the last couple of decades or so on the proposition that democratic states have not and are not likely to fight interstate wars with each other (and are less likely to engage in militarized conflict with each other than states in general) has produced many multivariate models of international conflict and war. These models as a group have characteristic strengths and weaknesses; this paper will focus primarily on the latter. Perhaps the primary flaw in most of these models, from this writer's point of view, involves their complexity. Virtually all recent models aimed at the evaluation of the impact of regime type on interstate conflict involve a number of control variables that is sufficiently large to make it difficult to interpret statistical results. Furthermore, the control variables are often introduced for no very good reason other than that it is not possible to publish papers that rely entirely on bivariate analyses in good journals.

But not all variables apparently or conceivably related to the outcome variable (such as interstate conflict) are equally good candidates for inclusion as control variables, and there are substantive distinctions between different types of control variables to which more attention would usefully be paid. This paper will briefly review the structure and evolution of multivariate models within the democratic peace literature with a view toward highlighting uncertainties and some confusion that those models produce in the aggregate. A discussion of how multivariate models of conflict might be more usefully constructed and interpreted will serve as the basis for the examination of one recently published multivariate

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. It has benefited from comments by all the participants on a panel devoted to this and related issues at the 2004 annual meeting of the Peace Science Society, and by the editors of this special issue as well as anonymous referees. Any remaining shortcomings are of course my responsibility.

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model of interstate conflict. That examination will demonstrate how more focused, simpler analyses can produce more informative, less confusing results.

In the Beginning: Bremer and His Precursors

In what was certainly one of the earliest analyses of this type, Rummel (1983) examined the relationship between regime type and international conflict for 62,404 observations of dyad-years, but his analyses covered only 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 the probability of interstate disputes, as well as the wars in which they became involved. Bueno de Mesquita and Lalman (1992) analyze 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), they find that no jointly democratic dyads became involved in war with each other.

Bremer's (1992) path-breaking paper built on and surpassed these precursors in three most important respects. First, it focused on "dangerous" dyads. Second, it dealt with a prolonged period of time. Finally, it provided a multivariate model of interstate conflict. Bremer analyzed not only the impact of regime type on interstate war, but also the possible impacts of proximity, power ratios, alliance ties, development, and militarization. Relying on these initial bivariate analyses (and on the resulting Z-scores), 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.

Also included in that table are rank orderings of the factors according to their impacts in the two multivariate models on which Bremer reports. (They are ranked according to the size of the proportionate increase in the probability of war that they apparently bring about.) He developed 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 important feature of 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, they would be of little value. But 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 ranks fifth in importance among the seven factors. However, in the initial multivariate analysis in Bremer (1992), democracy ranks second only to proximity. Then, the removal of militarization and the addition of the interaction term of militarization and alliances bring still more substantial changes in the rank ordering of the causal factors according to the importance of their impact. Democracy, for example, in the final model, ranks fourth. Power status moves from fourth to sixth, while alliance ties are ranked sixth in the original model, but second in the final, modified model. The ranking of development remains the same in both models, but in the bivariate analyses, development is *positively* related to the probability of war onset, while in both multivariate models the relationship

[1]Rummel (1983) also analyzed pairs of states for the duration of their existence between 1945 and 1965, and reports that none of the 276 jointly democratic pairs of states during that time period even *threatened* to use force against each other.

TABLE 1 Rank order of the impact of explanatory factors on interstate war, 1816–1965 (Bremer, 1992)

Bivariate analyses	Multivariate analysis	Revised multivariate analysis
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 the 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 that power differences are negatively related to the probability of war onset.

is negative. Exemplifying a main point of this paper, the model is too complex to allow readers to know why this change in the relationship between development and conflict takes place.

In a later paper, Bremer (1993) includes analyses of militarized disputes as well as war onset. Bivariate analyses suggest that "dyads without joint democracy are almost 50 ($e^{3.905}$) times more likely to originate wars than dyads with joint democracy" (Bremer, 1993, 241). The multivariate model in Bremer (1993) is identical to the first one presented in Bremer (1992), with one exception. In the later paper, the presence of a world leader or hegemon is included as an explanatory factor.[2] One finding in Bremer (1993) important to keep in mind in evaluations or interpretations of related work is that many explanatory factors seem to 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 on the probability of war involvement. (These results can be seen as anticipatory of a later argument that selection effects can obscure differences between factors that lead states to get involved in disputes in the first place, and those factors that lead those disputes to escalate to war. See Reed, 2000.)

Even more important is that including hegemony as an explanatory factor (surprise!) substantially changes the rank orderings of the variables by the import of their impact. With hegemonic presence included, democracy becomes by far the most important explanatory factor in the model addressed to war involvement; democracy is even more powerful in its impact on the probability of war involvement than geographic proximity. Recall that in Bremer (1992), in the culminating multivariate model, democracy ranks fourth in importance of impact. In Bremer (1992), alliance ties rank second in their impact on war involvement. In Bremer (1993) (with hegemonic presence added to the model), alliance ties have no statistically significant impact at all. In Bremer's (1992) multivariate analysis,

[2]Bremer relies on Thompson's (1988) coding of polarity to operationalize this concept.

the presence of one major power in a dyad has a positive, significant effect on the probability of war onset. In his 1993 analysis, the presence of a major power has no significant effect. *In short, the addition in Bremer (1993) of one explanatory factor to the model introduced by Bremer (1992) has several substantial consequences for the rank orderings of the explanatory variables by their "substantive" importance.*

So, the work by Bremer (1992, 1993) was important and path breaking, especially in its development of multivariate analyses of war onsets, as well as involvement in MIDs. But the results in those papers also raise several questions about the strength and even the existence of the impacts of what have become standard explanatory factors in multivariate models of interstate conflict. Those articles were published over a decade ago. Let us now review how research in subsequent years has clarified or resolved at least some of the issues raised by Bremer (1992, 1993).

The Evolution of Multivariate Models of Interstate Conflict

Published at about the same time as Bremer's earlier paper, Maoz and Russett (1992) report several important analyses that are quite informative. They evaluate 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 an equation simultaneously along with regime type. Less fortunate is their choice to focus only on dispute involvement. Since Bremer shows that what predicts dispute involvement may not always predict war involvement, and vice versa, we cannot be certain of the implications of Maoz and Russett's (1992) results for models of interstate war.

Nevertheless, two of their findings are of special interest, perhaps, in light of later research. We can assume that richer countries are more likely to become democratic than are poor countries, and Maoz and Russett (1992) provide some evidence that wealthier pairs of countries are less likely than pairs of poor countries to get involved in disputes. This finding reinforces Bremer's (1993, 245) conclusion that "dyads composed of two more developed states are about three times *less* likely to originate disputes and five times *less* likely to originate wars than dyads that contain one or fewer less developed countries."[3] It also 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 entirely by the impact of a prior, third, potentially confounding factor of wealth on both regime type and conflict.

However, that is not what Maoz and Russett (1992) report. Whether focusing on pairs of rich states alone, pairs of developing states in isolation, or pairs of states all of which are poor, Maoz and Russett (1992) find that democratic states are distinctly *less* likely to get involved in militarized disputes than mixed or jointly autocratic pairs of states. And, of special interest for an issue to be taken up again below, Maoz and Russett (1992, 256–257) also report that while, in general, wealthier pairs of states are less likely to become involved in militarized disputes than poorer pairs of states, among *democratic* states, wealthier pairs of states are *more* likely than less wealthy pairs to become involved in militarized disputes.

Another finding reported by Maoz and Russett (1992, 262) focuses on a factor not considered by Bremer (1992, 1993). They conclude that "stable states are far less likely to fight one another than expected, regardless of their regime type." Such a finding suggests that stability might confound the relationship between democracy and peace, that is,

[3]Though recall also that in *bivariate* analyses Bremer (1992) reports that pairs of wealthy countries are *more* likely to get involved in wars than poorer pairs.

that stability might make both democracy and peace more likely and therefore create a spurious correlation between them. Accordingly, Maoz and Russett (1992, 262), conclude that "political stability . . . may account for the low rate of disputes between democracies."

In the decade subsequent to 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, based to some extent on my idiosyncratic preferences, but intended to be representative and noteworthy.) They are marked in the aggregate by the following characteristics: (1) They aim at generalizations regarding interstate conflict, but they focus more on militarized disputes than on interstate war. (2) They tend to rely on a rather large number of explanatory factors, quite typically incorporated into the models simultaneously. (3) They are based on an ever-shifting list of predictor variables. All of these characteristics complicate comparisons of the results and the reaching of 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 or conflict in their own right. Let us elaborate on these points, relying on Table 2 as a point of reference.

One issue raised 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 show that "wars between allies are about three times more likely than one would expect . . . " This is a truly novel, counterintuitive hypothesis.[4] Maoz and Russett (1992) provide a rather stirring defense of that hypothesis. But Bremer argues that the positive bivariate relationship between alliance ties and the probability of conflict is misleading. He even suggests that it is contiguity that brings about this misleading positive, bivariate relationship, and supports this argument by providing multivariate analyses in which contiguity is included and in which the relationship between alliance ties and the probability of conflict turns negative. However, his analyses contain so many other control variables that it is impossible to tell whether it is in fact contiguity that changes the positive coefficient in his bivariate analyses to a negative coefficient in the multivariate models. Surely, readers would guess, this must be a rather straightforward issue that has been sorted out in subsequent years.

Guess again. 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." They control for five other relevant variables. Which of those changes the relationship between alliance ties and conflict involvement from positive to negative? There is no way of knowing.

Table 2 shows that while some studies report a significant negative relationship between alliance bonds and the probability of conflict (e.g., Oneal et al., 1996; Oneal & Russett, 1997; Russett, Oneal, & Davis, 1998; Reed, 2000; Russett & Oneal, 2001; Henderson, 2002), several suggest instead that the relationship is not statistically significant (Barbieri, 1996, 2002; Bennett & Stam, 2000; Oneal & Russett, 1999a). Each of the multivariate analyses in every one of these articles taken alone, and certainly in the aggregate, are sufficiently complex as to preclude any confident conclusions about the impact of alliance ties on conflict. It is also impossible to know which factor, if controlled for, changes an oft-reported positive, bivariate relationship between alliance ties and conflict to a negative relationship in several multivariate models.

Maoz and Russett (1992) found, to repeat, that political stability might account for the fact that democratic states do not fight one another. The analyses they present showing that

[4]Ray (1990) reports some modest supporting evidence for this hypothesis.

TABLE 2 Predictor variables in multivariate models of interstate conflict from a sample of analyses of dangerous dyads

Independent variables	Maoz and Russett, 1993	Oneal et al., 1996	Barbieri, 1996	Oneal and Russett, 1997	Russett, Oneal, and Davis, 1998	Oneal and Russett, 1999b	Reed, 2000	Mousseau, 2000	Bennett and Stam, 2000	Russett and Oneal, 2001	Henderson, 2002	Barbieri, 2002
Democracy	—	—	—				NS				NS	NS
Democracy, low				—	—			—	—			
Democracy, high				+	+							
Democracy, low–avg.												
Democracy						—				+		
Avg. democracy						—				—		
Political similarity											—	
Wealth	—											
Growth	—	—			NS		—				—	
Economic growth				+								
Development, high								—				
Development, low								+				
Development * democracy								—				
Alliance	—	—	NS	—	—	NS	—		NS	—	—	NS
Contiguity	+	+	+	+	+	+		+	+	+	+	NS
Distance						—		—	NS	—		
Capability ratio	—	—		—	NS	—	+	—	—	—	—	NS
Interdependence		—			NS		NS	—				+
Interdependence, low				—					—		—	
D Interdependence		—	+									
D Interdependence, high				—								
Interdependence trend					—							
Avg. interdependence						—						

TABLE 2 Predictor variables in multivariate models of interstate conflict from a sample of analyses of dangerous dyads *(Continued)*

Independent variables	Maoz and Russett, 1993	Oneal et al., 1996	Barbieri, 1996	Oneal and Russett, 1997	Russett, Oneal, and Davis, 1998	Oneal and Russett, 1999b	Reed, 2000	Mousseau, 2000	Bennett and Stam, 2000	Russett and Oneal, 2001	Henderson, 2002	Barbieri, 2002
Avg. trade										—		
Trade, low-avg. trade						—				—		
Salience			—									NS
Symmetry			—									NS
IGOs					—				NS			
IGO-avg. IGO						—				—		
Avg. IGO						NS				NS		
Power Status								+				
Both minor powers						—			—	—		
Hegemonic power						NS		—		NS		
Hegemonic defense burden						+						
Hegemonic status								+				
Joint Satisfaction						NS	NS			NS		
Democracy, low *Contiguity				NS								
Economic growth *Contiguity				—								
IIG equilibrium									NS			

stable states are peaceful whether or not they are democratic, and unstable states tend to be conflict prone whether they are democratic or undemocratic, is precisely the kind of evidence that is needed to show that a relationship is spurious, that is, that the correlation in question is brought about by a prior third factor that causes both the key independent variable and the outcome phenomenon. Although Maoz and Russett (1993) include political stability as a variable in a model 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, for all we know, might alter many of the results shown in Table 2.[5]

A common thread running through virtually all of the results shown in Table 2 is an emphasis on the relationship between democracy and peace. Reed (2000) is an exception. But he does report, in a stage of his 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, where he concludes that democracy is not related to the probability that disputes will escalate to war, he includes several control variables such as capability ratios, joint satisfaction, alliance ties, GDP growth, and interdependence.

I would argue that none of those are legitimately included as a control variable in a model aimed at evaluating the potential causal impact of democracy on peace. What should be included are potentially *confounding* variables, i.e., "*antecedent* third factor[s] that [bring] about a statistical association or correlation between two other variables" (Ray, 2003, 4), thus making that association spurious. In other words, Variable C is a confounding variable if it leads to or is a cause of both Variables A and B. *Intervening* variables, in contrast, are variables that intervene in a causal process leading from Variable A to Variable B, such that A leads to C (the intervening variable), which in turn leads to B. Controls for confounding *or* intervening variables will both tend to reduce a key relationship to 0. However, as Ray (2003), as well as King, Keohane, and Verba (1994, 173) argue, "In general, *we should not control for an explanatory variable that is in part a consequence of our key causal variable*" (emphasis in the original). Controls for intervening variables can eliminate the correlation between key causal factors and the outcome phenomenon, creating the impression that the key, hypothesized causal factor is in fact not related to the dependent variable in question.

Joint democracy *might* have an impact on capability ratios, joint satisfaction, alliance ties, GDP growth, and interdependence, for reasons that we cannot elaborate on here for lack of space. But if those reasons are valid, then Reed's (2000) conclusion that democracy is not significantly related to escalation to war, in light of the fact that he includes all of those potentially intervening variables in his multivariate model, 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 Henderson could point to many precedents to defend his choice to do the same. But in my view, all those precedents are unfortunate, even though none of them, it turns out, found that the inclusion of intervening variables reduced the

[5]Maoz and Russett (1992) do not provide *definitive* evidence that the relationship between democracy and peace is spurious. In the first place, to repeat, their dependent variable is MIDs, not war, so their evidence does not establish that there is no correlation between democracy and peace, if the latter is defined as the absence of war (rather than the absence of a MID). There is also the possibility that democracy leads to stability (see, e.g., Gurr, 1974), rather than, or in addition to, stability leading to democracy. If so, then a control for stability would eliminate the relationship between democracy and peace because it is an *intervening* rather than a *confounding* variable. The distinction between intervening and confounding variables will be discussed in due course.

relationship between democracy and peace to statistical insignificance. Furthermore, one reputedly confounding variable relied on by Henderson is political similarity, which is related to joint democracy 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, to repeat, 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.

As we noted above, Bremer (1992) found a positive bivariate correlation between wealth and conflict, which turned negative when wealth was embedded in a model containing several control variables. Maoz and Russett (1992) report that in general wealthy pairs of states are less likely to get involved in disputes. Maoz and Russett (1993) also report a negative relationship between wealth and conflict, but 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 confounded by the fact that for Maoz and Russett (1993), the measure of wealth reflects not only the degree of wealth in a pair of states, but also the differences in the level of wealth within dyads. This creates an index that reflects the interaction of wealth and differences in wealth in such a way that it is impossible to disentangle the relationships between wealth, differences in wealth, and conflict. And it is impossible to say from this analysis what the impact of wealth might be on the relationship between democracy and peace.

Oneal et al. (1996) exclude wealth as a control variable because "it never proved significant when INTERDEP [that is, trade-based interdependence][6] was in the equation." Since a main point in the process of being developed at this stage of the paper is that analysts typically include too many control variables, it might seem incongruous to criticize the exclusion of wealth by Oneal et al. (1996). But I am not in fact suggesting the addition of wealth to the list of control variables already included in the models of Oneal et al. (1996). Rather I want to assert that it might have been a mistake to exclude wealth from all further analyses only because it had no significant relationship with conflict in analyses with several other predictor variables.

Allow me to elaborate on this assertion by pointing out that my own high-powered medical research based on the internet (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 or accounting for the probability of heart attacks are heredity, or family history, cigarette smoking, high levels of 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 effects of the factors listed in Figure 1 based on the model construction strategy of Oneal et al. (1996) might find that hereditary factors, as indicated by family history, have no impact at all. People with parents who are/were smokers, and had high cholesterol levels, high blood pressure, and a tendency toward obesity are probably more likely to be smokers, have high cholesterol, and, high blood pressure as well as 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

[6]Let me suggest here in a footnote, to which such futile gestures are rightly relegated, that the use of such acronyms in any published work, perhaps even in tables, ought to be avoided for the sake of clearer communication. They are fit only for communications between adults and their own personal consenting computers.

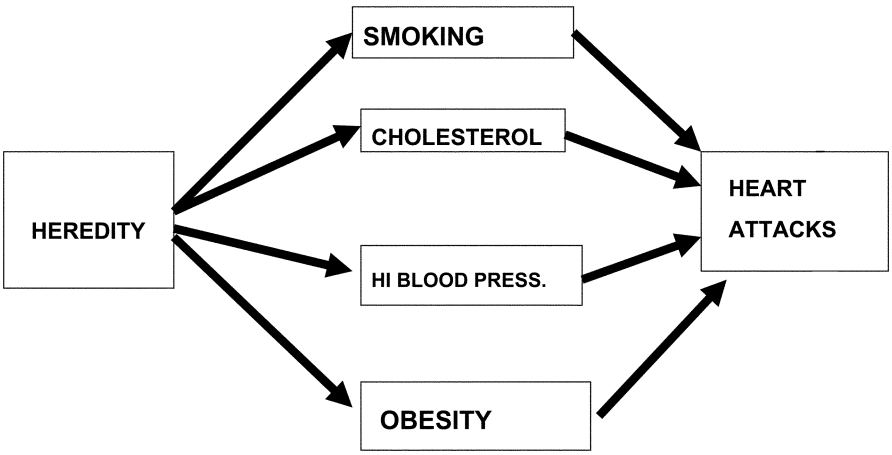

FIGURE 1 Plausible interrelationships among factors leading to heart attacks.

certainly be erroneous to conclude from such a result that genetic factors, or family history, have no important impact on the probability of heart attacks.[7]

Similarly, if the relationship between wealth and conflict is not significant in a model including, for example, interdependence and joint democracy as additional independent variables, it might be a mistake to conclude that wealth has no important impact on conflict propensity. It is also impossible to tell in the analyses presented by Oneal et al. (1996) whether or not wealth might be a confounding variable creating a spurious negative relationship 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, (2) more likely to trade a lot with each other,[8] and less likely to engage in interstate conflict.

When interdependence is included in the model presented by Oneal et al. (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 joint democracy and peace within dyads. This is a sign, perhaps, of troubles to come.

Perhaps in part because Oneal et al. (1996) dropped wealth from their model, it has tended to be absent from most subsequent work. Mousseau's (2000) paper is an exception. He argues quite persuasively that it is only among relatively *wealthy* states that democracy has a pacifying impact. There are reasons to doubt this conclusion. 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 between the impacts of each. Furthermore, though Mousseau is virtually alone among the analysts

[7]A reviewer suggested that I seem to be arguing here against a type of analysis that medical researchers in fact rely on quite regularly and successfully to uncover "risk factors" for heart diseases and other illnesses. I am not necessarily arguing that medical research should avoid what might be described as eclectic statistical fishing expeditions to uncover risk factors. I am just saying that if they were to construct a fishing expedition of the sort depicted in Figure 1, and the causal relationships at issue were structured in the manner I have posited, the results would almost certainly be misleading.

[8]Oneal et al. (1996, 18) acknowledge that "economically advanced countries have been among the most interdependent in the postwar period."

included in Table 2 to show sensitivity to the problem created by including intervening variables in models aimed at evaluating the impact of key factors or focal variables,[9] he nevertheless includes variables (such as trade interdependence and relative capability) in his most important model that might intervene in the process leading from democracy to peace. Thus, his estimate that democracy by itself has no important effect on the probability of conflict might be faulty.

Many, probably most, of the papers we have discussed in this brief review of some systematic empirical analyses of dangerous dyads provide estimates of "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. This information is of limited utility. Adding or deleting even one variable even in a rather large multivariate model can notably change the estimates of the size and even the signs of many coefficients, and even more so any estimate of the "substantive" effects of all the variables in the model. Even a brief perusal of the different sets of explanatory variables in Table 2 or in the literature generally proves beyond any doubt that there is no widely accepted, theoretically based agreement on which variables ought to be included in a multivariate analysis of dangerous dyads or interstate conflict in general. Therefore, any estimates of "substantive impact" of the type that are quite ubiquitous by this point are basically arbitrary, and not generalizable.

What Is to Be Done?

Evidence supporting the argument that such estimates of substantive impact are of limited utility is presented in a seminal article regarding the construction of multivariate models by Leamer (1983). In this paper, entitled "Let's Take the Con out of Econometrics," the author asserts that "though the number of observations of any phenomenon is clearly limited, the number of explanatory variables is logically unlimited" (Leamer, 1983, 34). He also observes that most inferences based on econometric analyses are "whimsical" in character, because "the list of variables [included in multivariate models] is partially conventional, often based on whatever list the first researcher happened to select" (p. 38).

To address this problem, Leamer (1983) suggests a method of analysis that has come to be known as "extreme bounds analysis," or EBA. He provides an example addressed to the deterrent value of capital punishment. In order to determine whether or not capital punishment does have deterrent value, he examines data on murder rates in states of the United States. He finds that a simple regression of murder rates on the probability of execution for convicted murderers, along with 12 control variables such as median income, unemployment rates, and percent of population in urban areas, suggests that executions have a clear, negative impact on murder rates. But when he includes different subsets of the control variables as might be suggested by "right wingers" or "bleeding hearts" or "rational maximizers" the estimates of the deterrent potential of capital punishment vary so dramatically (from strongly negative to strongly positive) that he concludes that "any inference from these data about the deterrent effect of capital punishment is too fragile to be believed" (Leamer, 1983, 42).

Would estimates of "substantive impact" so common in most recent articles on interstate conflict be more credible if they were based on "extreme bounds analysis?" I doubt it. Entering "Extreme Bounds Analysis" into Google Scholar returns some 61,500

[9]For example, at one point, Mousseau (2000, 488) says that "alliance links are intervening rather than confounding factors in any explanation of market democracy on international behavior. Statistical control for this variable is thus not appropriate."

citations,[10] (including many very recent ones), so it should not be dismissed too lightly. But as one set of critics argues, "the most serious defect in EBA becomes transparent: unless extreme bounds are presented for *all* possible classifications of variables... an observer cannot be certain that the selection does not constitute a 'con job...' EBA users report results for only particular variable categories and so are as arbitrary and selective in their modus operandi as the practices they criticize and claim to be improving on" (McAleer, Pagan & Volker, 1985, 298).

One possible solution to this problem is (believe it or not) to look at *almost* all possible classifications of variables, as argued by Sala-I-Martin (1997) in "I Just Ran Two Million Regressions," an article published, I might point out, in the *American Economic Review.* But an admittedly cursory review of practitioners of EBA, as well as Sala-I-Martin's article based on 2 million regressions (an earlier unpublished version of the paper was based on 4 million regressions), reveals that they are typically seemingly oblivious to or uncaring about problems that can be created by including intervening variables in the multitude of models they construct. The result is that there is a distinct tendency to conclude that *no* finding or coefficient is stable. Levine and Renelt (1992), for example, examine a reasonably exhaustive list of variables that are putative causes of economic growth and find that virtually none can withstand the rigors of EBA. Sala-I-Martin solves this problem by taking averages of results from huge numbers of analysis, but his results are subject to the impact of intervening variables as depicted in Figure 1. (He concludes, for example, that in spite of several reports to the contrary, ethnolinguistic fractionalization has no impact on economic growth. But many of the variables he includes in his models might intervene in any process leading from fractionalization to growth.)

Note, too, that Leamer's discussion of EBA in his article published in 1983 is aimed at fundamental, even elementary questions of whether the coefficient for capital punishment is negative and significant, not at an evaluation of its "substantive significance." If shifts in the sets of control variables included in alternative models make it impossible to come to any definitive or stable conclusions regarding the statistical significance or the signs of coefficients, how likely is it that estimates of "substantive significance" would withstand even the most preliminary scrutiny?

I would argue that at least an important step in the direction of resolving some of these issues would be to replace the question to which most multivariate models are apparently aimed. Instead of, "Does the relationship between the key variable X and outcome phenomenon Y persist if several control variables are added to a model?" the primary question to which multivariate models might more fruitfully be addressed would be, "Is there a correlation between variables X and Y, and is that relationship spurious?" Generally, models best suited to address that question effectively will have one control variable, that is, the alleged confounding variable. Any single paper, of course, could have large numbers of such trivariate analyses, and a set of trivariate analyses could serve as a basis for larger multivariate models.

This strategy of constructing multivariate models would not necessarily suggest that relationships of interest would be relegated to the ash heap of history simply because one potentially confounding variable is found that wipes out that relationship. The discovery of a variable that if controlled for does eliminate the key relationship should instead lead immediately to additional investigations directed toward the question of whether or not the relationship between the putative confounding variable and the key independent variable, or the relationship of the confounding variable with the outcome variable, might be spurious. And if it is discovered that either one of those relationships is spurious, then that potentially

[10]Entering "Democratic Peace" into Google Scholar currently generates some 48,000 citations.

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	*−.180*	−.024	*−.322*
	8.56	*−1.47*	*−0.15*	*−2.57*
	0.00	*0.14*	*0.88*	0.01
Contiguity		3.88		3.83
		28.45		*17.79*
		0.00		*0.00*
Distance			−1.09	−.287
			−14.97	*−4.52*
			0.00	*0.00*

Note. 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.

confounding variable should be excluded from future attempts to analyze the impact of the key variable on whatever outcome is to be accounted for.

This type of approach to multivariate modeling would, I believe, have led to more productive research in the wake of the earliest analyses of democratic peace. If the type of strategy of multivariate model construction advocated here had been widely relied upon in the last 10 or 20 years, perhaps we would not still be wondering if wealth confounds the relationship between democracy and peace, or whether alliance ties do reduce conflict between states, or whether political stability or political similarity might confound the relationship between democracy and peace.

Let us elaborate on this point by applying the kind of strategy of multivariate analysis I have in mind to an issue addressed in what is now the standard manner. The example analyzed will rely on a data set made available by Gartzke and Li (2003), who in turn rely on data sets originally generated by Oneal and Russett (1997, 1999a). 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 the series of inconsistent results mentioned above. Table 3, based on the data set relied on by Gartzke and Li, shows that pairs of allied states are more likely than 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 indicator of the geographic relationship between two states, does not by itself produce a negative relationship between the presence of alliance ties and the onset of MIDs. It takes controls for both contiguity and distance simultaneously[11] to reveal a negative relationship between alliance ties and the onset of MIDs (see Model 4). The analyses reported by Gartzke and Li (2003) are too complex, containing too many control variables to facilitate clear conclusions about the relationship between alliance ties and the probability of conflict, conclusions that could be based on the simpler, more incremental type of multivariate model building advocated here.

[11]Such controls for "independent" variables that are in fact related to each other by definition are not in general to be recommended. (See Ray, 2003.)

TABLE 4 Logit analysis of the relationship between trade interdependence and the probability of MID onset, all dyads, 1950–1992, with controls for 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	.024	.024	−.600
	9.78	*2.00*	*2.17*	*−6.59*
	0.00	*0.05*	*0.03*	*0.00*
Contiguity		3.90	3.39	3.22
		21.27	*15.26*	*13.86*
		0.00	*0.00*	*0.00*
Distance			−.277	−.352
			−3.83	*−4.50*
			0.00	*0.00*
Dependence				.567
				6.77
				0.00

Note. 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.

A persuasive response to this example 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 their analyses are possibly misleading in their implications about that relationship is at best a minor problem. However, in the case of Gartzke and Li (2003), and in the multitude of papers utilizing similar multivariate models, the results can be uninformative and even misleading, even about relationships of central interest.

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 (but is it?) that states that trade extensively with each other are also more likely than other pairs of states to become involved in serious disputes with each other. Table 4 shows a strongly positive bivariate relationship between trade interdependence and the probability of the onset of MIDs.

However, it is clear that the bivariate relationship between trade and conflict might be positive mostly because both trade and conflict are affected so fundamentally by geographic relationships 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. Therefore it is logical to expect that a control for contiguity will be necessary to uncover any pacifying impact that trade might have. Somewhat surprisingly, however, the results of an analysis of Model 2 in Table 4 show that even with a control for contiguity, the relationship between trade interdependence and conflict for all pairs of states in the system from 1950 to 1992 is *positive*, and significantly so. That relationship remains, as shown in the results of the analysis of Model 3 in Table 4, positive and significant even if *both* contiguity and distance are controlled for. It is only when, as in Model 4 in Table 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) is

added to the model that the relationship between trade *interdependence* and conflict turns significantly negative.

The results presented by Gartzke and Li (2003) seem to indicate that the positive relationship between trade dependence and conflict supports arguments by Barbieri (2002), while the negative relationship between trade interdependence and conflicts supports the arguments by Oneal and Russett (1997, 1999a). This is arguably a misleading interpretation of the data analyzed by Gartzke and Li. (I hasten to add that Gartzke and Li's approach to multivariate analysis is virtually identical to that relied on in virtually all of the research discussed in this paper.) Analyses of the data (not reported here) provided by Gartzke and Li (2003) show that the positive relationship between trade dependence and conflict exists on the bivariate level, and with a control for geographic proximity. Such a result does support theoretical arguments by Barbieri (2002) regarding the conflict-inducing impact of trade. The impression that the data analyzed by Gartzke and Li also support arguments by Oneal and Russett (1997, 1999a) is arguably misleading, since they do not argue that trade *interdependence* has a negative impact on conflict only if trade *dependence* is controlled for. The misleading impression is created by multivariate analyses of Gartzke and Li (2003) that are sufficiently complex to obscure the fact that even with contiguity and distance controlled for, the relationship between trade interdependence and conflict is positive.

The point here is *not* to make anything like a definitive or even authoritative contribution to the debate about whether international trade usually dampens or exacerbates international conflict. Oneal (personal correspondence, April 14, 2005) has informed me that his analysis of the same data ostensibly relied upon by Gartzke and Li shows that the relationship between trade interdependence and conflict is negative and significant with only contiguity controlled for. Any attempt to resolve this apparent contradiction between results obtained here with the data provided by Gartzke and Li and results reported to me by Oneal is (thank God) outside the scope of this paper. The argument here is that the results of analyses of models constructed in the fashion relied on by Gartzke and Li (and virtually everybody else), even assuming that their data are free of errors, tend to be misleading. That argument is intended to be at least a gentle criticism not so much of the paper by Gartzke and Li, but of many analyses of "dangerous dyads" since Bremer (1992) coined the phrase.

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 sometimes produced inconsistent or misleading findings. Many of these inconsistencies and the confusion they produce are both products to an important extent of multivariate models too complex to produce results that can be interpreted in a confident, consistent fashion. Multivariate analyses focused more directly on the revelation of spurious relationships, rather than currently more common models that contain eclectic (or even haphazard) combinations of rather large numbers of control variables, would produce results that are more transparent, interpretable, and generalizable.

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