Rule of Three, Let it Be? When More Really Is Better

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Abstract

Jim Ray and others in this issue raise serious questions about customary
procedures for the quantitative analysis of theoretically complex questions in the social
sciences. In this article we address Ray's use of work on the Kantian peace to illustrate
his points. We discuss how we agree and disagree with his five guidelines for research,
and then take up five substantive issues he has raised about the Kantian peace. With new
analyses to supplement our previous work, we show that none of his reservations is well-
founded. We discuss the costs as well as the benefits of rigid insistence on reducing the
number of independent variables in an equation.

The basic argument in Jim Ray's two articles (2003, 2005) is this: Too many independent variables in regression analyses spoil the pot. An excessive number can often, through complex interactions, produce results that are not stable from one specification to another, so we should follow Chris Achen's (2002) Rule of Three (ART) for parsimony, limiting ourselves to no more than three in the absence of a formal theory. Ray cites a number of works in international relations, published in leading political science journals, that violate this rule. He could also have cited research in other subfields of political science or articles in the major outlets of economics, sociology, or psychology. Research on economic growth routinely violates the rule of three. Robert Barro (2001), for example, estimates the effects of fourteen variables on the growth rate in per capita gross domestic product. Barro was the second most frequently cited economist between 1990 and 2000 (Coupé forthcoming). Science is, of course, not a democracy, but can so many authors, editors, and reviewers be wrong?

We agree with Ray's call for greater attention to the specification of regression models, to a point—but Ray gives as much bad advice as good. If applied rigidly, the rule of no more than three independent variables is as bad as rigorously following the poison ivy rule, which would have us avoid all the useful three-leaved plants just to escape those that leave an irritating rash.

Evaluating Ray's Five Guidelines

We generally agree with the first of Ray's (2003) five guidelines: "Do not control for intervening variables." We concur that in research on the Kantian peace alliance ties are best conceptualized as intervening. In that theoretical context, alliance is less a truly

independent influence on the risk of conflict between two states than a consequence of political and economic influences which more directly affect the risk of conflict. That is, alliances reflect not only strategic power realities, but also, as in the cold war, shared political preferences and commercial interests.[1] Of course some reverse causality may also apply from alliance to the other variables, in which case alliance would become what Ray refers to as a confounding (or competing) variable.

We included it in our model to meet objections (e.g., Gowa 1999) that alliances, especially in the Cold War era, account for the reduced risk of violent conflict that has been attributed to democracy. We were able to show that the alliances formed by democracies are not the principal reason they do not fight one another; non-allied democracies also are peaceful. In our best causal analysis using distributed lags (Oneal, Russett, and Berbaum, 2003), alliance actually showed a weak *positive* influence on the risk of violence. In light of these results, we would not now object to analyses that omitted it as a bow to parsimony; but its inclusion permitted us to say something about the means by which democracies are able to avoid conflict. The role of alliances is less than Gowa and many others suspected.

More emphatically, we regard preferences as measured in UN voting affinity (Gartzke 1998) as an intervening variable, and indeed show (Russett and Oneal 2001: 229-37) that the direct effects of democracy and trade on conflict hold at about the same level whether or not one includes some indirect effect through affinity. We appreciate the clarity of Ray's statement.

Ray's Guideline #2, "Distinguish between complementary and competing explanatory variables," sounds helpful, but some of the conclusions he draws from it are

not. We deliberately constructed our analysis to test Kantian (liberal) theory against realist theory. Realists offer several variables, individually and in combination, as competing explanations for the Kantian peace. Relative power, as measured by the capability ratio, is obviously at the heart of any realist theory. So too are alliances, geographic proximity, and the wide-ranging capacity of a major power to overcome many of the limitations of distance. If we had omitted any one of these variables we would have been rightly criticized for under-specifying realist theory, to the relative advantage of our liberal model. Our goal was to test the liberal view in a way that gave a "fair shake" to the *realist theory as a whole*. After all, early findings suggesting the possibility of a democratic peace (e.g., Russett and Starr 1981, ch. 15; Rummel 1983; Doyle 1986) were properly criticized for omitting competing realist variables.[2]

Our inclusion of three complementary and partially correlated Kantian variables (democracy, trade, and shared IGO membership) gives us only a limited ability to identify precisely their individual contributions. But not to do so would disadvantage the *liberal model as a whole*. Furthermore, the project moved beyond the democratic peace because one of us (Oneal) was concerned about the possibility of omitted-variable bias due to the exclusion of any measure of economic interdependence. Overall, our tests with all three liberal variables generally do show significant independent contributions from each to reducing the risk of violence. But since the relative impact of the three variables varies somewhat depending on the data, model specification, spatial and temporal domain, and other factors, we have avoided asserting that one was necessarily most or least important (Russett and Oneal 2001: 39-41, 172-73).

Ray's Guideline #3, "Do not introduce factors as control variables merely on the grounds that they have an impact on the dependent variable" leads him to endorse ART. Achen (2002: 446) allows that ART would not apply if a formal model specified a larger number of variables, and Ray (2003: 14) is "inclined to be even more generous . . . even if the theory providing justification for each variable in the model were not *formal*, strictly speaking . . ." if ". . . the variables in the model are the complete set of factors necessary to explain the outcome phenomenon." Our theory was not derived in a formal model, but we believe it was carefully articulated.[3] Theoretical justification for the specification of regression analyses is important; and we do not, of course, contend that our statistical analyses contain the *complete* set of factors necessary to explain interstate conflict—nor do we consider that aspiration realistic. Such a counsel of perfection is not helpful. The great danger with limiting analyses to no more than three independent variables is that we will end up with numerous islands of theory without knowing their commensurability or relative importance.

Ray's Guideline #4, "Do not control for variables that are related to each other or the key explanatory factor by definition," also makes sense only up to a point. For example, we agree that political similarity and democracy are related to each other by definition (though the former is of course broader), that political similarity as defined cannot possibly be termed a cause of democracy, and that to include both in an equation to explain conflict (Henderson 2002) is not an acceptable way to assess the effects of joint democracy on the likelihood of peace (Ray 2003:15-17).

But his subsequent application of this guideline to the inclusion of both distance and contiguity as explanatory variables—especially when trade or alliances are also

included in the regression—is mistaken. They are related by definition and thus correlated, but each taps part of a concept (geographic proximity) that is an important confounding influence on the relationship between trade and conflict: all else being equal, proximity increases states' opportunity and willingness to engage in conflict and increases trade by reducing transportation costs. Ray recognizes that without a control for proximity trade will appear positively related to conflict, rather than negatively when the controls are introduced, but objects to including both distance and contiguity. The point is to control for the effect of proximity, of which both distance and contiguity are imperfect, complementary measures. Distance is typically measured between states' capital cities, with valuable interval qualities. Yet two countries may be contiguous but have capital cities quite far apart (e.g., Moscow and Beijing). Including contiguity picks up the greater likelihood of trade or conflict for such a pair of states. A single combined measure might record distance as zero for all contiguous countries, yet for Russia and China that would exaggerate their closeness because their centers of political and economic activity are more distant. It would be best to modify contiguity for topography and ease of border crossing (Starr and Thomas 2002); but until long-term global data are available, and agreement emerges that it is the most appropriate measure, we defend the use of *both* distance and contiguity (Oneal and Russett 2003).

This leaves us with Ray's Guideline #5, "Control for possible differences between across space and over time relationships," and his attendant empirical analysis. We agree with Ray, and Achen, that both spatial and temporal variation in the relationships are likely, and in principle applaud efforts to explore such variation systematically.[4] Nevertheless, Ray's own empirical illustrations do not support him. He cites Zorn's 2001

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analysis, which suffers from some, though certainly not all, of the limitations of Green, Kim, and Yoon's (2001) fixed-effects analyses.[5] It is restricted to a relatively short period of time during which both key independent variables (trade and democracy) and the dependent variable (conflict) showed little variation over time for many of the dyads, notably those dyads falling on one side or the other in the cold war. Hence it is not surprising that the temporal relationships were different from the spatial ones. As we showed in our response to Green et al., the expected relationships reappeared as soon as the analysis was extended to a longer time period (1885-1992) with much more temporal variation. Recognizing that possibility provided the motivation for us to painstakingly expand our data set beyond that found in our early analyses of the post-World War II era.

Ray's second article (2005) covers some of the same ground as the first, including the invocation of Achen's rule of three (2002); but here he takes a longer look at conflicting empirical results, and actually suggests additional variables that should be included but rarely are![6] The thrust of his review of past research is that including or excluding specific variables from analyses already burdened by too many variables can dramatically change the relative size of the coefficients and even their signs. As our remarks above indicate, we agree that this can sometimes happen, most egregiously when the effects of geographic proximity are not properly taken into account. But look at his Table 2 covering six studies.

Oneal, Oneal, Maoz, and Russett (1996) drop a variable (wealth) from Maoz and Russett (1993) and add two new ones (trade interdependence and change in interdependence) to move from the democratic peace to a more comprehensive liberal peace. As Ray notes, the then-current continuous measure of joint democracy produced

noticeably weaker results than did a simple dichotomous measure indicating whether

both states are democratic or not. But we did not "resort" to a dichotomous measure "to

preserve" anything. Maoz and Russett had already used both, and noted that despite their

conceptual preference for a continuous measure the one they had been using suffered

from some measurement problems that made it less than optimal (Maoz and Russett

1993: 629) Oneal and Ray (1997) explored this issue in considerable detail.[7] Barbieri

(1996) produced different results for trade and alliances; but her research had a number of

differences in specification, case selection, measurement, and data so it took time to

figure out why her results were different (Oneal and Russett 1999, Oneal and Russett

2003, Gartzke and Li 2003), especially since her data were unavailable until recently.

What Ray's Table 2 actually shows for the three subsequent analyses by Oneal

and Russett is that even as trade and IGOs were added to democracy to form the core

variables of the Kantian model, and others were subtracted because they were not

robustly related to the probability of conflict, all three core variables, including the trade-

to-GDP ratio (significant at the .09 level in a two-tailed test in Russett, Oneal, and Davis

1998) remained statistically significant, as they have been in most of our subsequent

analyses (e.g., Russett and Oneal 2001; Oneal, Russett, and Berbaum 2003; Oneal and

Russett 2004).[8]

Moreover, in the six publications that Ray (2005) reviews, there are numerous

methodological differences from study-to-study, so to attribute the differences in results

to changes in the list of independent variables is unsupportable. He occasionally

mentions such other differences, but generally ignores them. Considering the different

decisions in research design that have been made, and that the applied statistical methods

for analyzing this kind of data evolved tremendously over the 1990s, one should expect some differences in the results, and that later studies would correct and replace earlier ones. Decisions on which independent variables to include are arguably among the least important.[9] It is a wonder that our findings could remain so robust through so many changes in analyses extending over a decade.

New Analyses for Ray's Five Substantive Issues

In discussing his guidelines for research, Ray (2003, 2005) raises five substantive issues.

1) He claims, without providing new evidence or citing previous research, that the causes of war and of militarized disputes are different.

2) He questions the robustness of the evidence that has been presented for the conflict-reducing effect of economic interdependence, arguing that these results depend upon a particular, complex specification that is inappropriate.

3) He argues that our efforts to test liberal and realist theories generally produce meaningless results because of the complexity of our regression analyses.

4) He presents results indicating that the effects of the liberal (and realist) variables in our model differ when estimated with information from time series rather than cross-sectional variation.

5) He suggests that the pacific benefits of democracy and interdependence are spurious and their conflict-reducing effects probably attributable to economic development, though he gives no evidence for this claim.

We now address each of these points with new statistical analyses. We find that none of his five reservations is well founded. We discuss them in order.

1) Disputes, Fatal Disputes, and Wars

Table 1 gives the results of three logistic regressions showing that the causes of wars and lower level conflicts are substantially the same. In column one, the dependent variable is the onset of a militarized interstate dispute (MID) of any severity; in column two, the onset of a fatal militarized dispute; in column three the onset of wars, fatal disputes with at least 1000 deaths in battle. We use the latest data from the Correlates of War project (version 3.04) downloaded from EuGene (2004), Gleditsch's (2004) data on trade and gross domestic products (version 4.1) for the post-World War II period, and our data for the years before World War I and the interwar years (Russett and Oneal 2001) which we have carefully compared to Barbieri's (1998). We focus on the effects of the liberal variables democracy and interdependence, and alliances and the balance of power from realist theory, while controlling for contiguity, the distance between capitals, states' status as major or minor powers, the years of peace since the last dyadic conflict, and the number of the states in the international system.

We include the lower and higher democracy scores in the regressions because past research indicates that democracies and autocracies are particularly likely to fight one another. We do not include the higher trade-to-GDP ratio because it has never proven statistically significant. Thus there is no evidence that asymmetric interdependence increases the risk of conflict.[10] The specification in Table 1 is essentially the model of interstate conflict that Ray (2003, 2005) criticizes. We add a control for the number of

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states in the system to take into account the dramatic growth in the number of sovereign nations that has occurred since World War II.[11]

Table 1 shows that the causes of militarized disputes, fatal disputes, and wars are quite similar. Democracy, economically important trade, and power preponderance reduce the likelihood of conflict at all three levels of violence, while the effect of alliances is less certain; the control variables perform as expected. These results are consistent with our previous work. It is good to find that the causes of militarized disputes and wars do not dramatically differ. It simplifies theorizing and facilitates testing. Wars are particularly rare events. The analysis of disputes or fatal disputes increases sharply the number of positive outcomes and provides important protection against the misleading consequences of historical contingency.

It is important, as we have emphasized, to consider the substantive implications of our results. A finding that is statistically significant but trivial substantively would be uninteresting. In Table 2 we set the continuous variables in our regression analyses at their medians or means and estimate the probability of a fatal dispute. We then adjust the variables of greatest interest one at a time. We focus on fatal disputes, as Hegre (2004) and others have recommended, to reduce the bias in the reporting of less violent incidents. Use of force at even a low level in Western Europe, e.g., rifle fire across an international border, would not go unreported in the Western media from which the COW data are gleaned; such incidents are apt often to have gone unreported in some periods in large parts of Africa, Asia, or even Latin America.

The results in Table 2 are encouraging. Each of the liberal variables has a substantial, independent effect on the likelihood of military conflict. If both democracy

scores and the level of interdependence are increased simultaneously from the 50th to the 90th percentile, the probability of a fatal dispute falls from .0058 to .0011, an 81% reduction. Democracy and interdependence also make a significant difference in the probability of a dispute or a war. The risk of a MID drops from .0153 to .0078 if the democracy scores and level of interdependence are increased, a decline of 49%; and the probability of a war goes from .0007 to .0001, a drop of 85%. Analyses of the post-Cold War era, using an indicator for the years after 1989 and appropriate interactive terms (Oneal and Russett 2004), show that the pacific benefits of democracy and interdependence are if anything greater in the contemporary period.

2) Testing the Robustness of the Benefits of Interdependence

Ray (2005) calls into question the pacific benefit of economic interdependence in particular, arguing in part that results like those we have just presented are dependent on an overly elaborate specification. To make his case, he introduces several control variables one by one in an effort to show that economically important trade has a beneficial effect only in certain specifications. Though we do not believe that scholars should routinely limit their regression analyses to three independent variables, it is important to consider the robustness of the results that are reported. We would have to have confidence that a particular specification is the correct one if the results depended on that choice.

To show that commerce reduces military conflict even with a minimum of co-variates, we follow Ray's (2005) lead and introduce sequentially the controls we use in Table 1. The analysis of all militarized disputes in our Table 3 allows easy comparisons to Ray's Table 4. Our Table 4 shows the same analyses using fatal MIDs.

The results for all disputes and for fatal disputes are similar. The first column of Tables 3 and 4 gives the results of having only the lower trade-to-GDP ratio on the right-hand-side of the regression equation. The coefficient is positive as expected: both trade and conflict are correlated with geographic proximity. The second column includes a control for direct, state-to-state contiguity. Unlike Ray (2005), we have not yet included a count of the years of peace. In both Table 3 and 4, there is clear evidence that interdependence is *correlated* with lower levels of interstate violence when the influence of contiguity is taken into account. Ray (2003a,b) advises that we justify the controls we use, but he offers no explanation for evaluating liberal theory while holding constant the years of peace. The correction for duration dependence suggested by Beck, Katz, and Tucker (1998) is useful for addressing the issue of endogeneity (Oneal, Russett, and Berbaum 2003; Oneal 2004), and there is clear evidence that past conflict both increases the probability of conflict in the current period and reduces trade. Nevertheless, the inverse correlation of interdependence and the risk of conflict is useful in itself as a predictor of dangerous dyads. With the addition of the years of peace and its splines in the third column, the coefficient of the lower dependence score becomes insignificant in the analysis with all militarized disputes; but it is still significant at the .003 level in the estimation with fatal disputes.

The estimated coefficient of the lower trade-to-GDP ratio is statistically significant in both analyses when the log of the capital-to-capital distance is added to the model, as seen in the fourth column of the tables. It becomes much more significant when a control for major-power dyads is introduced. Taking into account the expansion of the size of the international system reduces the estimated coefficients of the lower

dependence score and their significance; but a high level of trade is still clearly associated with lower levels of conflict (p < .001, single-tailed tests).[12] More importantly, the effects of interdependence are substantively important. Based on the results in the last column of Tables 3 and 4, raising the lower dependence ratio from the 50th to the 90th percentile reduces the risk of any militarized dispute by 22% and the risk of a fatal MID by 60%.[13] These estimated effects closely resemble those reported for the full model.

How do we justify our controls? Taking into account the growth of the number of states in the system simply acknowledges that the probability of a dispute for any given pair of non-relevant states has declined over time as membership in the international system has expanded.

The previous section discussed why we need two measures of geographic proximity. They are not highly correlated (-0.50). Distance discourages both trade and conflict. Borders increase the incentives for conflict: states fight over boundaries, resources, and irredentist claims. A border makes using military force easier. Everyone agrees that we should control for contiguity in testing the influence of more theoretically interesting variables. But to exclude the capital-to-capital distance would suggest that states half-way around the world from one another are as likely to fight as non-contiguous states in the same region. It would imply that Argentina and Israel were equally likely to attack Iraq's nuclear installation in the early 1980s. We include major-power status because large states typically engage in more disputes than does a minor power (Bremer 1992). The United States can use military force even at great distance; Myanmar cannot. Without these controls we could not specify the reasons why small, distant countries are unlikely to fight. They usually have little bilateral trade because of

the distance separating them, and little conflict for the same reason—not little conflict because they have little trade. These controls are especially necessary when all dyads, rather than just the politically relevant pairs, are analyzed.

3) Further Tests of Robustness

Ray (2005) argues that our efforts to test liberal and realist theories produce meaningless results because of the complexity of our regression analyses. We have shown in Tables 3 and 4 that support for the pacific benefit of economic interdependence is evident even in models with minimal co-variates. As a further test of robustness, we ran a series of analyses of fatal disputes that included the four controls in Tables 3 and 4 and all possible combinations of the variables of greatest theoretical interest: the two democracy scores, which were always entered together; the lower dependence score; the indicator of an alliance; and the capability ratio. The trade-to-GDP measure is always associated with lower levels of conflict at least at the .007 level. The lower and higher democracy scores and the capability ratio are also highly significant in all these tests ($p <$ .001), whether entered by themselves or in combination with other theoretical variables. The effect of an alliance is more dependent on the other terms in the regression equation. It is significantly related to a lower level of conflict when only the control variables are included ($p < .03$), but it is significant at the .05 level in only three of six regressions when the various combinations of the other theoretical variables are added. It is not significant in combination with either the two democracy terms or the lower dependence score. These liberal influences give a better account of the incidence of fatal disputes than does an alliance, as much of our past work suggests (Oneal and Russett 1997; Russett and Oneal 2001; Oneal, Russett, and Berbaum 2003).

It is not surprising that, save for the effect of alliances, the results in Table 1 are robust. The correlations among the variables of greatest theoretical interest are not large. The lower and higher democracy scores are correlated at 0.47. The next highest correlation is between the lower dependence score and the lower democracy score (0.13) and between the dependence score and the alliance indicator (0.13). The lower democracy score and the alliance indicator are correlated at 0.10. Excluding the trade-to-GDP ratio and the alliance indicator from the regression, on the grounds that they are intervening variables, does increase the apparent effect of democracy on the likelihood of conflict. Making both states democratic in this restricted analysis lowers the probability of a fatal dispute by 60% relative to the baseline rate (compared to 50% in Table 2). Acceptance of the higher figure necessitates discounting the influence of factors other than democracy on the interdependence of states and the pattern of alliances. In any event, the differences in the estimates seem minor given the limitations of our data and methods. Whichever results are preferred, it is clear that democracy has a substantively important effect in reducing interstate conflict.

4) Cross-Sectional versus Time-Series Evidence

Ray (2005) also challenges our findings on the grounds that our pooled cross-sectional and time-series analyses mask important differences in the relationship of the key variables across space and over time. He reaches this conclusion by applying the method recommended by Zorn (2001). Variation across space is captured by the mean value of a variable for each dyad over the entire time the dyad existed. Variation through time equals the raw score of a variable minus its dyadic mean. Like Zorn, Ray finds evidence for the pacific benefits of democracy and trade only in the cross-sections, not in

the variation through time. Indeed, higher levels of trade through time are significantly associated with greater, not less, interstate conflict in the results he reports. This same issue was raised by Green et al.'s (2001) recommendation of a fixed-effects regression model to insure that the hypothesized relationship was supported in the time series.

We applied Zorn's (2001) method using the latest data, 1885-2001. We used fatal militarized disputes and limit ourselves to the simple specification that Ray prefers, estimating the effect of democracy and trade while controlling only for contiguity and the years of peace. Our results are in Tables 5 and 6. There is strong support for both components of the liberal peace. Economically important trade reduces conflict both in the cross-sections ($p < .004$) and over time ($p < .05$), and the estimated coefficients for the two effects (-56.6 across space, -39.0 over time) are similar, as shown in Table 5. The support for the pacific benefit of democracy in Table 6 is even stronger: the estimated coefficient for variation across space is -0.0903 ($p < .001$); it is -0.0630 over time ($p < .001$). Similar results were obtained when we used all the control variables and when we added the dyadic means and differences for the capability ratio and the alliance indicator. In the latter test, support for the peace-inducing effect of a preponderance of power was evident in both the time series and cross-national components. There was no support for the pacific effect of an alliance in the variation through time ($p < .46$), although the dyadic means of the alliance variable were associated with reduced violence ($p < .005$).

We also estimated a fixed-effects model using the specification from Table 1 and the method recommended by Green et al. (2001). The coefficient of the lower democracy score (-0.0466) was significant at the .002 level; the coefficient of the trade-to-GDP ratio

(-66.1) was also very significant (p < .003).[14] These results are consistent with those we

reported in our reply (Oneal and Russett 2001) to Green et al.

5) The Influence of the Wealth of Nations

Ray (2005) suggests that the pacific benefits we have reported for democracy and

interdependence are spurious, their apparent effects probably attributable to economic

development. He presents no evidence for this claim. Bremer (1992) was the first to

control for GDP per capita. He noted that developed economies tend to be more

economically interdependent and suggested that this might account for the conflict-

reducing benefit of development. Indeed, Oneal, Oneal, Maoz, and Russett (1996)

subsequently reported that GDP per capita was not significant when the trade-to-GDP

ratio was also entered in the equation.

Mousseau (2000) confirms this result, though he also reports a significant

interactive effect between development and democracy. The conflict-reducing effect of

democracy is conditional on states' economic development, a result consistent with the

importance of markets as a source of liberal values and institutions. Mousseau, Hegre,

and Oneal (2003) report that joint democracy is not a significant force for peace if one

democratic state in a dyad has a GDP per capita below $1400 (constant 1990 dollars).

Fortunately, this level of income is low enough that 91% of the democratic dyads in their

sample of politically relevant dyads, over the years 1885-1992, were in the zone of peace.

We have confirmed in tests to be reported elsewhere (Oneal forthcoming) that the

pacific benefits of democracy and interdependence are not a consequence of economic

development. Both the lower democracy score and the trade-to-GDP ratio remain very

significant (p < .001) when the lower GDP per capita in the dyad is introduced. We do

find evidence of an interactive effect between democracy and development, but in 2000 only one percent of democratic dyads were below the threshold level at which the peacefulness of democracy is nullified.

Finally, we address Ray's (2003, also 2005) identification of the two basic aims of multivariate regression models. He is right that our primary purpose over the past ten years has been to test specific hypotheses about the liberal or Kantian peace and to compare the explanatory power of the liberal and realist theories; but we do not share his apparent disdain for constructing "the best, most potent multivariate model" (p. 2) for the purpose of predicting which dyads specifically are dangerous. To form and evaluate policy requires us to know whether promoting democracy and economic interdependence is apt to encourage peace in general; but we should not dismiss social scientific efforts to predict *which* pairs of states are most likely to fight and when. Applying theory to real-world problems is justified when our recommendations are based on scientific analyses of general patterns of behavior (Morrow 2002). If the experience of economists attempting to forecast the U.S. economy is any guide, this is apt to involve more variables than three. "Another thing we've learned," Princeton economist Mark Watson has observed, "is that to forecast the economy, it is better to use 70, or even 700, variables rather than 7. There's more information in the economy than can be captured in a small number of variables. It's better to average the information in a large number of variables than to select a few up front" (International Monetary Fund 2004).

Conclusion

Since we opened with Ray's invocation of Achen's Rule of Three, we close with some of Achen's other remarks which are germane. Achen cites the finding of a democratic peace as one of two "important discoveries" that began when "empirical researchers surprised everyone with strong evidence." But he goes further. Subsequently it "engendered substantial decision- and game-theoretic literatures" (2002: 442) that refined our understanding of international politics in important ways. That still was not enough, in light of possible interactions and the fact that "different groups of people have unique histories, respond to their own special circumstances, and obey distinctive causal patterns" (2002: 443). Consequently political scientists need to develop "intimate knowledge of their observations that would constrain our choice of estimators and discipline our formal theories" (2002: 447).

We concur. That is why the Kantian program's empirical research over the past dozen or so years has concentrated on checking for the influence of competing or confounding variables, looking for interactions and non-linearities (e.g., Lagazio and Russett 2004). It found important instances of them in a process of cumulation and approach to consensus (Chernoff 2004). A dialogue, between generalizations and supporting theory on one hand and the experience of individual cases on the other, is essential to scientific progress and to meaningful policy analysis (Russett 1970, 2005).

Methodological decisions should be determined by our theoretical interests. If the arguments for including statistical controls are compelling and comparative theory testing requires including multiple independent variables, we should not limit ourselves to any set number of regressors. But such investigations do require a variety of robustness tests.

As Achen (2005) notes, a special problem arises when variables are measured on scales of dubious linearity. Complex relationships demand careful scrutiny. Scientific research is a time-consuming process, and our analyses are never final. To reach consensus may require scores of tests by independent researchers, proponents of a theory and critics.

King, Keohane, and Verba (1994: 20) provide a better guide than rigid adherence to the Rule of Three: "Parsimony is . . . a judgment, or even assumption, about the nature of the world: it is assumed to be simple . . . never insist on parsimony as a general principle of designing theories, but it is useful in those situations where we have some knowledge of the simplicity of the world we are studying." Quantitative analysts must not simply empty the garbage can, but neither should they subordinate theory to the demands of a statistical orthodoxy. Strict adherence to the rule of three would at best produce incommensurable theories of unknown relative importance. The world is not always simple. Recall Mozart's response to the Austrian emperor's complaint that his composition had "too many notes." Rather, he said, "It has just as many as the music requires."

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Table 1: Models of the Onset of Militarized Interstate Disputes, Fatal Disputes, and War, 1885-2001

Variable		All MIDs	Fatal MIDs	Wars
Lower Democracy	ß	-.0686***	-.0961***	-.162***
	S.E.$_ß$	.0080	.0170	.030
Higher Democracy		.0383***	.0384**	.0426*
		.0067	.0112	.0244
Lower Trade-to-GDP Ratio		-32.3***	-95.8***	-45.8*
		9.0	25.9	26.9
Allies		.0753	-.199	-.562*
		.1011	.178	.346
Capability Ratio (log)		-.284***	-.410***	-.754***
		.030	.049	.096
Contiguous		1.13***	1.15***	.982**
		0.15	.25	.357
Distance (log)		-.289***	-.466***	-.365**
		0.053	.081	.161
Major Power		1.01***	1.12***	2.24***
		.13	.23	.40
Constant		-.489	-.643	-3.18**
		.406	.618	1.27
Wald Chi^2 (df)		3862.0 (13)	1345.3 (13)	472.7 (13)
p of Chi^2		.0001	.0001	.0001
Pseudo-R^2		.38	.28	.26
N		464,953	464,692	464,953

* $p < .05$; ** $p < .01$; *** $p < .001$ (one-tailed test). The estimated coefficients of the statistical controls (the years-of-peace and its cubic splines and the number of states in the international system) are not reported to save space; with a single exception, all are statistically significant.

Table 2: Annual Probabilities of the Onset of a Fatal Militarized Dispute, 1885-2001,
Based on Estimated Coefficients in Table 1

	p	Change
1. Democracy scores, trade-to-GDP ratio, IGOs, and capability ratio set at median for contiguous dyads; allies equals 0; Distance, years of peace, and system size at mean for contiguous dyads	.0058	0
2. Increase both democracy scores to 90th percentile	.0029	-50%
3. Decrease both democracy scores to 10th percentile	.0087	+49%
4. Decrease lower democracy score to 10th and increase higher to 90th	.0179	+207%
5. Increase trade-to-GDP ratio to 90th percentile	.0023	-61%
6. Increase both democracy scores and trade-to-GDP ratio to 90th percentile	.0011	-81%
7. Allies equals 1	.0048	-18%
8. Increase capability ratio to 90th percentile	.0023	-61%

Table 3. Logistic Analyses of the Relationship between Trade Interdependence and Onset of Militarized Interstate Disputes, All Dyads, 1885-2001

Dep. Variable MID onset	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Interdependence	28.2 5.98 .001	-22.5 -2.60 .005	-6.20 -1.32 .10	-11.3 -2.07 .02	-48.6 -4.69 .001	-25.2 -3.28 .001
Contiguity		3.82 31.54 .001	3.48 33.98 .001	3.04 14.72 .001	2.43 14.5 .001	1.33 6.66 .001
Years of Peace			-0.394 -18.36 .001	-0.395 -16.23 .001	-0.404 -17.81 .001	-0.393 -18.49 .001
Distance				-0.225 -2.77 .003	-0.475 -7.04 .001	-0.294 -4.37 .001
Major Power					1.85 15.47 .001	0.733 5.49 .001
System Size						-0.475 -11.94 .001

N = 553,866. Cells show coefficients, z-scores, and probabilities, one-tailed test; standard errors are adjusted for dyadic clusters. The cubic splines of the years of peace that are also included in the regression are not shown.

Table 4. Logistic Analyses of the Relationship between Trade Interdependence and Onset of Fatal Militarized Interstate Disputes, All Dyads, 1885-2001

Dep. Variable MID onset	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Interdependence	21.3 5.94 .001	-72.8 -3.01 .002	-53.1 -2.84 .003	-72.8 -3.30 .001	-122 -4.54 .001	-92.4 -3.68 .001
Contiguity		3.76 20.95 .001	3.71 22.49 .001	2.84 11.05 .001	2.45 10.43 .001	1.29 4.26 .001
Years of Peace			-0.285 -7.72 .001	-0.281 -7.81 .001	-0.289 -7.93 .001	-0.284 -7.93 .001
Distance				-0.459 -4.42 .001	-0.612 -6.35 .001	-0.493 -5.00 .001
Major Power					1.62 9.17 .001	0.634 2.76 .003
System Size						-0.410 -6.50 .001

N = 553,602. Cells show coefficients, z-scores, and probabilities, one-tailed test; standard errors are adjusted for dyadic clusters. The cubic splines of the years of peace that are also included in the regression are not shown.

Table 5. Relationship Between Lower Trade/GDP Ratio and the Probability of a Fatal Militarized Dispute, across Space and over Time 1885-2001

(AS=Across Space)
(OT=Over Time)

VARIABLES	COEFFICIENTS	Z-SCORES	SIGNIFICANCE
CONTIGUITY	3.72	21.61	.001
TRADE(AS)	-56.6	-2.64	.004
TRADE(OT)	-39.0	-1.63	.05

N = 553,602. The z-scores are derived from standard errors adjusted for dyadic clusters. Significance levels are based on one-tailed tests. The years of peace and the associated cubic splines that are also included in the regression are not shown.

Table 6. Relationship Between Lower Democracy Score and the Probability of a Fatal Militarized Dispute, across Space and over Time 1885-2001

(AS=Across Space)
(OT=Over Time)

VARIABLES	COEFFICIENTS	Z-SCORES	SIGNIFICANCE
CONTIGUITY	3.39	24.46	.001
DEMOC. (AS)	-0.090	-4.66	.001
DEMOC. (OT)	-0.063	-3.80	.001

N = 514,227. The z-scores are derived from standard errors adjusted for dyadic clusters. Significance levels are based on one-tailed tests. The years of peace and the associated cubic splines that are also included in the regression are not shown.

[1] "The governments, elites, dominant classes, and people of the free-market democracies feared not just for their physical security and national independence but also for their prosperity and their political and economic liberties. They formed strong alliances in order to preserve their way of life…" (Russett and Oneal 2001: 237). Trade, democracy, and shared IGO memberships typically explain about 20% of the variance in shared alliances.

[2] We have sometimes limited our analysis to either the realist or liberal variables to test the models separately, as when we tested the possibility that civilizational differences constitute a confounding variable for either or both (Russett and Oneal 2001: 253).

[3] Conceptually, it can help to think of the realist variables as providing the opportunity for conflict and the liberal variables (plus perhaps alliances) as providing restraints on the willingness to engage in conflict (Kinsella and Russett 2002).

[4] We would even qualify our endorsement of Green, Kim and Yoon's (2001) statement that time-series and cross-sectional analyses should in principle give the same answers. It is possible, for example, that recent democratization might disturb the peace normally expected of stable democracies, though we have found no evidence of this effect empirically (Oneal and Russett 1997; Russett and Oneal 2001).

[5] Green et al.'s fixed effects analysis eliminated all the dyads that were free of violence throughout the history of their relations, which meant also dropping a very large proportion of jointly democratic dyads. Other analyses (e.g., Russett 1995: 173-74, Maoz 1998, and the enduring rivalry literature represented by Hensel, Goertz, and Diehl 2000

and Diehl and Goertz 2000, ch. 2), have established the temporal relation between joint democracy and reduced conflict.

[6] One of these is political stability. In Maoz and Russett's (1992) contingency tables, this variable looked as though it might replace democracy as a significant influence; but Maoz and Russett's (1993) multi-variate regression analysis showed that democracy was always significant in equations that included stability.

[7] At our NSF-sponsored conference in 1995 we were already aware of this measure's potential for non-monotonicity, and solved the problem with help from conferees, including Ray. The revised continuous measure showed both trade and joint democracy to have significant effects (Oneal and Russett 1997, Oneal and Ray 1997).

[8] The measure of joint memberships in intergovernmental organizations has been the least consistently significant. Removing the increasing trend in the variable, which results from the increased number of IGOS, improves its performance. Better measures of the role of international organizations based on the institutional characteristics of the IGOs have also produced encouraging results (Boehmer, Gartzke, and Nordstrom forthcoming), as has a measure of the average level of democracy of an IGO's members (Pevehouse and Russett, forthcoming).

[9] A short list of the most important differences between early and later studies, would have to include: expansion of the temporal domain from 1950-1985 to 1885-2001 and the cases analyzed from those that are politically relevant to all dyads; better data on democracy, trade, MIDs, alliances, etc.; methods of handling missing data; changes in the measurement of joint democracy, trade dependence, and conflict (MID involvement or

onset, all MIDs, fatal MIDs, or wars); the inclusion of controls for temporal dependence (peace-years, GEE, or distributed lags); and the use of robust standard errors.

[10] Crescenzi (2003) gives a theoretical discussion of why conventional measures of asymmetry may not identify a general relationship.

[11] The 'System Size' variable corrects for the change in the probability of conflict due to an increasing number of states. When using logistic regression, we assume that the dyads have a constant probability of conflict after having accounted for the influences of the explanatory variables; but this cannot be the case when analyzing all dyads. We correct for the increasing number of non-relevant dyads as suggested by Raknerud and Hegre (1997). If no correction for the increase in system size were necessary, the estimated coefficient would be 0. In fact, it is highly significant ($p < .001$).

[12] All the statistical significances we report are based on single-tailed tests because the theoretical relationships are directional.

[13] The beneficial effect on a war is also substantial, a reduction of 28%, though for these rare events the significance level is .08.

[14] Over 95% of the cases in the pooled analyses are lost because the dyads did not experience a fatal militarized dispute. This is why Beck and Katz (2001) and King (2001) advised against using the fixed-effects model in logistic analyses of rare events.