

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

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Jim Ray and others in this issue question customary procedures for the quantitative analysis of theoretically complex questions in the social sciences. In this article we address Ray's use of research on the Kantian peace to illustrate his points. We discuss his five guidelines for research, indicating how we agree and disagree, and take up five substantive issues he has raised about our research. 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 a regression equation.

Keywords causes of war, democratic peace, liberal peace, economic interdependence

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 for parsimony we should follow a "rule of three" (ART) proposed by Chris Achen (2002), limiting ourselves to no more than three variables on the right-hand side of a regression equation in the absence of a formal theory. Ray cites a number of works in international relations that have been 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, psychology, or the biological sciences. 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, and Barro was the second most frequently cited economist between 1990 and 2000 (Coupé, 2005). Science is, of course, not a democracy, but can so many authors, editors, and reviewers be wrong?

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Ray raises issues relevant to the research of a majority of the readers of this journal, but since his article in this issue focuses on our work, we shall too, while remaining attentive to its wider implications. We agree with Ray's call for greater care in 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) guidelines: "Do not control for intervening variables." We concur that in research on the Kantian peace, alliance ties are best conceptualized as intervening. Alliances are less an independent influence on the risk of conflict between two states than a consequence of political and economic factors which our research shows are more important influences on the risk of conflict. That is, alliances reflect not only strategic realities, but also, as in the Cold War, shared political preferences and commercial interests.¹ Some reverse causality may run from alliance to these other variables, in which case alliance would become what Ray refers to as a confounding (or competing) variable; but as in analyses we report below, it is clear that the primary direction of influence is from democracy and economic interdependence to alliances.

The inclusion of an intervening variable in a regression analysis can often clarify causal relations in important ways. We incorporated the alliance indicator 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 had 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; nonallied democracies are also peaceful. Furthermore, alliances do not have significant pacific benefits once the liberal variables are introduced. In our best causal analysis using distributed lags, the alliance indicator actually shows a weak *positive* influence on the risk of violence (O Neal, Russett, & Bernbaum, 2003). 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 important than Gowa and many others suspected.

More emphatically, we regard preferences as measured in the similarity of states' votes in the United Nations (Gartzke, 1998) as an intervening variable. We have shown that the effects of democracy and trade on conflict are very similar whether these are estimated without considering the independent influence of preferences or by taking into account the direct effects of the Kantian variables, controlling for preferences as revealed in UN voting, and their indirect effects on conflict through their influence on preferences (Russett & O Neal, 2001, 229–37).

Ray's second guideline, "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.

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 & O Neal, 2001, 237). Trade, democracy, and shared IGO memberships typically explain about 20% of the variance in shared alliances.

If we had omitted any one of these variables, we would have been rightly criticized for underspecifying realist theory, to the advantage of the liberal model. Our goal was to test the liberal view in a way that gives a “fair shake” to *realist theory as a whole*. After all, early findings suggesting the possibility of a democratic peace (e.g., Russett & Starr, 1981, ch. 15; Rummel, 1983; Doyle, 1986) were properly criticized for omitting competing variables.²

Our inclusion of three complementary and partially correlated variables (democracy, trade, and shared IGO membership) gives us only a limited ability to identify their individual contributions. But not to include them would disadvantage the *Kantian 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 a measure of economic interdependence. Overall, our research shows that the three liberal variables make independent contributions in reducing the risk of violence. The evidence indicates that democracy and economic interdependence have remarkably robust pacific benefits; the contribution of IGOs is less certain (Oneal, Russett, & Bernbaum, 2003; Oneal & Russett, 2006). The relative impact of the three variables varies somewhat depending on the data, specification of the model, spatial and temporal domain, etc. It is important to note that new strong support for the benefits of IGOs has come with improved measures of their effectiveness (Boehmer, Gartzke, & Nordstrom, 2004; Pevehouse & Russett, 2005). We have avoided asserting that one liberal element is most or least important (Russett & Oneal, 2001, 39–41, 172–73).

Ray’s third guideline, “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.”³ Our regression model was not formally derived, but we believe it has been carefully articulated and justified theoretically.⁴ We do not, of course, pretend 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 fourth guideline, “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 a good way to assess the effects of joint democracy on the likelihood of peace (Ray, 2003, 15–17).

Ray’s subsequent application of this guideline to the inclusion of both distance and contiguity as explanatory variables, however—especially when trade (or alliances) is also included in the regression—is mistaken. They are related by definition and thus correlated,

²We 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 & Oneal, 2001, 253).

³We appreciate Kadera and Mitchell’s (2005) good-spirited illustrations of how to theorize rigorously even without a formal model.

⁴Conceptually, 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 & Russett, 2002).

but each taps only part of the broader concept of geographic proximity, which is the confounding influence that must be addressed: all else being equal, proximity increases states' opportunity and willingness to engage in either conflict or trade. Ray recognizes that without a control for proximity, trade will be spuriously related to conflict, but he objects to the means for resolving this problem. 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, but two countries with capital cities quite far apart (e.g., Moscow and Beijing) may share a border. 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 desirable to further modify contiguity to take into account topography and the ease with which borders can be crossed (Starr & Thomas, 2002), but until long-term global data are available, the use of both distance and contiguity is the best practical way of controlling for geographic proximity (Oneal & Russett, 1999; Hegre, 2005).

This leaves us with Ray's fifth guideline, "Control for possible differences between across-space and over-time relationships." We agree with Ray, and Achen, that it is important to confirm that theoretical relationships hold both spatially and temporally.⁵ Nevertheless, Ray's own empirical illustrations do not support his case. He cites Zorn's (2001) analysis, which suffers from some, though not all, of the limitations of Green, Kim, and Yoon's (2001) fixed-effects analyses.⁶ It is restricted to a short period of time (35 years of the Cold War) during which both key independent variables (trade and democracy) and the dependent variable (conflict) showed little variation over time for many dyads. Hence it is not surprising that the temporal relationships are somewhat different from the spatial ones in this case. We showed in our response to Green et al. that the expected benefits of democracy and interdependence are confirmed when the analysis is extended to a longer historical period (1885–1992) with more variation through time. Recognizing that possibility motivated us to painstakingly expand our data set back to the 19th and forward to the 21st centuries. We confirm below that analyses of the liberal peace over the longer period do not reveal important differences across space and over time.

Ray's second article (2005), in this issue, covers much 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!⁷ The thrust of his review of past research is that including or excluding specific variables from analyses with numerous explanatory variables can dramatically change the results. This can happen most egregiously when the effects of geographic

⁵We 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 little empirical evidence of this effect (Oneal & Russett, 1997; Russett & Oneal, 2001; Oneal, Russett, & Bernbaum, 2003).

⁶Green 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, 173–174; 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.

⁷One 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) multivariate analysis showed that democracy was always significant in equations that included stability.

proximity are not properly taken into account.⁸ But look at his Table 2, covering six studies.

Oneal et al. (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 (Ray, 2005). 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 measurement problems that made it less than optimal (Maoz & Russett, 1993, 629). Indeed, Oneal and Ray (1997) explored this issue in considerable detail.⁹ Barbieri (1996) produced different results for trade and alliances, but her research has a number of differences in specification, case selection, measurement, and data, so it took time to figure out why her results were different (Oneal & Russett, 1999; Gartzke & Li, 2003; Oneal, 2003), especially since her complete dataset was unavailable until recently (Barbieri & Peters, 2003).¹⁰

What Ray’s Table 2 actually shows for the three subsequent analyses by Oneal and Russett is that when trade and IGOs were added to democracy to form the core variables of the Kantian model, and others were subtracted because they proved not to be robustly related to the probability of conflict, the three liberal variables (including the trade-to-GDP ratio in Russett, Oneal, and Davis, 1998, which is significant at the .09 level in a two-tailed test) remained statistically significant, as they have been in most of our subsequent analyses (Russett & Oneal, 2001; Oneal, Russett, & Bernbaum, 2003; Oneal & Russett, 2006).

Moreover, the six publications that Ray (2005) reviews show 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 in the end ignores them. Considering the different decisions in research design that have been made, and that applied statistical methods 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 factors affecting the results reported (Bennett & Stam, 2000b).¹¹

⁸The failure to account properly for proximity accounts for the insignificant results reported by Keshk, Pollins, and Reuveny (2004) for the effect of trade on conflict, not their use of a simultaneous system of equations, as they claim. If capital-to-capital distance is added to their conflict equation, the pacific benefit of trade is evident. Trade also reduces conflict, even without distance being added to the specification, if their analysis is limited to contiguous pairs of states—dyads with the ability to engage in military conflict. There are substantial errors in their trade data.

⁹At an NSF-sponsored conference held at Yale in 1995 we were already aware of this measure’s potential for nonmonotonicity 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 & Russett, 1997; Oneal & Ray, 1997).

¹⁰Barbieri and Peters’s (2003) conclusion that interdependence increases interstate conflict depends upon the specification of their regression equation (dropping the major-power indicator and measure of distance and adding openness) and analyzing a non-randomly selected set of cases.

¹¹A 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.

It is a wonder that our findings have remained 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 regarding the liberal peace.

1. He claims 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 are probably attributable to economic development, though he gives no evidence for this claim.

We next address each of these points. We find that none of his reservations is well founded.

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 1, the dependent variable is the onset of a militarized interstate dispute (MID) of any severity, in column 2, the onset of a fatal militarized dispute, in column 3, the onset of wars and fatal disputes with at least 1000 deaths in battle (Maoz, 1999). We use the latest data from the Correlates of War project (version 3.04) downloaded from EUGene (Bennett & Stam, 2000a), Gleditsch's (2002) data on trade and gross domestic products (version 4.1) for the post-World War II period, and our economic data for the years before World War I and the interwar years (Russett & Oneal, 2001), which we have carefully compared to Barbieri's data (1998). We focus on the effects of the liberal variables democracy and interdependence because, as noted earlier, research on the role of IGOs is rapidly evolving. We also consider the influences of 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.¹² We add a control for the number of states in the system to take into account the dramatic growth in the number of sovereign nations that has occurred since World War II. The specification in Table 1 is essentially the model of interstate conflict that Ray (2003, 2005) criticizes.

¹²Crescenzi (2003) gives a theoretical discussion of why conventional measures of asymmetry may not identify a general relationship.

TABLE 1 Models of the onset of militarized interstate disputes, fatal disputes, and war, 1885–2001

Variable		All MIDs	Fatal MIDs	Wars
Lower democracy	β	-.0686***	-.0961***	-.162***
	S.E. β	.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)
<i>p</i> of Chi ²		.0001	.0001	.0001
Pseudo- <i>R</i> ²		.38	.28	.26
<i>N</i>		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.

As shown in Table 1, the causes of all 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.¹³

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 of little interest. 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

¹³It also helps avoid selection bias from focusing on only the top of the scale. For an early statement regarding efforts to distinguish between big (systemic) wars and small ones, see Bueno de Mesquita (1990).

TABLE 2 Annual probabilities of the onset of a fatal militarized dispute, 1885–2001, based on estimated coefficients in Table 1

	<i>p</i>	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%

go unreported in the Western media from which the COW data are gleaned; such incidents are often apt to have gone unreported during 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 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 & Russett, 2006), show that the pacific benefits of democracy and interdependence are if anything greater in the contemporary period.

Ray (2003) dismisses analyses of substantive significance like these because they focus on changes in small probabilities. He neglects to consider the very high human and financial cost associated with such rare events as fatal militarized disputes. The benefits of liberalism are a function of the cost of military conflict as well as the probability of its occurrence. Reducing the annual probability of a fatal MID by 81% is hardly trivial when we recognize that fatal disputes involve at least one fatality among the combatants and some involve millions of deaths. Moreover, civilian deaths in extended militarized disputes are often greater than military casualties (Li & Wen, 2005). Viewed from a systemic perspective, the benefits we have found for democracy and interdependence indicate that dramatic reductions in the number of militarized disputes and wars are possible with the spread of liberal institutions and policies.

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 the choice of explanatory factors included in the regression model.

To show that commerce reduces military conflict even with a minimum of covariates, we follow Ray's (2005) lead and introduce sequentially the controls used in Table 1. How do we justify our controls? We previously explained why two measures of geographic proximity are needed. Distance discourages both trade and conflict, and a border facilitates them. Everyone agrees that we should control for contiguity, but to exclude the capital-to-capital distance would suggest that states halfway around the world from one another are as likely to fight as noncontiguous states in the same region. Argentina and Israel were not equally likely to attack Iraq's nuclear installation in the early 1980s. Contiguity and distance are not highly correlated (-0.50). We also include a measure of major-power status because these states have lower loss-of-strength gradients than smaller states (Boulding, 1963; Bremer, 1992). The United States can use military force at great distance; Myanmar cannot. The potential for interstate violence exists when one state can reach another with effective military force. We need only common sense to predict peace if this condition is not met. Bolivia and Laos have little conflict because they are small and distant from one another, and little trade for the same reason—not little conflict because they have little trade.

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 nonrelevant states has declined over time as membership in the international system has expanded—a violation of an assumption underlying the logistic estimator (Raknerud & Hegre, 1997). The controls included in the final column of Tables 3 and 4 are especially necessary when all dyads, rather than just the politically relevant pairs, are analyzed.

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 because 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 column 2 of both Tables 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 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) helps to address the issue of endogeneity (Oneal, Russett, & Bernbaum, 2003; Oneal & Russett, 2004), and there is clear evidence that past conflict both increases the probability of conflict in the current period and reduces trade. With the addition of the years of peace and its splines in the third column, the coefficient of the lower dependence score becomes insignificant ($p < .10$, one-tailed test) in the analysis with all militarized disputes, but it is still significant at the .003 level in the estimation with fatal disputes.

The 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

TABLE 3 Logistic analyses of the relationship between trade interdependence and onset of militarized interstate disputes, all dyads, 1885–2001

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

Note. $N = 553,866$. 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.

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).¹⁴ 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%.¹⁵ These estimates closely resemble those reported earlier for the full model.

These results contrast sharply with those reported by Ray (2005) in his Table 4. He analyzes data posted by Gartzke and Li (2003) rather than our original data (Oneal & Russett, 1999), from which theirs were derived. We replicated Ray's analyses using our 1999 data, but were unable to reproduce his results. As in the results we report in Table 3, the estimated coefficient of interdependence is negative and very significant ($p < .006$), controlling only for contiguity. Even adding the peace-years variables, it is negative and significant at .03. In this last analysis, we have over 337,000 cases; but Ray reports having only 270,994 observations. Over 20% of the cases in our data are missing.

¹⁴All the statistical significances we report are based on single-tailed tests because the theoretical relationships are directional.

¹⁵The beneficial effect on a war is also substantial, a reduction of 28%, though for these rare events the significance level is .08.

TABLE 4 Logistic analyses of the relationship between trade interdependence and onset of fatal militarized interstate disputes, all dyads, 1885–2001

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

Note. $N = 553,602$. 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.

Additional 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 that support for the pacific benefit of economic interdependence is evident even in models with minimal covariates. 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 associated with lower levels of conflict in each of these tests, at least at the .007 level. The lower and higher democracy scores and the capability ratio are also highly significant in all 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 trade-to-GDP ratio. These liberal influences give a better account of the incidence of fatal disputes than does an alliance, as our past work suggests (Oneal & Russett, 1997; Russett & Oneal, 2001; Oneal, Russett, & Bernbaum, 2003; Pevehouse & Russett, 2005; Oneal, 2006).

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

TABLE 5 Relationship between lower trade/GDP ratio and the probability of a fatal militarized dispute, across space and over time, 1885–2001

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

Note. $N = 553,602$. 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. AS: across space; OT: over time.

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 are minor given the limitations of our data and methods. Whether the results of the restricted analysis or those of the full model are considered, it is clear that democracy has a substantively important effect in reducing interstate conflict.

Cross-Sectional Versus Time-Series Evidence

Ray (2003) also contends 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.¹⁶ 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. As noted earlier, this same issue was raised by Green, Kim, and Yoon (2001).

We applied Zorn's (2001) method using the latest data for the extended period, 1885–2001. We use fatal militarized disputes and limit ourselves to the simple specification that Ray prefers, estimating the effect of trade and democracy while controlling only for contiguity and the years of peace.

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 ($p < .05$), as shown in Table 5, and the estimated coefficients for the two effects (−56.6 across space,

¹⁶One problem with Zorn's method is that information from later years is used in estimating earlier effects; i.e., the level of trade or democracy in 2000 would influence the mean values of these variables, which then are used to account for conflict in 1886.

TABLE 6 Relationship between lower democracy score and the probability of a fatal militarized dispute, across space and over time, 1885–2001

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

Note. $N = 514,227$. 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. AS: across space; OT: over time.

−39.0 over time) are similar. The support for the pacific benefit of democracy, reported 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 last test, support for the peace-inducing effect of a preponderance of power is evident in both the time series and cross-national components. There is no support for the pacific effect of an alliance through time ($p < .46$), but the dyadic mean of the alliance variable is 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, Kim, and Yoon (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 ($p < .003$).¹⁷ These results are consistent with what we reported in our reply to Green et al. (Oneal & Russett, 2001).

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, nor does he acknowledge that democracy and trade might promote development. 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 et al. (1996) subsequently reported that GDP per capita was not significant when the trade-to-GDP ratio was also entered in the equation.

Mousseau (2000) confirmed 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). 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.

¹⁷Over 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.

In a test not reported in a table, we confirmed 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 in these analyses of the years 1885–2001 that development also reduces fatal disputes and that there is 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.

Final Thoughts on the Rule of Three

There are three major problems with the rule of three. First, it severely limits our ability to evaluate theories in nested tests, so the consistent application of Achen's rule would lead to unconnected islands of theory with no indication of their relative importance. Second, it does not allow for the use of all our knowledge to predict outcomes for particular cases. Ray (2003, 2005) is right that our primary purpose over the past ten years has been to test specific hypotheses about the liberal or Kantian peace, but we do not share his disdain for constructing "the best, most potent multivariate model" for the purpose of prediction. Consider the following analogy. Medical epidemiologists want to know whether smoking causes heart attacks, but practitioners should use all available information about a person (his smoking habits, sex, age, fitness, nutrition, weight, etc.) to determine the individual's overall risk of a heart attack.

Applying our theories to real-world problems is justified when our recommendations are based on social-scientific analyses (Morrow, 2002). To evaluate alternative foreign policies requires us to know whether promoting democracy and economic interdependence encourages peace, but we should not dismiss efforts to predict the probability of conflict in particular cases. If the experience of economists is any guide, this will involve more variables than three. "Another thing we've learned," Princeton economist Mark Watson recently claimed, "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).

The third problem with Achen's rule is that its application is not as straightforward as it seems at first. Achen (2002) wants to limit analysis to a small number of variables so that researchers can consider whether a general finding applies to subsets of cases, but he provides no rule for limiting the subsetting—a problem whether we perform regression analyses on restricted samples or use interaction terms to identify particular groups in a pooled analysis. If we divide our samples into minute subsets, what will lead us to reject the generalization being assessed? What should we conclude about a theoretical relation if it is not significant in a subset of cases, for example, among Asian dyads in the Interwar Years? The danger of ad hoc theorizing, too, is evident.¹⁸

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

¹⁸Clarke (2005) suggests experimental methods as preferable in principle, but of course he understands that true experiments (e.g., take 10 autocracies at random, then make 5 of them democratic and see if they behave more peacefully) are not possible. With Achen and Clarke, Starr (2005) urges quasi-experiments with subsets of the spatial-temporal domain, which can be useful. Nevertheless, such a technique can lead to so few cases of rare events (wars, or even MIDs) remaining that one tumbles into the pit which Spiro (1994) dug for himself.

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 is not enough in light of possible interactive effects 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 our research over the past dozen or so years has concentrated on checking for the influence of competing or confounding variables and looking for interactions and nonlinearities (e.g., Lagazio & 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 consideration of individual cases on the other is essential to scientific progress and to meaningful policy analysis (Russett, 1970, 2004).

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, the proponents of a theory, and skeptics.

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. . . . [N]ever 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 methodological 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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