

The Democratic Peace

Author(s): Bruce Russett, Christopher Layne, David E. Spiro and Michael W. Doyle

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Correspondence

The Democratic Peace

Bruce Russett
Christopher Layne
David E. Spiro
Michael W. Doyle

"And Yet It Moves" (Bruce Russett on the Democratic Peace):

In their introduction to the Fall 1994 issue on the democratic peace, the editors of *International Security* called it (p. 3) "the conventional wisdom."¹ If it has become conventional wisdom, or seems likely to do so, we should expect to see challenges to it. The theoretical edifice of realism will collapse if attributes of states' political systems are shown to have a major influence on which states do or do not fight each other. The dialectic of proposition and attempted refutation is a healthy necessity for developing any kind of scholarly understanding. The critiques published in this journal argue that the new "conventional wisdom" is, in terms of the old Scottish verdict, "not proven." But that "conventional wisdom" is not dispelled by either critique: the logic of their contents fails to match their snappy titles.

Christopher Layne and David Spiro offer three major objections:

1) To be valid, democratic peace theory (Layne, p. 13) "must account powerfully for the fact that serious crises between democratic states ended in near misses rather than in war," and cannot do so.

2) The number of wars between democracies is somewhat higher than proponents of democratic peace admit, because they engage in "intellectual suppleness" with "continual tinkering with definitions and categories" (Layne, p. 40), or "selectively adopt definitions of key variables so that data analysis yields the results they seek" (Spiro, p. 55).

Bruce Russett is Dean Acheson Professor of International Relations and Political Science at Yale University. He thanks Don Green, Gary King, Zeev Maoz, Barry O'Neill, James Ray, Dan Reiter, R.J. Rummel, and Spencer Weart for comments, and Soo-Yeon Kim for computations.

Christopher Layne is an unaffiliated scholar in Los Angeles. He is presently a consultant to the government contracts practice group of Hill, Wynne, Trop, and Meisinger, which represents major firms in the defense industry.

*David E. Spiro is Assistant Professor of Political Science at the University of Arizona. He is the author of *Hegemony Unbound: Petrodollar Recycling and the De-legitimization of American Hegemony* (Cornell University Press, forthcoming). He thanks Michael Desch, John Mearsheimer, Edward Muller, Tom Volgy, and Steven Weber for their comments.*

Michael W. Doyle is Professor of Politics and International Affairs at the Center of International Studies, Princeton University.

1. "Editors' Note," *International Security*, Vol. 19, No. 2 (Fall 1994), p. 3; Christopher Layne, "Kant or Cant: The Myth of Democratic Peace," *ibid.*, pp. 5-49; David Spiro, "The Insignificance of the Liberal Peace," *ibid.*, pp. 50-86.

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3) Wars are rare phenomena, and through most of modern world history, democracies are also rare. Thus the number of wars to be expected between democracies is so small that no statistical test can distinguish the actual number (zero, or very low, depending on who counts) from the very low number that would be predicted by chance. This argument—the main burden of Spiro’s piece, seconded by Layne (p. 39)—does *not* claim to have disproved the hypothesis of democratic peace. It says only that the evidence is so sparse that statistical tests cannot confirm the hypothesis.

John Owen’s article in the same issue in part considers the first two objections, so they require less attention here.² But since Owen does not address the third, it needs more extensive discussion, which I offer below. I conclude with some new analyses, stimulated by the critiques, that strongly support the democratic peace proposition.

NEAR MISSES, AND DOGS THAT DIDN’T BARK

We begin with the matter of why in particular instances democratic states have not fought each other. Layne argues (p. 38) that whereas “democratic peace theory identifies a correlation between domestic structure and the absence of war between democracies, it fails to establish a causal link.” Yet certainly the literature on the democratic peace has gone well beyond correlation, and has postulated a variety of causal mechanisms, involving perceptions of shared norms, institutional constraints, and strategic behavior.³ What Layne presumably means is that until recently, the democratic peace literature was light on the kind of in-depth case study analysis that would establish with reasonable force that the considerations identified in one variant or another of the theory actually were important motivators of individual and state behavior. To correct this omission he examines four crises between democratic states. He then looks at a selection of statements by major figures in government or affecting governments, and finds that although the crises ended without war, the participants’ calculations were exclusively concerned with matters central to realism’s focus on power and strategic interest, and claims (p. 38) that “democratic peace theory indicators appear not to have played *any* discernible role in the outcome.”

Process-tracing of decision-making can be enlightening, and not enough of it has been done on this topic. Layne’s effort is welcome, but his conclusions are not beyond contest. A poststructuralist, for instance, would have serious reservations about the ability of an observer to penetrate the self-justificatory and mythological functions of decision-makers’ texts to discern “real” motivations. Since I am no poststructuralist I

2. John M. Owen, “How Liberalism Produces Democratic Peace,” *ibid.*, pp. 87–125.

3. Bruce Russett, *Grasping the Democratic Peace: Principles for a Post-Cold War World* (Princeton, N.J.: Princeton University Press, 1993), chap. 2, discusses two sets of causal propositions at length; Zeev Maoz and Bruce Russett, “Normative and Structural Causes of Democratic Peace,” *American Political Science Review*, Vol. 87, No. 3 (September 1993), pp. 624–638, give a slightly different version. Bruce Bueno de Mesquita and David Lalman, *War and Reason* (New Haven, Conn.: Yale University Press, 1992), develop and test an elaborate game-theoretic explanatory process, as does D. Marc Kilgour, “Domestic Political Structure and War: A Game-Theoretic Approach,” *Journal of Conflict Resolution*, Vol. 35, No. 2 (June 1991), pp. 266–284. See also R.J. Rummel’s five-volume *Understanding Conflict and War* (Newbury Park, Calif.: 1975–81); and David Lake, “Powerful Pacifists: Democratic States and War,” *American Political Science Review*, Vol. 86, No. 1 (March 1992) pp. 24–37.

would not push the objection that far. Nonetheless, serious problems of interpretation are unavoidable, and difficulties regarding which statements are reported and which are not, and of obtaining agreement among observers, must not be evaded.⁴ Owen, for example, interprets two of Layne's crises, notably the 1895–96 Anglo-American confrontation, differently.⁵ He finds substantial evidence that considerations consistent with the expectations of democratic peace did appear, and made a difference. Furthermore, Layne does not explain why Britain decided that its strategic interests lay in accommodation with the United States, in the first instance, and France, in the second, rather than with Germany. Why did the British not consider America "another enemy," a threat along with other rivals like Germany, since they certainly (Layne, p. 15) considered the United States a latent "world power"? The calculus of strategic interest is not obvious without asking why some states were regarded as intrinsically more desirable and trustworthy friends.

But suppose we were to concur entirely with Layne that democratic peace considerations really were invisible, or nearly so. Indeed, from my own research I conceded much of the case that Layne makes for realism as applied to the Venezuela and Fashoda crises.⁶ In each of Layne's cases, power and strategic considerations *were* predominant. No vital issues were at stake over *Trent* and Venezuela, and in Fashoda and the Ruhr the weaker side had no hope of prevailing in war. Does that concession give away the game? It would if democratic peace proponents claimed that shared democracy is the *only* influence permitting states to avoid war—but that is nonsense. I am happy to grant that power and strategic interest greatly affect the calculations of all states, including democracies. States sometimes start wars they think they can profit from, and usually avoid those where a cost-benefit calculation indicates they will lose, or win but at unacceptable cost. They do not, however, always initiate a war just because they think they could win at some acceptable price.⁷

4. "1) Decision makers may not fully understand their own (often unconscious) motives or intentions. 2) If they do, they may not articulate them. 3) Over the duration of a crisis, there may be a change in their motives and intentions . . . that is not being articulated. 4) They may articulate conflicting expressions of intentions . . . 5) They may deliberately disguise their true motives and intentions." Paul Huth and Bruce Russett, "Testing Deterrence Theory: Rigor Makes a Difference," *World Politics*, Vol. 47, No. 4 (July 1990), p. 481.

5. For interpretations of Fashoda giving more credit to democratic peace interpretations, see James Lee Ray, *Democracy and International Conflict* (Columbia: University of South Carolina Press, 1995), chap. 5, and Spencer Weart, *Never at War: Why Democracies Will Not Fight One Another* (forthcoming 1995), chap. 13.

6. Respectively, "although important in preventing an Anglo-American war over this bagatelle, British strategic interests do not deserve all the credit for avoiding war"; "Considerations of any norm that these two nations should not fight each other were well in the background on both sides; war was avoided primarily for other reasons." Russett, *Grasping the Democratic Peace*, pp. 6, 8. I do contend, however, that the experience of near war stimulated in each case intense reconsideration about interests and the direction of foreign policy, in which the previous antagonists' views of the democratic norms and institutions they shared played a major role in changing the way they behaved toward each other.

7. In the formal expected-utility calculation of Bueno de Mesquita and Lalman, *War and Reason*, positive expected utility for war is a necessary condition for a state to start a war but is far from a sufficient condition.

Neither an unfavorable strategic cost-benefit evaluation nor shared democracy is a *necessary* condition for avoiding war. But, allowing for some possibility of irrationality or misconception, either may well constitute a virtually *sufficient* condition. For this reason, conceding most—or even all—of Layne’s argument does not gain him the day on the larger issue of whether democratic peace exists. Extending that argument commits the logical fallacy of inducing a principle of universal non-existence merely by finding a few cases of non-existence. Even if there were no evidence for democratic peace considerations in four cases, that would prove nothing about their putative absence in other instances.

Now, at some point after examining very many cases my objection would become rather silly, at least for an influence—shared democracy—which I contend really is important in international affairs. But we have Owen’s good case studies (twelve of them in his dissertation cited on pp. 88–89 of his article, not just the four discussed at length in the article), many of which find more evidence for democratic peace considerations. That is noteworthy because Owen’s cases are all from the period between 1794 and 1917, an era during which, I believe, such considerations were much less influential than they later became.⁸

Moreover, *any* research design focusing on crises misses all the dogs that did not bark—the crises that never erupted or never brought the participants to the brink of war. What about the many conflicts of interest between democracies that were settled amicably, without threatening war—even though considerations of power and strategic interest might well have argued against such a settlement? The Venezuelan and Fashoda crises were the last of their type between these two pairs of states. Never since has Britain engaged in a diplomatic crisis with a democratic France or the United States that even remotely approached a likely step to war itself. Why? Might shared democratic norms and institutions possibly have something to do with it?

WHO IS THE SUPPLEST OF THEM ALL?

Layne and Spiro’s charge of “intellectual suppleness” is perhaps the most colorful but not the most important item in dispute. Owen says (p. 88) that “most democratic peace theorists are meticulous in their definitions”; readers can look, for example, at my criteria or James Lee Ray’s and make up their own minds.⁹ But neither critic is above suspicion himself. Layne avoids tinkering with a definition by the simple expedient of never giving us an explicit one. The closest he comes is the statement (p. 43), “In the realm of foreign policy, France and Britain were no more and no less democratic than the Second Reich [Imperial Germany],” with the explanation (p. 42) that in all three

8. Russett, *Grasping the Democratic Peace*, p. 22. One important reason may be the denial of the franchise to women in the nineteenth century. There are no public opinion data on the earlier era, but over the past fifty years both American and British women have been much more averse than men to using military force. Lisa Brandes, *Public Opinion, International Security Policy, and Gender: The United States and Britain since 1945*, Yale University, Ph.D. dissertation, 1994.

9. Russett, *Grasping the Democratic Peace*, chap. 1; James Lee Ray, “Wars between Democracies: Rare, or Non-Existent?” *International Interactions*, Vol. 18, No. 3 (1993), pp. 251–276. Spiro says (p. 56) that various researchers have used different criteria for democracy, and implies that this weakens the case. Rather, the fact that essentially the same results hold across a range of definitions is evidence that the findings are robust.

countries “crucial foreign policy decisions were taken without consulting Parliament.” One might conclude from this that virtually no countries had democratically-controlled foreign policy. (Would the United States pass this test in most of the twentieth century?) If so, there would not have been much opportunity for any wars between “democracies,” and hence there could be no democratic peace! But he seems instead to want to include Imperial Germany among the ranks of democratic powers (p. 44), an idiosyncratic view, rejected even by Spiro (p. 69).

Spiro, however, wants to count Finland’s role on the Axis side in World War II as war against four or five democracies in each of the years 1941–44. In doing so he seeks consistency in applying a definition—reasonable enough, but at some cost in good sense. His argument is that other democracies declared war on democratic Finland, and that those pairs of “warring” democracies should thus count. I thought it grotesque to count them. So far as I can tell there is no record of combat casualties between Finland and any democracy during World War II. The definition used by the Correlates of War Project from which both Spiro and I drew our data is to count a “warring” state as part of a multilateral war only if it has at least 1000 troops in combat or suffers at least 100 battle-related fatalities.¹⁰ Spiro seems to mean that we could properly exclude Finland only if we also looked carefully at all other multilateral wars to see if there are other instances where a particular pair of states (presumably, for his argument, not a democratic pair) were identified as participants in a multilateral war but really inflicted few if any fatalities *on each other*. That might be a good way to proceed, but it would be a lot of work, so instead poor Finland is made to count 17 times.¹¹

Spiro further defends this decision on grounds of consistency and deference to the data’s originators (p. 74): “Singer and Small coded Finland as at war with the liberal alliance during World War II, and so should studies that use the data set.” He does not always find it convenient, however, to be consistent and deferential: he chooses to drop Mecklenburg-Schwerin in 1866, and all but four states during the Korean War, because they suffered fewer than 1000 fatalities. This allows him to cut the number of warring dyads (warring pairs of states) from 29 to 21 in 1866, and from 28 to 4 in each year

10. Melvin Small and J. David Singer, *Resort to Arms: International and Civil Wars, 1816–1980* (Thousand Oaks, Calif.: Sage, 1982), p. 67.

11. Spiro decides (p. 74) that Finland “threw in its lot with those of fascist powers against other liberal democracies.” He does not dispute my statement about no casualties, though he refers (p. 61) to Ohto Manninen, “Operation Barbarossa and the Nordic Countries,” in Henrik S. Nissen, ed., *Scandinavia in the Second World War* (Minneapolis: University of Minnesota Press, 1983). On p. 166 (not p. 85), Manninen reports that some Royal Navy planes bombed Finnish territory on July 30, 1941. Spiro does not mention Manninen’s statement on the next overleaf (p. 168) that subsequent “declarations of war did not mean at any stage of the Second World War that Finland had become involved in real hostilities with the Western allies.” This raid was four months before Britain, under great pressure from Stalin, reluctantly declared war on Finland; Winston S. Churchill, *The Grand Alliance* (Boston: Houghton Mifflin, 1951), pp. 526–535. The target was a German-operated nickel-mining operation in northern Finland; Weart, *Never at War*, Appendix. Finland took no hostile action against any Western ally, nor during the period of declared war did the Western allies shoot Finns. (Australia, Canada, and New Zealand joined Britain in a formal declaration of war lasting from December 1941 to 1944; the non-elected Free French government was briefly at war with Finland in 1944.)

from 1950 to 1953. Since all these dyads are non-democratic, their deletion greatly raises the likelihood that he will fail to reject the null hypothesis. Without the deletion, the four Korean War years would support the democratic peace by rejecting the null hypothesis at the .056 level or less. (Reversing his Mecklenburg-Schwerin adjustment brings the 1866 level down too, but only to .29.) In massaging the data, he tosses aside the very coding rules of Singer and Small noted above; i.e., the 1000 troops/100 fatalities minimum for participation. Has the fate of realism come to hang on such manipulations?

INSIGNIFICANT INSIGNIFICANCE

All of this might sound a wee bit *recherché* but for two considerations. A few democratic peace proponents have maintained the strong proposition that democracies *never* fight each other. I do not wish to defend that position here, partly because of my own doubts about its evidentiary basis and partly because it invites eternal nit-picking discussions that do not greatly interest me. It is enough to say, first, that wars between democracies are at most extremely rare events, and second, that even violent conflicts between democracies that fall short of war are also very rare. Application of the proposition to violent conflicts well short of war is useful in expanding the number of “events” that can be analyzed, and, more important, is integral to the theory.

Neither Finland nor Imperial Germany would make much difference save for the matter of few wars and few democracies, leading to the third allegation, that it is difficult to find enough cases to distinguish zero as a statistically significant number. Even so, the notion that the data do not support the democratic peace proposition becomes possible only by procedures that make it impossible to find statistically-significant results, even with zero democratic wars.

Spiro’s statistical analysis has many dubious and inconsistent assumptions. If we scrutinize that analysis, and examine some alternatives, it will be clear the lengths to which one must go to support the “finding” of non-significance. His major effort—comprising 12 pages of text, Appendix table, and graph—is devoted to chopping the data into yearly intervals and doing 165 separate year-by-year analyses to discover whether the frequency of democratic dyads actually at war was significantly different from zero.¹² In only 6 of the 165 years does he find the difference statistically significant, as

12. Spiro performs his test using a hypergeometric distribution. In simple English, he is usually asking what is the *exact* probability that one will find no liberal (democratic) dyads at war. Yet consistent with my position above, the correct hypothesis is only that liberal dyads have a lower propensity to engage in war. In 1941 through 1943 he computes the probability of finding exactly 4 (5 in 1944) democracies at war, to conform with his argument that Finland and the Western allies constitute warring democratic dyads. For those years, however, he should instead have computed the probability of the observed outcome of 4 (or 5), or fewer. Thus in 1941 it would be not just the probability of observing 4 democratic wars, but the sum of the probabilities of observing 4, 3, 2, 1, and 0 wars. This would give significance levels of .437, .719, .691, and .660 for 1941 through 1944. These are higher than in column 8 of his Appendix (p. 85), and would have made his null hypothesis fare even better. This is the same as Fisher’s Exact Test (one-tailed), which compares the test statistic with the hypergeometric distribution. In years with no wars between democracies, Spiro’s method does not differ from the method used here.

democratic peace theory would predict. (He avoids finding statistical significance for 8 other years by the way in which he defines a warring dyad.)¹³ Thus he fails to reject the hypothesis that democratic dyads are no different from other dyads. But of course; by splitting the data into small enough parts, he has guaranteed a low rate of war outbreak in each year, so most of the tests he runs will have zero statistical power. Even though there are never (except for Finland) any wars between democracies, with so few wars in any one year it is almost always mathematically impossible to reject the hypothesis of no relationship. He might as well have broken the time periods down into even finer units, like the month or week.

Any proposed correlate of peace, however strong, could be discarded through such a divide-and-conquer approach. Realism's assumption—that states' internal characteristics are irrelevant to peace—would thus be safe. Imagine a trial judge instructing the jury to divide the evidence into 165 pieces, and then to deliberate whether each fraction alone warrants a conviction. Someone could get away with murder. A juror should decide on the aggregate of the evidence presented, and so should the international relations scholar. This requirement to use "total evidence" was enunciated by Jakob Bernoulli in 1713, at the birth of our concept of probability.¹⁴

An obvious question to ask is: what is the probability, assuming independence over many trials, of consistently finding zero (or 4 or 5 in the Finland years) democracies at war. Instead of taking each individual-year "test" in isolation, what is the probability of producing this finding *year after year*? The answer requires multiplying the probabilities in each year; for example, the probability of finding no liberal dyads at war in 1980 is, from Spiro's table, .829; in 1979 .704, etc., so the calculation is .829 times .704, etc. The joint probability over 165 years is .00000000000000000002, which would appear to be impressive evidence for democratic peace.

Spiro does not do that, because it requires assuming that each year constitutes a test fully independent of every other year. The very factors that produce peace between two states in one year are likely to make them peaceful the next; indeed expectations built upon past peace encourage its perpetuation. Likewise, a war begun last year may well carry over. Yet to acknowledge this exposes a contradiction in how to interpret Spiro's repeated year-by-year tests. When he treats 1967 as one test of the proposition and 1968, 1969, and so on as quite different tests, he ignores the fact that most of the warring dyads are continuing participants in the Vietnam War. This makes a big difference on the impact that multi-state multi-year wars have on the analysis. Finland again is an example. So too are the continuities from World War I to World War II in what has been called the Thirty-Years War for German Hegemony. By this reasoning, year-by-year tests

13. In 4 years of World War I, and in 1940 and 1945, the results are statistically significant as I predict. For 1941–44 they are in the wrong direction. That is solely due to calling Finland "at war" with democracies; otherwise these 4 years would be significant in the theoretically predicted direction. His revision of the Korean War data avoids showing another 4 years as statistically significant. During the 66 years when no dyads at all were at war, the probability of no war between democracies in these 66 years was *a priori* 100 percent.

14. Rudolf Carnap, *Logical Foundations of Probability*, 2nd ed. (Chicago: University of Chicago Press, 1971).

are not at all independent of each other, and multiplying probabilities is not permissible. But if so, just what does it mean to take 165 different “samples”? If one year’s sample is much like the next, how persuasive is it to say that all those repetitions failed to turn up support for democratic peace?

Subsequently Spiro offers to aggregate the years in another way. Exact results for periods longer than a year are not reported, either in numbers or graphs. It is evident, however, that his procedure (pp. 74–75) does not enlarge the “sample” sizes, but rather is guaranteed to keep them small. He includes only countries that were continuously in existence over the longer period, and continuously liberal. The samples shrink as the time period is extended. In the longest (150-year period), the sample is reduced to only 12 countries (3 of them liberal). He says the results confirm the “no different than random chance would predict” model, especially in the longer periods, and (p. 75) that “the results are not at all what one would expect.” Given such reductions in the sample size, who should be surprised?

By contrast, consider some support for the democratic peace proposition. I use my own work as an example, but related work reaches nearly identical conclusions.¹⁵ Start with the recognition that not all dyads have an equal probability of being at war. A few states are great powers, with interests all over the globe, and the ability to land troops, naval bombardments, or nuclear weapons anywhere. For dyads that include great powers, war is in principle possible. War is also possible between neighboring states or near-neighbors. As Layne recognizes (p. 39), but Spiro does not, “only dyads meeting these preconditions are part of the appropriate universe of cases from which democratic peace theory can be tested.” Most dyads (e.g., Ghana and Burma) are politically irrelevant—too far apart to have border conflicts or to be much involved in each other’s affairs, and too weak to project power over long distances. On occasion—as in the World Wars and the Korean War—they may be drawn into conflict with distant states. But

15. Russett, *Grasping the Democratic Peace*, chap. 4; Maoz and Russett, “Normative and Structural”; also Stuart Bremer, “Dangerous Dyads: Conditions Affecting the Likelihood of Interstate War, 1816–1965,” *Journal of Conflict Resolution*, Vol. 36, No. 2 (June 1992), pp. 309–341; and Bremer, “Democracy and Militarized Interstate Conflict, 1816–1965,” *International Interactions*, Vol. 18, No. 3 (1993), pp. 231–250. Bremer’s work is important not only because we replicate each other’s findings, but because it covers the long period of Spiro’s analysis. Spiro’s treatment of this research is misleading. He says (p. 77) that Maoz and Russett did not analyze the earlier period “presumably because they suspected that democracy would not be statistically significant.” But I knew from a copy of Bremer’s first manuscript in mid-1991, before the Maoz-Russett multivariate analysis, that the effect of democracy was significant over the long time-span. Spiro also identifies (p. 76) “many” studies as showing, “at best, a very weak correlation between democracy and peace.” He cites two from 1984 and one from 1989, neglecting to say that all three authors later decided the correlation is robust. See Steve Chan, “Democracy and War: Some Thoughts on Future Research Agenda,” *International Interactions*, Vol. 18, No. 3 (1993), pp. 197–213; William Dixon, “Democracy and the Management of International Conflict,” *Journal of Conflict Resolution*, Vol. 37, No. 1 (March 1993), pp. 42–68; Erich Weede, “Some Simple Calculations on Democracy and War Involvement,” *Journal of Peace Research*, Vol. 29, No. 4 (November 1992), pp. 377–383. Whereas Rummel’s early empirical work was limited to a short time-period, his subsequent review of five other studies showed robust support for the democratic peace. R.J. Rummel, “Libertarian Propositions on Violence within and between Nations: A Review of Published Research Results,” *Journal of Conflict Resolution*, Vol. 29, No. 3 (September 1985), pp. 441–442.

under nearly any other other circumstance it is pointless to include them in an empirical test of potential war adversarsies. Thus we should concentrate on the roughly 12 percent of dyads in the international system for whom war is a real possibility.¹⁶

Furthermore, we do not begin with total ignorance (the equivalent of Spiro's random model for all dyads in the international system) about what kinds of countries go to war with each other. In addition to the effect of geographical proximity, good theory (much of it straight out of realism) suggests that rich countries are unlikely to fight each other, as are countries whose economies are growing rapidly; that states of relatively equal power are more likely to fight each other than are states of widely disparate military capabilities; and that states which share ties of military alliance do not have the same incentives to fight one another as do states not so allied. One should incorporate this knowledge into a test, controlling for the influence of these variables as well as of democracy. By doing so we make it *harder* to find an independent causal relationship for democracy, since many democracies also are wealthy, allied, etc. Nevertheless, we do still find it.

Equally important is the proposition that, not only are wars between democracies rare or non-existent, but democracies are more likely to settle mutual conflicts of interest (and there surely are such) short of the threat or use of any military force. Conflicts of interest arise, but democracies rarely escalate those disputes to the point where they threaten to use military force against each other, or actually use force at all (even at a level of violence far below the threshold at which we would call it a war). Much more often than other states, they settle their disagreements by mediation, negotiation, or other forms of peaceful diplomacy. This integral element of democratic peace theory constitutes a logical extension of the research program that began nearly two decades ago with wars. By ignoring it, Spiro is more than five years behind the curve.¹⁷

With these specifications, Zeev Maoz and I analyzed the year-by-year behavior of all politically-relevant dyads in militarized disputes (if any) during the period 1946–86.

16. Maoz and I discuss analyzing "politically relevant dyads" at length in our publications. Whereas doing so misses some conflicts (and a few wars) between dyads not politically relevant by these criteria, it picks up a greater proportion of conflicts and wars between democratic dyads than between non-democratic ones. Hence it does not bias the results in favor of our hypothesis. See Russett, *Grasping the Democratic Peace*, p. 74.

17. See Zeev Maoz and Nasrin Abdolali, "Regime Types and International Conflict, 1816–1976," *Journal of Conflict Resolution*, Vol. 33, No. 1 (March 1989), pp. 3–36. For the most recent extension, to democracies' use of negotiation and third-party mediation, see Dixon, "Democracy and the Management of International Conflict"; Russell Leng, "Reciprocating Strategies and Success in Interstate Crisis Bargaining," *Journal of Conflict Resolution*, Vol. 31, No. 1 (March 1993), pp. 3–41; Gregory Raymond, "Democracies, Disputes, and Third-Party Intermediaries," *Journal of Conflict Resolution*, Vol. 38, No. 1 (March 1994), pp. 24–42. Spiro says (p. 78) that Russett shifts to militarized international disputes from war although he "proposed to study the latter in his introductory chapter." Not so. Although that chapter begins with the extreme case (war), pp. 20 ff. and the theory in chap. 2 consider the full range of threats and use of interstate violence. Layne (p. 14) recognizes that realism, unlike the democratic peace literature, expects democracies to use "threats, ultimata, and big-stick diplomacy against another democracy." James Fearon, "Domestic Political Audiences and the Escalation of International Disputes," *American Political Science Review*, Vol. 88, No. 3 (September 1994), pp. 585–586, offers a formal explanation for why democracies are less likely to escalate disputes with each other.

The result was that democratic dyads were significantly (statistically) less likely to engage in conflict—whether wars or minor disputes—than were pairs of states where one or both members were not democratic, even allowing for the effect of the control variables.

Spiro, however, objects (pp. 77–79) to anything that combines 41 years of observations into a single analysis, because aggregating years violates the assumption of independence between observations. He does identify a well-known problem in the statistical procedure, known as pooled-time series analysis, which we used.¹⁸ We acknowledged that patterns of behavior in one year depend *in part* on behavior in the previous year, and took the standard methodological precautions to minimize the effects. There is no perfect statistical solution; the observations are neither completely independent nor so dependent that one need restrict the evidence just to what can be shown on a year-by-year basis. One important mitigation of the independence problem would be, unlike Spiro, to include a dispute only in the year it began, or if it subsequently escalated to a higher level of violence on our scale, to include it in the year of escalation. This would avoid counting wars or disputes which are merely continuations of the past.¹⁹

NEW LOOKS

If we grant that dependence between years does raise some problems for pooled time-series, other analyses—not previously reported—are still possible. Instead of using the dyad-year as a unit of analysis, take the whole *regime-dyad* as the unit of analysis. For example, the United States and Costa Rica constitute an always-democratic dyad. The United States and the USSR are an always non-democratic dyad. The United States and Argentina are a democratic dyad in the years 1966–72 and 1983–86; in all our other years they constitute a non-democratic dyad because of the character of the Argentine regime. (For simplicity I treat all democratic dyad-years as a single unit even if the years were interrupted; ditto for non-democratic years.) For each of these regime dyads then ask: did they ever initiate a dispute, or escalate one? We are not counting the number of disputes or escalations within each regime-dyad; we are only asking if a dyad experienced at least one. The result is 1251 units for comparison, one for each dyad over time. We then can ask, very simply, whether democratic dyads are less likely than non-democratic dyads, over their whole “lifetimes” (up to 41 years in these data), to begin or escalate disputes.

The answer, shown in Table 1, is overwhelmingly clear: yes. Comparing percentages in the last column, non-democratic dyads were “infinitely” more likely to make war on each other. They were four-and-a-half times more likely to use force against each other

18. His discussion, however, is confused. The first two reasons or problems he identifies are one and the same. His only objection to “enlarging the number of observations” is that the observations are not fully independent. Nor are we “sure of finding significant results” (p. 78) by pooling observations—we merely have not cut the data into so many small parts as to guarantee not finding significant results.

19. This distinction informed the presentation in chap. 1 of my book, although not the multivariate analysis. It cannot correct for non-independence among dyads at peace.

Table 1. Dispute Behavior with Regime-Dyad as the Unit of Analysis, 1946–86.

	War	No War	Percent with War
Democracy	0	169	0
Not Democracy	37	1045	3.4
	Use of Force	No Use of Force	Percent Using Force
Democracy	8	161	4.7
Not Democracy	229	853	21.2
	Any Dispute	No Dispute	Percent with Dispute
Democracy	12	157	7.1
Not Democracy	257	825	23.8

than were democratic dyads. As for disputes, non-democratic dyads were more than three times as likely as democratic dyads to engage in any sort of militarized dispute. These big differences confirm that the inhibition against violence between democracies applies at all levels, and that it is most powerful as a restraint on war. Statistical tests are fully appropriate, and these differences are highly significant: at the .004 level for wars, and the .0000001 level for use of force and for all disputes. This is powerful evidence for the relatively pacific behavior of democratic dyads.²⁰

Now the *coup de grace*. Take our data, but otherwise replicate Spiro's year-by-year analysis, using the same .20 probability level and, for comparison, counting disputes that continued from the past year. We know wars are too rare for us to expect significant results on a year-to-year basis, but uses of force and all disputes are much more common. I created 41 two-by-two tables (one for each year), with the number of all democratic dyads which did not use force against each other, the number of democratic dyads which did use force, and the number of all other dyads with and without use of force. I then did the same thing for disputes at all levels. Table 2 gives the results. Using Spiro's own method of analysis, more than four-fifths of the years showed the predicted statistically significant difference between democratic dyads and non-democratic ones.²¹

20. Zeev Maoz has performed an analysis like this on the entire 1816–1986 period, with very similar results to be reported elsewhere. It is essential to distinguish new or escalated disputes from continuing ones, since if a regime changes from democratic to non-democratic, or vice versa, one would not want to blame the new regime for simply inheriting an old dispute from its predecessor. Here and below I use Maoz's recently refined data-set; the results would not be substantively different using data employed for my book. Fisher's Exact Test gives the probability that the difference is attributable to chance. As explained in footnote 12, this test is *less* likely than Spiro's to reject the null hypothesis when there actually are disputes between democracies.

21. Deleting non-escalating disputes after the year of initiation avoids the problem of non-independence of conflicts over time, but it also cuts the number of events by one-third, reducing the ability to achieve statistical significance. Even so, 60 percent of the years still show a significant difference.

Table 2. Year-by-Year Tests of Disputes and Use of Force by Politically Relevant Dyads, 1946–86.

	Democracies Significantly Lower	No Significant Difference
Use of Force	32	9
Any Dispute	34	7

None showed a difference—significant or not—opposite to what democratic peace theory predicts.

By raising new objections to the evidence for the democratic peace, Spiro forced me to devise new tests. The result is that the evidence for the democratic peace is stronger and more robust than ever. I am grateful for the challenge.

CONCLUSION

There is no need to jettison the insights of realism which tell us that power and strategic considerations affect states' decisions to fight each other. But neither should one deny the limitations of those insights, and their inability to explain many of the instances when liberal states have chosen not to fight or threaten one another. The danger resides in "vulgar realism's" vision of war of all against all, in which the threat that other states pose is unaffected by their internal norms and institutions. The challenges posed in this journal to the theory of democratic peace, and to the fact of democratic peace, hardly compare with the force of the Inquisition. Nevertheless, Galileo's response works here: "*Epure si muove.*"

—Bruce Russett
New Haven, Conn.

On the Democratic Peace (Christopher Layne replies):

Because of space constraints, my response to Professor Russett is confined to three central points of disagreement: (1) he imputes to me the position that democratic peace theory is nothing more than statistical correlation; (2) he criticizes my research design on the grounds that my case studies cannot account for instances where "the dog did not bark"; and (3) he questions the reliability of the historical evidence on which my (or any) process-tracing cases studies are based.

Professor Russett notwithstanding, it plainly is not the case that my article dismisses democratic peace theory as mere statistical correlation. On the contrary, the main thrust of my article is precisely my detailed examination of democratic peace theory's causal logic. As I demonstrate, democratic peace theory relies on two asserted causal explanations: (a) institutional constraints and (b) democratic norms and culture. After demonstrating that institutional constraints do not explain the democratic peace, I test the explanatory power of democratic norms and culture, and conclude that this causal logic

does not hold up when examined in four key cases where it should apply. Once democratic peace theory's causal logic is shown to be threadbare, then, indeed, little is left but statistical correlations; democratic peace theory offers no convincing explanation of *why* democracies purportedly do not fight each other.

Next, invoking the "dog that did not bark" argument, Professor Russett claims (pp. 165–167) that I commit the "logical fallacy of inducing a principle of universal non-existence merely by finding a few cases of non-existence. Even if there were no evidence for democratic peace considerations in four cases, that would prove nothing about their putative absence in other instances." Professor Russett asserts that my methodological focus on near misses overlooks "the crises that never erupted or never brought the participants to the brink of war." This criticism simply misses the point: nothing can be learned from mute dogs because it is difficult (if not impossible) to prove why a non-event did not happen. The only way we can test democratic peace theory's causal explanations—and thereby determine what factors influence states to remain at peace—is precisely to examine instances where they have come to the brink of war without going over it, that is, crises. Professor Russett clearly wants to believe that democratic peace theory explains the "non-barking dogs" but there is no evidence to substantiate his belief.¹

Professor Russett also attacks my case studies on post-modernist grounds, implying that the historical evidence of what and why decision-makers did what they did is inherently unreliable. Lawyers and historically-trained IR theorists (I have been trained in, and practiced, both professions) know that evidence must always be sifted painstakingly to determine its probative value. One learns to look carefully at archives, diaries, memoirs, and policy statements to determine whether the evidence is consistent and corroborative. Historical evidence of this type is the fundamental data base for studying international politics. We are properly enjoined that such evidence must be used punctiliously. It is something rather different, however, to suggest—as Professor Russett comes perilously close to doing—that the historical record must be discarded entirely because it is fundamentally untrustworthy. "Post-modernism" may have a place in art museums but it should be kept out of the study of international relations.

In reality, rather than attacking my conclusions, Russett admits (p. 166) that, in the cases I studied, "power and strategic considerations were predominant." While this admission reflects well on Professor Russett's social scientific rigor, it also leaves him in a bind. He says that his concession does not give the game away. In fact, it ends the game decisively. Unable to demonstrate that democratic peace theory explains the

1. Illustrative is Russett's contention that democratic peace theory factors played an important role in explaining why, *circa* 1900, Britain pursued rapprochement with the United States rather than with Germany. In fact, between 1898–1901, Britain and Germany tried to form an alliance. The effort did not materialize because London and Berlin could not reconcile their conflicting strategic interests. Domestic political factors had little, if anything, to do with the outcome. Britain subsequently focused on the German threat, rather than on the (geographically and temporally) more distant American one for geopolitical reasons. Specifically, British policymakers recognized that Germany, the dominant land power on the continent, which possessed a powerful and growing navy, fit the classic mold of a potential European hegemon that could threaten Britain's security. These points are not controversial.

avoidance of war in near-miss crises, he is left only with his weak “non-barking dogs” argument.

Whatever else it may be, democratic peace theory is not a compelling explanation of international political outcomes. It is, however, a dangerous retrogression to the kind of “normative international relations theory” (itself an oxymoron) so popular in Britain and America after World War I. Indeed, in *Grasping the Democratic Peace* (p. 136), Professor Russett gives the game away again when he says:

Understanding the sources of democratic peace can have the effect of a self-fulfilling prophecy. Social scientists sometimes create reality as well as analyze it. Insofar as norms do guide behavior, repeating those norms helps to make them effective. *Repeating the norms as descriptive principles can help to make them true.* (Emphasis added.)

We are all entitled to our value preferences. However, these should never be conflated with the rigorous theory building and testing that must constitute the intellectual foundation of the study of international relations.

—Christopher Layne
Los Angeles, Calif.

The Liberal Peace—“And Yet It Squirms” (David Spiro replies):

The debate between Professor Bruce Russett and me is about the causes of peace. Russett’s recent work aims to support a commonly-accepted theory—the Liberal Peace—which argues that democracies do not fight one another. My article directly challenges this theory by demonstrating that the number of wars between democracies is not statistically different from what random chance would predict. Both wars and democracies are rare, and that is why there are not many wars between democracies.

My article did not try to “disprove” liberal theory. Instead, I showed that, in statistical tests, random chance accounts for the Liberal Peace better than regime-type. Since random chance, which we know does not explain peace or war, is statistically more significant than regime-type, it follows that the absence of wars between democracies is not empirical confirmation for liberal theory. Furthermore, I argued that the reason some studies find statistically significant results is that the authors have biased the results by selectively choosing definitions and methods.

Russett’s critique is lengthy and detailed, but I am confident that I have answers to all of his points. Because of space limitations, however, I will address only his four principal criticisms.

DEFINITIONS

Russett charges that my case against the Liberal Peace rests on manipulating data sets. He specifically objects to my exclusion of Mecklenburg-Schwerin from the Seven Years War in 1866 and of twelve minor participants (such as Belgium and Ethiopia) from the Korean Conflict. At the same time, he wants me to alter the original data sets so that Finland is not considered at war against the allied democracies during World War II. His objections are curious, since his own work does not include World War II at all,

and he himself excluded 10 of those same 12 minor participants in the Korean Conflict. I agree, however, that how we define participation in the World War II and the Korean Conflict is important, because it determines whether the Liberal Peace is significant during two of the biggest wars in recent history.

It is indeed difficult to decide how to define certain wars. Russett makes a reasonable argument for excluding Finland, even though it allied with the fascist powers; was formally at war with Australia, Britain, Canada, and New Zealand; and was bombed at least once by the RAF. He also argues for including nations such as Belgium in the Korean Conflict even though it suffered fewer than 25 casualties each year in combat, fewer Belgians than succumbed to falls from ladders.

My main point was that the issue of how to treat the data is quite contestable. Russett himself cannot decide whether to criticize me for excluding nations or for including too many nations. On page 168 of his response I include too many and on pages 168–169 too few, then on page 171 “reductions in the sample size” lead to my results and on the next page my failure to reduce the sample size leads to my results. His response is empirical confirmation that these issues are not clearcut.

What is most important, and what Russett conveniently ignores, is that his case for the significance of the Liberal Peace rests *heavily* on these definitional issues. These are issues over which Russett himself demonstrates confusion and inconsistency. If Russett were to change his mind about any single one of these highly contestable definitions, his results would no longer be significant. The argument for the Liberal Peace completely depends upon this selective choice of definitions.

SLICING UP THE DATA

Russett protests that the analysis I did for one-year periods makes it impossible to prove significance, because “by splitting the data into small enough parts [I] guarantee a low rate of war outbreak in each year.” This objection represents a serious misunderstanding of my methodology, and of the difference in statistical analysis between analyzing a sample and analyzing the entire population.¹ I looked at all nations (not a sample), and I compared the proportion that were at war to the proportion that were democratic during a set period of time. The length of the time-period is irrelevant to the number of nations that are at war, unless the wars we are studying were extraordinarily short and trivial.

Russett seems to be hung up on my analysis of one-year periods, but I also examined all of the possible 5, 10, 20, 30, 40, 50, 60, 70, 80, 90, 100, 110, 120, 130, 140, and 150-year periods between 1816 and 1982. He chooses to ignore my many analyses of longer periods, which support the argument that the absence of wars between democracy is

1. In another objection, Russett uses my results to calculate a joint probability of peace year after year, and then points out that the years are not independent from one another (which I knew, and which was why I did not calculate a joint probability). Then he criticizes his own assumption of independence, recognizes “a contradiction” in the way he interprets my analysis of one-year periods, and blames *me* for ignoring historical context. I find it difficult to respond to someone who presents an argument that I did not make, rebuts that argument, and then criticizes me for inconsistency.

statistically insignificant.² The reader might wonder, if Russett's primary objection to my article was the analysis of one-year periods, why my results for multi-year periods also contradict his work. Russett gives the reason in his response. His results depend upon a pooled time-series analysis, which means that he takes one-year observations and mixes them all together. (The dyad between the United States and Canada, for example, counts 41 separate times.) I pointed out in my article that there is no logical rationale for presenting data this way, and furthermore it violates several rules for proper statistical methods. Russett responds that my article identified "a well-known problem in the statistical procedure," and "there is no perfect statistical solution" to this problem. Having misunderstood my methodology, and having conceded that his own data analysis was faulty, Russett is forced to present entirely new data analysis instead of standing by his previous work.

RESTRICTING THE DATA TO RELEVANT DYADS

Russett's pooled time-series analysis considered only dyads that he defined as "politically relevant." No one can be sure of what criteria Russett used for political relevance, because he has not made the decision-rules or his data publicly available. The criteria seem to be based on whether nations and their colonies are contiguous or bordering the same body of water, and whether one of them is a great power. The result is that his analysis leaves out 78 percent of all dyads between nations.

In principle, it seems like a good idea to consider only nations that have the possibility of war. In practice, however, this is a difficult idea to implement. According to Russett, the dyads of nations that he does not consider relevant occasionally go to war with one another; and those "occasions" include "the World Wars and the Korean War" (p. 171). These dyads cannot be very "politically relevant" if the criteria for choosing them does not allow consideration of the major conflicts of this century.

I also grappled with a problem of how to define relevant dyads. Since the issue is the endurance of peace over time, when I analyzed (for example) a forty-one year period, I examined only nations that existed for the entire time.³ Russett includes a war between two nations, even if they existed for only one day during the single forty-one year period he examines. Because I examined every possible period for every possible time-frame, I am sure that the way I selected the data did not bias my results.

I believe the way I defined relevant dyads is correct. But even if I am wrong, the point remains that the case for the Liberal Peace is entirely dependent on tricky and

2. Russett says that I did not provide the detailed results of these analyses, but as I wrote in my article, all of the data, the computer programs, and the results are available to anyone on the Internet. In fact, it appears that Russett himself took advantage of this public access, since he quotes results that I did not have space to report in my article. "Spiro.exe" (a self-extracting zip file) is available from haavelmo.harvard.edu via anonymous ftp or gopher.

3. I devised my tests for the endurance of the Liberal Peace at the suggestion of Michael Doyle, who had commented on an early draft of the article. Before either Doyle (whose work established modern study of the Liberal Peace) or I had the idea of what tests of longer periods would show, we agreed that it would be appropriate to include only dyads that existed for the entire period. To include dyads of nations that existed for only part of the period, as Russett wants to do, is not a proper test of endurance.

highly contestable definitional issues. Russett consistently and across the board chooses definitions of key concepts and data and methods so that they always yield results favoring the Liberal Peace. My article pointed to problems with definitional issues, and reported results for both sides of each issue.

NEW DATA ANALYSIS

Russett has addressed the theory of the Liberal Peace in at least two books and three articles, and thus has had ample opportunity to present convincing arguments in its support. But rather than stand by this work, he now presents new data analyses, calling them a "*coup de grace*." Has he delivered the definitive proof of the Liberal Peace? Who knows? There are no citations on where the new data come from or how he manipulated it. There are no descriptions of the statistical tests or methods used, and no detailed results. This seems less like a *coup de grace* than a desperate *coup de theatre*. If Professor Russett publishes his results in an appropriate form, making his data, methods, and results publicly available for replication, we will be able to evaluate this *ad hoc* argument for the Liberal Peace.

—David E. Spiro
Tucson, Arizona

To the Editors (Michael Doyle on the Democratic Peace):

The collection "Give Democratic Peace a Chance?"¹ is remarkable evidence for the progressive development of the liberal research program. Christopher Layne offers a valuable exploration of hard cases, where war nearly occurred; David Spiro challenges the significance of the statistical regularity that liberal states are unlikely to go to war with each other; and John Owen takes a large step toward the deepening of the liberal paradigm by showing how the process of the liberal peace might have worked. What other research program in international relations displays its proponents and critics contesting on the same playing field, producing social science of this quality and drawing upon both statistical testing and case studies? Together, they point the way forward for further development, illustrating the importance but also the limitations of either case studies or statistical tests, considered alone.

I wrote "Kant, Liberal Legacies and Foreign Affairs," published in two parts in *Philosophy and Public Affairs* (Summer and Fall 1983) in order to show how Immanuel Kant's 1795 essay, "Perpetual Peace," could be constructed as a coherent explanation of two important regularities in world politics: the tendencies of liberal states simultaneously to be peace-prone in their relations with each other and unusually war-prone in their relations with nonliberal states. Republican democratic representation, an ideological commitment to fundamental human rights, and trans-national interdependence

1. Christopher Layne, "Kant or Cant: The Myth of the Democratic Peace," *International Security*, Vol. 19, No. 2 (Fall 1994), pp. 5–49; David Spiro, "The Insignificance of the Liberal Peace," *ibid.*, pp. 50–86; and John M. Owen, "How Liberalism Produces Democratic Peace," *ibid.*, pp. 87–125. My views on these issues have benefited from discussions with George Downs.

(to rephrase Kant's three "definitive articles" of the hypothetical peace treaty he asked states to sign) could, I argued, be seen as three necessary and, together, sufficient causes of the two regularities taken together. This causal structure distinguished Kantian liberal theory from the international implications of nonliberal democracy, commercial pacifism, and mixed republicanism.² (It also gave rise to difficult determinations of what did or did not constitute a liberal regime.) The first part of the essay focused on the liberal peace and its Kantian sources. The second part focused on exposing the dangers of liberal imperialism, liberal aggression, and liberal appeasement.

Christopher Layne's article raises serious and important issues that question the liberal argument I discussed. He chose as his cases "near misses"—episodes when supposed liberals came close to war. In those cases, he argues, the logic of power replaced the liberal logic of accommodation. At the minimum, he succeeds in warning liberals of the dangers of imperial pursuits of principled settlements (the Venezuelan Dispute), unprincipled and punitive peace settlements (the Ruhr Crisis), and the contest over undefined colonial assets (Fashoda). In addition to their strategic significance, each of these disputes was problematic from a liberal point of view; liberal interests—both ideal and material—were also at odds. But Layne's tests have problems.

First, case studies designed to test a theory should be selected not by outcomes (in this case peace or war), as Christopher Layne does when he seeks out near-wars in which to test decision processes, but according to the hypothetical causes: liberal republics and non-liberal states. Hard cases, such as those for which he searches, are useful to generate alternative hypotheses or to test and, when met, to confirm theories, but they are not the best tests to invalidate anything but iron laws. (Most advocates of liberal theory took the trouble to point out exceptions to the peace-proneness of liberal republics or democracies.) A fairer set of case studies, for example, would include a small random sample of liberal dyads, liberal-nonliberal dyads, and nonliberal dyads, and examine whether liberalism makes a difference and the liberal thesis was confirmed or disconfirmed.

And, second, in the next round of theory testing, case studies should not be as dependent on secondary literature as Layne's are. Historians, like the rest of us, write with a purpose in mind. They are not simple chroniclers. Those purposes shape their presentation of events, giving rise to the well-known disputes between radical and conservative historians on, for example, German policy before World War I. We cannot resolve an issue by quoting conservative historians against supposed liberal ones on, in his example, the authoritarian aspects of pre-World War I Germany. None of the historians wrote to probe these issues and their treatment is consequently incomplete. (The radical historians, for example, would not necessarily agree with the liberal thesis, nor the conservative ones with a balance-of-power interpretation.) Instead, while prior historiography will serve as a useful guide and check, we will need to examine the

2. I have reexamined the imperial, isolationist, pacifist, and internationalist implications of various forms of democracy, both "realist" and "liberal," in Doyle, "Voice of the People: Political Theorists on the International Implications of Democracy," in Geir Lundestad, ed., *The Fall of Great Powers: Peace, Stability and Legitimacy* (London: Scandinavian University Press/Oxford University Press, 1994), pp. 283–310; and in Doyle, *Ways of War and Peace* (New York: W. W. Norton, forthcoming).

primary sources with our new questions in mind. To test the liberal thesis on decision-making, moreover, we will need a wider investigation than is typical of conventional diplomatic history, tracing decision processes outside cabinets, through parliaments and pressure groups, to, sometimes, the public. We will need to look for distinctions between informed and uninformed publics (often aroused by crises), axiomatic and articulated assumptions,³ and issues on or absent from the policy agenda.

If further investigation sustains Layne's view of the crises he examined, we need to go a step further and note that the liberal failure not only took place in the inability to control the crisis, it also, and more importantly, preceded the crisis. When a liberal state is expecting war, it *should* look to its power. One of the signs of liberalism at work will be, not the war-crises resolved, but the issues and crises that did not arise. The thesis I drew from Kant focused on a *state* of peace distinguishing liberal relations from the *state* of war characterizing liberal-nonliberal and nonliberal-nonliberal relations. Kant, of course, was drawing on Hobbes's famous realist description of international relations not as war, but as a "state of war," which is "a tract of time, wherein the will to contend by battle is sufficiently known. "For," Hobbes continued, "as the nature of foul weather, lieth not in a shower or two of rain; but in an inclination thereto of many days together: so the nature of war, consisteth not in actual fighting; but in the known disposition thereto, during all the time there is no assurance to the contrary." War or peace are thus merely indicators of the "states" that permit them. States of war and peace are well-captured in studies such as those conducted by Anne-Marie Slaughter Burley, who focused on the differing treatments accorded to liberal and nonliberal states in American courts, and by William Dixon, who examined the management of conflict prior to the outbreak of a crisis.⁴

If the liberal thesis is anything like normal social science, we will discover exceptions—interliberal wars or interliberal crises—with some of the latter resolved by (from the liberal view) luck rather than by principled respect, institutional restraint, and commercial interest. Erik Yesson's study of Fashoda, using primary sources, came to just such a conclusion: the liberals were lucky, not effective.⁵ Passions, colonial uncertainty, and a long history of rivalry overwhelmed liberal restraint; peace was rescued by the balance of power. In many other instances, as liberals suggest (and as Dixon and Slaughter Burley found), differences will be managed long before they become violent disputes in the public arena. Rather than writing case after case of non-events, however,

3. Ernest May, *Lessons of the Past: The Use and Misuse of History in American Foreign Policy* (New York: Oxford University Press, 1973).

4. Anne-Marie Slaughter Burley, "Law Among Liberal States: Liberal Internationalism and the Act of State Doctrine," *Columbia Law Review*, Vol. 92, pp. 1907–1996; and William Dixon, "Democracy and the Management of Conflict," *Journal of Conflict Resolution*, Vol. 37, No. 1 (March 1993), pp. 42–68.

5. Erik Yesson, *Power and Identity in World Politics*, Ph.D. Dissertation, Department of Politics, Princeton University, 1992. Other difficulties for the liberal thesis are raised by Hongying Wang, "Liberal Peace? A Study of the Fashoda Crisis of 1898," paper presented at the annual conference of the American Political Science Association (APSA), 1992. But for a contrast favoring liberal explanations over realist in the Fashoda and Spanish-American War crises, see James Lee Ray, "Comparing the Fashoda Crisis and the Spanish American War," APSA, March 1994.

this is where the utility of statistically testing the significance of the liberal thesis will make itself clear.

What is the best statistical measure of the political significance of international liberalism? The test should probe whether liberalism makes a difference. Following a revolution in which a liberal state replaces a nonliberal state, would the liberal state in its foreign relations with other liberals and nonliberals behave in the same way, in the same circumstances, for as long as would have a continuing nonliberal state, and vice versa? Such a question is not readily testable. The keys are regime and position and duration. We can control for contiguity, income, etc., across an entire sample as, for example, Zeev Maoz and Bruce Russett did,⁶ but not for all those factors together with geopolitical position. Geopolitical position clearly counts: regions are not the same and some (such as Europe) have been much more dangerous than others. If we cannot test all these at once, we will need to settle for something less. One valuable research strategy is, as suggested above, to expand the dependent variables to events other than war: court cases, crises, trade treaties, secret intelligence-sharing arrangements, war plans, and alliances. The unit of time by which we measure duration is, moreover, inherently ambiguous. How long does it take to decide or to preclude, to avoid or to embark on war? A year, month, or week? (The Cuban Missile Crisis was resolved in "21 Days"; the Cold War took 45 years.) Each measure affects the level of statistical significance of the inter-liberal peace, as David Spiro points out. David Spiro usefully tries out year-by-year and decade-by-decade comparisons, putting aside the positional issue. Another useful (but also very incomplete) test would be to compare for each country its war experience during its liberal periods to that during nonliberal periods. John Owen uses case studies to address (at least partially) just this geopolitical issue by comparing U.S. relations with Britain and France under liberal and nonliberal regimes. History, as Spiro confirms, also provides its own test during world wars, when states are forced to choose on which side of an impending conflict they will fight; interestingly, liberals tend to wind up on the same side (with a few anomalies).

Can we rely on statistical data sets to decide anomalies? Finland's formal status as a belligerent of the Allies in World War II is driving much of the recent statistical differences. Ruling Finland out of the war data set by the 1000-battle-deaths criterion of Singer and Small is a useful statistical convenience, but does not resolve the issue.⁷ If today the United States and Britain suddenly attacked each other, and stopped before sustaining 1000 casualties, no advocate of the liberal thesis should regard the theory as vindicated. Here is where we need careful case studies. A good place to begin would be Allied and Nazi relations with Finland. Was Finland regarded as an enemy by the Allies and, if so, in a way similar to how the other enemy states were regarded? If yes, then it should be regarded as a disconfirming case; if not, not.

In the end, as with most theoretical disputes, the debate will turn on the alternatives. Liberal theory should not be compared to the statistical residual or to a richly described

6. Zeev Maoz and Bruce Russett, "Alliance, Contiguity, Wealth and Political Stability: Is the Lack of Conflict Among Democracies a Statistical Artifact?" *International Interactions*, Vol. 17, No. 3 (1992), pp. 245-268.

7. J. David Singer and Melvin Small, "Wages of War, 1816-1980," ICPSR Study 19044.

case study, but to the comparative validity of other theories of similar scope. Then we might be able to determine whether liberal internationalism is but a spurious correlation of some other causal model. To do this we will need to develop disconfirmable versions of at least the two other leading modern candidates for a general theory of world politics, realism and Marxism. The present difficulty of such a comparison lies in the current condition of the rivals. The most popular and developed current versions of realism see the theory confirmed by international systemic hierarchy (hegemony) or by equilibrium, by bandwagoning or by balancing behavior, all without offering us clear ways of distinguishing when we should expect one and when the other of those opposites. Marxism is confirmed for its adherents in equally confusing ways, depending on whether the theorists regard the mode of production (ownership) or rather the mode of exchange (market) as the more fundamental feature of modern capitalism. For some, existing communist states were (are) a rejection of capitalism; for others they are an integral part of world capitalism.

In short, before we can truly engage in a debate over a general theory in international politics, other traditions will need to catch up to the kind and quality of work represented in the Fall 1994 issue of *International Security*. If the liberal theory is to have its chance, the other traditions also need to be put to their chance.

—*Michael W. Doyle*
Princeton, N.J.