
Clear and Clean: The Fixed Effects of the Liberal Peace

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In their article in this issue, Donald P. Green, Soo Yeon Kim, and David H. Yoon claim, contrary to liberal theory and extensive evidence from pooled time-series analyses, that neither joint democracy nor economic interdependence significantly reduces the frequency of militarized interstate disputes when dyadic fixed effects are taken into account. Similarly, their fixed-effects analyses contradict theory and previous evidence that democracies have higher levels of trade with one another than do other types of states. Our reexamination, however, refutes both claims and reinforces previous findings. Their fixed-effects analysis of disputes produces distorted results because they consider a relatively short period of time, 1951–92, in which variation is limited in the binary dependent variable and in the key independent variables. When we analyze a longer period (1886–1992) with greater variation in the data, the results confirm that democracy and economically important trade have important pacific benefits. The differences between our analyses of the volume of bilateral trade and those of Green, Kim, and Yoon primarily arise from a seemingly minor methodological decision: how to treat zero levels of trade when taking the logarithm. A more reasonable method produces more reasonable results: democracies have higher levels of trade than expected on purely economic grounds. A fixed-effects model is seldom the best means of analyzing dyadic data. Indeed, our analyses illustrate the pitfalls that can occur in its use. Nevertheless, our findings regarding disputes and trade provide additional confirmations of liberal theories of international relations.

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A New Challenge

Methods of analysis in international relations have developed rapidly in the last few years. As recently as 1997 we published an article on the liberal peace in which we examined pooled cross-sectional time series of dyadic interstate relations using simple logistic regression analysis.¹ In the estimations reported in the tables, we did not consider whether there was heteroskedasticity in the error terms, account for the grouping of our data by dyads, or address the lack of independence in the time series. We had applied similar methods in other major journals.² Nathaniel Beck, Jonathan N. Katz, and Richard Tucker showed us the error of our ways.³ Their reanalyses of our data, correcting for these violations of the assumptions of regression analysis, confirmed the democratic peace but called into question the pacific benefit of economic interdependence. Subsequently, we and others have shown, with longer time series and in a variety of specifications, that the benefits of economically important trade are both statistically and substantively significant, even controlling for the history of dyadic disputes. Different methods of accounting for temporal dependence reinforce these findings.⁴ Other scholars questioned the liberal peace on the grounds that we had not devised adequate measures of the underlying concepts or that the phenomenon merely reflects the special character of the Cold War.⁵ In response, we have shown that our results are robust to changes in the construction of key variables and are valid over roughly a century.⁶

The latest critique of our work, from Green, Kim, and Yoon, challenges not only the empirical evidence for the liberal peace but pooled analyses generally, an increasingly popular method in the study of international politics. They question the pacific benefits of democracy and economically important trade on the grounds that we have not considered fixed, unobserved differences among the pairs of states. As a consequence, they worry that support for the liberal peace rests only on incompletely specified cross-sectional comparisons. Pooled analyses show that democratic states and those economically interdependent are more peaceful, but Green, Kim, and Yoon are not sure how they got this way. Such statistical associations may be useful for the purpose of prediction but are not reliable grounds, in their view, for advocating policies designed to promote democratization or globalization. Though a fixed-effects model does not address a myriad of other concerns they have about dyadic analysis, it would, Green, Kim, and Yoon argue, at least say more about whether democracy and trade are significant causes of peace. Their reexamination of dyadic involvement in militarized disputes, 1951–92, is not reassuring: neither democracy nor trade is significantly associated with peaceful interstate relations

1. Oneal and Russett 1997.

2. See Maoz and Russett 1993; Oneal et al. 1996; and Oneal and Ray 1997.

3. Beck, Katz, and Tucker 1998.

4. See Oneal and Russett 1999a,c; Bennett and Stam 2000; Hegre 2000; Mousseau 2000; and Hegre and Kim 2000.

5. See Farber and Gowa 1997; Gowa 1999; and Gartzke 1998.

6. See Oneal and Ray 1997; Oneal and Russett 1999b,c; and Russett and Oneal 2001.

when dyadic fixed effects are taken into account. They also question research showing that democracies trade more with one another than would be expected on purely economic grounds.⁷ Here, too, the special status of democracies is eliminated when they control for dyadic fixed effects.

Green, Kim, and Yoon's results are surprising, given that so many other statistical analyses—including those that look for changes in individual dyads over time⁸—have confirmed the pacific benefits of democracy. Here, we show that Green, Kim, and Yoon's results are misleading. Even taking into account dyadic fixed effects, there is strong evidence for the liberal peace for the period 1886–1992: democracy and economically important trade do reduce the likelihood of interstate conflict. We also show that democracies have unusually high levels of trade with one another, as Kant and other liberals anticipated.⁹

New Evidence for the Liberal Peace

To show that democracies and interdependent states are more peaceful, we present three analyses for the period 1886–1992. First, we estimate a simple pooled model with a single intercept. To incorporate the dynamic information in the time series, we adjust for autocorrelation of the first order (AR1) using the general estimating equation (GEE). We then reevaluate the liberal prescriptions for peace using fixed-effects (or conditional) logistic regression. Finally, we incorporate dynamics into the fixed-effects test using a distributed-lags model. In these analyses we report conservative tests of statistical significance based on robust standard errors that take into account the grouping of our data by dyads. The three techniques produce robust, statistically significant evidence in support of the liberal peace. More importantly, democracy and economically important trade have substantively significant pacific benefits. Green, Kim, and Yoon's results differ because the variance of key variables in the data from 1950 to 1992 is limited.

Like Green, Kim, and Yoon, we will not discuss our data in detail. We have documented our sources and the construction of our variables elsewhere, and the differences between our analyses of militarized disputes and theirs do not hinge on such matters anyway.¹⁰ For the post–World War II years, we use essentially the same data as Green, Kim, and Yoon. The point to emphasize is that here we analyze interstate conflict beginning in 1886, using information on economic interdependence from specialized sources.¹¹ The addition of the pre–World War I and interwar

7. See Bliss and Russett 1998; Morrow, Siverson, and Tabares 1998; and Mansfield, Milner, and Rosendorff 2000.

8. See Russett 1995, 173–74; Maoz 1998; Hensel, Goertz, and Diehl 2000; and Diehl and Goertz 2000, chap. 6.

9. Kant [1795] 1970.

10. See Oneal and Russett 1999c; and Russett and Oneal 2001.

11. Oneal and Russett 1999c. We are grateful to Soo Yeon Kim for helping to produce these trade data.

years accounts for the differences between the results we report and those of Green, Kim, and Yoon. Even with a fixed-effects model, our tests for the longer time span confirm the pooled analyses we have reported elsewhere.

First, we estimate the basic pooled model with a single intercept. We explain the onset of a militarized dispute, 1886–1992, as a function of seven variables, each of which is lagged one year.¹² Those of greatest theoretical interest are the lower democracy score in each dyad, the trade-to-GDP ratio of the state that is less economically dependent on its dyadic partner, the bilateral balance of power (or capability ratio), and an indicator of whether the two states in each dyad are allied. In addition, we control for whether the states are contiguous either directly or through dependencies, the distance separating them, and whether the dyad includes a major power. This specification—derived from Stuart A. Bremer’s early dyadic analyses—is the one we have emphasized in our previous work.¹³ We use the lower democracy score and lower trade-to-GDP ratio because they indicate the degree to which the state with the greater freedom of action is constrained from using military force. We do not include a measure of economic growth because we have not found it to be robustly related to the likelihood of dyadic disputes.¹⁴

We use GEE to generate our first results. GEE is a flexible, quasi-likelihood method developed specifically for pooled time-series, cross-sectional analyses.¹⁵ It can be used to estimate general linear models, for which researchers can specify the link function and the correlational structure of the error term. By specifying an autoregressive process in the error term, GEE provides an alternative method for taking into account temporal dependence, what Green, Kim, and Yoon call “dynamics.” We use the logistic curve, adjust for first-order autoregression (AR1), and report robust standard errors corrected for data clustered by dyad. As recommended by Green, Kim, and Yoon, we include only time series with at least twenty years of observations.

The estimated coefficients for our pooled analysis with a single intercept are shown in the first column of Table 1. The results are similar to those we have reported elsewhere. Using the cross-sectional as well as temporal information in the data, there is strong support for the liberal peace; both the lower democracy score and the smaller trade-to-GDP ratio help to explain interstate conflict ($p < .001$). To show the substantive importance of the liberal variables, we calculated the reduc-

12. In the analyses below we use the onset of a militarized dispute, rather than every year of a dyad’s involvement in a dispute, because this is preferred by most other researchers; but the results with the two dependent variables are similar both here and in general. Oneal and Russett 1999a.

13. Bremer 1992.

14. Russett and Oneal 2001, chap. 4. Heldt shows why using the simple growth rate, as an additive term in the equation, is an inadequate specification. Heldt 1999.

15. See Diggle, Liang, and Zeger 1994; and StataCorp 1999. We have explained our preference for GEE over the peace-years correction recommended by Beck, Katz, and Tucker 1998 in Oneal and Russett 1997 and 1999a; but we have also shown (Oneal and Russett 1999a,c), as have Bennett and Stam 2000, that there is strong evidence for the liberal peace when that method of correcting for temporal dependence is used.

TABLE 1. *Alternative regression analyses of the onset of militarized disputes, 1886–1992*

<i>Variable</i>	<i>Pooled with GEE correction for AR1</i>	<i>Fixed effects</i>	<i>Fixed effects with distributed lags</i>
Contiguity	2.01*** (0.19)	1.80*** (2.48)	1.61*** (0.29)
Capability ratio (log)	−0.296*** (0.051)	−0.092 (0.092)	−0.039 (1.50) ^a
Alliance	−0.341* (0.175)	−0.436*** (0.127)	−0.358* (8.50) ^a
Democracy (L)	−0.0555*** (0.0111)	−0.0327*** (0.0099)	−0.0538*** (21.35) ^a
Bilateral trade/GDP (L)	−50.4*** (14.8)	−18.4 (10.5)	−21.0* (7.80) ^a
Distance (log)	−0.453*** (0.057)	Dropped: no within- group variation	Dropped: no within- group variation
Major power	1.68*** (0.18)	1.51*** (0.30)	1.47*** (0.33)
Lagged dispute	—	—	1.88*** (115.2) ^a
Constant	−2.03*** (0.47)	—	—
<i>N</i>	115,942	18,792	15,715
Log likelihood	—	−2,784.3	−2,310.9
χ^2	1,148.35	145.78	271.73
Prob of χ^2	< .0001	< .0001	< .0001

^aIndicates χ^2 statistic, not standard error of the regression coefficient. The sum of the coefficients of the lagged terms is reported for these variables.

*** $p \leq .001$.

** $p \leq .01$.

* $p \leq .05$.

tions in the likelihood of a militarized dispute that result from moving from the tenth percentile among contiguous dyads to the ninetieth percentile on our measures of democracy and interdependence. As Green, Kim, and Yoon did, we compare these effects to the pacific benefit of two states being allied. All other variables are held constant at their means for the contiguous dyads. As seen in column 1 of Table 2, increasing economically important trade lowers the risk of conflict by 56 percent, twice the effect of an alliance. The benefit of increasing the level of democracy in the less democratic state cuts the incidence of militarized disputes by 64 percent.

The second column of Table 1 reports the estimated coefficients for a fixed-effects logistic regression analysis. Again the results are consistent with liberal theory. The lower democracy score is still significant at the .001 level, and the

TABLE 2. *Change in annual probability of a militarized dispute, 1886–1992 (based on the estimated coefficients in Table 1)*

	<i>Pooled with GEE correction for AR1</i>	<i>Fixed effects</i>	<i>Fixed effects with distributed lags</i>
Increase democracy (L) from tenth to ninetieth percentile	–64%	–44%	–62%
Increase in bilateral trade/GDP (L) from tenth to ninetieth percentile	–56%	–22%	–25%
Change alliance from zero to 1	–28%	–32%	–27%

trade-to-GDP ratio has a significance level of .08.¹⁶ An alliance is negatively associated with the onset of a dispute ($p < .001$) as before, but the capability ratio is now not significant ($p < .32$). The substantive effects are reported in the second column of Table 2. Democracy still has the greatest effect, reducing the probability of a dispute by 44 percent. An alliance lowers the risk of conflict by 32 percent, and greater dependence on trade lowers the incidence of disputes by 22 percent. These estimated effects are lower than those calculated in a simple pooled design, but they are of the same order of magnitude and are clearly important in practical terms.¹⁷

Why Do Our Results Differ from Those of Green, Kim, and Yoon?

As already noted, the addition of data going back to 1885 accounts for the differences between our fixed-effects analysis and that reported by Green, Kim, and Yoon. These differences are not so apparent in the pooled analysis, where we have nearly 116,000 observations and Green, Kim, and Yoon have about 94,000. Even there, however, the extra years provide assurance that the liberal peace is not just an artifact of the Cold War.¹⁸ But when indicators for the individual dyads are employed in the fixed-effects specification, we still have roughly 19,000 cases—a

16. For ease of comparison with Green, Kim, and Yoon’s analyses, we use here two-tailed tests of statistical significance. We have previously used one-tailed tests on the grounds that liberal theory clearly specifies that democracy and trade are expected to reduce the likelihood of interstate conflict. Thus, one-tailed tests seem more appropriate than the agnosticism implied by two-tailed tests. By following their choice, we refrain from making the strongest case for our theoretically based findings.

17. With fixed-effects regression, the probability of a dispute is estimated for each group and is conditional on the number of positive outcomes. We have calculated the reduction in risk assuming that the probability of a dispute was .043, the baseline rate for a contiguous pair of states estimated using the pooled analysis, and that the fixed effect is zero.

18. See Oneal and Russett 1999a,b; and Russett and Oneal 2001.

reduction of 84 percent from the pooled analysis—whereas Green, Kim, and Yoon have only about 6,000.¹⁹ Examining a longer historical period means that more dyads have experienced at least one dispute, so that these cases enter into the estimation. In our analysis of the period 1886–1992, 388 different dyads are included; only 201 remain if we limit our attention, as Green, Kim, and Yoon did, to the post–World War II period. Including the additional years and dyads not only increases the number of observations available for analysis but also produces substantially greater variation in the independent variables. Like Green, Kim, and Yoon, we find no evidence of the liberal peace if we restrict our analysis to the years 1951–92;²⁰ but the greater variation that results from expanding the analysis to 1886–1992 allows the effects of democracy and interdependence to become apparent.²¹ A fixed-effects test limited to the short period eliminates all the democratic, interdependent dyads that have been at peace throughout the years since World War II—pairs involving the countries of Western Europe, North America, and Japan among others. It also excludes many dyads in which one or both states experienced dramatic transformations in their political regime in the aftermath of either of the two world wars. As Green, Kim, and Yoon note, “variation is a necessary condition for a dyad to be informative within the context of a fixed-effects model.”²²

We wanted to be sure that the liberal benefits reported in the second column of Tables 1 and 2 extend across the whole period, not only the years before World War II. This is just the opposite of the concern raised by Henry Farber and Joanne Gowa, who have worried that the democratic peace is an artifact of the Cold War.²³ It would also be inconsistent with the view that economic interdependence was ineffective in constraining states at the time of World War I. To investigate the post–World War II effects, we created an indicator variable that equals 1 if an observation is from the period 1951–92 and zero if from an earlier year. We then interacted this with each variable in the fixed-effects analysis. The results, which are

19. Green, Kim, and Yoon (455) seem sensitive to the loss of information entailed by using the fixed-effects model with rare events. They report that they have 93,924 observations and say in a note to their Table 3 that 87,402 observations have no variation in outcome. This means, of course, that these observations are not included in the estimation process; but they insist that it “should be stressed that cases without temporal variation are not dropped from the analysis; each such case simply adds zero to the likelihood function and is therefore computationally irrelevant.” Their claim that this is “more than statistical trivia” is not reassuring. The Stata routine they used reports an *N* of only 5,820.

20. In our analysis of the post–World War II period, democracy and the bilateral trade-to-GDP ratio have positive signs, but neither is significant at the .05 level; alliance is negative and significant at the .01 level.

21. We calculated the standard deviation for our measures of democracy and interdependence, dyad by dyad, for the cases included in our fixed-effects estimation. The average value for the standard deviation of the lower democracy score is 3.11 on a twenty-one-point scale; it is only 2.13 for the years after 1949. For the trade-to-GDP ratio, the difference is greater when viewed as a percentage: 0.00185 for the cases included in our analysis of all years versus 0.00122 for just the years 1951–92—a reduction of 34 percent for the shorter, post–World War II period.

22. Green, Kim, and Yoon 2001, 455.

23. See Farber and Gowa 1997; and Gartzke 1998.

not reported in a table, reveal no statistically significant differences between the pacific benefits of democracy and interdependence before and after World War II. Thus, as we have shown in pooled analyses, liberal theory is supported over a span of more than one hundred years.²⁴

Do Democracy and Interdependence Cause Peace?

Green, Kim, and Yoon suggest that information in the time series can be exploited to enrich our understanding of the dynamics of interstate relations. But simply including a lagged dependent variable in the regression model is inadequate for several reasons, especially in logistic analyses of rare events. First, the lagged dependent variable is expected to be a function of past values of the independent variables. If conflict at time t is a consequence of democracy, trade, and so on at $t-1$, then conflict at $t-1$ will be a function of these variables at $t-2$. Then, adding a lagged dependent variable will reduce the apparent influence of the independent variables. Second, a dispute in any of several recent years, not just the last one, is likely to influence current interstate relations. Adding a single lag of the dependent variable ignores this possibility. Finally, the independent variables, too, may have long-term effects. A pair of states with a history of close economic relations, even if recently involved in a dispute, may be less likely to become embroiled in conflict than states that have never been interdependent. To properly assess our theories, we need to address this possibility. We employ a distributed-lags model for this purpose.

Clive W. J. Granger proposed that a variable X might plausibly be considered a cause of Y if past values of X can be used to predict the current value of Y more accurately than using Y 's past values alone.²⁵ Thus we determine whether we can better predict the current likelihood of the onset of a dyadic dispute, within a fixed-effects model, by using past values of the liberal influences than by using only

24. See Oneal and Russett 1999b; and Russett and Oneal 2001. Bennett and Stam provide valuable independent support for the liberal peace. Bennett and Stam 2000. They conduct twenty-four tests of the consequences of democracy and interdependence, 1886–1992. Six include dyadic fixed effects with dynamics, but Bennett and Stam's modeling of conflict dynamics is substantially richer than that employed by Green, Kim, and Yoon. Instead of a single lag of the dependent variable, Bennett and Stam use a spline function of the years of peace. Democracy is associated with peace at a high level of statistical significance in all six of these estimations, and interdependence significantly increases the prospects for peace in four—more than any other variable except democracy and geographical contiguity. Being allied, the influence that Green, Kim, and Yoon emphasize on the basis of an analysis limited to the post–World War II period, is significant in three tests. Bennett and Stam report the results of other analyses using a variety of estimators, sets of cases, and specifications; and they control for a number of other influences thought to affect the incidence of dyadic conflict. Notably, all their tests include an indicator, based on Bueno de Mesquita and Lalman's international interaction game, that conflict is likely. Bueno de Mesquita and Lalman 1992. This is important because many consider their model of the strategic behavior of self-interested, expected-utility maximizers to be the most sophisticated theory of interstate conflict. For other fixed-effects analyses confirming a negative effect of democracy on military disputes, see Schultz 2001.

25. Granger 1969.

the history of their past disputes. We include three lags of the dependent variable (the onset of a dispute) and three lags of each of the independent variables (democracy, interdependence, and so on) except for contiguity and major power status, which are considered strictly exogenous. Incorporating lagged values of our dependent variable, the onset of a militarized dispute, also provides some protection against accepting a spurious correlation as evidence of a causal relation, because these terms act as proxies for explanatory variables omitted from the regression equation.²⁶

Including three lags of the dependent variable controls for the dynamics of interstate relations more completely than the single lag used by Green, Kim, and Yoon, of course. Using an equal number of lags of democracy, the trade-to-GDP ratio, and other variables allows the past values of these theoretically interesting variables to influence the current likelihood of conflict. In this way persistent benefits of democracy, interdependence, and so on that ameliorate the harm done by a recent conflict can be detected. This is especially important for fixed-effects analyses of rare events because of the reduction in the number of cases available for testing. We determined the number of lags to be included by adding additional terms until none was statistically significant.²⁷

The results of estimating a fixed-effects, distributed-lags model are reported in the third columns of Tables 1 and 2. Instead of reporting the coefficients for all the lagged variables, we give in Table 1 the sum of the three coefficients for each of the variables on the right-hand side of the regression equation (the lower democracy score, the lower trade-to-GDP ratio, and so on), the χ^2 statistic for each of these sets of variables, and the probability that the coefficients are jointly insignificant. The sum of the coefficients indicates the net effect of a variable if its value remained constant over a three-year period. Their sum is more meaningful than the individual coefficients because the lagged values of each variable are highly correlated; consequently, the estimated coefficients of the various lagged terms should not be given much weight. We report the coefficient, standard error, and the probability associated with the Wald test for the strictly exogenous variables.

As seen in column 3 of Table 1, the hypothesis that the lagged values of the democracy score are jointly zero can be decisively rejected ($p < .001$). The three lags of the bilateral trade-to-GDP ratio are significant at the .05 level and the lagged values of the indicator of an alliance at .04. The substantive effects are shown in the third column of Table 2. With distributed lags, the fixed-effects model indicates that democracy reduces the likelihood of conflict by 62 percent, economically important trade by 25 percent, and an alliance by 27 percent. These results are consistent with the distributed-lags tests in a pooled design that we have reported elsewhere.²⁸ They are additional evidence that the classical liberals were right: democracy and interdependence causally affect the prospects for war and peace.

26. Burkhart and Lewis-Beck 1994.

27. Beckett 1993.

28. Oneal and Russett 2000.

Explaining Bilateral Trade

Thus far we have focused on interstate conflict, but the analysis of bilateral trading patterns also deserves attention. Green, Kim, and Yoon start with a common economic explanation of bilateral trade—the gravity model—and then add two political variables of interest: the lower democracy score in each dyad and an indicator of whether or not the members of a dyad are allied. As they note, the gravity model offers reasonably accurate predictions of trade volumes despite its somewhat shaky theoretical underpinnings. It is, however, consistent with the Heckscher-Ohlin theory and with other explanations of bilateral economic relations that predict a positive relationship between bilateral trade and GDP, and a negative one between trade and population.²⁹

Green, Kim, and Yoon's analyses with a fixed-effects model produce some unexpected results. One is a strong positive relationship between population and trade. This finding is surprising because, if GDP is held constant, trade would increase as GDP per capita declines. It seems unlikely that commerce expands as countries' inhabitants become poorer. Certainly it is impossible in the long term: trade would be greatest when average incomes are zero. More relevant to the interests of readers of this journal, however, is their finding that more democratic countries trade less with one another when fixed effects are taken into account.

Table 3 reports three analyses of the volume of bilateral trade flows. Again we consider the years 1886–1992. Column 1 shows the coefficients for a pooled regression analysis with dynamics. As Green, Kim, and Yoon did, we estimate this specification using OLS; but we report robust standard errors. This model includes a lagged dependent variable but no fixed effects. The use of a lagged dependent variable is less problematic with a continuous dependent variable and is the specification recommended by Nathaniel Beck and Jonathan N. Katz in their comment in this issue on Green, Kim, and Yoon.³⁰ The results of this analysis are similar to those in column 3 of Green, Kim, and Yoon's Table 2. Like them, we find that two democracies trade more than two autocracies or a mixed dyad,³¹ and allies have lower levels of trade than nonallies, other things being equal. All the estimated coefficients are highly significant.³²

29. For a comprehensive consideration of the gravity model, see Deardorff 1995. For a review of theories indicating a negative relationship of trade with population when controlling for GDP, see Mansfield, Milner, and Rosendorff 2000.

30. Beck and Katz 2001.

31. Nor does this simply reflect the greater peacefulness of democracies. We estimated an analysis that included an indicator of whether or not a dyad had experienced a militarized dispute in year $t-1$. The coefficient of this term was negative, as expected, and quite significant ($p < .003$); but the lower democracy score was significant as well ($p < .001$) and its coefficient declined very little (.00325 versus .00327).

32. We also estimated a pooled analysis using GEE with an adjustment for autocorrelation. The results were similar to the results produced using OLS (column 1, Table 3) except that the GEE analysis indicated that allies do trade more than nonallies.

TABLE 3. *Alternative regression analyses of bilateral trade, 1886–1992*

<i>Variable</i>	<i>Pooled with lagged dependent variable</i>	<i>Fixed effects with lagged dependent variable</i>	<i>Fixed effects with lagged dependent variable, zero values of trade dropped</i>
GDP (log)	0.0787*** (0.0025)	0.197*** (0.005)	0.259*** (0.005)
Population (log)	−0.0252*** (0.0021)	−0.212*** (0.011)	−0.203*** (0.013)
Distance (log)	−0.0742*** (0.0032)	Dropped: no within- group variation	Dropped: no within- group variation
Alliance	−0.0223*** (0.0068)	0.0463*** (0.0121)	0.0133 (0.0139)
Democracy (L)	0.00326*** (0.00033)	0.00123* (0.00052)	0.00458*** (0.00057)
Trade _{<i>t</i>−1} (log)	0.916*** (0.002)	0.791*** (0.002)	0.684*** (0.002)
Constant	−1.36*** (0.05)	−2.21*** (0.08)	9.63*** (0.11)
<i>N</i>	104,321	104,321	93,625
<i>R</i> ² overall	0.94	0.93	0.90
<i>R</i> ² within groups		0.78	0.67
<i>R</i> ² between groups		0.98	0.96

****p* ≤ .001.***p* ≤ .01.**p* ≤ .05.

In the second column of Table 3, we control for fixed effects, again including a lagged value of trade. Here our results differ substantially from those reported by Green, Kim, and Yoon for their fixed-effects model with dynamics (column 4 in their Table 2). We find, as in the previous analysis without fixed effects, that democracies have higher levels of commerce ($p < .02$) than would be expected on the basis of the gravity model. Our results also differ from Green, Kim, and Yoon's in other ways. Alliances now have a significant positive effect ($p < .001$) on commercial relations. And we do not get the odd finding that trade increases as states get poorer: the coefficient of the logarithm of population in column 2 is negative and very significant ($p < .001$).³³ The differences between our results and Green, Kim, and Yoon's are not primarily due to our using data from a longer historical period. Analyses we conducted for just the period 1951–92 are similar to what we report in Table 3. So what accounts for the differences?

Our data are similar but differ from Green, Kim, and Yoon's in important regards. First, they use as their dependent variable the natural logarithm of trade in current

33. Like Green, Kim, and Yoon, we do not report robust standard errors in column 2 of Table 3; Stata 6.0 does not allow this option.

dollars. We use the log of trade in millions of constant 1990 dollars. Constant dollars are appropriate because we are making comparisons through time as well as cross-sectionally. We use millions of dollars, like the International Monetary Fund, because the trade data are not accurate to the dollar. Second, before taking the logarithm we assigned a different value to the trade variable for dyads that report no trade. Some value must be imputed because the logarithm of zero is undefined. We use \$100,000; Green, Kim, and Yoon used \$1. It is this that accounts for most of the differences between our results and theirs.³⁴

To show this, we reestimate our dynamic fixed-effects model using only those dyad-years when some trade actually took place. This is helpful because any assigned value is to a degree arbitrary, but certainly the value chosen should not by itself determine the results. The coefficients using only the observations with nonzero values of trade are given in the last column of Table 3. They confirm that democracies have higher levels of trade than do other pairs of states. The coefficient of the lower democracy score is positive, nearly four times its value in column 2, and even more significant (t -score = 7.98). The same reversal in sign and change in significance level for the lower democracy score are obtained if the \$1 values of trade are dropped from Green, Kim, and Yoon's data. The greater consistency of the results we report in Table 3 compared to those in Green, Kim, and Yoon and the implausibility of their finding that trade increases as average income declines indicate that their results reflect their methodological choices rather than the character of interstate economic relations. Choosing a very low value for states with no commerce before taking the logarithm, especially, distorts their analyses. A comparison of the variance explained using \$1 and \$100,000 tends to confirm this. The overall R^2 in their fixed-effects analysis using the low value is .71; it is .93 for the same period with the higher value we have used.³⁵ A similar difference exists for the pooled model with lagged dependent variable.

Table 4 shows the substantive effects of democracy on bilateral trade. Because we are using linear regression analyses, the increased trade expected between two democracies is easily calculated. Using the coefficients produced by our pooled analysis with dynamics (column 1), an increase from the tenth to the ninetieth percentile in the lower democracy score, that is, going from -10 to +9 on the Polity III democracy-autocracy scale,³⁶ increases trade by 6.4 percent. The increase is 2.4 percent if the coefficient from the fixed-effects model with all cases (column 2) is used. It is 9.1 percent if dyads with no trade are excluded (column 3), suggesting that

34. If we use \$1 in place of zero values of trade and limit our analyses to 1951-92, the coefficient of the lower democracy score is -.00959, similar to what Green, Kim, and Yoon report, and statistically significant. If we use \$1 and the years 1886-1992, the coefficient is still negative but not significantly different from zero.

35. These comparisons are based on the statistics produced by Stata and are consistent with the log-files provided by Green, Kim, and Yoon. They report an adjusted R^2 of .77 in column 4 of their Table 2, which they calculated themselves using the squared correlation between observed and predicted values with an adjustment for the degrees of freedom.

36. Jagers and Gurr 1996.

TABLE 4. *Change in volume of bilateral trade, 1886–1992 (based on estimated coefficients in Table 3)*

Variable	Pooled with lagged dependent variable	Fixed effects with lagged dependent variable	Fixed effects with lagged dependent variable, zero values of trade dropped
Increase democracy (L) from tenth to ninetieth percentile	+ 6.4%	+ 2.4%	+ 9.1%
Change alliance from zero to 1	–2.2%	+ 4.7%	+ 1.3%

our analysis in column 2 understates the tendency of democracies to become economically interdependent.³⁷ The effect of an alliance is more variable. Allies trade 2.2 percent less than nonallied states if the pooled analysis is used. They trade 4.7 percent more if all cases are analyzed in a fixed-effects model or 1.3 percent more if only the nonzero values of trade are used in the estimation. In two of the three analyses, the effect of democracy on the volume of trade exceeds that of an alliance.

Implications for Theory and Policy

Nietzsche says in *The Twilight of the Idols*, “What does not kill me makes me stronger.” The challenge posed by Green, Kim, and Yoon provoked much thought and induced us to add to our statistical skills. The tests we conducted in response make our confidence in the liberal peace even greater. Nevertheless, we do not encourage the use of a fixed-effects model for analyzing pooled data. As we have shown in analyses of both dichotomous and continuous data, it can seriously distort results. Moreover, what kind of theory would purport to explain variation through time but not accrued differences across groups? The analyses of time-series and cross-sectional data should give us the same answers, as Green, Kim, and Yoon note.³⁸ Because fixed-effects models allow a unique baseline for each dyad, they require us to believe that cross-sectional comparisons contain no theoretically relevant information. This is less plausible than imposing a single intercept. They also require substantial variation in the independent variables, which may not be present over short historical periods.

37. Analyses we have reported for the politically relevant dyads in a pooled design (Russett and Oneal 2001, chap. 6) and for all dyads with a distributed-lags model (Oneal and Russett 2000) also suggest that the fixed-effects model understates the effects of democracy on the level of trade.

38. Green, Kim, and Yoon 2001, 458.

The incorporation of fixed effects is particularly inappropriate when we are considering rare events like the occurrence of a militarized dispute. It is simply impossible to think that the 97,150 annual observations of the experiences of the 2,751 dyads that managed to live in peace—84 percent of our total number of cases—tell us nothing about the causes of war. As we have seen, a fixed-effects model can lead to seriously misleading conclusions when there is limited variation in either the left- or right-hand side variables. After all, epidemiologists studying the causes of cancer do not normally limit their analyses to those who have already had the disease. They also look at individuals who have never had cancer.

Indeed, if the results of our fixed-effects analyses are accepted, it cannot be true that comparisons across dyads are irrelevant. Consider the onset of a militarized dispute. According to liberal theory, states should experience long periods of peace when they become democratic and economically interdependent. The fixed-effects tests we conducted provide added evidence that the classical liberals were right. *Ipso facto*, we should accept that the long-lasting peacefulness of some pairs of states is a result of their being interdependent and democratic. Green, Kim, and Yoon are right that it is important to consider whether the evidence for our theories is only cross-sectional. The ultimate goal is to develop explanations of conflict that are realistic, not just instrumental, so that we can intervene purposefully. Corroboration of our theories using fixed-effects analyses of behavior through time can aid in that enterprise, when the data permit; but it is only one technique among several alternatives. To continue with the analogy, it is useful to know whether quitting smoking improves the prognosis for someone who has already had a bout with cancer; but surely this is not the only way or even the best way to explore the health effects of smoking.

Nor should we believe that the result of Hausman's specification test (or an *F*-test regarding the joint significance of the intercepts) forces us to limit our analyses to those incorporating fixed effects. With a large number of cases, it is very likely, as Green, Kim, and Yoon note, that this test will indicate that there are statistically significant differences between at least some of the within-groups coefficients and those across groups. Once it is verified that these differences are not substantively important, as we have shown both for the pacific benefits of democracy and interdependence and for the effect of democracy on the level of trade, the best summary of the consequences of our theoretical variables is the average of the within- and across-groups effects. This is especially true because fixed-effects regression uses a great number of degrees of freedom and produces standard errors that are sometimes several times larger than those generated by pooled analysis.³⁹

Social scientific research on the causes of war has progressed rapidly over the past twenty years by focusing on the behavior of pairs of states observed through time. Dyadic analysis provides an important advance over studying international relations at either the systemic or the national level of analysis because it directly addresses

the questions of greatest concern to political scientists and policymakers alike: which states are likely to fight one another, and which will remain at peace? Certainly, understanding the causes of war and peace is a difficult undertaking, and we must subject our hypotheses to a variety of methodologically sophisticated, demanding analyses. Fixed-effects models have only a limited role to play, for the reasons explained in this issue by Beck and Katz and by Gary King.⁴⁰

We should also take care, lest increasing technical expertise lead to radical skepticism. Green, Kim, and Yoon warn of the dangers of pooling and offer the fixed-effects model as a remedy. Yet they “resist the temptation to draw any particular substantive conclusions about the sources of militarized disputes” from their own analyses. They complain of nagging methodological problems arising from reciprocal causation, inadequate measurement, selection bias, variability in the slope parameters across dyads and over time, interactions among the independent variables or between these variables and omitted regressors, and so forth. But all social science faces these difficulties. If our work is to be useful, researchers must navigate through Scylla and Charybdis, avoiding both type I and type II errors of inference. This requires that we minimize the joint danger of accepting as true what is false, because our tests are not sufficiently demanding, and of rejecting as false what is true, because our standards are unrealistic given the limitations of our data. Green, Kim, and Yoon do not achieve this balance.

Caution is essential. The world has ample experience of public policy made on the basis of untested, badly tested, or untestable theories. But there is abundant corroboration, from many different researchers using a wide variety of empirical techniques, that democracy and economic interdependence substantially reduce the danger of violent interstate conflict. There are good theoretical grounds for confidence, too. The bloody nature of our subject compels us to offer practical guidance when the science is so strong.

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40. See Beck and Katz 2001; and King 2001.

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