Trade and Conflict in the Cold War Era: An Empirical Analysis Using Directed Dyads

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Abstract

What is the relationship between trade and conflict in the post-World War II era. Using a directed dyad-year design, and a sample of both all dyads and of politically relevant dyads, this paper uses the generalized additive model to study the relationship between dyadic trade and militarized interstate disputes (both all disputes and those involving casualties). Trade appears to have a non-linear impact on conflict, with increases in dyadic trade sometimes being conflictual and sometimes being pacific. In any event, there is little evidence for a causal pacific impact of trade, but also little evidence that trade is inherently conflictual, other than it being an obvious necessary condition for trade disputes and also signaling that dyadic partners are in some interesting relationship.
1 Introduction

Looking at the postwar era, it appears that the finding that democracies are more pacific is relatively robust (Maoz and Russett, 1993; Oneal and Russett, 1997). There is somewhat more controversy on the role of trade, with Barbieri (1996) finding that trade leads to conflict, Oneal and Russett (1999) finding that trade is pacific and Beck, Katz and Tucker (1998) finding no statistically significant effect of trade on conflict.\(^1\) On a theoretical level one could argue that trade increases the opportunity for conflict (and many of the non-Cold War postwar conflicts have been trade conflicts), in that nations that have essentially no commercial relationship are unlikely to enter into at least some types of militarized disputes. On the other hand, nations that trade have an interest in the peaceful resolution of disputes: militarized conflict will impede trade, and hence be costly.\(^2\)

This paper examines dyad-years, looking for the presence of absence of conflict initiation in a given year from one dyadic partner to the other. Using directed dyads allows us to examine whether trade deters aggressive acts.\(^3\) Because trade data is only readily available in the post-World War II era, I limit my analysis to this period. Again, this mirrors previous analyses. As is also common, I examine both data sets containing all dyads and one limited to dyads with a relatively high \emph{ex ante} probability of conflict, the so-called “Politically Relevant Dyads” (Maoz and Russett, 1993).

The purpose of this paper is to use a somewhat new empirical method, the Generalized Additive Model (Hastie and Tibshirani, 1990) to more flexibility model the relationship between trade and conflict. This method allows us to easily see if trade has non-monotonic effects, and hence furthers the inquiry begun by Barbieri and Oneal and Russett. The paper builds on a previous effort by one of the authors (Beck, 1999) which used similar methodology but with undirected dyads. Since that paper still only exists as a conference version, much of the methodological argument of that paper is reproduced here.

\(^1\)Barbieri and Schneider (1999, 394) argue that “statistical studies of the trade-conflict relationship provide a mixed set of findings.” Barbieri and Schneider’s introduction to the special issue of the \emph{Journal of Peace Research} provides an excellent overview if the literature on trade and conflict.

\(^2\)Again, see Barbieri and Schneider (1999) for a review of the theoretical arguments.

\(^3\)It is not trivial to know which dyadic partner was the aggressor, and the solution used here is far from ideal. This issue is discussed below.
The previous paper and the current effort looks at both the standard measure of conflict, whether or not a dyad has engaged in a “Militarized Interstate Dispute” ("MID") (Gochman and Maoz, 1984) and a second dependent variable, which is based on whether a dyad engaged in a subset of the MID conflicts. MIDs, by definition, involve some use of militarized force. But militarized force can be a long way from war. In particular, events such as troop movements count as MIDs. The MID data set does have an ordinal variable, highest level of force used, but unfortunately this variable does not distinguish more and less significant MIDs. The MID hostility level codes range from one to five, where five is interstate war (involving over 1000 casualties). But the seizure of a single fishing boat results in a hostility code of four! This is particularly critical for examining the role of trade in conflict, since many conflicts are trade conflicts, and a number of those end up in fishing boat seizures. These seizures, which involve no casualties, hardly seem like war, yet if we used the MID hostility level scale, they would be coded as being in the category right below interstate wars. Fortunately the MID data also codes for the number of casualties. To eliminate less serious MIDs, I therefore examine the dependent variable “MIDC,” which is one if a dyad engaged in a MID which led to casualties. This should eliminate many of the “minor” trade MIDs, and hence should eliminate what is to my mind a spurious reason for trade appearing to be conflictual. It is interesting to note that for the post-World War II era, only about 30% of MIDs resulted in any casualties.

The next section of this paper lays out the methodology in more detail. Empirical results are in Section 3 with Section 4 concluding.

2 Methods

2.1 Generalized Additive Models

Empirical researchers make many assumptions in their use of statistical models. Linearity must rank as one of the more ubiquitous, albeit seldom acknowledged, assumptions. We rarely know the correct functional form. While we often have a prior that relationships are monotonic, for the trade-conflict relationship we lack even that. I thus use the generalized additive model

\footnote{4A longer term project would be to actually recode all the MID data, but such is beyond the scope of this paper.}
("GAM") which allows each independent variable to have a non-parametric relationship to the dependent variable (Hastie and Tibshirani, 1990). I have discussed the utility of this model elsewhere (Beck and Jackman, 1998) so need not repeat that discussion here. While I am particularly concerned with non-parametric estimation of the effects of trade on conflict, I also allow other effects to be estimated non-parametrically since tests indicate the non-parametric estimates are superior to linear estimates. To estimate the smooth for trade, I use Loader’s (1999) \text{LOCFIT} smoother.\footnote{This seemed to work better than LOESS or spline smoothers for the trade data, which has this large clump of very small values. \text{LOCFIT} is still being developed, and, as indicated, some of the estimated standard errors seem to be a bit odd (though that may be a result of the very difficult data set). For the other control variables I used a cubic smoothing spline. All analyses were done in \text{SPLUS/2000}, using the June 14, 1999 \text{LOCFIT} library for \text{SPLUS/2000}.}

### 2.2 Duration Dependence

Empirical studies of the effect of trade on conflict have often used the dyad-year design (directed or undirected), typically involving a straightforward logit analysis.\footnote{While logit analyses involve a non-linear transformation, they assume a linear relationship between the independent variables and a "link" function, which is then transformed non-linearly into the probability of interest. For simplicity, I refer to such models as linear. The relevant methodological discussion is essentially the same as in Beck and Jackman (1998).} Many recent works correct for duration dependence by adding a smooth term in duration (\text{PEACEYRS}) to the logit.\footnote{The relevant methodological discussion may be found in Beck, Katz and Tucker (1998) and need not be repeated here.} Because I am using \text{SPLUS} for this paper, I use a smoothing spline in duration (\text{PEACEYRS}), rather than the natural splines discussed in the earlier article. This has no consequences of any interest. All specifications reported in this paper include the smoothed \text{PEACEYRS} variable.

In their recent article, Oneal and Russett (1999, 427) argue that the "generalized estimating equation" ("GEE") of Liang and Zeger (1986) is a superior way to correct for duration dependence. But their use of the GEE correction is problematic, at best. The argument about why it is sensible to include \text{PEACEYRS} is in Beck, Katz and Tucker (1998) and need not be repeated in here. Suffice it to say that if one accepts that the datasets we are analyzing are event history datasets, as they are, then the use of
the PEACEYRS variable in the logit corresponds exactly to estimating a Cox (1972) proportional hazards model with an unspecified baseline hazard. The GEE procedure assumes that the hazard of conflict is constant over time (that is, in the event history jargon, there is no duration dependence).\footnote{Not to hammer this too hard, but the GEE logit estimated by Oneal and Russett has a time invariant form for the hazard of a dispute, with temporal issues only being relevant to correct problems relating to the estimation of terms in variance-covariance matrices. Looking at any of the GEE estimations produced by Oneal and Russett, one would conclude (by force) that the probability of the onset of a dispute does not depend on how long the dyad has been at peace. This is, to put it simply, at strong odds with what the data actually suggest about this. Oneal and Russett’s (1999, 427) analogy of including the PEACEYRS correction to including antecedent causal variables such as the money supply in a regression including inflation is therefore simply not a good analogy.}

In event history analysis this is always taken to be a testable assumption (easily testable within the PEACEYRS framework) rather than an untestable assumption that all analysis is conditional on. Since the test for duration independence clearly rejects that hypothesis, I cannot understand Oneal and Russett’s (1999, 427) argument that the GEE is superior to the PEACEYRS correction in that it gives their “theoretically specified model primacy in accounting for duration disputes.” It only does that by incorrectly assuming duration independence.\footnote{The GEE has other problems for the type of data being analyzed here. It was designed for large \( N \), small \( T \) panels, rather than the time-series-cross-section data analyzed here. All the good properties of the GEE depend on asymptotics in \( N \), whereas for our data the \( N \) is fixed (and asymptotics are in \( T \). Perhaps GEE is good for such data (and it is probably better to use the GEE over ordinary logit), but there is literally no evidence for this proposition.}

2.3 Measurement

Dependent Variable

Militarized Interstate Dispute. This study uses two dependent variables. The first is the commonly used measure of whether or not a dyad engaged in a militarized interstate dispute (MID) in a given year.\footnote{The dispute dataset was obtained at the Peace Science Society website at: \url{http://pss.la.psu.edu/cow/20data/mids/210.zip}.} While earlier studies typically used interstate war as a dependent variable, recent research examines militarized interstate disputes, of which interstate wars are a small subset. The former require a substantial number of battle deaths, while the
latter include any event involving the threat or actual use of military force.\textsuperscript{11}

Many of the MIDs are actually trade conflicts (boarding of fishing boats in disputed waters and the like), and obviously trade will increase the likelihood of such conflict. These conflicts, moreover, are often nothing like what we think of as wars, in that no casualties ensue. I thus use a second dependent variable in this study, “MIDC” which is one whenever a dyad engages in a MID that involves any casualties. While such conflict is a far cry from interstate war, it is more serious than the minor trade conflict which results in no casualties.\textsuperscript{12}

Since interest here is in whether trade reduces conflict, rather than whether trade reduces the length of conflict, the dependent variable is only coded as one for the first year of a conflict. For conflicts which endure over several years, the latter years of conflict are censored observations, and so do not enter the data set.\textsuperscript{13} Oneal and Russett (1999, 436) argue that one should not just look at the onset of disputes, but treat all years where a dyad is in dispute as a dispute. Treating all conflict years as conflicts is identical to assuming that a long conflict is identical to a sequence of short conflicts. Furthermore, the coding for multiyear conflicts is problematic in the dyad-year context. Assume a conflict starts in, say, the Fall of one year and then ends in the Spring of the subsequent year. Oneal and Russett would prefer to code this as two years of conflict. While it might be admirable to want a theory of the durations of both war and peace, it seems unlikely that the identical specification is appropriate for both cases. But this is exactly what Oneal and Russett must assume for their analysis of 'involvement' rather than 'onset.' This paper will only analyze what looks like their 'onset' variable.\textsuperscript{14}

\textsuperscript{11}The details of the Militarized Interstate Dispute (MID) dataset can be found in Gochman and Maoz (1984). We restrict our tests to those types of conflict involving actors that are both members of the nation-state system as defined by the Correlates of War Project \url{http://pss.la.psu.edu/intsys.html}.

\textsuperscript{12}Many MIDs are coded as having missing data for the casualties variable. For those observations, Matt Baum attempted to see if any newspaper sources indicated that the MID involved real hostilities, that is, either casualties or shooting. Baum will return to these efforts this summer.

\textsuperscript{13}It should be stressed that the first year of a new dispute is always coded as a one, even if it immediately follows a year with another dispute. I only censor ongoing disputes for the same dispute, as indicated by the same MID code.

\textsuperscript{14}Reading Oneal and Russett (1999), it is unclear to me if their onset variable includes the first year of new disputes immediately following other disputes. We surely have no disagreement that the onset of new MID disputes belong in the data set, even if such
We use directed dyads. This means we must know which partner instigated a MID. This is not easy to discern from the COW data set. Here we simply uses the COW indicated first mover. It is well known that this measure is problematic. But other fixes, such as using whether a state was revisionist, are also problematic. There is no doubt that this is a very serious problem in our analysis; the resolution of this problem must await extensive (and expensive) recoding of the COW data. There is no doubt in our minds that this recoding would be a worthwhile effort; directed dyads allow for the use of rational economic models whereas undirected dyads make sensible modeling difficult, at best.

In the dataset containing all dyads, there were 979 MIDs (that is, dyad-years with a MID after excluding ongoing disputes). Of these, only 292 were MIDs involving casualties. Limiting the data to PRDs yielded 742 MIDs with 251 MIDs. Thus about two thirds of the conflicts typically analyzed are conflicts which resulted in no casualties. It should also be noted that almost all the MIDs occurred between PRDs.

**Independent Variables**

**TRADE**

This paper is about trade and conflict. While a number of other variables enter the specifications, interest focuses on the effect of trade. Measuring trade exposure in undirected dyads is problematic, since one must measure exposure of both partners, or form one or more indices. The situation is much less complicated for directed dyads. Presumably the moving actor takes trade into account in deciding whether to move. Thus we only need be concerned with trade exposure of the moving partner. While we experimented using both imports and exports of the moving partner (as a proportion of GDP), because of multicollinearity (and perhaps other measurement issues) we ended up measuring trade as the dyadic imports plus exports of the moving partner as a proportion of the moving partner’s GDP.

Missing data is a major issue here. Following the lead of Oneal and Russett (1999), I use data which imputed values for trade for dyad-years where it was reported as missing in the IMF Direction of Trade dataset. The IMF dataset makes it hard to distinguish missing data from the near absence of trade. It is clearly wrong to simply drop dyads because they seemingly

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\[\text{disputes immediately follow a prior dispute.}\]

\[15\text{See the discussion in Oneal and Russett (1999).}\]
contain missing data, when most of these dyads simply do not trade (and also probably do not conflict). Unlike Oneal and Russett, the data set I use did not simply assume that all IMF “missing data” indicated zero dyadic trade. Instead, an informed decision was made about whether the code was a result of truly missing data or just reflected less than some minimal trade figure.\footnote{I should not make too much of this “informed” decision, since in the end most of the filled in trade values were zero. The IMF only reports trade figures for IMF nations. This would lead to exclusion of the Warsaw Pact nations, who clearly play a non-trivial role in post-War disputes. For non-IMF nations, World Bank data was used. Interpolation was used where a series had some gaps. GDP data comes from the Penn World Tables (release 5.6), with missing GDP data filled in a hopefully intelligent manner. Specific details on the coding are available from the author. Missing data were actually filled in by Kristian S. Gleditsch, then working at the Harvard Data Center and now on the faculty of the University of Glasgow. While he made all specific decisions, these were done under my direction, and always in consultation with me, so I take all blame for egregious imputation rules. We clearly need better measures of trade, including the requirement that the exports from A to B match the imports of B from A, a constraint the IMF does not meet!}

As (Oneal and Russett, 1999) have clearly shown the missing trade data issue is critical, and is one that eventually all researchers using trade in large-N analyses will have to deal with. To give but one simple example of how critical this issue is, I ran a simple linear logit model on the PRD data set using the trade measure used here (that is, with all missing data filled in). The coefficient on trade in that logit was -.3 with a standard error of 2.7. Re-running the identical regression, but dropping all observations which Oneal and Russett coded as missing (that is, dyads where one partner was not in the IMF) yielded a coefficient on trade of -4.1 with a standard error of 4.4. While both estimates are not significant, the change in coefficient estimates based on what dyads have missing trade data obviously makes it critical that conflict scholars agree on a reference trade data set.

It should be noted that the dyadic trade figures as a proportion of GDP cluster very tightly around zero. For PRDs, 95% of the dyads have a trade figure under 6%, and 98% are under 12%; the corresponding figures for all dyads are 1% and 4%. Since the confidence intervals for the effect of trade on conflict are extremely wide where there is very little data, the graphs showing the impact of trade on conflict are sometimes limited to the lower trade region which contains about 90% or more of the cases. Table 1 shows the quantiles of the trade variable for the two data sets.
Table 1: Quantiles of Trade Variable (in percent of GDP)

<table>
<thead>
<tr>
<th>Quantile</th>
<th>PRDs Only</th>
<th>All Dyads</th>
</tr>
</thead>
<tbody>
<tr>
<td>10%</td>
<td>.0002</td>
<td>0</td>
</tr>
<tr>
<td>25%</td>
<td>.009</td>
<td>0</td>
</tr>
<tr>
<td>50%</td>
<td>.11</td>
<td>.005</td>
</tr>
<tr>
<td>75%</td>
<td>.64</td>
<td>.08</td>
</tr>
<tr>
<td>90%</td>
<td>2.94</td>
<td>.42</td>
</tr>
<tr>
<td>95%</td>
<td>5.933</td>
<td>1.18</td>
</tr>
<tr>
<td>98%</td>
<td>11.45</td>
<td>3.32</td>
</tr>
<tr>
<td>99%</td>
<td>15.86</td>
<td>6.18</td>
</tr>
</tbody>
</table>

Control Variables

Democracy. For simplicity, I use a binary variable measuring whether both dyadic partners were democratic. While this loses some information, previous analysis (Beck and Tucker, 1998; Beck and Jackman, 1998) shows that the dichotomous joint democracy variable does not do a great disservice to the data. The democracy measure is constructed with Polity III data that were obtained from ftp://isere.colorado.edu/pub/datasets/polity3/policymay96.data. As is typical, the dyad is coded as democratic if both partners score above six on the Polity scale (Oneal and Russett, 1997).

Capability Asymmetry. Presumably if one side is going to be a clear winner, the probability of conflict will be low; a balance of forces should lead to greater conflict. Here I use a measure of capability symmetry, CapAym as a control. This measures is based on the relative power capability formula of Ray and Singer (1973). Their formula measures CapAsym by

\[
\text{Asymmetry} = \sqrt{2 \times \left( \frac{\text{CapA}}{\text{CapA} + \text{CapB}} \right)^2 + \left( \frac{\text{CapB}}{\text{CapA} + \text{CapB}} \right)^2} - 0.5 \quad (1)
\]

where CapA and CapB represent the respective composite indicator of national capabilities scores for states A and B.\(^{17}\) A score of zero on this measure

\(^{17}\)These scores come from Bennett and Stalm’s (1998) Eugene.
indicates a completely symmetrical dyad whereas, a value of one represents a perfectly asymmetrical one, with all force held by one partner.

*Similarity of Alliance Portfolios* States with dissimilar interests are more likely to enter disputes, since they presumably have more to gain from such disputes. The standard way to measure this in the dyad-year context is the similarity of the two partners’ alliance portfolios. Here I use SimInt, which is coded using Signorino and Ritter’s (1999) Tau-b measure, which ranges from -1 (complete dissimilarity) to 1 (complete similarity).\(^{18}\)

*Distance* The variable *Distance* is the distance, in kilometers, between the capital cities of the two partners.\(^{19}\) Distance will clearly be a non-trivial determinant of conflict in the data set consisting of all dyads. Note that I do not use a contiguity dummy variable in the specification, since *Distance* enters each specification non-parametrically.

*Peaceyrs* Each specification contains a variable, *Peaceyrs*, which is the number of years since the last dyadic conflict ended (or, before the first conflict, the number of years since the first year the dyad was observed).

In this paper we do not use an alliance variable. The alliance data are problematic post-1984, and in any alliances are likely to be endogenous. While this changes the specification from that used in Beck (1999), this change in specification does not appear to have had dramatic results.

### 2.4 Case-Selection

Since reasonable trade data is only available for the post-World War II period, this study analyzes data from 1946–1992. This period is the mostly widely studied one. There is no reason to believe it is a typical period, and, in particular, the Cold War nature of many disputes may make it an atypical period. But if we want a dyad-year large N study of the effect of trade on conflict, we have little choice but to study the post-World War II period.

\(^{18}\) Data were obtained from Richard Tucker’s “The Similarity of Alliance Portfolios, 1816-1984, version 2.50” web site (http://www.fas.harvard.edu/rtucker/data/affinity/alliance/similar/similar.html) and updated to cover 1985–92.

\(^{19}\) Data from Bennett and Stam’s (1998) Eugene.
(The data stops in 1992 for a variety of reasons, most of which are not theoretical. But stopping in 1992 does avoid mixing the Cold War era with the post-Cold War era.)

Which dyads should be analyzed? Some nations have such a small opportunity to interact with each other that their likelihood of conflict is inordinately low. Thus, recent focus has fallen on pairs of nation-states that have what Most and Starr (1989) call “opportunity” to wage conflict on each other. More specifically, the Maoz and Russett (1993) notion of Politically Relevant Dyads (PRDs), which consist of all dyads containing either at least one major power or two geographically contiguous partners, has become the standard for case selection in quantitative studies of war and peace. Following the lead of this literature, I analyze PRDs below.

It is the case, of course, that not all conflicts occur among PRDs. It also may be the case that trade is a good indicator of whether non-PRDs may come into conflict; non-PRDs that trade, after all, at least have some relationship, even if they are not geographically contiguous. Moreover, while the probability of a dispute in a PRD is greater than the similar probability for a random dyad, there are a non-trivial number of disputes that take place among non-PRDs.

Thus, for example, we have data on 34140 dyad-years for PRDs. These contain 1085 MIDs, for a rate of 3.2% per dyad-year. The full data set has 294691 dyad-years, with 1326 MIDs. Thus while the dispute rate for all dyads is only 0.5%, including all dyads does yield 241 more MIDs.\footnote{Since we drop the second and subsequent year of all ongoing disputes, these are 241 different MIDs.} For serious disputes, involving casualties, the figures are 345 serious disputes involving PRDs and 416 involving any dyad (a rate of 1% for PRDs and 0.1% for all dyads). We thus analyze both data sets, giving us four sets of analyses (PRDs and all dyads, analyzed for all MIDs and MIDs involving casualties).\footnote{I do not examine all possible dyads, limiting myself to dyads where each partner had a population in excess of a million people. While some “microstates” are defined as being part of the international system, in practice they lack sufficient data to be analyzed, and in practice they do not engage in interstate conflicts of any importance.}

For very practical reasons, I decided to create data sets which consisted of all dyad-years with conflict (of the relevant type) and a random sample 10,000
dyad-years with no conflict.\footnote{After dropping ongoing dispute years, the PRD datasets had 10193 and 10706 cases, for MIDs and MIDCs, respectively; the corresponding figures for the full datasets were 10281 and 11022 cases, respectively. The data set as analyzed had no missing data on any of the independent variables, so all analyses are based on these N's.} Given the sample sizes reported in the previous paragraph, this meant a sampling rate of about 3\% of all non-conflictual dyad-years and a sampling rate of about 30\% for all non-conflictual dyad-years involving PRDs. This sampling was done after the computation of PEACEYRS, so the dynamics are correctly maintained in the sampled data set. As King and Zeng (1999) have shown, such sampling on the dependent variable is statistically benign. Such sampling reduces computational time from minutes or hours per analysis to seconds or minutes. King and Zeng discuss issues related to inference from the sampled data back to the population. Since here I only report the equivalent of logit coefficients, these issues are not particularly relevant to my study.

3 Data Analysis

The results of a GAM estimation with linear trade terms are in Table 2. Since all the other terms (except joint democracy) were entered as smoothing splines, only the coefficients for trade and democracy are shown in the table. The coefficient for democracy, which is a dummy variable, provides an excellent numeraire for examining the impact of trade on conflict; it is much easier to examine the underlying linear function (in “link” or latent units) than its non-linear logit transform. The advantage of the latter is that we end up with estimates of the effect of a variable on the probability of a dispute, which is easy to interpret. The downside of this is that the effect varies with all the other independent variables. The effect of movements in trade on the underlying latent dispute variable can easily be compared with the effect of a movement from being an undemocratic to a democratic dyad. We know the latter effect is non-trivial, so we can use this knowledge to gauge the effect of trade on conflict.\footnote{All the splines performed significantly better than their linear counterparts, and all the linear counterparts were themselves statistically significant. Since interest here centers on the effect of trade, only passing reference is made to the effects of the other variables, which are shown in Figures 1–4.}

Since the rest of this paper focuses on the trade variables, I should at
Figure 1: Political Relevant Dyads, All Disputes

(a) Distance

(b) Capability

(c) Similarity of Interests

(d) Peace Years
Figure 2: Political Relevant Dyads, Disputes with Casualties

(a) Distance

(b) Capability

(c) Similarity of Interests

(d) Peace Years
Figure 3: All Dyads, All Disputes

(a) Distance

(b) Capability

(c) Similarity of Interests

(d) Peace Years
Figure 4: All Dyads, Disputes with Casualties

(a) Distance

(b) Capability

(c) Similarity of Interests

(d) Peace Years
Table 2: The linear effect of trade and democracy on conflict: 1946–89

<table>
<thead>
<tr>
<th>Var.</th>
<th>PRDs Only</th>
<th></th>
<th></th>
<th>All Dyads</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>MID</td>
<td>MIDD</td>
<td>MIDC</td>
<td>MID</td>
<td>MIDD</td>
<td>MIDC</td>
</tr>
<tr>
<td></td>
<td>(\hat{\beta})</td>
<td>se</td>
<td>(\hat{\beta})</td>
<td>se</td>
<td>(\hat{\beta})</td>
<td>se</td>
</tr>
<tr>
<td>Trade</td>
<td>.027</td>
<td>.010</td>
<td>.006</td>
<td>.02</td>
<td>.061</td>
<td>.015</td>
</tr>
<tr>
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<td>-1.32</td>
<td>.35</td>
<td>-.51</td>
<td>.17</td>
</tr>
</tbody>
</table>

least discuss briefly the effect of the controls.\textsuperscript{25} Not surprisingly, democracy has a strong and highly statistically significant effect pacific effect; this effect is similar in magnitude for both all dyads and PRDs only; it is about 50% greater for MIDD’s than for all MIDs.\textsuperscript{26}

Peace has its usual effect, dramatically lowering the probability of conflict in the first decade, with the effect declining or disappearing after that. Distance also has the expected effect, with the probability of conflict declining monotonically with distance. This indicates that simple contiguity is not enough to capture geographic causes of conflict. Not surprisingly, the effect of distance is much greater when we look at all dyads; again, those dyads at a great distance are probably unlikely to engage in conflict in any event. For PRDs, capability asymmetry appears to monotonically increase the likelihood of conflict. Looking at all dyads, however, we see that moving

\textsuperscript{25}These effects remain reasonably constant as I allowed trade to enter non-linearly, so I do not repeat this discussion for the later specifications.

\textsuperscript{26}Since I do not compute probabilities here, substantive effects can only be judged by comparing coefficients and relevant variable ranges in link units (that is, the units before they are subject to the inverse logit transform). To understand link units, note that the logit model has

\[
P(MID) = \frac{x_i \beta}{1 - P(MID)}
\]

where \(x_i\) is the vector of explanatory variables for observation \(i\). The link units are units of the right hand side of Equation 2. Since Democracy is a dummy variable, a change from being a non-democratic to a democratic dyad decrease conflict by between .5 and 1.5 link units. A decrease in the link function of one unit is equivalent to adding about five years of peace or moving the dyad about 2000 kilometers further apart.
from the highest levels of symmetry towards asymmetry initially lowers the probability of conflict, though after one side has about twice the force of the other increasing asymmetry increases the probability of a conflict. It is likely that many of the symmetric dyads consist of small countries that are simply unlikely to conflict for completely non-strategic reasons. Finally, as interests become more similar, dyads are less likely to engage in conflict; this effect is dramatically larger in the dataset containing all dyads.

3.1 Trade: Linear Analysis

We now turn to the variable of interest, Trade. In the linear analysis trade appears to be conflictual, not pacific. The coefficient on trade is positive in all four analyses, though it is only large in relationship to its standard error for all MIDs. Thus, even with a simpler linear analysis, it appears that the finding that trade is conflictual is a result of trade being associated with more non-serious MIDs, which are often MIDs over trade. Since the claim of this paper is that the linear analysis is misleading, we will not continue with this interpretation, but rather move directly to the non-linear analysis.\footnote{In the previous work, we did find some small pacific effects of trade on undirected dyadic conflict, but the effect was not large. It should be noted that a simple logit analysis using the same independent variables also yields the puzzling positive sign of trade on conflict.}

3.2 Trade: Nonlinear Analysis

Trade was entered into each specification using Loader’s 1999 loqfit smoother. This smoother worked much better with this data set than the more common smoothing splines or loess smoothers. Except as noted, we used the default smoothing parameter (.7). In all cases, the smooth significantly out performed the linear analysis; results are in Table 3.

The plots of the smooths for PRDs (both MIDs and MIDCs) are in Figures 5 and 6; the corresponding smooths for the full data set are in Figures 7 and 8. Figures are shown with and without pointwise 95\% confidence intervals. The estimated standard errors are generally very small (usually under .02 for the vast range of the data). However, there are a few points with larger standard errors. The plots emphasize those few larger standard errors, and so are hard to read. The vast majority of the standard errors indicate that the bends in the impact curves are real, not statistical artifacts. These bends
Table 3: Tests of non-linearity of impact of trade on conflict: 1946–89

<table>
<thead>
<tr>
<th></th>
<th>PRDs Only</th>
<th>All Dyads</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>MID</td>
<td>MIDC</td>
</tr>
<tr>
<td>Nonpar DF</td>
<td>7.7</td>
<td>6.1</td>
</tr>
<tr>
<td>$\chi^2$</td>
<td>20.66</td>
<td>13.36</td>
</tr>
<tr>
<td>Prob</td>
<td>.01</td>
<td>.04</td>
</tr>
<tr>
<td>Deviance</td>
<td>4016.7</td>
<td>1730.6</td>
</tr>
<tr>
<td>DF</td>
<td>10677.4</td>
<td>10165.8</td>
</tr>
</tbody>
</table>

can be better seen in graphs that do not show the confidence intervals. The graphs are also clear when limited to the vast majority of dyads that have small amounts of dyadic trade; readers should carefully observe the scales on both axes. Figure 9 superimposes the four smooths for interpretive purposes.

Note that the effect of trade on conflict is much easier to interpret in the directed dyad case than in our previous undirected dyad analysis; the latter paper used clumsy dyadic measures which introduced multicollinearity and made interpretation difficult. The story here appears more straightforward. The non-monotonicity of the effect plots tells us why we often get mixed findings on the Pacific impact of trade, and why sample issues are so critical.

Looking first at the PRDs, we see that the probability of conflict increases as dyads move from having no trade to a small amount of trade. The size of the effect is about .4 link units for MIDs and .6 link units for MIDCs. This is non-trivial; it is just about half the effect of a dyad becoming democratic. Note that this effect occurs in the range of data actually observed.

But is trade causal here? Unlikely? Which PRDs do not trade? Since they either abut or include major powers, there is little reason for them not to trade unless they are political enemies. Thus, for example, North and South Korea did not trade. Thus it is likely that the PRDs which did not trade were those that were inherently conflictual. Comparing those enemies to dyads with low trade is a comparison of political enemies with dyadic partners that are at least on good enough terms to trade. It seems unlikely that increasing trade from zero to a small level would causally lead to a decrease in conflict.

For all MIDs we then see that as we move from low trade to slightly higher
Figure 5: Effect of Trade on Conflict, PRDs, MIDs
Figure 6: Effect of Trade on Conflict, PRD, MIDCs
Figure 7: Effect of Trade on Conflict, All Dyads, MIDs
Figure 9: Comparison of Effects of Trade on Conflict
trade that conflict actually increases. The effect is not great, but it appears to be more than noise. But when we limit the analysis to MIDCs, the bump is eliminated. Trading partners have militarized disputes, often over trade. These disputes usually are not at all like wars. Eliminating them from our study gives us a much clearer picture of the conflictual impact of trade.

Finally, for both MIDs and MIDCs, a move towards higher trade does appear conflictual. The size of this effect is similar to the effect of making a dyad non-democratic (though the standard errors increase as trade increases, given the extreme skewness of the trade data). Since we see similar increases in the probability of both MIDs and MIDCs in the high trade range, this finding is not due to high trade being associated with more non-serious trade MIDs. At this point we are unclear as to why increased trade, at the higher levels of trade, appears to be conflictual. Perhaps our coding of MIDCs does not eliminate all trade related MIDs.

Turning to all dyads, it appears that a move from no trade to a slight amount of trade is conflictual, with the impact rivaling the impact of a move away from democracy. But again, is this a causal impact. Dyads that do not trade are likely to be dyads that have nothing to contest; such dyads have no motive to fight. Only dyads that have something to contest are likely to fight. So the finding that as trade increases from zero its effect is conflictual is a bit like finding that marriage increases the divorce rate!

We then note that as trade continues to increase its effect is pacific, particularly for the MIDCs. As with the PRDs, trade eventually appears conflictual, but only for the relatively small percentage of dyads with high levels of dyadic trade. As before, we must leave the explanation of this to future papers.

4 Conclusion

Empirical studies of the effect of trade on conflict have provided mixed results. These different findings are at least partly the result of different ways of measuring trade, or, more precisely, different ways of handling missing trade data. This paper has attempted to improve on the treatment of missing data, though our treatment is far from satisfactory.

The Correlates of War data also conflate many types of militarized conflict. Much of our thinking about trade and conflict is really about trade and war, but most of the COW conflicts involve fairly trivial disputes. Worse
yet, many of these disputes are trade disputes. Non-trading partners clearly do not seize each others fishing boats! Again, we have attempted to use a better measure of serious militarized conflict here, though much remains to be done.

Finally, we have moved from undirected to directed dyads. This appears to make for much more sensible modeling. But it is unclear whether the COW aggressor was really the dyadic aggressor. We all know of the famous case of Poland starting WWII by attacking Germany.

We use a non-linear analysis in this paper. It shows that the effect of conflict on trade is not monotonic. Analysis of the various different effects indicates that much of the apparent effect of trade on conflict is really a proxy for other effects, both on the motive and opportunity side. A randomly chosen dyad that does not trade is also unlikely to fight; a dyad that should trade (for geographic reasons) but does not is likely to be a hostile dyad. The analysis in this paper indicates that the relationship between trade is non-monotonic, but that also many of the interesting non-monotonicities have little if anything do with causal impacts of trade on either conflict or peace. At this point, it would be very hard to conclude that trade causes a decrease in conflict. To return to Starr and Most’s terminology, trade can be a shorthand for both motive and opportunity. We should probably being to examine these underlying factors, rather than aggregate trade per se, if we wish to further understand the causal determinants of international conflict.
References


King, Gary and Langche Zeng. 1999. “Logistic Regression in Rare Events Data.” Paper presented at the Annual Meeting of the Society for Political Methodology, College Station, TX, July.


