

On ignoring missing data and the robustness of trade and conflict results: A reply to

Barbieri, Keshk, and Pollins

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Abstract

Barbieri, Keshk, and Pollins (2009) introduce a new Correlates of War trade data set and express various opinions on issues related to analyzing data on trade and how these may impact our inferences on the relationship between trade and conflict. Since there are 33 references to my name in their paper and the authors believe the expanded trade that I have generated to be highly problematic, I respond to some of the issues raised, clarify the specific sources of disagreement, and outline my thoughts on how trade data can be improved and best used in empirical analyses. Barbieri et al. (2009) could be interpreted as suggesting that inferences on the pacifying effect of trade on conflict are fragile and sensitive to decisions on the data. However, their paper in my view provides few new empirical results and does not change my own interpretation of the evidence that dyadic trade reduces the risk of conflict.

Keywords: trade, missing data, networks, conflict

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Introduction

Barbieri, Keshk, and Pollins (2009) introduce a new data set on trade. New data are always a good thing, and the authors deserve credit for making the Correlates of War trade data set available to the scholarly community. The authors also express various opinions about specific issues with regard to examining data on trade and how these may impact our inferences on the relationship between trade and conflict. Since there are 33 references to my name in their paper and the authors believe the expanded trade that I have generated to be highly problematic, I respond to some of the issues raised, clarify the specific sources of disagreement, and outline my thoughts on how trade data can be improved and best used in empirical analyses. Barbieri et al. (2009) primarily comment on issues regarding trade data, but their article could be read as suggesting that inferences on the pacifying effect of trade on conflict are fragile and sensitive to decisions on the data. Although I have not done research in this area myself, their paper in my view provides few new empirical results and does not change my own interpretation of the evidence that dyadic trade reduces the risk of conflict.

1. Should we ignore missing data?

How to best handle missing data is a core question raised in the analysis of trade data, and indeed data analysis more generally. The appropriate answer depends on how one evaluates the relative costs of excluding observations with missing data and the adequacy of alternative approaches. In brief, listwise deletion from the data matrix is likely to be unproblematic if data are missing at random, but increasingly problematic as data are missing in ways associated with our central variables of interest (see King et al. 2001;

Rubin 1976). It is highly implausible to treat trade data as missing at random, as data are much more likely to be missing in instances where there either is no trade or very low trade (as far as I can see Barbieri et al. 2009 do not dispute this). I discussed in greater detail in Gleditsch (2002) how listwise deletion seems problematic for analyses of trade and conflict, as one is likely to exclude a large share of dyads with no or negligible trade, and showed empirically that a large number of disputes onset dyad years indeed have missing trade data.

The next question then is whether one can come up with more plausible estimates or guesses of trade values, and how the potential benefits of avoiding the perils of listwise deletion compare to the possible problems of introducing potentially imperfect and inaccurate imputations. Statistical techniques for imputation may be helpful when we have good indicators to predict the distribution of values for missing observation. This is not something that I have worked on myself, but the results for the two stage model of trade and conflict in Hegre, Oneal, and Russett (2010) suggests that this could be a promising avenue. My approach in the expanded trade and GDP data was to try to improve on listwise deletion through a simple approach to capitalize on plausible guesses on the likely distribution of trade volumes for many of the missing observations, based on the observed data and cases where there is unlikely to be notable trade. I will return to defend some of these choices below, but refer to my 2002 paper for further details on why I believe that my suggested approaches are preferable to listwise deletion.

Barbieri et al. (2009) seem much less concerned with the problems of listwise deletion than I am, and argue that missing data ought to be treated as missing. I disagree, but it is of course legitimate for people to question the adequacy of the alternative

procedures that I suggested, and my data provide full identification of the origin of all observations so that people can choose to exclude the estimates they find more questionable. However, I do not believe that Barbieri et al. substantiate their implied argument that the alternative estimates are likely to be worse than deleting missing data, and their paper does not provide any new results to help us evaluate the argument. On p. 482 they offer some hypothetical reasons why dyads assumed to have no trade could have some trade. However, if trade is very likely to be low in these instances then an imputed value of 0 is likely to be less problematic than dropping the observation altogether. Likewise, not all dyadic trade is balanced, but the real question is whether it is more problematic to assume this as an approximation in the absence of reported data on trade flows than simply deleting the observation. The former can be supported in part based on the strong observed correlation (about 0.91) between reverse flows in the observed trade data. Barbieri et al. (2009) note that many reverse flows have deviations of 10% or more, but a 10% cut-off threshold seems an odd criterion given the cited IMF estimate of a 10% average deviation between imports and exports (p. 474-5). Moreover, studies indicate that no actual observed networks have properties resembling a random network – where the existence/size of links from A to B are independent of the links from B to A (Rapoport 1957) – but instead are *either* highly reciprocal, where links from A to B are similar to links from B to A, or fully directed, as in the case of citation networks, where work in the past is unlikely to cite future work (see Garlaschelli and Loffredo 2004). The analyses presented by Barbieri et al. (2009) on MID frequencies by missing and observed trade data are not very informative, as it is already known and relatively easy to show that a) many dyad years with MIDs have missing data, b) the likelihood of missing trade data

increases with distance between states, and c) the volume of trade declines with distance. In sum, although this is an issue where people legitimately may have different views, I am not convinced that it is better to exclude observations not reported than to seek alternative estimates, although it would certainly be helpful to try to continue to improve on the tentative estimates that I suggested (more on this issue below).

2. Some thoughts on what can be done to improve data on trade?

Although I had not seen the Barbieri et al. (2009) paper prior to it appearing in print, I have had various conversations with Pollins about what can be done to improve trade data and how best to do this (Ann Arbor, October 2003 and Columbus, April 2005). I suggested then that we should try to pool efforts to help achieve progress in this area. That remains my position. I have unfortunately not had as much time as I would have liked to work on updating my own trade data set, but I would welcome greater collaboration among scholars in this area.

First, I believe that it is less useful to exclusively compare the final data sets as Barbieri et al. (2009) do, and more productive to compare the actual input sources and code used to generate the data sets to see where differences originate and where improvements can be made. My own data are generated by a set of Perl scripts that in principle can be replicated on any computer, using exclusively open source software. I make no claim to being a particularly good programmer or that my code is always free of problems or unintended consequences. However, the manner in which my data are created is transparent, and it is possible to inspect the code to see where the final observations come from and to inspect potentially suspect observations and revise the

code accordingly. By contrast, Barbieri et al. (2009) only provide the final data without any of the original input sources or the code used to generate the data.

Second, instead of always accepting observed data as given or missing, it would be helpful to do more work on cross-evaluation and check observed and missing data against alternative sources to improve on existing estimates. For example, both Barbieri et al. (2009) and I rely on alternative data for Taiwan and use a similar proportional estimates approach to derive separate trade figures for Belgium and Luxembourg when these are reported jointly. But one could likewise imagine consulting alternative data sources and area specialists on important dyads for which data are missing or potentially suspect. This would seem the best way to resolve doubts concerning particular dyadic flows that Barbieri et al (2009) repeatedly raise as possibly important for our inferences. It is certainly possible that highly asymmetrical trade relationships may be more prone to conflict (on this point, see Kinne 2009: 178). In this case, using simple reverse flow imputations may underestimate the asymmetry associated with conflict. However, it seems preferable to actually investigate alternative information on the extent to which we may have large asymmetries in such cases rather than simply contend ourselves with excluding these cases as “missing”.

3. Do the new COW trade data change our inferences about the effects of conflict on trade?

Beyond their general criticism of my data, Barbieri et al. (2009) also suggest that we should be sceptical of existing research on the effects of trade on the risk of dyadic interstate conflict, since much of this relies on my dataset. This is not a topic that I have

contributed to myself, but I believe the authors unfairly cast doubt on the contributions of previous research, without substantiating clear grounds for doubting their results.

First, regardless of the merits or my data, or the lack of merits, it seems unfair to criticize existing research for not using their dataset when this has not been publicly available until recently – the precise data is unclear to me (the codebook states September 2008 for version 2.01). Making replication data available is clearly one of the reasons why Oneal and Russett's work on trade and conflict has generated so much attention (see Oneal et al. 1996; Oneal and Russett 1999a; Oneal and Russett 1997; Oneal and Russett 1999b; Oneal and Russett 2001). Indeed, it is only because their data have been made available that others have been able to inspect their data and identify issues they believe are problematic. By contrast, in cases where data used have not been made publicly available, notably Barbieri (1996) in the trade and conflict literature, it is not possible for others to try to replicate the published results and assess how the conclusions and differences from other studies may depend on data considerations or particular influential observations as opposed to for example model specification.

Second, Barbieri et al. (2009) simply assert that we should be sceptical of existing research, but do not actually demonstrate that using their data changes previous inferences with regards to the effects of conflict on trade. If they believe that other researchers are wrong on this issue, why not try to show this using their data instead of simply asserting that we should be sceptical? Why should the onus should lie on researchers that have produced empirical evidence suggesting that trade reduces the conflict rather on than Barbieri, Keshk, and Pollins to provide empirical evidence to the contrary? Since Barbieri et al. (2009) provide no evidence to the contrary, I have yet to be

convinced why I should doubt previous research suggesting capitalism and trade is associated with less conflict between states (see Gartzke 2007; Hegre, Oneal, and Russett 2010; Oneal and Russett 2001).

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