

A Problem with Peace Science:
The Dark Side of COW

When they conduct statistical tests of their hypotheses about the conflict behavior of states students of peace science generally employ the Correlates of War project's data on war and militarized interstate disputes. This essay contends that this practice has a dark side: an opportunity cost in questions not asked. It first distinguishes holistic from particularistic conceptualization, and then contends that the COW project adopts the latter. That scholars adopt particularistic conceptualization is not—of itself—a problem. The problem arises when the vast majority of scholars studying a phenomenon do so.

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This is an essay about conceptualization. It may appear to be a complaint about what we have done, but it is really a complaint about what we have not done; it is a story about opportunity cost. That is, I pick on an influential research project, the Correlates of War (COW), not because it has had a negative impact on what we know about conflict processes (it has not), but because our seemingly uncritical acceptance of its specific conceptualizations of war and force limits the research that we do and the lack of discussion of the issue suggests that we are unaware that this is so. I could make these same points by talking about the dark side of the Uppsala Conflict Data project and the Interstate Crisis Behavior (ICB) project, but since they are used less frequently, fewer people would pay notice. That said, this is a call not for eschewing COW-like conceptualization, but for exploring the theoretical consequences of adopting alternative conceptualizations.

I begin by distinguishing what I will call holistic conceptualization from what I will call particularistic conceptualization. COW-like conceptualization is particularistic conceptualization. It takes a specific phenomenon and conceptualizes it in a manner that separates it from other phenomena that a holistic conceptualization would not ignore. Put another way, a holistic conceptualization will conceive of a dimension and establish typologic categories as distinct points along a continuum. Particularistic conceptualizations focus on specific categories and abstract them from the underlying dimension that a holistic conceptualization would also specify.

The Peace Science community has invested—apparently uncritically, I will

charge—in two distinct types of particularistic conceptualization, each of which is best exemplified by two well known and widely used COW data sets. The first type of particularistic conceptualization tries to conceive of conflict as a state-phase concept. The second focuses attention on a particular type of behavior, leaving all other behavior unconceptualized, and ultimately treated in much empirical work as—at worst—irrelevant and—at best—an undifferentiated residual category. Doing so likely introduces bias into the statistical studies that form the foundation for empirical knowledge of peace science.

Of itself, particularistic conceptualization is unlikely to impose substantial opportunity costs on a scientific community. Indeed, holistic conceptualization also imposes opportunity costs. So this is not a story that advocates holistic conceptualization as superior to particularistic conceptualization. Instead it is a story about the skewed distribution of particularistic conceptualization: when the vast majority of scholars working in a community adopt one type of conceptualization the shadow (i.e., opportunity cost) cast will be quite substantial.

I lay out my argument in four steps. First, I discuss state-phase conceptualization and then the COW project's conceptualization of war. Next I turn my attention to the COW project's conceptualization of force (aka militarized interstate disputes). Having addressed those two examples of particularistic conceptualization I briefly describe and discuss Cliff Morgan's holistic conceptualization of conflict resolution. I then discuss the holistic conceptualization of cooperation/hostility of the events data movement.

State-Phase Conceptualization is Holistic

If you think back to your middle school coursework in physics you will recall learning about states of matter. In particular, you will likely recall learning about three distinct states of matter in which we might encounter H₂O molecules: solid, liquid, and gas. What is interesting about these three different states (or phases) is that they are brought into being at very specific thresholds over temperature (at sea level):

* Solid: below 32 degrees Fahrenheit

* Liquid: above 32 and below 212 degrees Fahrenheit

* Gas: above 212 degrees Fahrenheit

Further, these thresholds produce very manifestly different qualitative outcomes, *yet variance within those thresholds does not have an important impact*. H₂O molecules do not have greater or lesser ‘liquidness’ as one raises the temperature from 33 to 179 degrees, nor do they exhibit more or less ‘solidity’ as one lowers the temperature below 32 degrees.¹

The underlying dimension over which these states phases change is temperature. The state phase switches are interesting because variance within ranges does not have an effect, but there are dramatic effects across the thresholds.

¹ I believe there is another state phase at absolute zero, but I am uncertain about this, and it does not change the point.

If one adopts a holistic conceptualization then one can make connections quite naturally across the different phases and study H₂O molecules at all states of matter. Alternatively, one can adopt a particularistic conceptualization and study ice, water, or vapor in isolation. With both conceptualizations readily available no opportunity costs are paid. However, prior to the conceptualization of H₂O molecules having different states across temperature, the questions one could ask were limited. That limited ability to ask questions and conduct research is an opportunity cost.

The Particularism of COW's State Phase Conceptualization of War

Very well, I trust you enjoyed the brief return voyage to middle school science, but what—you might be asking—does this have to do with the Peace Science community and the COW project? The COW project uses a binary conceptualization of war, but is not a proper state phase conceptualization: it is a particularistic conceptualization that may, at first blush, appear to be based on an underlying dimension that is capable of establishing distinct phases, but it is not. The well known 1,000 battle deaths threshold is not a threshold over which the phenomenon of interest changes states or phases. In fact, battle deaths are not an independent variable that has a 'limited to no impact' on the phenomenon of interest, except at specific thresholds. As a consequence, we are left with the well known issue of selecting arbitrary thresholds: we might choose 25, 100, 500, 1,000 or any of a myriad other values as the number of deaths we need to observe prior to declaring a

state of war to exist.

Does COW's conceptualization of war establish state phases? No. The underlying dimension--battle deaths--does not produce qualitative changes in the phenomenon of interest at specific threshold values. The operational definition of 1,000 battle deaths creates a binary indicator to distinguish 'not war' from 'war,' but it is not the case that as the number of battle deaths varies above and below that threshold that we observe a qualitative shift in an observable phenomenon that we label 'war.'

The fact that a state-phase conceptualization of war is not holistic is not, of itself, a reason to eschew a binary conceptualization that relies on thresholds along a given dimension. Declaring a state of war to exist above a certain number of battle deaths, is a reasonable conceptual move *for some purposes*. A problem with Peace Science—in my opinion—is that our community invests roughly zero person hours specifying the purposes for which such a conceptualization is useful and those purposes for which it is not.

The above discussion lays the foundation for making a case that a state phase conceptualization of war based on the number of human beings whose lives were lost in the process is limiting. Such a definition will be useful for some purposes, but were such a definition to become so prevalent that a community of researchers tended not to notice its prevalence (and I believe that it has), then such a state of would have a dark side: some questions would be well explored, others would be poorly explored.

Let us consider a well known area of inquiry: the democratic peace. Those who use a COW-like conceptualization to define war can profitably investigate questions such as:

* Do democratic dyads participate in events that produce greater than 1,000 battle deaths with less frequency than mixed dyads?

Research has shown that the answer to this question is: yes. That is a useful thing to know.

Unfortunately, particularistic conceptualization casts a shadow of darkness; it makes us less likely to consider research that explores causal relations between and among the other behavior to which a holistic conceptualization would naturally draw our attention. To be sure, this purported reduced probability is not--of itself--likely to have a considerable impact on the research agenda of a community of scholars. However, if that conceptualization becomes the default way to think of a phenomenon of interest within a scholarly community (and fans of Thomas Kuhn's, 1960, argument about change in science might find his work a useful a framework for understanding the role that the COW project plays in the Peace Science community), then the shadow cast would be quite substantial. A data set with a large spatial-temporal domain has a non-trivial prospect of becoming so widely used that people do not think about alternative conceptual and operational definitions, and I submit that the COW project has attained that status in this community, and

that this state of affairs has a dark side: opportunity cost.

To go about this another way, the number of dyadic battle deaths is an integer count of dyadic intensity. We can think of several “intensity dimensions” that might interest us. For example,

- * Personnel Mobilized
- * Personnel Vanquished
- * Duration
- * Expenditures
- * Munitions Expended
- * Countries Involved
- * Size of territory under dispute

The point is that there are many possible dimensions of intensity of war. The particular dimension(s) that will interest us will depend on the questions we wish to address, but if we limit our conceptualization of intensity of war to a state phase (or any other binary) conceptualization we will unintentionally ignore many potentially fruitful avenues of inquiry.

What About MIDs?

One might object to this line of argument on the grounds that it ignores a COW project data set that focuses on different types of behavior, not a state-phase

conceptualization. Indeed, the militarized interstate dispute (MID) project focuses its attention on the threat, display and use of force as choices made explicitly by governments, and distinguishes these from other types of inter-state behavior such as bargaining (Gochman & Maoz 1984). It certainly isn't a flawed state-phase conceptualization based on arbitrary thresholds along an underlying dimension.

The critic is quite correct. Yet the COW project's conceptualization of MIDs is nonetheless particularistic: by failing to conceptualize the range of potential behavior (i.e., conceptualizing MIDs as a value [or range of values] of a variable) the MID conceptualization casts the same dark shadow cast by the state-phase conceptualization of war. In other words, though the MID project does not adopt a state-phase approach to conceptualization, its conceptualization is nonetheless limited in that it lumps all non-force (or non-MID) behavior into a residual, non-differentiated, and unconceptualized category.

To continue with the democratic peace example, the MID project helps us answer the question:

* Do democratic dyads use force with less frequency than mixed dyads?

Again, the answer to this question is: yes (e.g., Maoz & Russett 1993). Yet the three category typological conceptualization of MIDs makes it easier for researchers to investigate other questions about the democratic peace such as:

* Are democracies less likely than autocracies to back down once they have threatened to use force?

Again, we know the answer to this question: yes (e.g., Fearon 1994). And knowing that is interesting, but by limiting the conceptualization to a portion of the behavior that makes up international politics the MID project treats all other behavior as undifferentiated, and therefore reduces the prospect that we examine the impact of different types of non-force behavior on force and on those other types of behavior. Put differently, projects like MID focus on a portion of the behavior that states use to influence other states and as a result implicitly treat all other types of behavior as not relevant to the study of force in international politics.

I hasten to add that the COW project is not guilty of poor conceptualization. As noted above, many interesting questions can be explored using such a conceptualization. However, to the extent that we fail to recognize that such a conceptualization is only one of many possible conceptualizations, and that such a conceptualization makes it less likely that we ask certain types of questions, then we--the Peace Science Community, not the COW project--are needlessly paying opportunity costs.

Bias in Contemporary Studies?

Above I asserted that the use of COW's measures of war and MIDs likely introduces

bias into our empirical inferences. I briefly defend that claim before turning my attention to some available holistic conceptualizations. The MID data have replaced the COW Interstate War data as the standard dependent variable in this community,² so I focus on the MID data, but the point holds equally for any other binary indicator of conflict or violence (and to other binary conceptualizations of conflict behavior).

The case for bias rests on the belief that conflict behavior short of the events coded by the MID project (e.g., hostile foreign policy speeches, the withdrawal of diplomats, etc.) have a non-trivial impact on the probability of observing a MID *and* also co-vary positively with the variables that scholars include on the right-hand side of their equations that use a measure of MIDs as the dependent variable. If this belief is false, then there is no bias in contemporary studies of MIDs. If it is accurate, however, then the relationships reported in the literature are biased.³

If we conceptualize conflict behavior holistically then the use of a MID measure introduces two types of zeros in our dependent variable: zeros that accurately represent the absence of (directed-dyadic or dyadic) conflict behavior and other zeros that represent cases of (directed-dyadic or dyadic) conflict behavior below the MID threshold. This is the familiar problem of selection bias due to truncating the values of the dependent variable one observes (King, Keohane & Verba 1994:129-32).

While one could adopt an econometric approach to try to address this issue

² A search of “militarized interstate disputes” using scholar.google.com produced 587 articles, books, and papers. A search for “interstate war” cow data” produced 222 articles, books, and papers.

(e.g., adapting a model such as the hurdle Poisson or zero inflated Poisson), a likely superior alternative would be to use a measure that captures the range implied by a holistic conceptualization.⁴ As noted, this critique of biased findings rests on potentially erroneous beliefs about the conflict process. For example, if unmeasured lower-level conflict behavior has no impact on the probability of MIDs, or if the right hand side variables that we include in our analyses of MIDs do not have the same-signed impact on lower-level conflict behavior, then contemporary studies are not biased.

Morgan's Holistic Conceptualization of Conflict Resolution

There are, undoubtedly, useful discussions about conceptualizing war. One luxury I have afforded myself in this essay was to not burden myself with reviewing the literature on conceptualizing war and peace. Instead I will reference a single such work and do so not because it is the best such available (it may or may not be), but simply because it is useful and it popped into my head when I gave pause to consider this issue.⁵ T. Clifton Morgan (1990) implores us to adopt a definition of war that embraces Clausewitz's claim that war is one means of engaging in politics. Put another way, war is not a thing apart from 'politics as normal.' Instead it is one mechanism for producing political outcomes. Morgan argues that we should

³ I am grateful to Dave Clark for drawing my attention to this.

⁴ One could also try to include measures of conflict behavior below the level of a MID as right hand side variables, but doing so would fail to capture the impact of the other independent variables on low-level conflict behavior. See King, Keohane & Verba (1994:195-6) for a discussion of bias in the context of endogenous relationships.

understand war as one particular value of a variable—conflict resolution—and proposes a four-category typology:

- * Voting
- * Adjudication
- * Bargaining
- * Force

The remainder of his essay makes the case that war is neither conflict resolution via voting nor adjudication, and explores the advantages and disadvantages of understanding war as a form of bargaining rather than a form of force. He advocates conceptualizing war as conflict resolution via force.

I find Morgan's essay stimulating, but it is, alas, published in a journal that few members of the Peace Science Society read, and has a very limited audience (a search at scholar.google.com produced 4 citations, two due to myself). Since my limited efforts to improve the audience for this article have not borne much fruit, I thought I would take it on myself to wax philosophic on this issue and see whether I could stir some set of us from our seemingly uncritical use of what I am calling particularistic conceptualization.

To do so let us now consider what we might more readily investigate were we to adopt Morgan's conceptualization. Observe that we can reduce the four point typology into a binary one by collapsing three of the four categories of dispute

⁵ Though see Beer (2001) for an interesting exegesis.

resolution into ‘not force,’ contrast it with ‘force’ and ask a similar question:

* Do democratic dyads use force with less frequency than mixed dyads?

However, Morgan’s conceptualization fairly begs us to ask other, richer questions:

* Are democratic dyads more or less likely than mixed dyads to resort to adjudication, bargaining, and force than mixed dyads?

Morgan’s conceptualization expands considerably the democratic peace agenda, seamlessly connecting recent work on:

* Bargaining (e.g., Fearon 1995, 1998; Wagner 2000; Filson & Werner 2002, 2004; and Powell 2006)

* Adjudication (e.g., Dixon 1996; Hensel & Mitchell 2001, Mitchell & Hensel 2005)

* Foreign Policy Substitutability (e.g., Most & Starr 1989; Bennett & Nordstrom 2000; Clark & Reed 2005; Morgan and Palmer 2006).

Rather than leaving these research agendas disconnected or linked in ad hoc kinds of ways (e.g., they are all parts of IR), this conceptualization draws them together in a rather explicit fashion: they are different means of conflict resolution among which states select. State behavior, then, is the focus of inquiry and theory is

focused on choices governments make rather than states or phases that emerge from structural characteristics of the international system and the units themselves.

Let me put a fine point on that: David Singer and his colleagues were motivated by an interest in using the scientific method to evaluate the realist arguments that dominated the field when the COW project was born, but few have observed that by adopting a flawed state-phase conceptualization of the phenomenon of interest research has been unintentionally steered toward structural characteristics of states and the international system and away from the behavior of states (i.e., the choices they make). It is not just that the dominant theories of the day focused on structural characteristics (and with Waltz, 1979, the structural characteristics of the system, then with the democratic peace, the structural characteristics of polities), but that the conceptualization of the most widely used data project in the field also plays a non-trivial role in leading this community to focus on such questions. I submit that the Peace Science community is like a team of horses with blinders about which we are unaware: we are comfortable marching down the path neglecting potentially fruitful linkages across research agendas and other avenues of inquiry. This is the counter-factual comparative static: if we had alternative large-N data bases constructed on behavior (or choice) focused conceptualizations, we would have a richer, more robust research community.

To be sure, a choice focused research agenda has developed and has even

been feted recently by the Nobel Laureate committee, but that agenda has yet to spawn a data collection project that is grounded in an appropriate conceptualization. Indeed, when scholars working in this community want to test hypotheses they turn to the COW project for their data (e.g., Fearon 1994; Schultz 2001; Clark & Reed 2005). I suggest that this is a comfortable practice not only because it is costly to collect data, but also because people working in this community accept the state-phase conceptualization without thinking about it. Doing so is *not* costly in the sense that it leads one to make errors. Rather, uncritically accepting such a conceptualization imposes an opportunity cost that we do not appear to recognize that we are paying.

Events Data: Conceptualizing War/Force as Values along a Continuum

Morgan has offered us one alternative to COW-like conceptualization of conflict and war, but are there others? There is no limit to conceptualization, but let me draw our attention to an alternative that has been available for decades and draws bits of attention from time to time. The events data approach focuses attention on behavior and uses a cooperation/hostility dimension over which to code actions. One weakness of the event data community is that little attention has been given to consideration of the strengths versus opportunity costs of using a cooperation/hostility dimension to conceptualize interstate (and intrastate) behavior. A considerable virtue, on the other hand, of this approach to conceptualizing interstate (intrastate) behavior is that it neither produces state-

phases with arbitrary (and meaningless) thresholds nor does it relegate a set of behavior to an undifferentiated and unconceptualized category.

Does adopting a behavior-focused conceptualization à la the events data approach illuminate questions that the Peace Science community does not ask? I submit that it does, and offer one as an example:

* What impact does diplomacy have on likelihood of war?

Governments spend a non-trivial amount of money on diplomats, embassies, etc. Yet if one were to read the Peace Science literature on war one would not anticipate that this was so. Why not? Diplomats are absent from that literature: their behavior simply does not exist. Alternatively, if one reads press accounts of events that precede wars and non-wars, one will read a great deal about the behavior of diplomats. What can account for this?

One possibility is that Peace Science scholars are all structural-realists: we believe that diplomatic behavior--which is a unit level variable--has no impact on the decision to go to war. If one were to merely examine the regression tables in this literature and ignore the text in which the tables are embedded, one could well find support for such a conjecture: the variables that populate many of our regression models are—by and large—the types of variables structural realists would favor. To be sure, this is less true over the past 15 years than it was over the preceding 15 years, but one will be hard pressed to find many variables that

measure diplomatic behavior. Nevertheless, I suspect that few of the non-realists among us believe that diplomacy is irrelevant.

I submit that a major reason why we do not consider the day-to-day behavior of diplomats is that we uncritically adopt a state phase conceptualization of war that defines it over a battle death threshold. The battle death threshold is not important here--the binary conceptualization is. Two undifferentiated spaces separated by a threshold exist: not war and war. The state-phase conceptualization leads us to focus on explaining what pushes us over that threshold. What goes on below the threshold is uninteresting and shunted aside.

I do not wish to advocate the position that diplomacy influences the probability of war. Perhaps it does, perhaps it does not. That is ultimately a question to be settled by careful construction of theory and evaluation of hypotheses produced by theory.⁶ I am merely asserting that it is an interesting question and yet one that has received little attention within the Peace Science community. I am further claiming that conceptualization as represented by the most widely used data project in our community has played some role in our ignoring the question. By drawing attention to this state of affairs I hope to play a small role in making us more self aware of this opportunity cost and hopefully more self conscious about the conceptualization we do when it comes to peace science.

Conclusion: Conceptualization Matters

At this juncture it is probably reasonably clear that the argument is straining under

its burden: “Surely Professor Moore,” the critic might intone, “you do not expect us to believe that conceptualization in the COW project and its prevalence as a source of data is primarily responsible for the failure of Peace Science scholars to ask questions that you think should have been asked.” Indeed, I confess that I do not. I do, however, submit that our failure to recognize and discuss the conceptualization of war and force has in fact limited the research agendas of the community of scholars who gather at the Peace Science meeting. Given the frequency with which peace science scholars use the COW project’s data it strikes me as eminently reasonable to identify COW as a primary conspirator in producing this state of affairs. My goal here is to draw attention to the issue and hopefully spur us to become self conscious about conceptualizations that we too frequently embrace without explicit consideration, and begin to recognize the opportunity costs of doing so.

I have argued that conceptualization matters; it influences our research agendas; it influences the questions we ask and, ultimately, the policy implications we produce. I have lauded an alternative conceptualization advocated by Cliff Morgan and suggested that adopting it provides exciting possibilities for making explicit connections across what are now treated as distinct domains of inquiry. The payoff of making such connections will arrive in the production of theories of interstate relations that have a larger explanans than existing theories.

⁶ Kurizaki (2005) is one possible foundation.

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