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Theory, Data, and Deterrence: A Response to Kenwick, Vasquez, and Powers

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Kenwick, Vasquez, and Powers question whether empirical evidence supports the claim that defense pacts deter conflict as our prior research has concluded. We review the theoretical argument for why defense pacts should deter conflict and consider the challenges inherent in evaluating deterrence using observational data. We then consider whether the research design choices of Kenwick et al. improve upon our research design. We demonstrate that claims that defense pacts deter conflict are robust to many of these changes in research design, and we argue that the consequential difference, while perhaps appropriate for testing the Steps-to-War argument, is not appropriate for testing the deterrent effect of defense pacts. We conclude by noting that a deterrence effect of defense pacts is not necessarily incompatible with aspects of the Steps-to-War argument, and we suggest profitable new directions for testing the Steps-to-War approach.

In a recent article published in the *Journal of Politics*, Kenwick, Vasquez, and Powers (2015) question whether alliances really deter aggression as our past research has led us to conclude (Johnson and Leeds 2011; Leeds 2003b). As we explain below, we do not believe that their research design is appropriate for testing the theoretical argument we have made in our past work. Kenwick et al. limit their analysis to the first five years in which a state has any defensive alliance. Given that most theories of alliance formation argue that states seek allies when they view the probability of conflict as high, focusing on the immediate aftermath of the decision by a state to seek its first ally biases the analysis against finding any deterrence effect. While we do not believe that Kenwick et al.'s analysis calls into question whether alliances deter, as they suggest it does, we also do not believe that our analysis suggests that the Steps-to-War argument (Senese and Vasquez 2008) is without merit. We conclude by suggesting some directions for future research in analyzing the approach.

WHY DEFENSE PACTS DETER AGGRESSION

To begin to understand how to prevent or avoid war, we first need a theory of what causes war. Our approach draws from the widely (but not universally) accepted theory that war occurs as a result of failed bargaining, generally due to the existence of private information, incentives to misrepresent that information, and commitment problems (Fearon 1995). Most of the time, states are at peace with one another because the states tacitly agree that the distribution of benefits between them is roughly reflective of the distribution of power; neither side believes it can successfully compel a change in policy at acceptable cost through the use of force (Powell 1999). This does not necessarily mean that states have no differences with one another or that they believe that the current distribution of benefits is ideal. Rather it means that both sides view the costs of changing policy to be greater than the expected gains. Occasionally, however, something will change such that one side believes that it is worthwhile to demand a change in policy, with the explicit threat that the

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state will be willing to use military force to obtain the change if the negotiating partner does not concede the demand. This is when we see the initiation of militarized interstate disputes. These disputes escalate to war if the two sides are unable to come to a policy agreement without paying the costs of war, which Fearon (1995) argues should only occur as a result of information or commitment problems.

Essentially, therefore, we argue that peace is often the result of deterrence. What keeps international actors from demanding changes to the status quo is not that they view the status quo as ideal but rather that they view the status quo as acceptable given the costs that they would incur to change it and/or the probability that they would succeed. Deterrence occurs when an actor does not take an action he or she might otherwise take because of the expectation of negative consequences. Thus, states are deterred from making demands backed by the threat of military force when their expected utility from war is lower than their value for the status quo.

Defense pacts are a particular type of military alliance that specifically commit members to assist one another with military force in the event of attack on the sovereignty or territorial integrity of a member. A number of theorists have argued that because of the costs associated with forming, maintaining, fulfilling, and abrogating alliances, defense pacts are often credible signals that other states will assist a member state if attacked (Fearon 1997; Morrow 1994; Smith 1995, 1998). Empirical evidence suggests that most of the time when alliances are invoked by war, they are fulfilled, despite the fact that alliances of doubtful reliability are probably disproportionately targeted (Leeds 2003a; Leeds et al. 2000). To the extent that defense pacts are credible, they should serve to reinforce the status quo by increasing the potential costs of changing the status quo by force and decreasing the probability that a challenger expects to succeed in doing so. This leads us to conclude that, all else equal, states that have allies committed to defend them in the event of attack are less likely to be the targets of demands for change in the status quo backed by the threat or use of military force because potential challengers, aware that they may have to fight a multilateral war, are more likely to view the costs of attempting to compel change to be unacceptable. In other words, potential challengers are deterred.

HOW CAN A THEORY OF DETERRENCE BE TESTED USING OBSERVATIONAL DATA?

Theories of deterrence are among the oldest in international relations, and yet it has long been recognized that evaluating theories of deterrence empirically is especially challenging (e.g., Achen and Snidal 1989). To test if an implemented policy (e.g., an alliance) has a deterrent effect,

we would need to compare an actual outcome (e.g., war/peace) for a country that implements the policy with a counterfactual outcome we would observe for the same country in the same time frame if it did not implement that policy. This is not possible. Experimental research designs get around this problem by comparing average outcomes for two groups: a treatment group that implements the policy and a control group that does not. Randomization in assignment to treatment or control conditions assures that the groups are on average identical in terms of both observable and unobservable determinants of the outcome except for the implementation of the policy being studied; differences in outcomes can thus be attributed to the implementation of a policy. In observational studies, however, subjects are not randomly assigned into two groups; they self-select themselves into either group. Countries that choose to implement a policy are likely to be systematically different from those that do not in terms of both observable and unobservable factors. Defense pacts are no exception. It is very hard to imagine ever being able to hold all else constant while varying only the existence of defense pacts.

Both our prior work and that of Kenwick et al. face this problem. In Johnson and Leeds (2011), we evaluate the deterrence effect of defense pacts using a sample of all directed dyad-years from 1816 to 2000. The dependent variable codes whether the potential challenger initiates a militarized interstate dispute against the target. The key independent variable identifies whether the potential target has a defense pact that is applicable to a dispute with the potential challenger. We also include a set of variables to control for the baseline probability of conflict within a dyad. We do this to hold constant factors that may be related to both the presence of a defense pact and the initiation of a militarized interstate dispute in a dyad when estimating the deterrence effect of defense pacts. Using this research design we find that a potential challenger is less likely to initiate a militarized interstate dispute against a potential target that has a relevant defensive alliance (table A1, fig. A1; tables A1–A7 and figs. A1–A4 are available online). Kenwick et al. make a number of alternative research design choices and do not find support for this conclusion.

We note three potentially important differences between the research designs of Johnson and Leeds (2011) and Kenwick et al. (2015): (i) using directed dyad decades, Kenwick et al. examine only the first five years after a potential target becomes allied through a defense pact; later periods and cases in which the potential target is forming an additional alliance with a new partner are not included in the analysis; (ii) Kenwick et al. employ a pre-processing matching technique; (iii) Kenwick et al. use a different set of variables to identify similar cases than Johnson and

Leeds use as control variables. Below we demonstrate that the Johnson and Leeds' results are robust to the latter two changes. The factor that leads to different conclusions is the sample of alliance-years that are included in the analysis.

Immediate and long-term effects of alliances

The consequential difference between Johnson and Leeds (2011) and Kenwick et al. (2015) is whether the effect of alliances should be observed throughout their existence or only for a short time after gaining a new ally. The samples analyzed in the two studies are quite different. Kenwick et al. analyze directed dyad decades dropping all cases in which a potential target did not form a new alliance in the last five years and also all cases in which the potential target formed an alliance but was already a member of an alliance in that period. The second criterion means that the formation of a new alliance immediately after one expires, the formation of an alliance with a new partner, or the formation of a bilateral alliance among states who are members of a multilateral alliance (or vice versa) are not included in the data. In other words, Kenwick et al. study the effects of alliances only for the first five years that a state has any defensive alliance commitment from any other state. They also include in their analysis a set of directed dyad decades in which the target does not form an alliance; these are drawn from cases in which the target has no defensive alliance throughout the 10-year period. Thus, the sample includes nonallied targets and targets who have formed their first alliance in the last five years but no cases in which a target has had any defensive alliance for more than five years.

Kenwick et al. argue that testing the Steps-to-War argument requires a focus on alliance formation and not on the existence of an alliance. They note that, "The Deterrence Hypothesis, on the other hand, predicts that defensive alliances should always deter, regardless of the time since formation" (Kenwick et al. 2015, 6). We agree with their assessment of the deterrence hypothesis. Where we part company with them is on this statement: "It is also questionable whether forming a defensive alliance can be considered to have deterred, or failed to deter, an action undertaken more than five years after its formation, as the alliance was already in place when the action was to be contemplated in the first place" (Kenwick et al. 2015, 10).

In our study, we were attempting to evaluate the general, not the immediate, deterrent effect of defense pacts. General deterrence occurs when an actor chooses not to make a demand/threat because he/she assesses the consequences to be undesirable. Immediate deterrence occurs when an actor backs down from a prior demand/threat because he/she reevaluates the consequences and determines that they are undesirable. We believe, for example, that the existing mil-

itary of the United States, which has been quite strong for many years, serves as an effective general deterrent to many who might otherwise challenge the United States. While it is reasonable to doubt the immediate deterrence effect of the US military strength, since an actor who makes a demand while well aware of this strength might be less likely to back down from that demand because of it (Fearon 1994), it does not logically follow to argue that the military no longer has a general deterrent effect after it has persisted for five years. Similarly, and more relevant to the existing study, it is hard to believe that the Soviet Union and East Germany no longer factored NATO (North American Treaty Organization) into their decisions about policy toward West Germany after 1959. The median duration of defense pacts in the Alliance Treaty Obligations and Provisions data is approximately 10 years (fig. A2), and there is no reason to assume that defense pacts stop serving their purpose after five years (Leeds et al. 2002).

This may be a case in which the appropriate sample for testing different theories may be different. We do not dispute the Kenwick et al. claim that testing the Steps-to-War theory requires examining only the time immediately after a state obtains its first ally. We disagree, however, that one should question deterrence theory based on this limited sample. The Kenwick et al. sample includes only 6% of the cases that Johnson and Leeds code as having a relevant defense pact as having an alliance treatment. As an example, when the Warsaw Pact is formed in 1955, it results in an alliance treatment for only one directed dyad in the Kenwick et al. data: the instance in which Albania is targeted by Bulgaria. For most potential challengers, Albania experiences an alliance treatment in 1927, Czechoslovakia in 1935, the USSR in 1935 and 1936, and Bulgaria, Hungary, and Romania in 1947.¹ Hungary experiences another treatment in 1997, but none of the other Warsaw Pact members appear again in the data set as potential targets following the five years after 1927, 1935, 1936, and 1947, respectively. Thus, in the Kenwick et al. study, we cannot know if the Warsaw Pact had a deterrence effect; its formation is not coded as an alliance treatment, and with the

1. Romania has a treatment vs. potential challenger Yugoslavia in 1948, and the USSR has a treatment vs. potential challenger Japan in 1937 and Mongolia in 1945. Poland and the German Democratic Republic never appear in the Kenwick et al. data set as potential targets with an alliance treatment; this could be due to missing data for the years prior to alliance formation, since Poland and the German Democratic Republic formed alliances shortly after regaining their independence. It appears that the Kenwick et al. research design may not code alliance treatments for any state that forms its first defense pact less than four years after gaining/regaining independence. These comments and all of our analysis are based on the data used to produce panel *a* in fig. 1 in the Kenwick et al. article.

brief exception of the Bulgaria-Albania dyad, the member states are not included as potential targets in the data set at any time during the alliance's existence. By including only the first alliance, Kenwick et al.'s research design excludes cases in which states gain allies with stronger capabilities. For example, in 1955, Albania goes from having an alliance only with Bulgaria to being part of the Warsaw Pact, including, among others, the USSR. For at least some challengers, an alliance with the Soviet Union and a number of other states might have a stronger deterrence effect than an alliance with Bulgaria (Johnson, Leeds, and Wu 2015).

Limiting the allied sample to the first five years after a state forms its first alliance is particularly consequential since most theories of alliance formation argue that alliances are formed in response to threat. States are motivated to pay the costs of alliances when they most fear conflict. While Kenwick et al. and we both make an attempt to include variables that capture the baseline probability of conflict, we recognize that we are very far from being able to capture that perfectly; it is quite possible that when states form alliances, they view the underlying probability of conflict as high due to factors not captured fully in our included variables. As we have noted in our work, uncovering a deterrence effect under these circumstances is particularly challenging (Johnson et al. 2015, 317–18). Since, theoretically, alliance formation is correlated with confounding factors that increase the probability of conflict, it should be particularly difficult to see a deterrence effect in observational data in the period shortly after alliance formation. Given that the Steps-to-War argument is based on short-term effects of policy change, this may be an appropriate research design for testing the Steps-to-War argument, although others have raised concerns about endogeneity biasing toward finding support for the Steps-to-War argument (Slantchev 2009). We do not, however, believe it is appropriate to draw conclusions about the deterrence hypothesis with a research design that is inappropriate for testing it. We object to using evidence from this limited sample to answer the question posed in the title of the Kenwick et al. article: "Do Alliances *Really* Deter?"

Robustness to model specification

In addition to using a different sample, Kenwick et al. use a different modeling technique. Exact matching, which they employ, is intended to simulate experimental conditions by creating a treatment group and a control group similar in all aspects except the treatment. The challenge is that it is only effective at creating experiment-like conditions if one can both identify and measure all the differences that might affect selection into treatment and not be caused by treatment itself. Kenwick et al. rely on 10 variables. The pre-

processing matching process results in dropping only 1.7% of the observations because they do not match. It is not surprising, therefore, that skipping the matching step and including the covariates as control variables produces nearly identical effects (table A2).

We re-evaluated the Johnson and Leeds data using coarsened exact matching (Iacus, King, and Porro 2012), which is appropriate given that some of the covariates included in the Johnson and Leeds model are continuous (tables A3–A6, fig. A3). All of the pre-processing matching strategies we adopt result in larger estimates of the deterrence effect than the one reported in the original Johnson and Leeds study. Model dependence is not affecting the inferences drawn from the Johnson and Leeds data; the result is robust to pre-processing the data to reduce imbalances between directed dyad-years with defense pacts and without defense pacts.

Another difference between the two studies is the set of variables included to measure the baseline probability of conflict. Both studies share three variables in common, operationalized identically—joint democracy, challenger offense pacts, and challenger neutrality pacts. From there, the choices diverge. Kenwick et al. include target offense pacts, target neutrality pacts, contiguity, past disputes, rivalry, and whether the challenger or target is a major power. Johnson and Leeds include the challenger's likelihood of winning (i.e., the capability ratio), distance between the target and challenger, foreign policy similarity (Signorino and Ritter's (1999) S-score), and the number of years since the last dispute in the dyad as well as the square and cube of this variable. Substituting the Kenwick et al. control variables in the Johnson and Leeds research design produces a negative and statistically significant relationship between target defense pacts and the initiation of disputes, although one that is smaller in magnitude than the result with the original control variables (table A7, fig. A4). Thus, the Johnson and Leeds result is also robust to differences in control variables.

Other points of divergence

Kenwick et al. argue that the nuclear era should be analyzed separately from the nonnuclear era. Their argument is that the existence of nuclear weapons makes the costs of war so high that there is a large status quo bias that accounts for most deterrence. Given concerns about the credibility of a threat to use nuclear weapons in any but extreme circumstances, we are not convinced that nuclear weapons have fundamentally changed the effects of other variables on dispute propensities, and therefore we do not find the argument that the samples should be analyzed separately convincing.

Kenwick et al. also argue that the most relevant dependent variable for a study of the effects of alliances on conflict is war and not the initiation of militarized disputes.

Again, this depends on the theory one is trying to test. The theory we have laid out leads to a hypothesis that we should see fewer demands for change to the status quo backed by threats to achieve those demands by force when potential targets have defense pacts. Whether demands for change to the status quo are resolved peacefully or escalate to war depends on other factors related to bargaining success and failure (e.g., Fearon 1995). Thus, war is not the relevant dependent variable for testing our argument. While the MID variable does not capture our concept perfectly, and it surely is a noisy proxy for the underlying concept, we wish to capture demands backed by threats, not only the subsample of cases that escalate to war.

MOVING FORWARD: THE FUTURE OF THE STEPS-TO-WAR APPROACH

We conclude that the claim that defense pacts deter the initiation of militarized interstate disputes is robust to changes in modeling techniques and model specification. The research design decision that is largely responsible for the differences in the results reported by Kenwick et al. from the results of Johnson and Leeds is the decision to consider only the first five years in which a target state has any relevant defense pact, which comprises only 6% of the observations in which Johnson and Leeds code a target defense pact. Given that both we and Kenwick et al. agree that the deterrence hypothesis does not predict that the effect should be limited to the first five years, it would be unfortunate to make judgments about the theory based on a limited sample when the broader evidence is commensurate with the hypothesis. This is particularly true given that the limited sample chosen is most subject to concerns about our inability to observe and measure aspects of threat.

We recognize that there are many research design challenges that we have not overcome; studying deterrence with observational data is inherently challenging. Yet, given the compelling, internally consistent, and well-developed body of theory that this hypothesis is drawn from, combined with empirical support, we believe it is prudent to conclude that, all else equal, defense pacts have a deterrent effect. We see no reason to update negatively about the bargaining theory of war or the role of defense pacts in it.

We do not believe, however, that this necessarily discredits the Steps-to-War approach. We make no claim to have tested the Steps-to-War approach directly with our analysis. Neither the Senese and Vasquez (2008) nor the Kenwick et al. (2015) discussions of the Steps-to-War suggest that defense pacts lead to war directly. Instead, the Steps-to-War approach suggests that, when a state forms a defense pact, it might increase the sense of threat felt by a potential opponent who will be motivated to form alliances (possibly offensive ones) and

whose citizens might install a more hard line leader. Both Kenwick et al.'s and Johnson and Leeds' analyses control for the existence of offense pacts for the potential challenger, which increase the probability of dispute initiation. If those offense pacts are actually caused by defense pacts, controlling for offense pacts is inappropriate; this is an example of post-treatment bias (King and Zeng 2007). Neither analysis considers the assumption of power of hard-liners. A productive route forward would be to determine whether defense pacts have more indirect effects on war through encouraging the formation of offense pacts and/or changing the domestic political dynamics of potential challenger states. We reject the claim that deterrence arguments and the Steps-to-War argument are in a direct competition with one another in which only one can have merit.

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