
Dyadic Research Designs: Progress or Postmortem?

An INTERNATIONAL STUDIES QUARTERLY ONLINE symposium

Scott Wolford

Allison Carnegie

Tara Slough

Sarah Croco

Kathleen Gallagher Cunningham

Cassy Dorff

Brandon J Kinne

Patrick McDonald

Toby Rider

William Kindred Winecoff



DeRaismes Combes, Managing Editor

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INTRODUCTION

Scott Wolford
University of Texas at Austin

To what extent are international relations scholars constrained in their ability to answer important questions by what has become the workhorse unit of observation for analyzing relational data: the dyad? [Skyler Cranmer and Bruce Desmarais](#) argue that focusing on dyadic data has led scholarship to fail at properly characterizing relationships of interest in international relations data, while [Paul Diehl and Thorin Wright \(2016\)](#) and [Paul Poast \(2016\)](#) offer conditional defenses of using dyadic data. However, all three acknowledge that many of the problems identified in [Cranmer and Desmarais \(2016\)](#) carry with them both inferential and substantive merit. Journal space prevents a fuller exploration of this debate in the pages of *International Studies Quarterly*. Thus, to facilitate this important discussion, we invited eight scholars to weigh in on the question of dyadic data.

Our contributors (listed alphabetically) represent a range of traditions in empirical work—from network analysis to standard dyadic analysis—and in substantive orientation—from international conflict to civil conflict to international political economy. [Allison Carnegie and Tara Slough](#) acknowledge that all models are essentially that—models—and that researchers must be aware of what limitations their empirical models impose when they attempt to measure features of or characterize relationships between variables in their data. [Sarah Croco](#) wonders about the extent to which, by perhaps overfitting dependencies, analysts can trade one kind of bias for another if the use of network analysis is not sufficiently grounded theoretically. [Kathleen Cunningham](#) notes that, while dyads have improved the quantitative study of civil war, the unit faces specific challenges (say, of the endogenous observability of rebel groups, who are defined by their value on a possible outcome variable) for which a move to network analysis might to be a solution. [Cassy Dorff](#) addresses the problem of how to ensure that students are made aware of the costs and benefits of different research designs, how they can develop facility with network approaches and thus be equipped with the tools to choose among different research designs responsibly.

Next, [Brandon Kinne](#) clarifies the role of a particular model, the ERGM, in properly characterizing relationships in one's data, estimating dependencies (while not forcing them), and showing the value of such work in IPE. [Patrick McDonald](#) emphasizes some theoretical reasons to model interdependencies, focusing especially on the problems of pairing dyadic data with systemic theories, which suggest that the statistical relationship known as the democratic peace is an artifact of failing to model hyper-dyadic relationships. [Toby Rider](#) argues that Cranmer and Desmarais seem to conflate theoretical and empirical models of dyads. He cautions against associating theories with any single research design, and notes the inherent problems posed by confronting theoretical models with empirical models in any meaningful “testing” relationship. Finally, [Kindred Winecoff](#) defends Cranmer and Desmarais (2016) against specific challenges from [Diehl and Wright](#), as well as Poast. But he also notes an additional potential problem with dyadic data (also discussed by McDonald) that stems from its inflation of the number of observations in international-relations data.

This amounts to what will likely prove an long-lasting debate for the field. We hope that these collected contributions can contribute further to an important issue in the advancement of the field.

THE PROMISES AND PITFALLS OF DYADIC DATA

Allison Carnegie and Tara Slough
Columbia University

All methods have their limitations. Our inferences—whether design—or model-based—rely on a set of assumptions, only some of which are testable. It follows that we can scrutinize any commonly used method in terms of the plausibility of its assumptions; these assumptions tend to be more heroic under some research designs than others. In our view, the debate over dyadic design in International Relations (IR) matters a great deal. Although we agree with some of the criticisms advanced in this debate, we oppose abandoning the use of dyadic design altogether. Work in the field of international trade shows how the integration of theoretic and methodological advances in IR may provide a productive way forward.

The now-sizable literature on the use and limitations of dyadic data in political science has motivated the adoption of better practices in specifying models that treat the dyad as the unit of analysis. Nevertheless, [Cranmer and Desmarais \(2016\)](#) correctly question the plausibility of the assumption of conditional independence even *despite* recent advancements. We would also hasten to add the important assumption of conditional ignorability of "treatment assignment" (conditional on covariates) to generate unbiased estimates of "treatment effects" in dyadic models. Failure of either assumption to obtain (among others) may bias our estimates.

So where do these issues leave practitioners? Greater awareness of the limitations of those assumptions that undergird dyadic design should drive the adoption of different approaches when these prove feasible. The series of recent developments outlined in the present articles—including ERGM models, k-adic models, and other methods to improve inference with dyadic data—showcase some of the ways forward. Nevertheless, these discussions reveal outstanding issues. For example, the exaggerated Type-I error rate of all of the estimators assessed in the Monte Carlo analyses by Cranmer and Desmarais indicates much room for further development.

However, it should also lead us to better understand the limitations of our inferences and to find ways to research using dyadic designs. The use of (dyadic) gravity models in the trade literature in International Political Economy (IPE) exemplifies this approach. These gravity models have played a central role in IPE for decades. They challenge Cranmer and Desmarais' assertion that the availability of data drives theoretical innovation toward dyadic analysis. Gravity models of trade derive *explicitly* from longstanding theoretical models of trade. That is, they closely map theoretical models of trade to research design (See [Head and Mayer \[2013\]](#) for an overview of this literature). Since existing formal theories of trade tend to feature two players, the possibility of amending models to account for potential "hyperdyadic" influences may present a theoretically fertile approach. These are precisely the influences underlying Cranmer and Desmarais' concerns about the potential violation of the assumption of conditional independence in empirical models. Therefore, our sense is that theory should guide the choice of empirical strategy in this domain—and, in doing so, facilitate the best use of new methods.

In sum, the contributions by [Cranmer and Desmarais \(2016\)](#), [Poast \(2016\)](#), and [Diehl and Wright \(2016\)](#) further an important debate on the use of dyadic data in IR. The thorough discussion of the issues and limitations inherent to dyadic analysis provides a helpful resource to practitioners and methodologists. But the practicable implications for empirical IR scholarship remain less clear. Certainly, practitioners should be aware of limitations of the empirical strategies and models they utilize. At the same time, it is not evident that we have arrived at—or will soon arrive at—a one-size-fits-all solution to the ills of dyadic data. In this respect, work on international trade may provide a useful model, as it closely matches theoretical propositions to research methodologies.

RECONSIDERING DYADS. AGAIN.

Sarah E. Croco
University of Maryland, College Park

It was with great interest that I read the exchange between Skylar Cranmer and Bruce Desmaris (2016), Paul Diehl and Thorin Wright (2016), and Paul Poast (2016) on the use of dyads in international relations research. Having written about dyads (and dyad years) in the past, I was sympathetic to Cranmer's and Desmaris' concerns. The lack of independence between observations, especially in the context of multilateral events, and other "hyperdyadic" influences merits attention, but I am not fully convinced that the call to abandon the dyad in favor of a more networked approach is the best course of action. My reasoning is twofold.

First, as the other pieces in the exchange argue, the bias problem Cranmer and Desmaris raise is likely not as pernicious as the authors make it out to be. The actions of other states outside of the dyad may be highly relevant in some contexts, but matter less so in others. It is telling that one of the primary examples Cranmer and Desmaris cite to illustrate the potential for bias is World War I. I am guilty of doing the same in my earlier work on dyads—it's a juicy case that is nearly impossible to resist when discussing interdependence. I wonder, though, if we are putting too much weight on this case and the other (thankfully few) large, multilateral wars throughout history. We know that states behave strategically, considering the moves of potential third parties, but this doesn't mean the actions (or possible actions) of other states always accelerate the spread of war like they did in June of 1914. As Diehl and Wright point out, this influence can wax and wane. While they were speaking in the context of 3rd parties affecting the behavior of enduring rivals, there is no reason to believe the degree of interdependence between states will remain constant.

Second, I am not confident that the method Cranmer and Desmaris propose to fix the problem—an exponential random graph model (ERGM)—represents a silver bullet. One could argue that any attempt to account for missing factors is a step in the right direction. Yet applying this fix also requires the researcher to make choices about what characteristics best capture the linkages between states. Some of the possibilities Cranmer and Desmaris suggest are intuitive (e.g., being sanctioned by the same state), but others (e.g., UNGA voting similarities) strike me as a catch-all that may be imposing ties across states that don't exist in reality. In this instance, we may be trading one source of bias for another.

In sum, I found myself softening my views on the downsides of dyadic research. I agree with Poast's compromise that we should "use dyads, but proceed with caution". When dealing with phenomena that are obvious multilateral events (e.g., treaties involving more than two states), alternate approaches, such as networks or k-adic designs, may be more appropriate.

DYADS AND CONFLICT – BEYOND INTERSTATE WAR

Kathleen Gallagher Cunningham
University of Maryland, College Park

The latest wave in the quantitative study of civil war began in the IR subfield, but it was not dominated by a dyadic design. Instead, a country-year approach yielded studies that largely explained what type of countries are prone to civil war, and it was only in a 2009 [special issue](#) of the Journal of Conflict Resolution that the use of dyadic data became the norm (Cederman and Gleditsch 2009).

The new “dyadic” normal can be challenged on a few fronts, some of which mirror [Cranmer and Desmarais’s critiques](#). First, some wars are [multi-party](#) (Cunningham 2011); some have more rebels (or external actors) than others. While qualitative studies have delved more into the [complex dynamics between rebel groups](#) (Christia 2012), the quantitative literature has yet to do so to the same degree (though some recent work on [side-switching of militias](#) (Otto 2017) is moving in this direction).

A second challenge for the dyadic design centers on whom is in the dyad. Actors in civil conflicts are not necessarily unitary, and the [complexity of the opposition](#) (Cunningham 2014) is now being explored on a number of fronts, including its nature (with respect to [dimensions](#) (Bakke et al. 2012) and [power](#) (Krause 2014)), [causes](#) (Seymour et al. 2015) and [consequences](#) (JCR 2012). Moreover, there is typically a clear distinction between the “action” and the “actor” in classic studies of international politics that center on things like war, treaties, or trade agreements. Yet, when we look beyond states, actors are often identified by their action (i.e. rebels rebel, opposition movements protest, non-governmental organizations lobby for specific policy changes). This differs to some degree from Cranmer and Desmarais’s critique because this actor-based challenge is a call to consider the composition and constitution of actors in ways that states are typically not addressed in quantitative conflict studies. Similar issues might arise with study of non-state actors in international relations.

The interdependence critique could also be leveled at dyadic studies of civil war, though perhaps differently than for state-to-state theories of conflict. In a number of instances, states face multiple challengers in what are essentially different civil wars (for example, see India’s multiple low-level wars). Dyads in conflict with the same state are clearly not totally independent. Even across borders, however, there can be [links between actors](#) (Gleditsch 2007) that challenge the state.

Though there is some move toward network analysis in studies of civil conflict, this has generally been in the area of conflict patterns amongst non-state actors. The usual method for addressing such issues continues to be clustering standard errors, or more rarely using [multilevel modeling](#) (Bakke et al. 2008). A key challenge for employing more nuanced network analysis is the lack of information about the relationships between actors, or even the identification of a stable set of actors.

MAKE IT SO: INTERDEPENDENCE AND THE NEXT GENERATION

Cassy Dorff
University of New Mexico

I commend all of the authors in this collection ([Cranmer & Desmarais 2016](#), [Diehl & Wright 2016](#), and [Poast 2016](#)) on a job well done: each highlights important dimensions of the debate for, or against, critiquing dyadic designs. All of the articles meaningfully contribute to the growing need for scholars in our field to better pair theoretical assumptions with appropriate statistical analyses.

My first reaction centers around conceptualizing the outcome of interest—be that war, trade, rivalries or nearly any other international relations favorite. [Diehl and Wright's \(2016\)](#) claim that connections outside of a dyadic relationship might not influence the dyadic relationship itself, such that the effect of state A's external rivalries on State A's rivalry with State B might not drive the relationship between A and B—seems to slightly miss the main argument of Cranmer and Desmarais, as well as other work done by myself and scholars of latent networks. That is, those of us who often conceptualize the dependent variable in matrix (network) form.

I accept that, in many cases, the role of multiple relationships might not be the *main* driver of an outcome of interest. But to argue that this then diminishes the utility of the network approach sorely misses the point, particularly when we imagine how to construct a dependent variable as a network. In these cases (among others), we need to consider A and B's dyadic relationship in context of *all other* relationships (or dyads) that actor A or B are involved. Likewise, while some MIDs might only occur between a unique dyad pair, it does not follow that each actor in this unique pair is absent from all other pairs. At least one of the actors likely appears in multiple cases (imagine rows) of the data, violating the critical assumption of independence. Thus, while I agree with Diehl and Wright that we often err in assuming independence, I have a hard time understanding how the assumption of "complete interdependence" is similarly misguided.

My second reaction centers on a broader implication of this debate: how can we learn new research designs? As Diehl and Wright correctly point out, the dyadic research design has a long history in International Relations (IR). But as others note, this dominant framework is merely the the simplest way to analyze a dyad. Other fields—computer science, sociology, social psychology, biostatistics—are far ahead of us when it comes to advancing different ways to assess relational data. Since there is often limited room for pedagogical questioning within research articles, I'll raise the point here: how can we better lessen the "start up" costs of learning network analysis? We owe it to our students to answer this question. We can begin by granting network-oriented IR literature a commonplace role in our IR syllabi (or any conflict-oriented course, as these debates *certainly* apply to intrastate conflict). Along with this, we can, at the very minimum, teach students the fundamental differences between the accompanying data designs at the structural level. In an ideal world, a network analysis

course would be as common as Maximum Likelihood or game theory and would include texts like [Jennifer Victor](#)'s *Handbook of Political Networks* but with a stronger pedagogical orientation. If political scientists fail to seriously embrace the task of learning and utilizing network analysis, they will not only forfeit this opportunity to the many government and commercial professionals already adopting these tools, but they will also systematically limit the field's ability to engage in this complex, intertwined world of politics.

STRAW MEN AND RED HERRINGS IN THE DYADS DEBATE

Brandon J. Kinne
University of California, Davis

Dyads are resilient. Despite numerous challenges to their reign, dyadic models continue to dominate empirical international relations (IR) scholarship. [Cranmer and Desmarais \(2016\)](#) take aim at dyads yet again, carefully summarizing the key reasons why dyadic models are inadequate for empirical IR scholarship while also offering network-analytic methodologies as a viable alternative. In response, both [Diehl and Wright \(2016\)](#) and [Poast \(2016\)](#) provide thoughtful, guarded defenses of the dyadic approach. Both sets of defenders articulate concerns that should not be taken lightly; and, given the ubiquity of dyadic models, I suspect that such concerns are held by a large segment of the discipline. Constructing and estimating an inferential network model, such as an exponential random graph model (ERGM) or one of its many derivations, is not a trivial task. Is it worth it? Are we really better off abandoning dyads in favor of more complex models? Yes, we are. I shall focus my comments on three issues: (1) model selection, (2) the substantive importance of interdependence, and (3) multilateralism versus bilateralism.

(1) Cranmer and Desmarais rightly point out that IR data systematically violate the i.i.d. assumption that is foundational to regression models. [Diehl and Wright \(2016\)](#) counter that “the dyadic approach is appropriate if one is concerned primarily with an outcome based on two actors,” offering such examples as interstate conflict, rebel-government violence in civil wars, and enduring rivalries. With regard to rivalries, [Diehl and Wright \(2016\)](#) state that “assuming that linked rivalries are always connected is almost as misleading as starting with the expectation that they are fully independent.” Referencing Arab-Israeli rivalries in particular, the authors conclude that “[a]ssuming full case independence seems misguided, but then again so too does assuming complete interdependence.” These observations reflect important questions about methodological assumptions. Isn’t the assumption that everything is connected just as faulty as the assumption that everything is independent? Yet, this framing of the issue is a false dichotomy, and it risks propping up a straw man caricature of the network approach.

First, we must clarify that network analysis is not divorced from dyads. Rather, dyads are the building blocks of networks. Substantive bilateral relationships—the probability of a conflict, an alliance, a trade agreement, an investment treaty, or whatever—can still be the primary focus. We can still estimate the probability that a particular bilateral tie will emerge. Network models, in part, allow us to make such probabilistic estimates more accurately, by accounting for the overall structure of the network rather than limiting the analysis strictly to attributes of and relations between some ij pair of states.

Second, and most importantly, network models do not assume complete *interdependence*. Rather, they simply make no assumptions about *independence*. An inferential network model thus allows the analyst to explicitly model the various interdependencies that might exist within a network. Consider the example of a preferential trade agreement (PTA) network (e.g., [Manger, Pickup, and Snijders 2012](#)). Perhaps, when forming new PTAs, a

given i prefers j partners that already have many PTAs in place (a so-called “preferential attachment” or “rich get richer” effect). Or, perhaps i prefers j partners that have already signed PTAs with i 's own PTA partners (a “transitivity” or “friend of a friend” process). Such effects, despite their endogeneity, are easily incorporated into an ERGM or similar network model, which then allows us to *explicitly test whether those interdependencies matter or not*.

Assuming the model is otherwise well specified, if ties in the PTA network do not exhibit transitivity, the corresponding parameter estimates for transitivity effects will be indistinguishable from zero. Similarly, if there is no preferential attachment process in the PTA network, this fact will be reflected in the estimates and standard errors. In fact, if the network effects are null, then the ERGM estimates of specified exogenous covariates (e.g., distance, GDP, democracy, and all the other things that might matter for PTAs) will be identical to those in a standard logit regression model (Wasserman and Pattison 1996). Put differently, inferential network models allow us to model the various exogenous influences that IR scholars typically care about, while also allowing us to determine whether interdependencies—i.e., network effects—are present. If network effects matter, we can estimate their magnitude while also reducing bias in estimates of relevant covariates. If network effects don't matter, we are left with parameter estimates comparable to those produced by regression. In contrast, pooled dyadic regression requires us to assume *ex ante* that no dependencies exist in the data, while providing zero information about whether that assumption is true. Which approach seems more sensible?

(2) There is an unfortunate tendency, even among proponents of network analysis in IR, to view endogenous network influences as statistical nuisances that must be “controlled.” Certainly, as Cranmer and Desmarais (2016) make clear, if a given ij dyadic tie is not independent of, say, an ik tie (or a ji tie or a jk tie or any other tie), then we have an estimation problem. Yet, we must keep in mind that interdependencies in IR data are *substantively interesting phenomena themselves*. Why does the ik tie influence the ij tie? Such endogenous effects do not simply appear *sui generis*. Indeed, there is a long, rich theoretical tradition in IR of exploring the various ways in which states strategically respond to one another's actions (e.g., Deutsch et al. 1957; Keohane 1986; Schelling 1960). Interdependence is the natural state of affairs; it is not a statistical nuisance, but something that happens for palpable political, economic, and social reasons. Cranmer and Desmarais (2016) suggest this possibility in their discussion of economic sanctions. Numerous other network examples abound. Warren (2010) finds strong evidence that states use knowledge of their antagonists' alliances to inform their own alliance making. Manger, Pickup, and Snijders (2012) find that states pursue transitivity in their PTA ties in order to avoid the negative effects of trade diversion. In work on diplomacy, I have shown that states prefer to establish embassies in countries that already host large numbers of embassies (i.e., a preferential attachment effect), as this strategy allows a resource-constrained diplomatic corps to establish indirect contacts with diplomats from across the globe (Kinne 2014).

In a separate project, Jonas Bünte and I explore network effects in bilateral government-to-government lending—i.e., loans made by one sovereign government to another (Bünte and Kinne 2015). Employing temporal ERGMs (Desmarais and Cranmer 2012), we find that, as part of an intense competition to exercise influence in valuable markets, states condition their lending on the lending patterns of their competitors. Lenders use the existing ties within the network to determine which borrowers offer the greatest potential returns (e.g., political influence) and the lowest potential losses (e.g., credit defaults). In short, the network provides lender governments with strategically valuable—and otherwise unavailable—information.

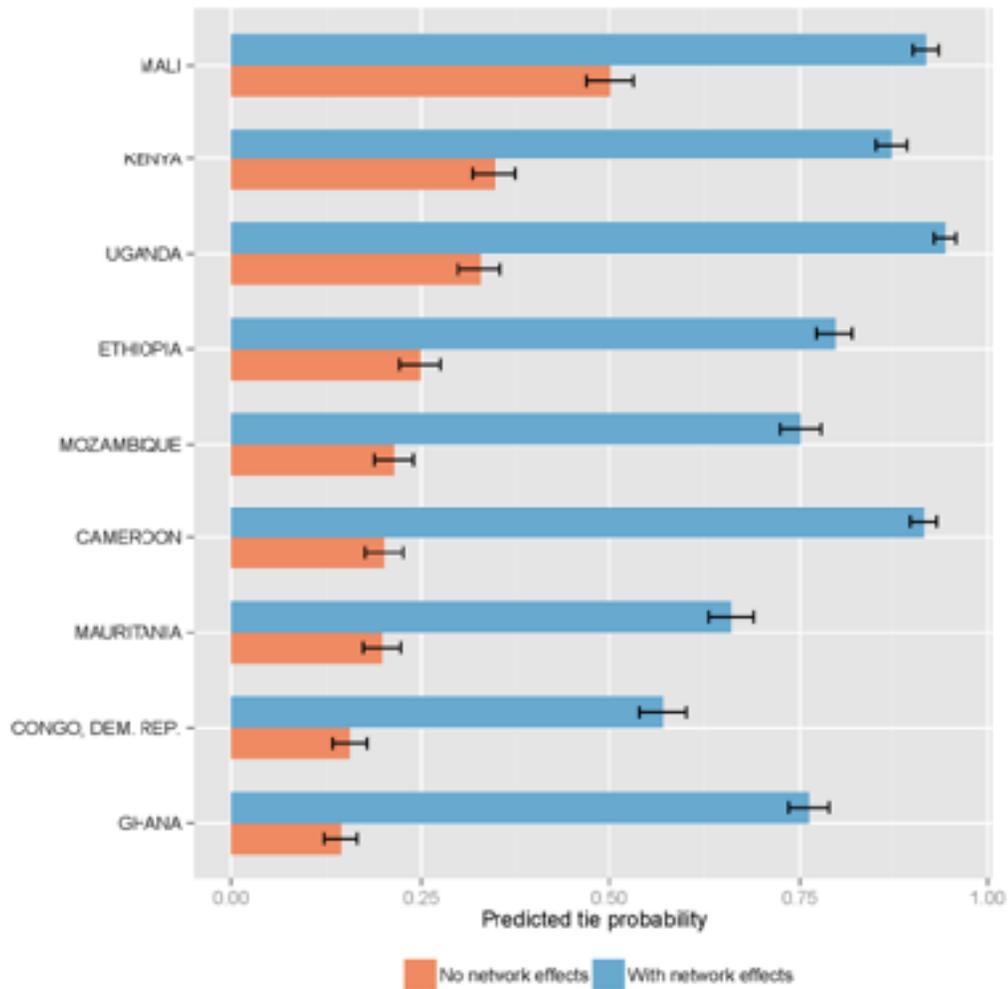


Figure 1: Predicting Chinese Loans, 2010

To make substantive sense of these influences, we examine China’s controversial and highly publicized loans to African governments. Figure 1 shows “true positives,” i.e., African countries to whom China extended bilateral loans in 2010. We employ out-of-sample prediction—using data up to but not including the year 2010 as a training set—to compare a network model to a standard regression model. The goal is to successfully predict the newly created 2010 China-Africa loans. The regression model fares poorly, correctly predicting a loan in only one case (Mali). The network model, on the other hand, accurately predicts all nine of these new loans. In other words, if we wish to understand why, when, and where China makes bilateral loans, traditional monadic and dyadic covariates tell us very little. We must look to the network to understand China’s lending practices. More generally, when we ignore network influences, we do not merely risk biased parameter estimates. We risk ignoring the most substantively interesting and fundamental aspects of international relations.

(3) As both [Poast \(2016\)](#) and [Cranmer and Desmarais \(2016\)](#) note, multilateral ties pose a particularly difficult problem for dyadic models. [Cranmer and Desmarais \(2016\)](#) favor

network models for such data, while [Poast \(2016\)](#) discusses the relative merits of both network models and k -adic approaches ([Poast 2010](#)). While I strongly agree that multilateral phenomena clearly do not fit within a dyadic framework, I worry that over-reliance on this particular limitation may constitute a red herring, distracting our attention from the difficulties in modeling multilateral data while also minimizing the importance of network methodologies for bilateral data. Network models of multilateral data are not easy to estimate. A standard ERGM assumes that network ties, though they may be interdependent, are separable. An ij tie may influence, say, an ik tie, but we nonetheless assume that these are distinct ties, where, in principle, one can exist without the other. With multilateral data, however, ties are often created as a package, via summitry or multilateral bargaining processes, and no one tie can exist in isolation. The North Atlantic Treaty, for example, cannot plausibly be interpreted as simply an aggregation of separate bilateral ties. Estimating an inferential network model on such data may yield misleading results. For example, multilateral ties are likely to yield networks with high levels of transitivity, but it's not clear that a traditional transitive closure process is actually responsible for these levels of transitivity.

Just as importantly, multilateral relations—the bulk of which involve formal treaties, agreements, organizations, or other international legal phenomena—are vastly outnumbered by bilateral relations. Indeed, the number of bilateral instruments annually deposited with the UN Treaty Series is orders of magnitude larger than the number of multilateral instruments, despite the fact that, as a rule, bilateral treaties are far more underreported than their multilateral counterparts. And trends toward bilateralism have only increased in recent years. These bilateral ties hold special relevance for network analysis, as they are often strategically formed in response to—or in anticipation of—the ties of others. And while [Diehl and Wright \(2016\)](#) are certainly correct that, in principle, good theory should have some sense of when dyads are independent and when they are not, this expectation, in practice, places a heavy, almost omniscient burden on theorizing. Ultimately, despite good theory, the question of whether dyadic relations are independent is empirical. If interdependencies exist, then our models must account for them, even if our theory cannot explain them. Network-analytical models are appearing with greater frequency in the discipline's journals, and I have yet to see a network model of IR data, in any issue area, that shows the assumption of independent dyadic observations to be empirically correct. Network influences are the norm, not the exception.

SYSTEMIC EFFECTS, STATEHOOD, AND DYADIC RESEARCH DESIGNS

Patrick J. McDonald
University of Texas at Austin

These three articles ([Cranmer & Desmarais 2016](#), [Diehl & Wright 2016](#), and [Poast 2016](#)) productively extend an emerging debate about the theoretical and empirical utility of dyadic research designs in International Relations (IR) scholarship. While they obviously reach different conclusions, together they reflect a deeper reorientation in the field. I support that reorientation, which manifests as varying levels of discomfort with the relative inattention to systemic factors in IR research after the Cold War. This metatheoretical orientation toward domestic and dyadic levels of analysis potentially interacts in pernicious ways with empirical research using dyads. In this brief post, I underscore these risks and highlight another that is not specific to the dyadic approach but further complicates empirical conclusions reached through it.

I begin at the same place as [Diehl and Wright \(2016\)](#) do—with the inherent path dependence in research processes. The widespread adoption of a dyadic analysis coincided with the end of the Cold War and a related frustration with the inability of prominent systemic theories to anticipate or explain this monumental change in world politics. In response, the field embraced domestic explanations of international outcomes ([Oatley 2011](#)), perhaps most prominently in democratic-peace research. The adoption of an empirical strategy capable of examining how a comparison of the domestic attributes of two states could shape interactions between them facilitated this broader theoretical orientation. As part of this mutually reinforcing process, the modeling assumptions associated with a dyadic design reached the status of standard operating procedures. They shaped the presentation and interpretation of potential new contributions to this debate. I have seen this as both an author and a reviewer. For example, manuscripts that try to evaluate some claim related to the democratic peace within a monadic or state-year research design are often asked to confirm those findings with dyads for the sake of comparability.

This path dependence in research practices and the underlying assumptions associated with dyadic research identified in [Cranmer and Desmarais \(2016\)](#) and [Poast \(2016\)](#) increases the risk that these reinforcing theoretical and empirical orientations foreclose the search for alternative explanations at the systemic level for important outcomes like military conflict. My own research on the democratic peace ([McDonald 2015](#)) illustrates how these theoretical and empirical orientations helped mistakenly reaffirm the robustness of the democratic peace. It rests significantly on the premise that the regime type for any single state in the international system (and thus for dyads as well) reflects an equilibrium that is jointly set by internal coalitional attributes and a larger set of systemic factors. Consequently, shocks associated with great power politics influenced these internal regime outcomes, induced waves of democratization ([Gunitsky 2014](#)) more broadly in the system, and also shaped conflict patterns among these democratic states.

This research (in process) has uncovered a deeper set of theoretical and empirical modeling challenges that stem from the near universal adoption of states as the principle organizational form in international politics. These challenges are often magnified when using dyads, because they match states with other states to create the sample.

For example, the existence of hierarchical relationships (e.g. [Lake 2009](#)) challenges a reliance on legal recognitions of sovereignty to identify both states and dyads. Reflective of the hyperdyadic dependence discussed in [Cranmer and Desmarais \(2016\)](#), membership in a hierarchical order generally means that a state does not have the foreign policy independence that is implied by standard monadic and dyadic research designs.

More broadly, systemic waves of statehood in the twentieth century ([Reus-Smit 2013](#)) have shocked the population of states upward, influencing empirical conclusions reached through dyadic analysis in multiple ways. The growth of states alters the relative balance of observations from any single year in a standard time-series cross sectional data matrix. Years with more states will comprise a larger proportion of the sample. Given state growth in the twentieth century, the temporal aggregation of observations into a single sample creates recency effects in which coefficient estimates rest more heavily on the correlations among variables in later years. This can obscure changing relationships over time. The explosion of sample size generated by moving from a monadic to a dyadic design compounds these effects. This tendency has helped to obscure important changes in the relationship between regime type and peace over time.

The decision to rest units of analysis on statehood creates a second problem that opens larger questions about endogeneity in the study of conflict. Statehood often emerges as part of a peace settlement among multiple states. In the most prominent waves of statehood and democratization that followed the ends of World War I and the Cold War, statehood, democracy, and peace for many political units were all effectively mutually constituted. Consequently, the absence of conflict for many of these newly independent states—and their resulting dyads in the ensuing period—may simply reflect the terms of the larger international settlement that recognized them. Apart from altering the temporal composition of the sample, these systemic shocks point to a series of omitted variables that could challenge existing conclusions.

Finally, the values of prominent control variables in dyadic studies of conflict are influenced by the number of states that exist in the international system ([McCormack and McDonald in process](#)). Two states are less likely to be contiguous when there are more states in the system. A dyad is less likely to contain a great power when there are more states in the system. The time counter measuring the duration since a conflict was observed in a cross-sectional unit is also reset when a state gains entry to the international system. Moreover, these statehood-induced changes in the levels of these control variables are all magnified when moving from a monadic to a dyadic research design.

Thus, I read this debate over the utility of dyadic research designs as part of a larger call to think more broadly about how systemic factors shape prominent questions in the study of international relations. Despite substantial progress, prevailing research procedures centered on the use of dyads have subtly pushed the field away from such perspectives. This conversation helps reintegrate them in a productive way.

EMBRACING THE LIMITATIONS OF MODELING CHOICES: DYADIC DESIGN, THEORY, AND SIMPLIFYING ASSUMPTIONS

Toby J. Rider
Texas Tech University

Cranmer and Desmarais (2016) highlight a number of critiques they deem sufficient to abandon the dyad as a unit of analysis. Not wishing to cover the same ground as Poast (2016), as well as Diehl and Wright (2016), I focus on two other issues: i) Cranmer and Desmarais's critique of dyadic theory, and ii) their implicit equation of assumptions with assertions.

First, Cranmer and Desmarais focus primarily on empirical issues, but they also posit that “thinking dyadically” poses a “serious challenge to the development of sound, consistent, and complete theoretical explanations...” I take their point to be that dyadic theorizing produces narrow and incomplete theories. But a theoretical model is nothing more than a tool used to simplify the world and isolate relationships of interest, of “limited accuracy, [only] partially represent reality, and are purpose build [by the user]” (Clarke and Primo 2012, 53).

Researchers structure theories around the questions they wish to ask, and we should judge them as such, not by our own standards of what type of theory we believe the research should have pursued. Cranmer and Desmarais appear to advocate the latter. But scholars *should* be able to theorize about dyadic relations, with a focus on a narrow set of conceptual relationships, and assume away, for analytical simplicity, the influence of other factors. This is not to imply that extra-dyadic factors don't matter. It's a use of theory as theory was intended—to abstract away from the real world in ways that are useful if not completely accurate. We eventually may develop organizational models encompassing national, dyadic and system level factors to explain the same phenomenon. But that is not a standard by which to judge any individual theoretical model.

When scholars translate their relational or dyadic theories into empirical models we rightly see that reflected in their choice of design—the dyad. Assuming away other dyadic or systemic influences is easier in a theoretical model, and key assumptions underlying our statistical tools must be taken seriously, but it is reasonable for scholars interested in dyadic outcomes to focus on dyadic analytic models.

Second, Cranmer and Desmarais argue that a “focus on the independent dyad neglects the fact that what happens at other levels of aggregation of the day has implications for which the higher-level outcomes are comprised.” They appear to be treating what are clearly reasonable assumptions in our models as *assertions*. No reasonable scholar asserts that these other factors do not matter at all. They develop theories at a level of analysis they think most appropriate for engaging that question, then choose an empirical strategy that fits the concepts and levels of analysis dictated by the theory. And that very well may be the dyadic approach.

Take Ethiopia and Somalia rivalry, two countries engaged in an enduring rivalry over control of the Ogaden region. Scholars might quite naturally wish to theorize about bilateral relations, decisions to seek outside alliances/support, emergent rivalry, arms competition and war. The rivalry was undeniably influenced by extra-dyadic factors. Somalia's 1969 coup potentially contributed to a renewed pursuit of the Ogaden region, and Ethiopia's engagement in Eritrea presented Somalia an opportunity to launch a war in 1977. Likewise, Cold War superpower competition provided opportunities for each to acquire arms, advisors, and allies. Scholars can rightly choose to focus theoretically and empirically on any or all of those relationships and levels of analysis. But a theoretical decision to focus on the bilateral relationship does *not* represent an assertion that a coup or the Cold War did not "matter" empirically.

This is not to trivialize the concerns highlighted by Cranmer and Desmarais, but merely to note that scholars have good reason to pursue dyadic theory and, under some proper conditions, dyadic empirical analysis. We can hope that their critique pushes us to think more carefully about the other factors we may need to account for when specifying our dyadic analyses. And if there is knowledge to be gained about relationships from network analysis, then by all means we should pursue them. But we should pursue them not instead of but in addition to the dyadic approach. Different theoretical and methodological approaches need not be in competition if they can each contribute to our collective body of knowledge.

AGAINST DYADIC DESIGN

William Kindred Winecoff
Indiana University, Bloomington

As I begin I should note that I'm on Team Cranmer and Desmarais (2016) all the way. I was at UNC at the same time as they were, and Skyler was on my dissertation committee. That gave me the opportunity to argue about these and other issues over the course of several years, which I did. (Sorry for being difficult, Skyler; it's how I learn.) In fact, I have made many of the same arguments that Paul Poast (2016) and Paul Diehl and Thorin Wright (2016) make in their contributions to this excellent symposium, in person. And it is because I have already lost those arguments that I can say, with confidence and humility, that I am on Team Cranmer and Desmarais.

At the same time I am surprised by the extent to which Poast (2016) and Diehl and Wright (2016) are *also* on Team Cranmer and Desmarais. Their responses to the Cranmer and Desmarais critique are tepid. They are not tepid enough, I argue below, but they both accept all of Cranmer and Desmarais's main points at least partly (and many of them in full). I am surprised because there has been a parallel debate in international political economy (IPE) circles regarding many of the same issues, and in IPE the issue appears to be far more contentious. While I was very disappointed to see zero references in this symposium to the IPE discussion – which hasn't exactly been hidden: it has taken place since 2011 in journals such as *International Organization* and *Perspectives on Politics*, at conferences like the annual meetings of APSA and ISA, and on prominent weblogs and social media – I was encouraged that all of the participants are willing to accept the need for reflection and critique.

The first step, as they say, is to admit you have a problem. The problem that Cranmer and Desmarais articulate is that dyadic research designs conducted on relational systems (as are all international political and economic systems) must make two strong assumptions about hyperdyadic dependence, i.e. interdependence beyond the “level” of the dyad: first, that it does not exist; second, that if it exists it is not important or interesting. They rightly claim that hyperdyadic dependence will usually exist, and I would go further to claim that there are very few theories (as opposed to detached hypotheses) in which hyperdyadic dependence does not play an important role. In fact, I struggle to think of a single theory in international relations in which that is the case.

Cranmer and Desmarais also claim that hyperdyadic dependence is often important and is usually interesting. To drive this point home they distinguish between hypothetical expectations about covariance, which are often modest and narrow, and models of the entire data-generating process. These are by necessity less modest, less narrow, and require *systemic thinking*. When I say “systemic” I do not intend to bring to mind functionally undifferentiated billiard balls colliding on a bed of pure anarchy. I mean what Herbert Simon described in 1962 as a collection “of a large number of parts that interact in a nonsimple way” (p. 468). A dyadic interaction contains the smallest number of parts that can interact. And, in a regression framework, it is difficult to specify a model containing nonsimple effects.

So in a sense what Cranmer and Desmarais are asking is this: “Do you think the world is simple, or complex?” They believe it is complex, and I believe that most other IR scholars

do too. But our commonly-used models are too simple to capture this complexity, so we too often make less-interesting theoretical propositions than we otherwise might. Cranmer and Desmarais characterize this state of affairs as “theoretical myopia driven by the availability of data” rather than any principled theoretical choice. But the problems do not end with theory. Indeed, dyadic designs can also lead to poor statistical inferences when hyperdyadic dependence exists.

Thus, Cranmer and Desmarais’s charge is that IR has long committed sins of omission, and that we remain in sin. They call us to repent, with haste, and change our ways. Poast (2016) and Diehl & Wright (2016) do not share Cranmer and Desmarais’s sense of urgency despite accepting nearly the whole of their critique. Their responses sometimes recall Augustine of Hippo: “Grant me chastity and continence, but not yet!” I believe this is a mistake for reasons I’ll describe below, but first let me try to characterize the two types of objections that PP and DW primarily make: first, that in practice the problems Cranmer and Desmarais identify are not (always) that big of a deal; second, that even in the face of potentially-severe problems, a dyadic design can make sense if it is chosen for reasons of utility or theoretical parsimony.

For example, Diehl & Wright (2016) argue that dyadic designs have generated many knowledge gains in the past. Of course they have, and Cranmer and Desmarais say the same thing in their initial critique. But the question is whether it is appropriate now to continue in our old ways, given that all of the low-hanging and most of the higher-hanging fruit has already been picked from the dyad tree?

Diehl & Wright (2016) argue that it might be, if an outcome of interest occurs at the level of the dyad. I appreciate Zinnes (1980) as much as the next guy, but referencing a nearly forty-year old address does not on its own constitute a justification of a practice in the face of powerful theoretical and statistical arguments that the practice is suboptimal in the present day. They seem to know that dyadic hypotheses can be tested in a hyperdyadic context, but I’d like to reinforce the point: an exponential random graph model (ERGM) can test the probability of a relationship forming between i and j given a set of covariates just as well as a dyadic regression can. In fact, Cranmer and Desmarais provide (in this paper and their others) the statistical proof that dyadic regressions are simply special cases of the more general, and flexible, ERGM. Thus it is worth repeating: there is really nothing you can do with a dyadic regression that you can’t do with an ERGM; the reverse is not true.

Thus, it would require a very brave theory to force us to choose a dyadic regression over an ERGM given relational data, regardless of what we think about “levels of analysis”.

Unlike Diehl & Wright, Poast (2016) admits the inferiority of dyadic models whenever there is any multilateral process, but argues that the assumption of independence of dyads is not so problematic. I would like to make two points about this, briefly. The first is that it is unknowable, *ex ante* or *ex post*, how big of a problem it is to assume independence when there is interdependence. There is no statistical test for this, nor is there any robustness check or post-estimation procedure that can tell us how much our estimates are biased when we wrongly assume independence. But what we do know for certain is that there is bias, every time, in an unknown direction, to an unknown degree. I don’t know about you but this makes me uncomfortable.

The second defense that Poast (2016) makes against Cranmer and Desmarais’s critique is that the bias from wrongly assuming independence can be partially mitigated by engaging in extreme statistical exertions. I’m not sure of the sequence in which the authors exchanged

their drafts, but Cranmer and Desmarais actually discuss several of these in their piece – the spatial approach proposed by Neumayer and Plumper (2010) and the k -adic approach developed by Poast (2016). While certainly a major improvement over prior practice these are partial fixes, are limited in some key respects (including unidimensionality), and are far more labor-intensive than simply switching to a model that does not assume independence in the first place.

There are other problems that Cranmer and Desmarais did not mention, or barely mentioned. I have already blown past my suggested word limit so I will briefly discuss only one. That is that dyadic regressions multiply data, which artificially inflates the number of observations, which artificially shrinks standard errors. From the perspective of inference this is problematic. In a world of 200 states there are nearly 40,000 dyadic observations per year, which are treated independently in dyadic regression. This strikes me as a very strange way to think about the world at any particular point in time, but it also makes me skeptical of IR papers with around a million observations in which almost every variable has three stars next to its coefficient. Again, one might propose this or that statistical tweak to partially mitigate this problem, but I agree with Cranmer and Desmarais that there is a better way forward.

So I conclude where I began: I am on Cranmer and Desmarais. I also needed to be convinced, but now I believe their critique is much-needed, long-overdue, and should be taken seriously by scholars, reviewers, and editors. There may be times when data limitations or some other suboptimality force us to use models that we know are not the best of all possible worlds. But whenever we have dyadic data we can, and should, do more with them than we typically have done.

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