1) In 1995, James Fearon (1995) published a paper entitled "Rationalist Explanations for War" where he argued that conflict is the result of private information, commitment problems, and/or issue indivisibilities. Write an essay briefly summarizing Fearon's argument. Describe in some detail how Fearon's framework has been applied by other scholars in studies of interstate and intrastate conflict. Which of these three theoretical mechanisms seems to have the greatest effect on conflict? Do you think Fearon's dismissal of issue indivisibilites as a serious cause of conflict has been detrimental to the study of conflict? Why or why not?

Fearon's (1995) influential essay on the "Rationalist Explanations for War" begins with the ostensibly reasonable assumption that war, a costly outcome, is never preferred by the parties involved. This he demonstrates with an expected utility model: between B's preferred outcome (0) and A's preferred outcome (1) there exists a continuous range of possible peaceful settlements. A and B enjoy a "bargaining range" filled with viable solutions bounded by their expected utility for war. Given some known probability (p) for one state to win – whether accurate, shared or not – and risk-neutral or risk-averse states, the bargaining range will never be empty (366-68).

Fearon (1995) uses this initial proposition to pose a puzzle: if war is not a preferred outcome, why does it occur? After dismissing an array of more traditional Neorealist explanations, including positive expected utility and anarchy, Fearon locates three conditions under which rational actors could "…fail either to locate or to agree on an outcome [within the bargaining] range, so avoiding the costs and risks of war" (390): issue indivisibility, incentives to misrepresent private information, and commitment problems. He does not consider the three possibilities equally useful, however, and this may be controversial.

Issue indivisibility would arise if, due to an insufficient number of feasible solutions to the problem, the bargaining range was left effectively empty. Fearon (1995, 389-90) raises three faults of this rational explanation of war. First, he finds most issues in international relations sufficiently complex and multidimensional, thus feasibly divisible into many different settlement opportunities. Second, the opportunity to utilize side-payments and issue linkage always exists. Third, if the number of options is truly limited to a certain number with none mutually preferred, a random alternating allocation mechanism would still be preferred to war. When issues appear indivisible, he concludes, "…the real question is what prevents leaders from creating intermediate settlements, and the answer is likely to be other mechanisms (often domestic political) rather than the nature of the issues themselves" (390).

The first rationalist explanation for war which Fearon (1995, 395-6) lends real credence, then, is the incentive to misrepresent private information. States may choose either to exaggerate or to conceal their true resolve and capability, because they want to obtain a favorable settlement (as well as avoid war). The advantage in exaggeration is possibly increasing chances of deterrence success; in concealment, is hiding vulnerability or revisionist intentions. The result of misrepresentations, however, is the calculation of moves based on inaccurate information. This serves to increase the incidence of unintended wars.

Finally, Fearon (1995, 401-8) argues that the commitment problem also explains the incidence of war between rational actors. Under the commitment problem, as opposed to the information mechanism, the actors possess accurate knowledge of each other's capabilities and intentions. This knowledge, however, makes settlement impossible: one party cannot be trusted to uphold any agreement. At least three mechanisms may give rise to the war-prone commitment problem. When a significant offensive advantage exists for party A (i.e. the prospects for victory are better when attacking than defending), party B cannot believe that preventative war will not be launched by A no matter the agreement on the table. Second, if party B is much weaker or has many accumulated negative experiences at the hands of A, the specter of unacceptable future demands makes initial appeasements intolerable. Finally, Fearon (1995, 408) demonstrates one circumstance in which the "issue indivisibility" of territory can be explained through an alternative mechanism: when the territory on the bargaining table is of significant strategic value, due to resources or tactical position or trade routes, any compromise by party B actually entails complete surrender to A's ideal point; once A gains the strategic advantage inherent in the slice of territory, it will be unable to resist taking the rest.

We might trace the influence of Fearon's (1995) argument through the literature on civil and interstate war. As of the hour of writing, Google Scholar (academic resource *par excellence*) reports 472 citations for this single article. If accurate, this would place Fearon (1995), after 12 years, at the number four rank for the top cited papers in the first 100 years of the *American Political Science Review*.[1] A brief review of the articles citing Fearon (1995) indicates that the number may be largely attributable to passing reference. Few appear to give him credit for the theoretical motivation of their paper, and I could locate no direct, simultaneous test of his three explanations.

A bit more abstractly, evidence of Fearon's (1995) framework may lay in the direction of research pursued in conflict studies in the intervening years. Surely, within the civil war literature we can locate a pronounced emphasis on the commitment problem. Probably bolstered by a further evaluation of this explanation within the specific context of ethnic conflict in Lake and Rothchild's *The International Spread of Ethnic Conflict: Fear, Diffusion, and Escalation* (Fearon 1998), the commitment problem seems eminently appropriate in many cases of civil war and ethnic conflict. As noted by Walter (1997) the absence of guarantees and excellent reasons for distrust often combine in civil conflicts to prevent peaceful internal settlement and prolong violence.

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[1] The November 2006 issue of the journal celebrates its centennial and tallies the third ranked article at 543 citations since 1962 (Sigelman 2006, 670). 1995, however, saw a disproportionate number of "high-impact" articles, indicating that Fearon (1995) might not really be that special (Sigelman 2006, 667).

Fearon's (1997) own work also blazed the trail for incorporation of information problems in the interstate war literature. In the 1997 article, Fearon investigates the relative effectiveness of two manners of signaling – accumulation of audience costs and sinking of costs – in the transmission of accurate information. The model indicates that "tying hands" by generating audience costs for backing down, though riskier, tends to be more effective. Since democracies enjoy a healthy benefit in producing such costs, it becomes likely that different types of states will manage to communicate resolve with different levels of success. Many subsequent models and empirical tests lend support to this proposition (Schultz 1999; Bueno de Mesquita, Morrow, Siverson and Smith 1999; Gelpi 2001; Filson and Werner 2004).

The newest branch of the conflict literature traceable to Fearon (1995) encompasses attempts to account for the "indivisibility" of territorial issues. Walter (2001), describing the importance of future considerations for a state deciding whether to grant territory to rebel factions, harkens directly back to Fearon's (1995) second type of commitment problem, though she does not directly acknowledge this link. DeMarchi and Goemans (2001) challenge Fearon (1995), claiming that the assumption of "single-peaked" preferences common in rational actor models is largely inaccurate and indefensible; they argue that the indivisibility of territory stems from complex preferences within domestic populations. Mitchell and Prins (2001) pursue a different tack, taking on maritime as well as territorial issues and considering the regime type of disputants; even territorial disputes can be settled peacefully provided joint democracy. Hassner (2003) traces the cultural ideas surrounding a specific type of indivisible territory: sacred spaces. In what constitutes perhaps a similar vein, Hensel (2000, 58-9) defends the psychological or intangible value of territory as producing "effectively indivisible issues". Hensel and Mitchell (2005, 279-80) subsequently test this hypothesis, but find rather that states more frequently compromise on territories imbued with intangible rather than tangible value. Goddard (2006), returning perhaps to Fearon's (1995) misrepresentation argument, contends that the indivisibility of issues is an illusion created by the tough bargaining stances adopted by leaders.

In terms of empirical support, it is difficult to assess which of Fearon's (1995) three explanation performs best. The explanations are not generally tested simultaneously, and the forms of test undertaken in separate lines of the literature are not readily comparable. The civil war literature on commitment problems musters significant theoretical appeal (Fearon 1998) and some sound empirical investigations (Walter 1997). Valuable auxiliary hypotheses from this line of research include the vital impact of external guarantors on the resolution of civil conflict and the supply-side determinants of such mediation. While these are valuable contributions, the commitment problem evident in civil conflicts represents generally only one reason why territory can become an indivisible issue and only one type of commitment problem in general.

The information problem continues to influence formal models, producing interesting hypotheses. These hypotheses, though frequently supported, must be tested using proxy variables – usually regime type – as the actual value of states' resolve, let alone the degree to which they might misrepresent it, is unobservable.

Finally, the attempts to account for the indivisibility of territory are wide-ranging. Explanations drift from bargaining strategies (Goddard 2006) to psychological effects (Hassner 2003; Hensel and Mitchell 2005) to underlying preferences (deMarchi and Goemans 2001) to the character and history of disputants (Mitchell and Prins 1999). If cumulative knowledge is to develop in this arena, some unifying framework for analysis must be developed. The recency of this research agenda's development accounts for a good deal of the dispersion in explanations. A bit, however, probably falls on Fearon's (1995) initial rejection of issue indivisibility. One may infer, though the causal relationship is far from clear, that the rationalism argument – while spurring on formal models of bargaining, commitment and information issues – delayed the nascent interest in territory and issue politics evident a few years prior to Fearon's publication date (c.f. Vasquez 1993; Diehl 1992).

It has not been counterproductive to investigate the alternative mechanisms of rational war onset. Much interesting information and even some accumulation within the democratic peace literature followed from Fearon's (1995, 1997) information argument. Moreover, we should not blame Fearon (1995) for "stifling" the study of contentious issues. His aim, rather than dismissing its importance as a mechanism, was to caution readers: the indivisible issue is not interesting *per se*. Rather, progressive theory in this vein must seek to unravel the domestic and relational conditions under which issues such as territory *become* indivisible.

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4) There has been renewed interest in the study of leaders in international relations. Discuss the utility of studying leaders as opposed to states or regimes in international relations. What theories and studies have been put forward to understand how variation in leaders influences international conflict? Evaluate the contribution of these studies to our understanding of conflict. What are some additional areas of research that would build upon these existing studies? Why?

Within international relation's dominant Neorealist paradigm, "first image" theory receives little respect. "[Human nature] cannot by itself explain both war and peace, except by the simple statement that sometimes he fights and sometimes he does not" (Waltz 1954, 29). It is true, of course, that sometimes leaders fight and sometimes they do not. This is reason to abandon all hope, however, only if there exist no intelligible conditions under which one or the other holds. Absent an understanding of these conditions, no theory of the state or system level – save perhaps chaos theory – can proceed. Without minimally rational decision makers, the world of international politics must itself be irrational, beyond the reach of social science. A significant portion of theory in international relations, therefore, relies specifically on the assumption that leaders do respond predictably – if not particularly intelligently – to their international and domestic context. Notably, the democratic peace literature demonstrates a fair amount of cumulative empirical support for its theories relating the domestic incentives of leaders to their proclivity for foreign conflict (c.f. Ray 1995).

The motivations and incentives acting upon decision makers exert a vital impact on the course of international relations. The study of leaders, resurgent in the past few years and especially buoyed by the development of the Archigos dataset (Goemans, Gleditsch and Chiozza 2006), promises a uniquely valid test ground for many of our theories as well as an increasingly sophisticated understanding of the conduct of foreign policy. Attention to the variation in leader characteristics and domestic incentive structures, rather than producing the indeterminate results predicted by Waltz (1954), actually explains empirical puzzles generated by theories at higher levels of analysis. Take two examples in turn, the polarity debate and the monadic democratic peace.

Waltz's (1979) systematic reformulation of realist thought produced a revelation in balance of power thinking: the beauty of the small and smaller great power systems. Specifically, Waltz (1964) contended that the lowered uncertainty and increased stakes of a bipolar world drastically reduced the chances of buck-passing and poor decision-making which produce systemic wars. Deutsch and Singer (1964) counter with a laundry list of pacifying mechanisms unique to multipolar systems wherein many great powers experience the benefits of frequent and diverse interaction. Fuzzy empirical results depicting different patterns across time failed to silence the contention between the two systemic interpretations (Singer, Bremer and Stuckey 1972).[2] Beuno de Mesquita (1981), however, demonstrates a plausible explanation for the confusion based upon leader characteristics. Conditions exist under which either highly concentrated or highly diffused Great Power systems contribute to peace and stability: individual level variations in risk-acceptance condition the link between power and the likelihood of war.

The democratic peace literature has struggled against allegations of inconsistency due to the relative weakness of findings regarding the monadic conflict propensities of democracies (c.f. Rosato 2003). Though the original formulations of the democratic peace mechanisms clearly indicate dyadic relationships, there remains a strong case for the institutional constraint mechanisms (Maoz and Russett 1993) and norms against violent conflict resolution (Dixon 1994) to operate on individual leaders. Some findings do indeed indicate a monadic pacifism in

[2] Mansfield (1992) eventually dispelled the cross-century variation in effect of Great Power concentration with a non-monotonic specification. The quite low R2 for his model (.22 at best) suggests that the actual power concentration tells only a small part of the Great Power war story.

democracies (c.f. Ray 1995; Leeds and Davis 1999), but the relative weakness of these relationships compared with the dyadic democratic peace remains problematic. Keller (2005) provides an intriguing explanation based on leaders' variation in "constraint internalization". Those democratic leaders who tend to respect institutional constraints exhibit lower levels of conflict when presented with a crisis than do those who tend to throw the rules out the window. This explanation may augment the Bueno de Mesquita, Morrow, Siverson and Smith (1999) insight that democratic leaders may be able to successfully target weaker states, explaining why some democratic leaders take advantage while others do not.

The utility of leader level theory and analysis, therefore, lies in its ability to account for variation in outcomes unavailable to other levels of analysis. Considerable empirical evidence (Gelpi and Grieco 2001; Chiozza and Choi 2003; Horowitz, McDermott and Stam 2006), for example, points to the importance of leaders' level of experience on their likelihood of conflict. Gelpi and Grieco's (2001) argue that inexperienced leaders – more prevalent in democracies due to term limits and elections – concede to international challenges at a higher rate than more experienced leaders, inviting more frequent targeting. Chiozza and Choi (2003) also posit that length of tenure may affect the incentives of leaders as they gain experience, legitimacy and reputation. Consistent with these arguments, Horowitz et al. (2005) find escalatory behavior to increase with age.[3]

Over the last ten years, Bueno de Mesquita and various colleagues have developed a highly influential model of foreign policy behavior explicitly built around the institutional incentives of various regime types. The work, culminating in *The Logic of Political Survival*

[3] Wolford's (2007) formal model forwards a contradictory logic: overly concerned with building a "tough" reputation, new leaders are more likely to escalate crises.

(2005), contends that leaders for whom the “selectorate” represents a narrow swathe of the population need not consider the broad public’s welfare in their policy decisions. Rather, their continued tenure in office is secured by provision of private goods to the subset of the “selectorate” actually necessary to maintain power, the “winning coalition”. Thus democratic leaders, motivated by the public good, choose their wars wisely in order to maximize victory and should be targeted less due to their incentive to respond fiercely (Bueno de Mesquita and Lalman 1992; Bueno de Mesquita and Siverson 1995; Bueno de Mesquita, Morrow, Siverson and Smith 1999).

Besides new developments of regarding domestic factors’ affect on international outcomes, investigating leaders also provides an ability to test the mechanisms proposed by existing theories. Many extant explanations of conflict emphasize the interaction between domestic accountability – or lack thereof – and international relations. Most focus explicitly on the impact which domestic concerns have on international conflict behavior. Studies which investigate the domestic impact of international outcomes on leaders thus test the viability of the mechanisms forwarded by the domestic constraints -> international outcomes arguments as well as develop independent theory. Below I discuss the outcome of tests for the mechanisms behind diversionary theory,[4] the “gambling for redemption” proposition (Downs and Rocke 1994).

Diversionary theory posits that in times of domestic turmoil, leaders have an incentive to engage in violent foreign policy in hopes of triggering an in-group/out-group dynamic and “rally” the population behind the flag. Systematic analyses, however, demonstrate a contradictory dynamic: for most leaders, the initiation of conflict becomes more likely the better the prospects of continued tenure. Ostrom and Job (1981) initially reported this finding for

[4] For a helpful summary and critique see Levy (1996).

American presidents, who despite being driven by political concerns in their foreign policy decisions, risked conflict only when riding on a substantial "approval buffer". Chiozza and Goemans (2003) confirm the suggestive results in a much wider pool of leaders. Mitchell and Prins (2004), considering the international context as an additional constraint on leaders nicely locate the boundaries of the diversionary logic. Their analysis accounts for the findings of Chiozza and Goemans (2003) and Ostrom and Job (1981); most leaders follow the pattern of withdrawing from international conflict when faced with domestic trouble. Under conditions of standing rivalry, however, Mitchell and Prins (2004) locate a clear and dramatic increase in the probability of conflict as internal economic conditions decline.

The "gambling for resurrection" proposition forwarded by Downs and Rocke's (1994) formal model also receives bounded support. They argue that if the prospect of losing office is the only restraint on leaders, and that losing military campaigns communicate an increased or definite likelihood of losing office, leaders actually have an incentive to escalate conflicts that are going badly. If they face losing office either way, "giving it their all" becomes rational. Goemans (2000) examines the actual likelihood of leaders suffering severe punishment as a consequence of failed war involvement. He theorizes and finds empirical confirmation for the variation of punishment across regime type and severity of loss. All leaders face increased risk of office after disastrous defeat. Leaders of mixed regimes, however, face substantially higher risks of severe punishment (losing office and suffering additional harm) even when their loss is less than spectacular (570). Goemans (2000) then analyzes the actual duration and cost of wars, finding that a mixed regime on the losing side significantly drags out conflict and drives up mortality (574-5). Colaresi (2004a) further finds that conditions of rivalry significantly depress the riskyness of disastrous defeat by about half.

Chiozza and Goemans (2004) conduct a test for relevant to all theories which claim that war-involvement significantly affects leaders' ability to hold onto office[5]. Their findings indicate that winning war never provides a significant boost in tenure; losing, however, significantly damages the ability of autocrats and mixed leaders to hold onto office. Democrats are not significantly affected in either direction by loss in military conflict, whether they initiate or are targeted.[6]

My interpretation of the result is that leaders who govern through institutions viewed as domestically legitimate may be relatively insulated from the vagaries of international conflict. Marinov's (2005) evidence for a contradictory pattern in economic conflict provides a refinement of this observation. In a mirror image of military conflict loss, economic sanctions destabilize *only* democratic leaders (Marinov 2005). To make sense of this finding, consider that sanctions are generally a tool used and legitimized by democracies. The imposition of sanctions – a legitimate, nonviolent policy tool which nonetheless causes harm to the public – causes democratic publics to question their leaders in a manner which poorly conducted military campaigns can not. Autocratic and mixed leaders, governing on the grounds of superior force and the ability to repress, lose face through military defeat rather than incurring the disapproval of the liberal international community.

The evidence, thus far, indicates that the causal arrow does track in both directions: international behavior appears to be conditioned by leaders' domestic incentives, but so too are international impacts conditioned by leaders' domestic incentives. This second finding is of essential importance to good foreign policymaking. The effectiveness of various foreign policy

[5] Including, according to the authors, Fearon's (1995) rational proposition that war is *ex-post* inefficient.
[6] This finding is a bit different than that reported in Goemans (2000), because conflict loss is not disaggregated into moderate and disastrous outcomes in the 2004 analysis.

tools will not be consistent across targeted states. We have seen this through the results of negative foreign policy behaviors, namely conflict and sanctions, as described above. In the future, I hope to fill a gap in this literature by investigating the variable effectiveness of friendly foreign policy tools across different target states. The initial evidence with respect to foreign aid receipt detailed in the methods section above provides good cause for optimism. I believe this research contributes not only through emphasis on a different class of policy tools (friendly versus hostile). Investigations of military conflict and imposition of sanctions importantly focus on how leaders' own international policy decisions affect their tenure prospects. The foreign policy impact on tenure demonstrated by my work on foreign aid stems explicitly from the outside; demonstrating a means by which powerful leaders wield influence over the tenure of *other* leaders.

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C-1: Select the ideal-typical, academically oriented book on international institutions for each decade from the 1960s through the 2000s. The books should exemplify the most important characteristics and trends in scholarship during each of those decades. Provide a brief description of each book and explain how it exemplifies the overall evolution of research on international institutions.

I choose five books that I think are the most influential in the study of international institutions. These are: *The Logic of Collective Action* (Mancur Olson 1965), *The Anarchical Society* (Hedley Bull 1977), *After Hegemony* (Robert Keohane 1984), *Interest, Institutions, and Information* (Helen Milner 1997), and *After Victory* (John Ikenberry 2001). Before explaining their central arguments and implications, first of all, I briefly introduce three different views on international institutions. I definitely believe that such introduction will make it easier to figure out why such books are important in the study of international institutions. Then, I describe the key argument of each book and its contribution.

It seems clear that there exist fundamental differences among IR scholars with respect to how international institutions affect state behavior. Thus, I look at the key arguments from different views and differences between them. First, let us look at realist side. In his discussion about the relationship between regime and state behavior, Krasner (1982) argues that the way researchers approach studying institutions or regimes affects how they think they affect state behavior. In this line, he proposes a structural view in which institutions arise from certain causal variables, such as shared interests or power, and act as an intervening factor between these causal factors and the state outcomes rather than a direct causal factor. Therefore, he concludes that institutions have no effects on state behavior. Probably, his assertion will represent most of the arguments among realists.

These arguments are also well unveiled in other work, especially about international organization's (IO) effect about international cooperation. For example, Waltz (1979) argues that cooperation is possible when states share similar interests and perceive no threat from each other, which is difficult to get in the system of anarchy. Gilpin (1981) also extends this argument by suggesting that cooperation is only likely if it is enforced by a hegemon acting in its own self-interest. Thus, he emphasizes the role of a hegemon in leading to cooperation. Presumably, Mearsheimer (1994) takes the harshest position against the role of international institutions. He argues that institutions do not change state behavior, and they do not increase the prospects of international stability or peace. Institutions solely reflect distributions of power between states. He also critiques institutionalists who believe that institutions can change the pay-off structure of cooperation. However, he contends that states look at relative, not absolute gains, and so win-win situations are not desirable for states. In this way, realists maintain a skeptical view in which institutions and IOs are solely a reflection of the balance of power, and are used by the major powers to force weak states to behave.

A second group of scholars called Neo-liberals institutionalists agree with realist's view to the extent that states are self-interested actors. However, they assume that international cooperation is seen as a collective action dilemma. Thus, they believe that regimes and institutions emerge when there are certain issue areas that have beneficial outcomes that are more easily reached in the presence of international institutions. More specifically, they argue that institutions change the nature of the bargaining game to states, and so increase cooperation. Institutions do change state behavior, but not state preferences. In response to Mearsheimer, for example, Keohane and Martin (1995) critique realist approach as unscientific, since realists don't test their assumptions about the world. Instead, they argue that institutions change state behavior in a number of ways: they change the incentives for states to defect from agreements, they reduce transaction costs, they link issues, and they provide focal points for cooperation and bargaining with setting appropriate options. Institutions also help alleviate the fear that other state is receiving unequal gains from cooperation, because IOs operate using reciprocity. Institutions overcome the multiple equilibrium problem with international bargaining, since they set boundaries on which policies are acceptable to pursue, and so simplify the world, benefiting state leaders. Abbott and Snidal (1998) also argue that states create and use IOs, since IOs possess both centralization and independence that aids collective action and makes them attractive to states.

A third group of scholars called constructivists, however, makes a direct challenge to the realist approach. Unlike Krasner, first of all, they argue that basic causal values give rise to both institutions & state behavior. For example, Haggard and Simmons (1987) suggest that regimes serve to reshape the international environment in 2 ways: they alter international environment in which states interact; and they change state preferences and character (Jepperson, Wendt, and Katzenstein, 1996). In particular, some scholars reemphasize the second characteristic in which institutions and IOs actually transform state interests and identities (Wendt 1994). Finnemore and Sikkink (1998) argue networks, including IGOs, are a method to spread global norms. Norm entrepreneurs can bring the norms they wish to promote to an organizational platform (NGOs or IGOs), which provides information and expertise to state actors, and allows entrepreneurs access to a relevant audience, such as state leaders, professionals, or public.

They continue to argue that organizations may also act as "agents of socialization" and norms also may become institutionalized in IO rules and procedures. Haas (1989) also makes an argument that epistemic communities and international regimes can mobilize actors at the domestic level, empowering domestic actors to change the behavior of states.

Given these different views on international institutions, let us move on to each specific book and look at where it is located in the study of international institutions.

The Logic of Collective Action (Olson 1965): This book is the classical piece on the collective action problem. Basically, Olson explains that groups suffer from the collective action problem because although the individuals have a common incentive to obtain collective benefit, they have no common incentive to pay the cost to get the benefit. The can be overcome by shame, for example, if the group is small. If there is asymmetry, then there is a tendency for the small to exploit the large. Some groups are privileged, which means that the necessity of the good is so important that one actor will bear all the costs while the good remains public. Olson's answer to overcoming the collective action problem is the use of 'selective incentives,' which are incentives such as prestige, respect, friendship, and other social and psychological objectives. Also, a large group can be split up to make them small.

I believe this book has a significant impact on the following studies about international institutions. The issue of collective action problem became at the center of international cooperation and a lot of scholars investigated the possibility of cooperation under such collective action problems. These studies include the ones by Hardin (1968), Oye (1986), Oneal and Diehl (1994), Wendt (1994), Moore (1995), Ostrom (1998). In particular, his discussion about the role of the number of players in international cooperation has been developed by many scholars in international institutions. For example, Oye (1986) points that as members increase, the likelihood of cooperation decreases, since transaction and information costs rise, the likelihood of autonomous defection rises, and feasibility of sanctioning defectors decreases. In this respect, he argues that institutions and regime can provide rules of thumb and collective enforcement mechanisms, resulting in more cooperation. Furthermore, the number of member also became a critical issue in IOs in terms of its relationship with members' compliance, leading to debate. The study by Koremenos et al. (2001) makes a suggestion about how rational design of IO can increase compliance among member states. In this respect, they argue that the more severe the enforcement problem (greater the collective action dilemma), the more restrictive the membership. The more uncertainty exists about member preferences, the more restrictive the membership, since the restrictive membership allows states to learn more about each other and serves as a costly signal. The more severe the distribution problem, the more inclusive the membership, since more members reduces the effect of zero-sum games and also expands the possibility of trade-offs between members. However, such prescription is still controversial. For example, Kydd (2001) argues that contentiousness of NATO expansion to East European states generates a dilemma for institutional design. He argues that it was uncertainty over preferences (over distrust) that made the inclusion of Poland, Hungary, and the Czech Republic so controversial, especially for Russia. Rejecting the dominant rationalist view that is described previously, Kydd argues that the restrictive nature of NATO expansion to states meeting very specific criteria can in fact increase trust between NATO states and Russia. This is because restrictive membership indicates that NATO is not pursuing expansionism for its own sake or to threaten Russia.

The concept of ‘privileged group’ has also been developed well in the following years, especially focusing on the role of hegemon in international institutions. The studies by Keohane (1984) and Ikenberry (2001) discuss this issue in detail.

The Anarchical Society (Hedley Bull 1977): Bull is not totally against realism. He does hold some commonality with realists such as the world is anarchic, states are important, and the balance of power is important, too. But, unlike realists he believes that some semblance of order exists, but the domestic analogy is not a perfect one. The “institutions” (different connotation here) that Bull believes provide order are: BoP, international law, diplomacy, war, and great powers. He discusses three world views: Hobbesian, Groatian, and Kantian. He primarily put himself in the latter two, although the primacy he gives to the BoP suggests that he may be a bit of a Hobbesian as well. The goal of states is to maintain the system even at the cost of war. Order is preferred to justice, especially if you are a great power. Among his discussion of those five “institution,” his view on international rules and laws as maintaining international order gives importance to the study of international institutions. He agrees that rules help to provide precise guidance to states, but they are mere intellectual constructs and do not really create order. For rules and laws to be effective, he assumes that they should be made, communicated, administered, interpreted, enforced, legitimized, capable of adaptation, and protected against changes that may undermine it. He argues that international law is important because it establishes the idea of a society of states, establishes rules regarding violence, and provides a means to mobilize compliance. However, it is not necessary or sufficient for international order and may actually hinder international order.

I believe his views on international order have two important implications in the study of international institutions. The first is that it has a large impact on realists' argument on international institutions. The second is that it generated debate realists and constructivists, especially about the role of legitimacy that international institutions are assumed to have. As discussed in introduction, first, his view becomes a basis of realists' argument that international institutions have no effect on state behavior including compliance. Since the balance of power and maintaining the system is the primary goal of states, following rules and laws will never be chosen. This strong argument is exactly reflected in the studies by Waltz (1979), Gilpin (1981), Krasner (1982), and Mearsheimer (1994).

Second, his skeptical view on justice or legitimacy, such as human rights can be violated in the face of some elements of international order, generated controversial with constructivist approach. As described earlier, a group of scholars raised a question against Bull's approach and investigated how norms and legitimacy that institutions have can change state preferences and their behavior. For example, Haas (1989) examines how epistemic community change states environmental policies in Mediterranean Sea. Besides this, they also argue that norms as appropriate conduct provide reference points for acceptable state behavior (Wendt 1994; Simmons 1998; Hurd 1999). In a similar vein, IOs possess legitimacy; they set standards of acceptable behavior for community members. Thus, legitimacy reflects an acceptance to a moral authority (Abbott and Snidal 1998; Hurd 1999).

After Hegemony (Keohane 1984): Keohane argues that cooperation must be distinguished from harmony. Harmony refers to "a situation in which actors' policies automatically facilitate the attainment of others' goals (i.e, Invisible Hand)," whereas cooperation requires that "the actions of separate individuals or organizations be brought into conformity with one another through a process of negotiation, which is often referred to as policy coordination" (p.51). He argues that it is important to define cooperation as mutual adjustment rather than to view it simply as reflecting a situation in which common interests outweigh conflicting ones. In other words, we need to distinguish between cooperation and the mere fact of common interests, since discord sometime prevails even when common interests exist. Especially where uncertainty is great and actors have different access to information, hurdles to collective action and strategic calculations may prevent them from realizing their mutual interests. This is the reason the mere existence of common interests is not enough. At this juncture, he strongly argues that institutions play a significant role in reducing such uncertainty and limiting asymmetry in information. In other words, institutions can provide information, reduce transaction costs, make commitments more credible, establish focal points for coordination, and in general facilitate the operation of reciprocity. Thus he argues that international institutions are most likely to form when states share interests, policy areas are dense, and so states need rules to operate in these complex situations (*ad hoc* agreements are good enough for low-density issues). In other words, IOs help simplify the policy area by setting constraints on acceptable bargaining options), provides information and allows states to monitor each others actions, and finally allow the reputational calculations increasing compliance to take effect. For the role of hegemon, he argues that hegemon can help create shared interests by providing rewards and punishments for defection; but other states working together can supply the same effect.

His work has a tremendous effect on the liberal institutional group in IR, supporting that institutions change the bargaining game to states and so increase cooperation. For example, the study by Keohane and Martin (1995) described earlier reflects his points well. It deeply influenced another group of scholars, arguing that states form IOs due to IOs' certain attractive characteristics, such as centralization and independence (Abbott and Snidal, 1998). They argue that centralization, first, is appealing to states because of two reasons. The first is that IOs allow greater communication and information, allow faster responses to sudden developments, set rules that shape state interactions, strengthen issue linkages by locating issues within common organizations, and provide useful consultative and supportive functions. The second is that centralization allows member states to pool resources like information or assets, reduce uncertainty by setting contingencies for appropriate action. They also argue that independence as an important feature of IOs is appealing. First, they define independence as acting with autonomy and perceived neutrality, and argue that IOs provide such efficiency and legitimacy. It is because IOs are believed to be better able to serve as neutral sources of information, as neutral trustees of resources, as an impartial distributor of resources, and as a neutral arbiter in cases of legal disputes between members.

Keohane's work continues to contribute to the discussion of rational design of institutions. For example, Koremenos, Lipson, and Snidal (2001) argue that distribution, enforcement, large number of actors, and uncertainty make cooperation difficult even with repeated interactions, and that's why states create institutions, especially to create rules to govern behavior. Among several variables, the argument about centralization is relevant. They argue that the more uncertainty about behavior, the more centralization, since uncertainty is noise, and centralized information helps to reduce this noise. The more uncertainty about the state of the world, the more centralization, since all actors benefit from joint information gathering and also helps to overcome "cheap talk" (Morrow, 1994), in which states have individual reasons not to share fully or honestly. The greater the number of actors, the more centralization, since more actors makes bargaining more difficult; "centralized bargaining reduces transaction costs" (788). The more actors, the more valuable centralized information becomes, since large numbers interacts with uncertainty – centralized information is even more valuable because it improves ability to monitor defections. The more severe the enforcement problem, the more centralization. It is based on the fact that, assuming states will resist centralization to maintain their sovereignty, organizations that can withhold resources have leverage over weak states. Even in the absence of enforcement mechanisms, institutions that can impose reputational costs will greatly reduce cheating. Even some centralization can improve domestic enforcement capacity – the organization itself does not need own enforcement agents.

In an empirical sense, Keohane's argument is also supported by Fortna's work (2003). She examines whether post conflict agreements are epiphenomenal. That is, are they simply symbolic of what the states would have done anyway, or do they have an independent effect? She argues that the mechanisms within a cease-fire agreement lead to durable peace, such as changing incentives to break a ceasefire, reducing uncertainty, and preventing accidental violations from triggering another round of fighting.

Interests, Institutions, and Information (Milner 1997): Milner (1997) raises empirical puzzles like why nations cooperate each other and when and under what terms countries are able to coordinate their policies in an issue area (p.5-6). To understand those puzzles, she first launches a strong challenge against realist theory, arguing that cooperation among nations is affected less by fears of other countries' relative gains or cheating than it is by the domestic distributional consequences of cooperative endeavors (p.9). Cooperative agreements create winners and losers domestically and therefore generate supporters and opponents. Thus, she argues that the internal struggle between these groups shapes the possibility and nature of international cooperative agreements.

To support her argument, Milner (1997) claims that states are not unitary actors. They are not strictly hierarchical but are polyarchic, composed of actors with varying preferences who share power over decision making (p.11). Even if the domestic struggle for political power and the survival of the state are critical, the behavior of states changes in polyarchy. For example, she states that having two players make a decision leads to a different outcome than if just one does, assuming their preferences differ. Therefore, the search for internal compromise becomes crucial and, thus, international politics and foreign policy become part of the domestic struggle for power. In the process of domestic politics, she argues that three factors are important to the extent of polyarchy; the policy preferences of domestic actors, the institutions for power sharing, and the distribution of information. She then concludes that states create international institutions to constrain domestic actors and "lock in" certain behaviors. It is because state leaders fear domestic retribution for cooperative agreements, and so can "pass the buck" of responsibility to an IO.

Milner's work is making contribution to the study of international institutions by providing an insight of the connection between institutions and domestic politics, which is never incorporated in the previous studies of cooperation. Most of all, her systematic and logical explanation between domestic politics and cooperation provides a clear picture of how these two levels work together and brings huge implications to understanding cooperation. Her theory also becomes a critical basis of several empirical studies later. For example, Pevehouse (2002) considers how joining regional IOs might help a young democracy consolidate its reforms. He argues that both winner and losers from democratization could ruin the prospects for consolidation, but these problems are assuaged with integration into IOs. Joining IOs provides more credible commitments than domestic acts because they set in place mechanisms to increase the costs of anti-regime behavior (sanction or expulsion), costs associated with membership are credible commitments, and joining IOs creates unique audience costs. He finds that joining regional IOs leads to increased longevity for new democracies. In addition, Milner's study makes scholar pay attention to how competing national interests influence international cooperation. For example, BDM et al. (2005) look at the relationship between competing winsets and international cooperation. Also, Schultz (2005) examines how cooperation varies depending on the relationship between hawks and dove in domestic politics. Dai (2005) also investigate the effects of domestic actors on whether states comply with agreements.

After Victory (Ikenberry 2001): Ikenberry basically argues in this book that hegemons create institutions to lock in their preferences and convince weaker states to follow the rules of such institutions. He argues that, following major wars, the old order is destroyed and a new hegemon looks to create a new legitimate international system. His argument, however, is different from hegemonic stability argument in a sense that hegemon is not a necessary enforcer of international order. He argues that hegemons realize that they will not be the preponderant power forever, so they create a system that locks in their preferences. They convince weaker states to abide by the order by setting constraints on their own action rather than policing the agreement, since it is too costly. Instead, they give weaker states an incentive to work within the new system. In this respect, he suggests that institutions are sticky and they have increasing returns. Although the initial cost of setup is large, he argues that learning gives an IO advantages, such as enhancing commitments and raising the cost of changing the system. He also adds that democracies make the best postwar institutions because of transparency, decentralized policy process and open and decentralized system.

Most of all, Ikenberry makes a contribution by touching one of the controversial issues in the study of international institutions. His contribution can be viewed in two ways. First, his argument is quite opposite to Bull's view on the relationship between rules and order. Unlike Bull's argument that institutions do not create international order, Ikenberry argues that institutions can provide a stable international order and hegemon plays a significant role in that process. In a practical sense, such competing view demands us to look at how institutions, especially created by hegemon, have performed in terms of maintaining order. For example, we can examine some of the institutions, such as the UN or IMF made by the US after WWII and whether they actually contributed to stabilizing international order. Nevertheless, some empirical studies provide bad news for Ikenberry's argument. Although it is not a direct test on the role of hegemon, it seems that many institutions suffer from internal confrontations and do not create a stable order among member states. For example, the study by Gallarotti (1991) focuses on macroeconomic reforms by IOs such as IMF. He explains that the attempt to stabilize the dollar by G-7 just exacerbated an already negative situation. Instead of working to come to a compromise among many international states, he argues that many situations would work out better if each state pursued its own good. He emphasizes that this is especially true when considering macroeconomic policy in which states have competing economic models. The other argument that he points to is that the failure of international institutions lies in the tendencies to intensify existing disputes. He claims that institutions can be a vehicle through which collusion and alliance building can take place. Institutions such as the UN can actually cause more confrontations because they force opponents to come face-to-face to confront a variety of issues, which escalate confrontations that might have been ignored otherwise. In addition, it seems that Voeten's finding (2004) is to some extent consistent with the second point by Gallarotti (1991), causing more confrontations. He examined different agenda in the UN, such as the Middle East, sanctions, arms control, and human rights and find that there was actually a widening gap between the US and the rest countries in terms of their preferences. Although a lot of reasons are conjectured (i.e., balancing or lesser form of resistance), this demonstrates that the UN creation by the US itself does not automatically increase cooperation among members in the UN. This study sends a message that, as long as there is fundamental difference in states' preferences, maintaining cooperation among member states is still complex even under the hegemon.

Second, he makes a contribution by bringing a new insight to enforcement problem in international institutions. Given the debate about hegemon as an enforcer between realist and liberal institutionalist (Gilpin 1981; Keohane 1984), he provides an important idea that when we need to examine the characteristics of the enforcer or hegemon, such as *American* hegemony rather than American *hegemony*. This is a different approach to enforcement problem in a sense that the previous studies (realist and liberal) have focused only on the capability of the hegemon as a means of enforcement.

C-2: Some scholars argue that trade promotes peace between states, others argue that peace promotes greater trade, while others argue that the processes are endogenous. Briefly explain the causal mechanism for each approach and evaluate the empirical record for each. Is there a theoretical or empirical way to reconcile these multiple viewpoints?

In this study, I first look at different views about trade-conflict relationship and their empirical findings. Second, I look at the endogeneity between trade and conflict. Then, I conclude that there is reconciliation between these two arguments. However, I also argue that such reconciliation is not perfect, but still has some problems and issues that should be considered in future research.

The relationship between economic interdependence and conflict has received considerable theoretical attention. Much controversy remains, however, and there are a number of contending propositions. Since Montesquieu and Kant, first, supporters of commercial liberalism have argued that economic interdependence will promote peace. Angell (1910) conceived of war in times of high economic interdependence as a highly unlikely event of collective irrationality. Deutsch (1957) maintained that trade and other forms of exchange would help foster the development of a sense of community that would make war unthinkable. Viner (1985) makes a similar argument that increasing contact among traders and consumers located in various countries fosters a sense of international community, as well as the development of mutual respect and harmonious relations. More recently, Polachek's (1980) expected utility approach claims that growing interdependence renders warfare more costly. He bases its theory on Richardo's theorem of comparative advantage, claiming that specialization in production will increase trade and in turn will grow the income of the average citizen. Rosecrance (1986) makes an interesting argument that international trade replaces conquest as a means of exchange, thus lowering the expected levels of conflict. What we previously gained through war can be gained through trade. He argues that economic trade can remove economic-based incentives for conflict, leading to a more peaceful international environment. Stein (1993) continues this tradition by arguing that open commerce dampens political conflict by promoting economic dependence. Open trade encourages specialization in the production of goods and services, rendering private traders and consumers dependent on foreign markets. These players have an incentive to avoid wars with key trading partners. This 'binding commercial liberalism' is also supported by Kim (1998), Oneal and Russet (1997, 1999, 2000, 2001), and Oneal, Russett and Berbaum (2003).

Marxists and dependent theorists support the idea that trade would result in warfare. Lenin's imperialism claims that the search for new markets would result in global tensions that ultimately lead to arms struggle between the oppressors and the oppressed in the world. Dependent theorists further articulated that trade and economic dependence tend to benefit powerful states and elite interests within developing societies, while those actors and states lacking power endure the costs of dependence, rejecting the portrayal of trade as mutually beneficial (Dos Santos 1970; Galtung 1971). Both Barbieri (1995) and Hegre (2001) find support for the argument that symmetrical trade may promote peace, whereas asymmetrical trade creates tensions causing conflict.

Interestingly, neorealists like Hirschman (1945), Waltz (1979) and Grieco (1988) suggest gloomy predictions about the impact of economic interdependence on the likelihood of conflict. Hirschman (1945) argues that when one partner in a trading relationship needs the trading relationship more than the other, this can cause the less-needy state to have leverage over the other. Thus, this may be a source of tension as the less-needy state wants concessions or advantages from the dependent state. Waltz (1979) argues that close interdependence increases contact and consequently raises the chances of conflict. Although economic exchange makes an economy more efficient, it will be difficult to draw any cooperation through trade, since trading also feeds your potential enemy. In line with relative gains problem, other groups of scholars, such as Gowa and Mansfield (1993) and Gowa (1994), make a similar argument that 'trade follow the flag' due to security issues, such as alliance. Basically, they argue that trade creates security externality. Between allies, this externality is positive; higher national income means higher military spending, and both allies benefit from each other's spending. Between adversaries, the externality is negative. Higher military allocations by one state make the other less secure. Thus security matters determine who trades with whom.

I believe that such differences exist not only in theory, but in empirical findings. Since Russett (1967) found the positive effect of trade on war, there have been a number of studies assessing the interdependence-conflict relationship. Wallensteen (1972) highlights that wars are most likely between asymmetrically-related states. Considering other variables in a dyadic level, Polachek (1980) and his following works find that trade has a negative impact on conflict. In 1996, Oneal et al. and Barbieri presented the very opposite results of trade-conflict relations, relaunching the debate. Oneal et al. and Oneal and Russett (1997, 1999, 2001) find that trade has an inverse relationship with conflict, supporting the position of commercial liberalism. Crescenzi (2000) finds that, using a framework of exit costs, trade increases low level of conflict (MID), but decreases high level of conflict, such as war. Meanwhile, Barbieri (1996, 1997, 2002) finds that only mutually beneficial trade promotes peace, while asymmetrical does not.

Such differences in empirical findings also takes place when we look at studies based on different levels of analysis. While most studies were implemented in dyadic level, there have been some studies looking at trade-conflict relations in a monadic level. For example, Domke (1988) examines the conflict-propensity of states at monadic level and finds considerable support for the thesis of Rosecrance (1986) that trading states are more peaceful than more autarkic states. Barbieri (2003) corroborate these findings, which is the opposite result to the ones in her dyadic analysis. Surprisingly, there has been little empirical research on the relationship between trade and conflict at the system level. The main example is Mansfield (1994) who finds that major power wars are less likely during periods of high trade, but that they are more likely during periods of economic openness (level of trade barriers).

A related research question is obviously whether militarized conflicts impinge on the level of trade. This question has important implications for theories about trade-conflict relationship. For example, the liberal hypothesis that trade deters conflict rests on the assumption that conflict reduces trade and, hence, the welfare gains from trade. If this is the case, then, the evidence that states trade with the enemy during wartime, in other words, conflicts do not reduce trade, would undercut the central causal mechanism of the liberal proposition. At the same time, such evidence would also undercut the strong implication of realist theory that relative gains concerns will lead one or both adversaries to terminate trade in order to deny the other the ability to convert relative gains into usable military power. In this aspect, there were some empirical studies about the effect of conflict on trade. However, I don't believe that any final judgment is possible, due to their different findings. For example, Mansfield's (1994) system level study shows that less trade is conducted during periods in which major powers are involved in wars against each other or against other states. Pollins' (1989) study on bilateral trade flows shows that cooperative political relations increase trade. Gasiorowski and Polachek (1982) find that Granger causality for short lag periods run overwhelmingly from trade to conflict and not from conflict to trade. If this is true, this is a puzzling finding because it simultaneously supports the liberal prediction that trade depresses conflict while undercutting the central causal mechanism of the liberal hypothesis – the anticipation that conflict reduces trade and consequently the welfare gains from trade will deter states from conflictual behavior. Reuveny and Kang (1996) find that although the causal relationship between conflict and trade is dyad-dependent, it is largely reciprocal. The studies by Giltner (1997) and Morrow (1997) has shown against this realist determinism that bilateral economic exchange is also possible between adversaries, 'trade does not follow the flag'. Barbieri and Levy (1999, 2001) came to the conclusion that the effect is rather negligible. They argue that this poses problems to both liberal and realist scholarship. Using a larger sample of dyads, Anderton and Carter (2001) came to the opposite conclusion. In particular, they find that long wars disrupt trade, but the effect of short wars is less clear.

There is the other group of scholars that argues so called 'rational expectations theory'(Morrow, Siverson, and Taberes 1998; Morrow 1999; Li and Sacko 2002; Long 2004; Li 2006). The theory argues that firms maximizing profits would decrease their economic activities beforehand if they anticipate militarized disputes in trading or investing countries. Expectations of militarized disputes increase their transportation costs, freight and insurance, because of the risk to property and employees. Knowing these results, firms assess political relations between states and anticipate conflict before it comes to military blows; economic actors leave markets where the risk of conflict threatens their profits and therefore militarized conflict does not affect economic activities (trade or investment) when it actually occurs.

Meanwhile, there is also a group of scholars that the relationship between trade and conflict should be understood in the aspect of endogeneity. In particular, such endogeneity argument is supported by the coercive side of trade as a policy instrument.

One important common feature across different theoretical views is that, as Stein (2003) points out, trade has been treated as an independent variable that affects the incidence of conflict. However, I pay attention to the fact that trade itself is not exogenous to political calculations and decisions. That is, interstate cooperation and conflict affect trade. Intergovernmental agreement is often a prerequisite for trade. Moreover, governments sometimes encourage trade with specific countries for political purposes and use a variety of levers to affect trade levels, such as solidifying alliance and signaling commitment (Skalnes 2000). In the deterrence literature, the empirical findings suggest that trade ties do indeed deter (Huth and Russett 1984). Also, trade itself is affected by interstate cooperation and conflict even without governmental intervention. It is because traders and foreign investors prefer stability and avoid conflict that makes their economic activities uncertain (Solingen 1998). In contrast, countries that are rivals or enemies purposely constrict trade between themselves (Stein 1984). In econometric analysis, such an endogeneity is also supported (Reuveny and Kang 1996, 1998).

The argument above is also supported by the studies that trade generate conflict and that states sometimes use trade as an instrument of coercion. The studies by Conybeare (1987) and Levy and Ali (1998) provide good evidence that a history is replete with trade wars and trade disputes. And these arise only among countries with extensive commercial relationships that are typically politically close, such as the US versus its allies during the Cold War. As in Copeland's "theory of trade expectations," also, we need to look into the expectations of future trade, as commercial liberalists argue. If trade between states is substantial, but is expected to decline significantly in the future, the economic opportunity costs of war are substantially lower that if trade is expected to continue at current levels. Indeed, if states expect trade to decline, and if they expect to lose access to vital goods in the process, they may be tempted, in some situations, to resort to military force to seize those goods. As the US currently imposed sanction on North Korea or plans to impose on Iran and Sudan, moreover, we need to realize that trade has been used as an instrument of statecraft, pursuing political rather than economic objectives. The extensive trade ties provide states with a tool to compel a change in others' policies by cutting off the ties. The use of economic sanctions as an instrument of coercion thus implies that trade does not preclude conflict and does not necessarily lead to cooperation. In this respect, trade can be both a source of conflict between states and a coercive diplomacy in interstate disputes (Stein 2003).

Given these arguments, I disentangle such complex relationships between trade and conflict in a broader context of rational explanations of conflict. There have been actually several efforts to take this approach in previous studies. First of all, I will explain the basic idea of such rational approach in trade-conflict relation. And then, I will point out some problems with that and explain why trade and conflict should not have any relationship, as opposed to the expectations by commercial liberalism.

Basically, a rational view of interdependence theory offers similar predictions as liberals. The rationalists like Gartzke, Li, and Boehmer (2001) argue that trade levels among states are not private information. Thus, trade by itself cannot cause binding since either side knows the other's constraints when entering a costly bargaining contest. What trade does give a state, however, is a broad pallet from which to select "signals" through state economic policies or through markets. They advance that governments of states that are highly integrated in economy are better able to signal their true intentions. It is because foreign policy a government pursues gains in credibility with the size of the opportunity costs that political violence would create. Thus, they predict that major conflicts will be deterred since states can use trade as a signal to show resolve. This is also somewhat consistent with Hegre's (2000) argument that increased trade increases communication and thus decreases misunderstanding, resulting in less conflict. In a similar vein, Stein (2003) argues that economic sanctions work as costly signals. When conflicts of interest arise, states look at the range of instruments they have to signal their concerns and the intensity of their preferences. In relationships in which there is some trade, economic sanctions are an intermediate step between mere diplomacy and military measure, working as a credible signaling device that carries resolve. Thus higher levels of trade are associated with lower levels of military disputes because trade provides a mechanism for costly signaling, whereas lower levels of trade are associated with militarized dispute both because the parties have little or no trade and because the use of economic sanctions has substantially reduced the trade by the time of the emergence of the militarized disputes. Then, he argues that trade does not reduce conflict; it provides an instrument of statecraft short of military action. It is the very coercive potential of trade sanctions that exist in states with commercial links that provides the opportunity to avoid militarized disputes.

Presumably, the first problem of this explanation is that it still treats trade as an exogenous variable. Since it was explained earlier, I would not point out the problems related again. The second problem with a costly-signaling argument is that the trade links between states are known and strategic interaction is inherently built in the reactions between states (Lake and Powell 1999). State make decisions in interaction with specific others whose choices they are interested in affecting and whose responses must be anticipated and incorporated in decisions. International conflict and cooperation, especially in bilateral relations, are the product of a strategic calculus. Thus, I believe that assessing the impact of trade on international conflict need to incorporate trade into such a calculus.

In this aspect, I am more consistent with the strategic approach to trade-conflict relationship by Morrow (1999). The key insight in strategic interaction is that states calculate and anticipate reactions to their strategic choices. States anticipates other's actions and reactions, and steps both taken and avoided reflect a calculus of expectations. The logic of anticipated reaction creates major problems for empirical work and assessing the implications for what is actually observed. Trade is known and observable to the parties prior to any conflict. Conflict exists in the shadow of trade. A state calculating the initiation of some conflict anticipates its trade partners' possible reactions and its own response and so on. The trade costs absorbed by the initiator may make it less willing to initiate a dispute. But the initiator is also aware that the trade costs of conflict will also make the target more reluctant to sustain the dispute, and this emboldens the initiator. The trade link thus has the simultaneous effect of dissuading and emboldening an initiator to a dispute. And there is no basis for inferring which effect is systematically stronger. Thus, the result should be that trade links have no net effect on the initiation of disputes. The 'true' explanation of peace or conflict may lie in some other attributes characterizing the relation between states.

Although I strongly support the strategic interaction approach to trade-conflict relations, I also see some problems with the approach, but the problems will not be only this case. I believe that these problems will be equally applied to trade-conflict literature in general. The most obvious problem of the strategic approach (as well as other arguments) is that the argument is usually based on mercantilism describing a government as a welfare maximizer and a unitary player in trade. Such unitary assumption will be ok if we attempt to debate about neorealist's relative gains reasoning. However, it gets into trouble when we acknowledge that both the decision to wage war and to liberalize an economy have important domestic repercussions. For example, we can assume that the economic well-being of population may not be necessarily the top priority of political leaders. If economic interdependence invites political changes such as democratization or a strengthening of the political opposition, leaders may opt against it. In this case, trade costs of conflict or sanction of the target may not be an important factor to the leaders.

Only the costs for their home country, especially for the survival of the leaders, will be the most important.

In addition, previous studies about domestic effect of economic interdependence provide a good direction for future strategic interaction approach. For example, Schneider and Schultze (2003) stress on the domestic redistributive effects of trade, distinguishing between the export, the import-competing and a military-sector as the crucial actors. Given tax increase from economic interdependence, the military is in favor of both interdependence and conflict since military tensions will increase its budget. Depending on a government's position in a continuum between the military and the import-export sectors, we will be able to expect which attitude toward conflict the government can adopt. For example, if the military is politically weak or the war is too costly, states will be more peaceful in times of expanding economic ties. In a similar vein, the sector-specific model like the Ricardo-Viner model provides a promising avenue of research. Assuming that factors are specific for particular industries and cannot easily move across industries in case of changing market conditions, the model implies that political constituencies in trade policy will build up along the division between the export- and the import-competing sectors (Rogowski 1989 also makes the similar argument).

Another important factor that strategic approach is missing is that such interactions around trade and conflict can change depending on specific types of trading goods. Thus, it has been suggested that we need to consider the role of asset-specificity in trade goods when we study trade-conflict relationships. For example, Morrow (1997) makes a distinction between typical trade goods like commodities, capturable goods, and military goods. He argues that a peaceful outcome is the highest when states trade in military goods. However, empirical test by Souva (2002) shows the opposite case in which he find that trade in strategic goods increases the likelihood of conflict, whereas trade in non-strategic goods decrease. Thus the question of how different types of goods influence conflict remains unsolved.

In addition, there are other possibilities that may constrain such interactions between states. Mansfield and Pevehouse (2000), for example, find that dyads with preferential trading arrangements (PTA) are less likely to fight each other, while trade has little impact on conflict for nonmembers to such arrangements. They talk about the future stream of gains from participation in a PTA and emphasize the role of PTAs in maintaining a state's access to key international markets and also in providing insurance against the possibility of protectionist measures by their trading partners in the future. Then, states' anticipation and reaction are conditions by each other as well as by outside institutions, making the game more complex.

As I explained so far, the relationship between trade and conflict is complicated and need a very extensive study to be fully understood. However, I showed that there is a possibility of reconciliation between different arguments, especially focusing on strategic interaction approach. At the same time, other conditions are recommended to be incorporated in future research.

Question: Identify a literature within conflict that ought to attract more attention and one that ought to be allowed to die on the vine. Defend your choices.

ANSWER #1:

The study of conflict, as any study subject to scientific inquiry, constitutes an ever-changing body of knowledge. New theories come to being, new methodological approaches are being put forth, and new bodies of literature begin to blossom. Old theories and bodies of literature are often times dismissed and heavily criticized. Scientists change focus of their scientific inquiry. Our knowledge about international conflict is, thus, constantly changing. Just as the phenomenon of life, theories and bodies of literature are born, they develop, flourish, and then oft times decline in a more or less abrupt fashion. Thus, some topics once popular become just as a dried-up well in the middle of a desert. Sometimes these bodies of literature are just waiting to attract more attention and blossom again. Some topic, however, once dried up should be allowed to die on the vine of the conflict tree.

In this essay, I identify two bodies of conflict literature, among which one should be allowed to die on the vine, and the other should definitely attract more scholarly attention. To me, the literature devoted to arms races exhibits all features of a scientifically unhealthy branch of the conflict tree. Literature devoted to the role of context in conflict (geography and history), should, on the other hand, be allowed to blossom even more. The remainder of this essay proceeds as follows. First, I defend my view that the arms races literature should be allowed to die on the vine of international conflict. Secondly, I turn my attention to the literature devoted to the role of context in conflict as an example of a body of work that ought to attract more scholarly attention. Finally, I offer some concluding thoughts.

Literature devoted to arms races- why should we allow it to die on the vine?

Arms races relation to war should constitute an interesting and highly vibrant area of scientific inquiry. Scholars engaged in research devoted to this subject have been for a long period of time trying to solve quite a few thought-provoking puzzles. First of all, what is the relation between arms races and conflict escalation? How often to disputes between rapidly arming countries escalate to war? (Wallace, 1979, Diehl, 1983, Sample, 1997, 1998b)? Do nuclear weapons have a significant effect on the arms race and dispute escalation (Sample, 1998b)? Do other factors present in an arms race increase the probability of a war breaking out (Sample, 2002)? These and other very interesting questions have defined this body of literature.

The questions mentioned above are, obviously, very interesting and thought-provoking. They should, therefore, provide highly elucidating, both theoretically and methodologically, debates. When we look at this body of literature, we also see that it continues to attract a considerable amount of attention; several analyses have been conducted in the recent times (example: Sample, 2002, Diehl and Crescenzi, 1998).

Why, then, do I identify research on arms races as the body of literature that ought to be allowed to die on the vine? I will answer this question by elaborating upon some rather negative characteristics of this body of work that sharply distinguish it from others. Unfortunately, majority of scholars studying the relationship between arms races and war have been endlessly engaging in mostly fruitless methodological debates. In other words, rather than constructing a sound theory (theories), most scholars have been devoting much time and effort to issues such as appropriate samples, measurement of unusual military build-ups, and so on. Theoretical developments have been, on the other hand, almost non-existing. For all the attention given to arms races, very few of the authors since Richardson (1960) have even attempted to develop a well-organized theoretical argument, which would explain exactly why arms races are connected to war in a causal fashion (Diehl and Crescenzi, 1998). Perhaps arms races are only correlated with escalation of disputes?

In my opinion, the state of this body of literature has not yet provided a sufficient explanation of the nature of this relation. Thus, the present specification of the relationship between arms races and war is much too weak to warrant almost exclusive empirical focus that has so far characterized arms races literature. Certainly, never-ending debates over measurement, indexes and sample sizes are hardly justified if the theory is weak, not to say nonexistent.

Thus, despite at least one significant effort to provide a sound theoretical basis for research (Diehl and Crescenzi, 1998), arms races research has sadly disappeared from the group of vibrant and healthy developing bodies of work devoted to conflict. Unless scholars follow the call of Diehl and Crescenzi to engage in "explicit theory building on when and how arms races may cause wars," this body of literature will slowly die out. Method, measurement should never occupy our minds to the point that we forget about theory. Quarrels concerning measurement can, of course, be beneficial, but they should never take our eyes of a theory. Because of the reasons stated above I have identified this area of study as the one that ought to be allowed to die out. Perhaps some efforts to move the scientific discussion onto a much more theoretical turf can rescue the arms races research from its pessimistic future. In order to keep the scholarly tree of conflict healthy and vibrant, only well developing branches should be kept up and allowed to blossom. The arms races literature has, unfortunately become an almost theoretically dead branch on the tree of conflict. Theoretically unproductive branches like this one ought to be allowed to die on the vine.

Literature devoted to the context of conflict (geography and history)- why should it attract more attention?

Quite a few scientists of international conflict suggest that phenomena occurring in our present world cannot be fully explicated without concentrating on their context (Sprout and Sprout, 1969, Goertz, 1992). These scholars suggest that history, geography, and other types of context can play a crucial role in our explanations of conflict. What is a context and why should the literature devoted to the relationship between conflict and context attract more attention?

According to Harold and Margaret Sprout, environment, milieu, or context "connotes some idea of relationship, both in popular usage and in technical vocabularies of special fields. Something is conceived to be encompassed- that is to say, environed- by something else in some meaningful relationship" (Sprout and Sprout, 1969:107). This definition reminds us that phenomena occurring in the world, including conflict between nations, never take place in a vacuum. States and other international actors are embedded in their surroundings and this reality affects their international and domestic undertakings.

There exist numerous studies, which incorporate into their analyses the notion of context. Some scholars have looked at how geography can constitute a facilitating condition for conflict (Starr and Thomas, 2002, Bremer, 1992, Diehl, 1985). The main finding in this area is that war is more likely to occur between states that are geographically proximate. Other scholars studying the relationship between geographical context and conflict, argue that geography can very often be a source of conflict by making it easier or more difficult to fight an opponent. It can also provide a reason that states become involved in a conflict. The major finding in this area is that territorial issues make an important difference in conflict behavior, beyond the effect of contiguity (Hensel, 1996).

In their definition of the notion of milieu, the Sprouts argue that the idea of context refers also to intangible factors, such as social patterns, and understandings. Thus, quite a few scholars have focused on the relationship between history and conflict. The argument made by this body of literature is that history can help us define a situation, interpret its meaning. Humans and states tend to learn from success or failures that they experience in the past. Past militarized disputes, crises, or rivalries can very heavily influence present relations between states by pushing them towards either more cooperative or more conflictual direction. Here the prominent body of literature is the one devoted to enduring rivalries (Diehl and Goertz, 2001, Hensel, 1999, Hensel, 2001), with its main finding that most conflict occurs between long-time rival countries.

The rivalry perspective on international war and other types of conflict represents a rather dramatic shift in both methodology and theory. Scholars that do not focus on rivalries usually use disputes or wars as the basic unit of analysis. Those who study war in a rivalry context introduce an important historical dimension into the analysis. In fact, most scholars do not study wars in the rivalry context. The focus on enduring rivalries is a relatively recent one and has just recently begun to spread beyond just a small number of conflict scholars.

Why should we, then take context into consideration when studying conflict? Why do I think that literature devoted to the relationship between geography, history and conflict should attract much more scholarly attention? Perhaps the best answer to this question can be given by looking at benefits that the conflict literature can accrue from incorporating context into our analyses.

First of all states and other actors involved in conflict, whether international or domestic, do not exist in vacuum. All of states are surrounded by geography, all of them carry with them their past history. This fact alone represents a sufficient reason for why in analyzing conflict we cannot disregard the notion of context. Secondly, the usage of the notion of context can bring to the study of conflict a very important methodological benefit. Due to an enormous complexity of the world that we live in, numerous levels of analysis have been employed in order to elucidate our understanding of factors standing behind conflict. Thus, for a long time the concept of the level of analysis has occupied a extremely important place in the general discussion of world politics as a discipline. Scholars have tried to explain patterns of conflict by focusing on divergent levels of explanation. Causes ranging from the structure of the international system, interactions between contending nation-states, or characteristics of individual decision-makers have been explored. The notion of context provides us with a solution to this problem, due to its ability to link historical, individual, nation-state, and world systems together (Goertz, 1992).

Thirdly, the idea of conflict also provides a bridge for incorporating to the study of conflict many interesting and highly relevant theories from other disciplines, such as geography, psychology, economy, and sociology (Sprout and Sprout, 1969). Insights from all of these disciplines may very well help us solve puzzles of international and domestic conflict.

The notion of context constitutes a useful and important construct that can add numerous insights to the study of conflict, both domestic and international. Geography and history should always constitute indispensable elements of our scientific inquires regarding conflict. The body of literature, which studies the relationship between conflict and context has produced numerous valuable findings. These findings are related to the impact of both contextual factors mentioned in this essay, namely history and geography. Stemming from this body of literature is the “territory and conflict” literature, the “war-weariness hypothesis” research, and the “rivalries” research. All of them have significantly increased our understanding of conflict. Numerous “contextual” issues, however, remain unexplored. These exist plentiful other ways, in which geography, history, and other types of context can determine severity, occurrence of international and domestic violence. Because of the novel findings that this literature has produced I assert that the link between milieu and conflict should be pursued much further. Obviously context matters. States when engaging in acts of violence do not exist in vacuum, there are embedded in their geographical surroundings and in their past history. Perhaps more attention from scholars would lead us to discover new crucial facts about conflict.

Concluding thoughts

The study of conflict constitutes an ever-changing amassment of scientific knowledge. Some areas of conflict research are vibrant and continuously produce novel insights into the dynamic of domestic and international acts of violence. The research devoted to the link between conflict and context is such a vibrant and healthy branch of the conflict tree. Other areas of research due to their inherent weaknesses are often times destined for decline. Such is body of the research devoted to arms races. Lack of solid theoretical grounds and never-ending methodological quarrels have contributed to the present pessimistic state of this body of literature. It is always discouraging to see an area of scientific inquiry stifle and decline. But, unless more theoretically productive steps are taken, the arms races literature ought to be allowed to die on the vine. It is always encouraging to see a research agenda flourish. Due to its productiveness and novel findings, the literature on context ought to attract even more attention. Change in the study of conflict is, overall, beneficial. That is how we might find one day the answer to the question: “What causes conflict?”

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ANSWER #2:

Two Literatures in Conflict:

Power Transition Theory (a rising star)

Power-transition theory has been one of the most successful research programs in International Relations in that the diligent work of the students of Organski produced a sound theory that has been empirically supported in different contexts in several studies. Indeed, in his analysis of the current state of IR theory, Midlarsky (2000) considers power transition one of the two "mature" IR theories along with the democratic peace theory.

Organski (1958) and his followers challenged the realist view on the anarchical nature of the international system. Organski describes a hierarchical international system in which established patterns and international orders enforced by the dominant power exist despite the absence of formal rules. He labels these ordered patterns the "status quo" (p. 325). And evaluations of the status quo are one of the primary determinants of state behavior in the power-transition approach.

Another central element in power-transition theory is the importance of *relative power*. Indeed, Organski originally developed his theory as a rival to the dominant balance-of-power theory. Power-transition theorists argued that war was most likely when the powers of the dominant state and the challenger are the closest. Thus, *power parity* is more war-conducive than preponderance. Therefore, for example, power-transition theorists disagree with the classical deterrence theory which argues that secure second strike capacities create international stability: "symmetry among nuclear arsenals combined with conventional parity sets the stage for not only war, but also nuclear war," (Kugler and Lemke 2000, 147). Yet as power-transitionists themselves admit, status quo evaluations are found to be more important empirically than is power parity in anticipating major (Kim 1991, 1992) as well as minor (Lemke and Werner 1996, Lemke 2002) power wars.

King, Keohane, and Verba posits that those theories that are put forward as applying to everything and everywhere are either presented in a "tautological manner" (in which case they are neither true or false) or in a way that allows "empirical disconfirmation" (in which case we will find that they make incorrect predictions) (1994, 103). They go on to argue that most useful social science theories are valid under particular conditions (for instance, interstate relations where the use of force is not at issue) or in particular settings (for instance, in developed but not in less developed countries) (ibid). Indeed, this is where the main virtue of power-transition theory lies. Power transition theory elaborated the power-conflict relationship by introducing the *dissatisfaction* condition. Contrary to the dominant realist claim which argued that power inevitably leads to conflict, power transition scholars argued that power leads to conflict only when it is coupled with dissatisfaction with the status quo: "should a satisfied state undergo a power transition and catch up with the dominant power, there is little or no expectation of war," (Kugler and Lemke 2000, 133). Several empirical analysis (such as Kim and Morrow 1992; Lemke and Werner 1996, Lemke 2002) supported the power-transition arguments and thus power transition theory helped us better understand many "no conflict" cases which were considered "deviant" cases from a traditional realist point of view.

The importance of "dissatisfaction" for international conflict is later recognized by scholars from outside the power-transition literature as well. In his attempt to explain the creation of "zones of peace" in the Third World, Kacowicz (1998) identifies satisfaction with the territorial status quo as the most important factor in the creation of zones of peace. Rousseau *et al* (1998) also find a powerful relationship between 'satisfaction with the status quo' and crisis behavior.

Operationalizing it as “favoring the maintenance of the current borders” in territorial disputes and as “not actively seeking to overthrow another regime” in anti-regime disputes, they find that a change in status from dissatisfied to satisfied decreases the predicted probability of using major force from 34% to 5% and increase the predicted probability of never using force from 27 % to 72 % (1998, 524).

Another merit of power transition research program is that it sheds light to some other theories of international relations as well. Lemke and Reed (1996), for example, argue that the prominent “democratic peace” can be understood as a “subset” of a larger “satisfied peace” anticipated by power transition theorists. According to power-transition theory, democratic peace is actually a natural result of the democratic countries’ satisfaction with both the global system and the hegemon that leads it. Democracies rarely fight each other because “they have little over which to fight,” (1996, 147).

Power-transition theory has an important limitation, however. Like most other system-level theories, a limitation of power transition theory is that it deals with certain types of interstate conflict (great-power wars or hegemonic wars) and this limits its realm of relevance. Power transition theorists deal with specific types of relations among a highly confined set of states.[1] Given the influence of major powers on the international system, a theory of international conflict can be excused for starting from the major powers; yet an *international* conflict theory is not supposed to stop at major powers, because minor powers constitute the majority of states and

[1] In fact, Lemke’s (2002) study stands out as an exception to the major power bias of systemic theories. In his book, Lemke attempts to apply power transition theory to the hegemonic wars at sub-systemic (regional) level. However, even in this study the unit of analysis is not *all* interstate wars, but only the wars that occur in a hegemonic rivalry context, which comprise only a minority of wars among minor powers.

account for the bulk portion of interstate conflicts. Thus, a limitation of power transition research program is that it does not address an important portion of interstate conflict.[2]

Despite it limitation regarding the realm of relevance, given its theoretical vigor and wide empirical support as well as shedding light to some other theories of international conflict, I believe that power-transition theory deserve a central place among the theories of international conflict and additional attention of the students of IR.

Enduring Rivalries and IR: (a literature with tautologies)

The 'rivalry' approach to the militarized conflicts can be regarded as analyses of the historical contexts of interstate conflicts. As the two famous scholars of international rivalry put it, "at the heart of our project and the rivalry approach lies the claim that one cannot understand disputes, crises, and war without considering the rivalry context," (Diehl and Goertz 2000, 69). Therefore, Diehl and Goertz criticize the war understanding of the Correlates of War Project on the grounds that it analyzes conflicts "as if they were independent of one another, and generally without regard to the history or future prospects of the rivalry," (70).

The rivalry literature classifies rivalries into three groups: isolated, proto, and enduring. They distinguish among these three types of rivalries primarily by their duration and dispute occurrence. Isolated rivalries involve one or two disputes in a period of ten years or less. Proto-rivalries are defined as those rivalries that generate three to five disputes in a fifteen year period. Enduring rivalries are those rivalries that involve six disputes or more and last for at least twenty years (Berkovich and Diehl 1997, 308). Because of the relative conflict intensity in enduring

[2] Kugler and Lemke admit this limitation in their review of power transition literature for the second edition of *Handbook of War Studies*: "Technically, power transition research only makes predictions about dyads that include the dominant power, and thus tests of power transition's war hypotheses have a very limited empirical domain indeed," (2000, 134).

rivalries, most scholars of international rivalry have focused on the conflict in the context of enduring rivalries.

I believe in the importance of analyzing the history of conflict-prone dyads. I agree with Diehl and Goertz that the war-proneness of certain dyads presents a theoretical and empirical puzzle. However, I cannot overcome my reservations regarding the current conceptualization of "enduring rivalry". Because enduring rivalries are currently operationalized according to the conflict densities between states, some of the findings of the enduring rivalry literature turn out to be tautological, which trivializes their importance. Diehl and Goertz argue that enduring rivalries have such special characteristics as "dispute proneness," "war proneness," "increasing territorial changes," and "violent territorial changes (2000, 58). However, it seems to me that all these characteristics are natural outcomes of the higher probability of their occurrence due to the fact that enduring rivalries have higher numbers of conflicts. I specifically cannot understand the wisdom behind such statements as "we expect that a large number of militarized international disputes will occur in the context of an enduring rivalry," (ibid) and "a series of results indicate that enduring rivalries account for almost half of all militarized disputes," (Goertz and Diehl 2000, 222). Both naturally follow from the way the rivalry literature categorizes rivalries.

An important question that goes unanswered in the current enduring rivalry literature is whether the causes that lead two states into a militarized rivalry are different from the causes that lead two states into any type of militarized relationship. If not, then we can view enduring rivalries as "repeated conflicts between the same two states" and this would render some of emphasis put on enduring rivalries redundant. The enduring rivalry literature has yet to demonstrate what is special with enduring rivalries. A starting point might be to find out if the special characteristics cited above apply to individual conflicts within enduring rivalries. This would suggest that the context of rivalry makes conflicts within an enduring rivalry substantively different than the isolated conflicts between non-rival states.

A related literature which is affected by the problems of the enduring rivalry approach is the conflict management literature. Since most studies in the conflict management literature use enduring rivalries as their unit of analysis, critiques against the enduring rivalry approach becomes relevant for some aspects of the conflict management literature as well. For example, Bercovitch and Diehl find that 53 percent of the enduring rivalries received "at least one" mediation attempt, whereas only 45 percent of the proto rivalries and 10 percent of the isolated rivalries received that mediation (1997, 311). They then interpret this as "comforting and logical in that the international community directs its attention to the most serious problems and the ones that reappear on the international security agenda," (ibid). However, application of the "at least one mediation attempt" criterion onto the enduring rivalry plane creates problems.[3] If we are to stick to the enduring rivalries sample, "average mediation per dispute" might be a better indicator for the density of mediations then "at least one mediation" indicator. A rivalry with 7 disputes is by definition more likely to attract mediation than a rivalry with 2 disputes; so far as international community's focus on international conflicts is concerned, a more fruitful question would be which rivalry receives more international mediation when mediation attempts are weighted with the total number of disputes in a rivalry.

Another problem in the conflict management literature arises from drawing conclusions from the analysis of enduring rivalries only. Andersen *et al*. (2001), for example, find that enduring

[3] If we think that each individual dispute has a base probability of attracting international conflict management, then because enduring rivalries include 5 or more disputes more than the isolated rivalries, it is very natural and logical that rivalries with more disputes end up with higher percentages of mediation involvement. To make it more concrete, if we assume that the base probability of involvement of an international mediation attempt in an interstate dispute is 10 percent (the percentage of international mediation attempts for isolated rivalries), in that case for an enduring rivalry with, say, 7 disputes the probability of involving at least one international mediation attempt would be 52 percent ($1-.9^7$); and this is roughly what Bercovitch and Diehl find in their studies.

rivalries are quite resistant to influences that produce changes in their dynamics, and they present this finding as a support for Diehl and Goertz's "punctuated equilibrium model". Yet definition of enduring rivalries implies that for a rivalry to become an enduring one some mediation failure is necessary anyway. The exclusion of lower level rivalries (isolated and proto) from Anderson et al.'s study undermines the value of their finding. Maybe some proto rivalries were potential enduring rivalries but somehow did not evolve into an enduring rivalry due to some mediation-related factors. Indeed, Bercovitch and Diehl (1997) find that mediation has a significant moderating effect only within proto-rivalries.

I should note that William Thompson (2001) offers an alternative approach to the conceptualization of rivalry. His approach emphasizes perceptions, rather than disputes, which requires a great deal of interpretation of the "political histories of individual state's foreign policy activities" by the students of IR (567). Thompson does not set any minimal duration in advance either. Thus, his is a qualitative approach to the international rivalry concept. Although I view Thompson's approach superior to the current conceptualization of rivalry in terms of escaping endogeneity and tautology, his approach brings forward its own problems. As Thompson himself admits, it is more labor-intensive, more subjective, and less replicable than the rivalry conceptualization that centers on dispute occurrence and duration. Thus, I am not sure if Thompson's approach solves more problems than it creates.

Mostly due to the embedded endogeneity in the conceptualization of enduring rivalry and the resulting tautology in some of its finding, enduring rivalry literature has not helped me much to better understand international conflict. Therefore, unless the enduring rivalry literature centers around less endogenous and more productive conceptualizations of international rivalry, I see it no harm to the discipline in letting the current enduring rivalry literature die on the vine.

ANSWER #3:

In this essay I select two areas within conflict literature, one that requires more attention and another that deserves less. I follow these selections with justifications for my choices. For increased scrutiny I choose the area of issues, and for a decrease in effort I find the study of system structure to be less beneficial.

Issues

I define issues as specific causes or catalysts that lead to the outbreak of conflicts, including wars. Issues are not only the sparks that cause wars to ignite, but short of overwhelming military power, they also influence escalation and where and how these clashes stop. I believe issues to be the key to the onset and resolution of conflict.

The field of issues is relatively new. Since the mid 1990s it has received more attention than previously was the case, but most of that attention has concentrated on *territorial* disputes, most likely because historically it has been one of the more contentious areas, and probably also because it is easier to get data on this issue than most others. While disputes over territory is a very important topic to investigate, there is great potential in expanding the literature to all issues to determine their saliencies and ways that disputes over given issues might be resolved.

Until the last decade or so, the study of issues had yet to be seriously explored. For a number of years various scholars advocated that works on political and conflictual matters should include an analysis of the issues that generate or underpin conflict (Diehl 1992; Mansbach and Vasquez 1981). Even so, it wasn't until the mid 1990s that researchers began to move in that direction, and only in the last few years has the practice become somewhat prevalent.

Claims, militarized conflicts, and war, which are points along the same continuum, occur because of an underlying reason, which has been defined as "the stakes over which two parties contend" (Holsti 1991). An issue is the catalyst that causes the initial disagreement that can lead to higher levels of conflict and even war. Without understanding these specific causes, I contend that we cannot effectively address the prevention or termination of potential or resultant conflicts.

To begin with, there is a host of potential issues that conflict can develop from; Holsti (1991) identifies over two dozen of them. They range from tangibles like territory, to rights such as air and water navigation, to emotional matters that include ethnicity and religion, to security concerns and national liberation. Other scholars have recognized additional potential points of contention. Klare (2001) believes ownership and control of natural resources can lead to conflict, and in his Issue Correlates of War (ICOW) data set, Hensel (2001) introduces river and maritime claims.

Issues tend to blend together in many disputes, making their identification and resolution more difficult. Vasquez concludes in *The War Puzzle* (1993: 41) that wars can start for a number of reasons, therefore attempts to assign only a single cause to a conflict may in many cases be difficult. For example: was the Iraqi invasion of Kuwait in 1990 an attempt to eliminate Iraq's substantial financial debt to the Kuwaitis, obtain a seaport, increase its wealth by taking the oil fields, enhance its position as the regional hegemon, or some or all of the above? Failure to recognize and deal with the potential of multiple causes leaves a hole in conflict literature.

Specific issues and their saliences have much to do not only with the onset of conflict, but also with escalation. Diehl (1992) concurs, arguing that researchers must take these into account in order to explain onsets and escalations of conflict, and concludes that foreign policy behavior varies by issue area, and that states are more willing to fight for issues they regard as important. Goertz and Diehl (1992) posit that the salience of an issue does not have to be the same for both sides. They propose that the use of different measures for the two sides will provide a clearer picture because not all salience is shared. Thus, they acknowledge the complexity of the effects of issues, and the importance of the study of issues. An example of their point is the 1962 Cuban missile crisis. It took place literally at the door step of the United States, but was on the other side of the world from the Soviet Union. President Kennedy was resolved to attack the missile bases if another solution could not be found (Allison 1971: 228). Kruschev recognized this and knew the existance of missiles in Cuba was a more salient matter for the Americans than the Soviets. Hence the crisis ended. To ignore the salience aspect is to miss the significance of the outcome.

Not all issues are the same, nor do they produce the same type of results. In an early look at issues, Hensel (1996) shows that territorial disputes produce different forms of conflict behavior than do less salient issues. An implication is that more in-depth studies of issues should provide us with new information about the onset and escalation of conflict.

Continuing with territorial issues, Huth (2000: 96) argues that salience is not the only reason these disputes escalate. He posits other potential explanations: (1) the high utility placed on controlling the territory; (2) the ease for leaders to mobilize domestic support for territory; (3) appropriateness of military force to achieve goals (territory is easily identifiable both as a desired goal and as an attained objective); and (4) the propensity of authoritarian leaders to dispute territory. I realize Huth was writing an article about territory, but some or all of those reasons could also be used to expand the study and knowledge of other issues. For instance, (2) and (3) could be applied to ethnic and religious disputes and (1) and (3) go with disputes over restricted air routes (i.e., the Berlin air corridors during the Cold War) and waterways (Bosporus Strait and Sea of Marmara separating the Black Sea from the Mediterranean).

Huth maintains that testable implications of territorial conflict theory provide “a fruitful agenda for future research in which scholars examine the different causal pathways by which territorial disputes lead to international conflict” (2000: 85). I see no reason why this concept should be limited to territory. Territory has provided us with a considerable amount of information in various studies, and most likely will continue to do so, but we should not become myopic where issues are concerned. There are many others, each with its own characteristics.

Referring back to Huth’s four reasons for territorial dispute escalation (or in my view, issue dispute escalation), (1) and (3) would apply to resource wars as predicted by Klare (2001). Up to this point there has not been much to observe in regards to the resource issue (the 1990 Iraqi invasion of Kuwait could be argued to be at least partially a resource war), but as oil, natural gas and other minerals become more scarce, the probability of such wars increases.

Mitchell and Prins (1999) find evidence that the underlying issues of disputes may have a lot to do with whether or not a disagreement becomes a war. One of the implications from their discovery is that the issues democracies compete over are less contentious than those that arise in non-democratic dyads. The democratic peace theory hypothesizes and supports the tenet that democracies seldom, if ever, go to war with each other. In evaluating the issues that do arise between democracies, Mitchell and Prins find that in the post World War II period these often involve fishing, maritime boundaries and resources of the sea. The two researchers are unable to determine the extent to which these issues are contentious due to the limitation of their data, but they suspect that many such claims are settled peacefully. Additional study may be able to tell us if that is so and, more importantly, why.

Adding to the complexities of the issues debate is that they continue to be relevant even after a conflict has begun. Greig (2001) posits that the nature of an issue impacts the prospect of reaching a settlement through mediation. He finds that territorial issues are more likely to result in a mediation agreement than are non-territorial issues. This is in tandem with Hensel's (2001) finding that territorial issues have a low likelihood of being referred to binding forms of settlement attempts. Greig also surmises that because territory is a divisible good rather than a winner-take-all item, this increases the likelihood of agreement between territorial disputants, though he provides no actual evidence to support this.

Andersen et al. (2001) contribute that some issues make dispute settlements more difficult than do others. Working with enduring rivalries, they find that territorial disputes are consistently more dangerous than other contentious issues. According to their research, the implications are that different issues have different effects on decisions about whether and how to use military force and on the outcomes of settlement attempts.

This corpus suggests that most of what we know about issues comes from studies that are either about or include territory. Diehl (1992) believes that probably has much to do with data limitations. I propose (working paper to be presented in Chicago in April) that territorial claims are less likely to be referred to third party settlement attempts, but river and maritime claims are more likely. This indicates that there are differences in the effects of issues, but more importantly is an example of the kind of comparisons that can be made with the right data.

New data sets are needed in order to provide this "right data." There are a number of conflict data sets, but few have much to offer in the way of issues. The PRIO data set (Strand et al. 2003) has an incompatibility (issues) category for each conflict, but it is limited to only two, territory and government, and provides little additional information on the specifics of either. Diehl sees the construction of data sets as controlling the questions that can be asked.

The ICOW data set (introduced by Hensel in 2001 and still under construction) is being created specifically to evaluate the effects of issues. It should enable us to learn more about them, but for at least the next few years it will incorporate only three: territory, rivers, and maritime. Although there are clearly more than three in the world of conflict, these three will, no doubt, have much to teach us. If it is our goal to discover the secret of anticipating, preventing and terminating war, I submit we must understand the underlying causes and their effects on the international community. We have a good start on territorial studies; from them we have learned the importance of issues. However, as I indicated above, it is time to widen the scope and include others.

The Study of System Structure

Selecting a literature within conflict that ought to be allowed to die on the vine is much more difficult than choosing one that should be studied more intently. I have trouble accepting the premise that a certain category of information is useless, or that there is nothing else we can learn from it. I prefer to begin this section by reframing the requirement to selecting a literature area that perhaps has the least to offer in the future and so deserves fewer resources than others. I say this simply because one never knows when a sizable nugget may be found in an old mine.

Having said that, I turn my attention to the study of the structure of the international system. I contend that this aspect of conflict, which is a substantial part of neorealism, has provided most of what it is capable of providing to our knowledge base. In the remaining part of this essay I explain why I have arrived at that conclusion. System structure is a part of a narrow system-centric theory. I see it as having already made most of its potential contributions, and believe it to be too narrowly focused to provide much more useful information.

To begin with, this strand of neorealism is important and useful. Waltz (1979) sees the anarchic international order and the distribution of power as significant influences on international relations, and I do not disagree. His balance of power concept has developed into its own theory. Though it does not seem to be accepted to the extent it once was, having been challenged by transition theory, it has generated much research and debate. However, Waltz's contention that all states behave pretty much alike because of the anarchic system, the so called "billiard ball effect" where only size of the ball and the shape of the table matter, is over-simplistic and much too restrictive to fully understand the underlying causes and complexities of IR in general and conflict in particular.

I suggest there are many things that influence state action regarding conflict besides the structure of the international system. These should not be underestimated in determining causes for and resolutions of conflict. As I demonstrated in the first part of this essay, issues are both a complex and highly important influence, yet the system structure approach makes no allowances for them.

The concern about issues leads directly to another area that neorealism's system structure totally excludes: preferences. Domestic pressures from within a state can influence leaders as to the type or extent of action they take. Moravcsik (1997) asserts that state preferences are not fixed (i.e., survival) as Waltz would declare, but change based on the preferences of the individuals and groups in a state's society. State behavior is therefore determined not by the structure of the international system but by the configuration of interdependent state preferences, usually based upon costs and benefits. Groups that exert such pressures include supporters and competitors of the national leadership, segments of a state's economy, to include labor and capital, and other special interest groups such as environmentalists and religious organizations.

Internal pressures are likely to be more intense in democratic societies, but even in autocracies preferences of the ruling elite can guide or influence a state's actions. These pressures are real and often substantial, but are ignored by studies of the system structure.

Institutions have become important in preventing or settling conflict. Organizations such as the United Nations, the European Union, and NATO give nations places to discuss and attempt to resolve problems. While any state has the right to walk away from such organizations at any time, it seldom happens, and most states seem to make efforts to remain in "good standing" with these organizations. Keohane and Martin (1995) explain that institutions can make cooperation easier and more effective. The United Nations is certainly not perfect, but would any responsible government really want to eliminate it? But once again, the system-centric approach does not recognize the roles played by institutions, except that they are allowed to exists only if the powerful states choose.

Leaders of states face great complexities in trying to lead their countries, both from domestic and international perspectives. Adherence to the system structure disregards this crucial aspect of determining a state's position. Putnam's (1988) two-level game demonstrates my point. His concept likens international negotiations to simultaneous board games and ties together domestic and international politics. The leader has to make the best arrangement he can for his country, but he also has to sell that arrangement to his domestic supporters in order for the deal to be accepted and thus have a chance to work. As before, this complexity is irrelevant when the world is viewed through the structure of the system.

Governments react to different problems through different lenses. Allison (1971) provides three examples, those being a rational actor/state centric model, an organizational model, and a model featuring interactions between a number of key players in the government.

Allison believes that the action taken and outcomes achieved depend on which model is used to arrive at a decision. System structure theory makes no allowance for this.

System structure also excludes what is by most standards a very important aspect of international conflict: the democratic peace. This phenomenon has demonstrated that democracies seldom, if ever, go to war with each other. The natural conclusion is that there is something that causes democracies not to attack each other. Since, according to theory about system structure, all countries act based on the international system structure and not peculiarities of domestic governments, logic dictates that either the democratic peace does not exist or no states should be going to war with each other.

I do not conclude that study of the system structure has no value. However, I suggest that it is so narrow and excludes so much, that most of what may be gleaned from it already has been.

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Section 2 (Conflict), question 2

Question: When it began, the study of conflict was driven by political activism. Today, scholars are driven by substance, theory and/or methods. How has this change affected what or how we study? Has its overall impact been good, bad or negligible? What, if anything, can or should be done to reverse it?

By now, I have been a graduate student for nearly three years. Many of my experiences during these last few years have helped prepare me for the job market that awaits. Among them, one of the most valuable was the opportunity to fill the student's seat on a job search. First, I watched as the committee winnowed a pool of one hundred or more qualified applicants down to a dozen. Then, I listened as those dozen were discussed and analyzed, and winnowed once more to a list of four. It became clear that networks are valuable: When I hit the job market, who I know and what they think of me may matter as much as the work I have done or purport to do.

Of all the valuable insights I gained participating in a job search, one in particular struck a chord. On the table was a list of six names; the goal was to choose four to interview. It came up that one candidate had taken a year off during his/her dissertation work, then returned, finished and entered the market. Though I had this information already, it hadn't struck me as particularly important; after all, it isn't unheard of for a graduate student to take time off, is it? The committee's concern was that s/he had lost "the fire in his/her belly". The fear, it seemed, was that a promising young career might sputter and die if the investigator lacked the burning desire to pursue his/her work. Whether the fire burned for normative reasons, or for substance, theory or method was never discussed.

This anecdote may generalize from the individual case to the study of conflict in general: For lack of a more appropriate, succinct or accurate phrase, academic inquiry is driven by belly-fire. When the modern scientific study of conflict began in the 1960s, committed peace activists held the reins. Today, scholars are more interested in the substance, theory, or methods of their work than in pursuing normative goals. Despite this change, scholars today have no less fire in their bellies than had those scholars a half century ago. It is the fire, not the kindling, that drives scientific progress. The more intrigued a scholar is by her work, the more aggressively and attentively she pursues that work. Work that is pursued aggressively and attentively is likely to yield more careful and accurate scientific insight than work developed in pursuit of tenure, a pay raise, or something equally uninspired. This is why scholars—particularly successful scholars—choose topics of genuine interest.

So long as our work is driven by fire, we needn't ask what stokes that fire; the change from a normative motivation to academic motivations is undeniable, but is inconsequential for the scientific study of conflict. What we study and how we study are independent of why we study, but endogenous to whether we care. Ultimately, research programs live and die by the belly-fire of investigators. And this, I argue, is what leads a research program to progress or degenerate.

Two shifts in the study of international conflict

After the First World War, several scholars began major efforts to understand the causes of war (e.g., Sorokin 1937, Wright 1942). In the 1960s, with World War II fresh in their minds, social scientists revisited that work with a mind to picking up where it left off. At the University of Michigan scholars, established the *Journal of Conflict Resolution* and a center of the same name; 'peace science' was born. As Singer (1976:119-20) recalls, the development of centers, journals, research projects and training programs was driven by "a very strong ethical commitment to the reduction of elimination of large-scale violence from the human scene".

Today, that normative drive is largely absent. In our core seminars we learn that norms lead to biases, and in our research design courses we learn the dangers of bias. To apply terms like ‘good’ and ‘bad’ to our research opens us to criticisms like ‘data mining’. Instead, we are interested in the substance, theory and/or methods that we use. I submit as evidence Susanne Hoeber Rudolph’s 2004 APSA Presidential Address (2005:5), which traces how American ideology has shaped “the very concepts and methods we use to acquire knowledge about…society and politics”.

Why we study international conflict has clearly changed. Once we studied conflict to end it; now, we study conflict to understand it. A second major shift has been in what we study and how we study it. However, as I argue briefly, these changes are independent of the changing motivations for our work.

While there is no question that how we study has changed since the 1960s, that change is independent of our altered purpose. Instead, it is a function of scientific progress: As time progresses, our methods and models mature. We increasingly borrow from other disciplines (e.g., economics, biology), adding to the set of resources at our disposal. In the same vein, there is no doubt that what we study has changed. Again, though, that change is independent of a move away from normative inspiration. Rather, it is a function of passing time: As history unfolds and our world changes, old phenomena become less relevant and new questions are raised. Thus we are less interested now in nuclear deterrence than the bipolar Cold War environment led us to be, and more interested in the internal conflict that has been more prevalent than either interstate or extrastate conflict since the end of World War II (Gleditsch et al. 2002:623).

Although the questions we ask and the answers we fashion are different now than they were fifty years ago, those differences have little to do with the shift from political activism to scientific inquiry. Instead, we study the things around us that we find interesting. As time passes we avail ourselves of new and improving methods of inquiry. The shift from normatively driven research to scientifically driven work has not affected our ability to understand international conflict. Instead, our insights are limited by the extent to which the fire burns in the bellies of scholars.

Belly-fire and the study of international conflict

I argue that the value of a research program at any point in time may be judged not by its investigators' motivations, but by the zeal they bring to their work. To support that claim, I review two very different programs: the geographic context of conflict and arms races. Each began in the 1970s, driven by scholars with fire in their bellies. Since then, the geographic context of conflict has been driven by a small group of investigators with a genuine passion for their work. As a result, it has developed into a promising and progressive research program. On the other hand, the empirical arms races literature has been engaged and then dismissed by a cadre of researchers. As a consequence of this lack of interest (fire), the empirical arms races literature has failed to engage its central puzzle. The literature is nonproductive and lacks an obvious logic; its ability to contribute to our understanding of conflict is questionable.

Geography as a context for conflict

Over the past thirty years, many scholars have observed that contiguity breeds conflict (e.g., Gleditsch and Singer 1975, Starr and Most 1983, 1985, Gleditsch and Ward 2001, Gleditsch 2002). This observation raised a new question: What makes states willing to fight, or, what explains the conflict-proneness of contiguous dyads? One body of work has suggested that states fight over issues, and especially over territory. This 'issues approach' is first discussed in Diehl (1992) and further developed by Vasquez (1995). It begins by recognizing that contiguous dyads are the most likely to engage in armed conflict. In Siverson and Starr's classic framework, this is because proximity provides the opportunity for war, while disputes over contentious issues provide the willingness to fight. "This means that not all issues are equally likely to give rise to war" (Diehl 1992:281). Instead, different issues influence conflict propensity differently.

Of all the issues that can give rise to war, territorial issues are most likely to do so (Diehl and Goertz 1988:287; Goertz and Diehl 1992, Huth 2000). This is so because territory is the most salient of all contentious issues. As Hensel (1998:1-2) explains, "territory is widely seen as a type of issue that is especially salient to decision makers…[and therefore] territorial issues produce very different forms of conflict behavior than less salient issues".

At least three explanations have been offered for the particular salience of territory. Vasquez (1995:283) turns to inherent biology: Human beings have a territorial tendency "to occupy and, if necessary, defend territory". Hensel (1996) offers two explanations, one physical and one psychological: Physically, territory contains natural resources, houses populations, and provides security and national defense. Psychologically, it embodies national identity and cohesion, national honor, and speaks to a state's reputation. For example, despite its apparent lack of physical resources or anything tangibly valuable, Argentina's claim over the Malvinas is seen by the claimant as a matter of national honor, and thus it persists. In sum, if countries fight over issues, and territory is the most salient issue of all, then "conflict over territory should be different from conflict over other issues" (Hensel 1998:6-7).

Like all research programs, this one is vulnerable to criticism. In this case, although the theory linking territory to conflict is intuitive and well developed, it has not been properly tested in the literature. As the discussion above highlights, the study of geographic contexts for conflict rests on three key claims: Territory is the most salient of all contentious issues. As an issue becomes more salient, countries are increasingly likely to fight over it, and elements of that fighting will be different (e.g., more fierce). Therefore, conflict over territory should be qualitatively different (e.g., more fierce) than conflict over other issues. The crucial point to note from the three keys is that issue salience, not territory, is the engine of territorial conflict**.** In other words, territory is a subset of contentious issues, which may be ranked according to salience. It is the salience, not the territory, which accounts for particularities and regularities in conflict. It follows that any theory of changing salience will supersede territory in the Lakatosian sense: it will be able to explain changing conflict patterns over all contentious issues, rather than simply territory. If the study of territory and conflict is to remain a useful research program, then, it must begin to account for variations in salience across issues.

I assert that this decades-old research program has never properly tested its central claim. For example, Hensel (1998) shows that where two countries fight over territory, the conflict will be more severe, more likely to end in decisive outcome, and more likely to recur than conflict over other types of issues. so territory is an important covariate of conflict severity, outcomes and recurrence. He does not show, though, that territory is the most salient of all issues, and thus that salience accounts for observed empirical regularities. My charge is a serious one, and if these scholars lacked a fire in their bellies they might turn their attention to more promising avenues of research. However, because its investigators are driven by a fire to understand it, the program can overcome its weaknesses and contribute to our understanding of world politics. Consider, for example, Hensel's 2001 paper. In it, he recognizes and accepts that the theory has not been properly tested; further, he introduces a new dataset, the Issue Correlates of War (iCOW), designed to measure the value—salience—of territory.

Because its researchers care deeply about their subject matter, the geographic contexts research program is able to surpass its weakness and help us understand conflict. If these scholars lacked the fire in their bellies, they might instead abandon this work. There is, of course, a precedent for such behavior. As I argue below, researchers investigating arms races were faced with a central puzzle they had trouble explaining. Rather than flame the fire and hurdle over that trouble, they turned to a debate on empirics and eventually redirected their attentions elsewhere.

Arms races

After Richardson's (1960) seminal study of arms races, Wallace (1979) initiated an empirical inquiry into the phenomenon. Wallace (1979:6) defines arms races as "runaway accelerations in which the pattern of normal arms competition is transformed into a runaway arms race through some combination of domestic, diplomatic and strategic pressures [characterized by] a sharp acceleration (significant increase in the rate of increase) in military capability". He wonders whether serious disputes between nations engaged in an arms race have a greater probability of leading to war than those disputes between nations exhibiting normal patterns of military competition. Evidence supports the affirmative response. In fact, Wallace (1979:14) finds that in great power disputes between 1816 and 1965, "a high arms race score for a pair of nations correctly predicts the outbreak of war 23 out of 28 times, and conversely, a low score correctly predicts the nonescalation of a dispute 68 out of 71 time".

The puzzle with respect to arms races, then, is explaining why some (but not all) arms races lead to war. Scholars have recognized this riddle, noting that "arms races simply increase the probability of war and do not ensure that war will occur instantly" (Vasquez 2000:400; also Sample 1997). However, rather than engaging the puzzle, the program has unfolded as an unsatisfying debate about empirics. As Diehl and Crescenzi (1998) observe, contributions to the literature make just noticeable differences or just publishable differences, and fail to advance our understanding of arms races.

Using different data and different operationalizations than Wallace, Diehl (1983, 1985) finds only a weak relationship between arming and dispute escalation. Diehl and Kingston (1987) test the propensity of major powers undergoing military buildups to be involved in disputes on all major power nations from 1816 to 1976, and find that "measured nominally or intervally, military buildups have yet to have a demonstrable link with the onset of military conflict" (Diehl and Kingston 1987:808). They also use dyads to analyze whether mutual military buildups influence whether two nations become involved in a dispute with one another, and find no significant relationship. Most recently, Sample (1997, 1998) argues that while the relationship is never as strong as Wallace suggested, there is a significant positive link between arming and dispute escalation given complete bivariate testing. Specifically, given mutual military buildup, most of the cases that do not immediately escalate to war either do so within five years or are cases involving nuclear weapons. Thus, while arms racing is not always an immediate sufficient condition for dispute escalation to war, it does clearly increase the probability of war in the prenuclear era.

There is evidence that these scholars are aware of the theoretical and empirical weaknesses of this research program. Diehl and Kingston (1987:811) write that their series of negative findings have "important theoretical and policy implications for the study of military buildups". Sample (1997, 1998) observes that a causal explanation is missing from the literature. However, Diehl and Kingston do not detail the implications to which they refer, and Sample does not offer the causal story she says is missing. If their interest in this area were genuine—if the fire burned in their bellies—I would expect them to attack the puzzle enthusiastically. Unlike the geographic contexts literature, then, it seems the arms races phenomenon never lit or flamed academic belly-fire. As a result, the program has ground to a halt, and it is unclear that it helps us understand conflict.

When the modern scientific study of conflict began in the 1960s, political activism lit a fire in scholarly bellies. Since that time, the activist spirit has waned and scholars have turned to substance, theory and method to fan the flames of inquiry. However, that shift has not affected the questions we study or the way we study them. Although those things have changed, they have done so as a function of changing history, new experiences and methodological maturity. Instead, the shift from activism to academic interest has led to a new kind of kindling for 'the fire in our bellies'. Where the fire burns strong, as in the geographic contexts research program, we accumulate knowledge. Where the fire wanes, as in the empirical arms races literature, we do not. As long as the fire burns in each of us as individual scholars, we needn't worry about the shift from normative to scientific motivations, and we needn't do anything to reverse it. Instead, we can and should realize that genuine progress comes from genuine interest, and conduct our research accordingly.

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Section 3 (IPE), question 2

Question: What explanations have been offered to explain why developing states today are more likely to liberalize trade than were developing states thirty years ago? Which is/are the most compelling?

Over the last three decades, countries across the globe have adopted freer trade policies in what Rodrik (1994) has called a 'rush to free trade'. In 1994, the Uruguay Round of multilateral trade negotiations under the General Agreement on Tariffs and Trade (GATT) concluded successfully. This advanced trade liberalization among many developed countries. For example, the average tariff for the developed countries was reduced from 6.3% to 3.8% (WTO 1996:31). In addition, it advanced trade liberalization between the developed and developing countries. GATT also helped reduce non-tariff barriers to free trade (NTBs), both by reducing trade barriers in many areas of key interest to less-developed countries, such as textiles and agriculture and by bringing many developing countries under the rules and auspices of the World Trade Organization. Without question, there has been a far-reaching increase in the liberalization of trade barriers around the world (Rodrik 1994).

One set of countries that has been particularly likely to liberalize is the developing world. With the exception of Cuba and to some extent Brazil, most of Latin America has wholeheartedly embraced deregulation and free markets since the 1970s. Many other lesser-developed and developing countries have also chosen to unilaterally liberalize their trade policies (e.g.: Ghana, India, Mexico, Poland and Turkey). Why are developing states today more apt to liberalize than were developing states thirty years ago? Explanations for this phenomenon have proceeded along two lines. Some scholars have focused on the domestic determinants of trade liberalization, while others have concentrated on the relevance of the international environment.

Below, I briefly review the development of the academic literature on international trade. Scholarly interest began among economists, but quickly spread to students of political science.

Then, I narrow the focus to those theories that can explain why developing states have been increasingly likely to liberalize. Domestic influences include domestic actors (policymakers, interest groups and individuals) and are grounded in the observation that most development involves democratization. International pressure may come from informal or formal institutions. Ultimately, international explanations for trade are more responsive to critics than their domestic counterparts. At the same time, they are more able to explain new facts like the current boom in trade liberalization among developing states. As a consequence, the theories of liberalization that I find most compelling are those focused on new or unique international institutions.

How we came to care

Some of the first academic studies of politics and international trade were by economists (e.g., Hirschman 1945, Cooper 1960, Kindleberger 1970). These scholars were largely interested in the composition and direction of trade flows, and the welfare effects of trade. They asked questions about why particular countries import and export certain goods or services to certain other countries (Milner 2002). This early work inspired a large body of research; in fact, much work on international trade addresses this question. For example, the Heckscher-Ohlin theorem builds on Ricardo's (1996[1878]) theory of comparative advantage to explain trade flows. The theorem holds that a capital-abundant country will export from capital-intensive industries., while labor-abundant countries will import those goods. Simultaneously, capital-abundant countries will import labor-intensive goods, and labor-abundant countries will import capital-intensive goods (Ohlin 1933, Leamer 1995). Economists have also been interested in trade barriers, concluding that "free trade is the best policy for most countries most of the time" (Milner 2002:448).

In terms of trade liberalization, then, the puzzle for economists has been why countries inevitably employ some protectionist policies, when free trade is economically superior: why *don't* countries liberalize? Political scientists began investigating trade shortly after the economists (Gilpin 1975, Krasner 1976, Keohane and Nye 1977). Like recent economists, these scientists focused on the issue of protectionism. Unlike economists, though, political scientists tend to see protectionism as the norm (Milner 2002). Therefore, the issue for political scientists is why a country would ever liberalize its trade policies or adopt free trade: why *do* countries liberalize? These scholars have focused their work primarily on explaining the nature of protectionist and free trade policies, and changes in them over time. In other words, scholars have attempted to address the puzzle of why some (but not all) states adopt or relax protectionist policies some (but not all) of the time. In particular, they have focused on two sets of factors in explaining trade liberalization: internal influences including the preferences of domestic actors and the effects of domestic political institutions, and the role of the international system for affecting domestic choices. Below, I discuss this work as it applies to trade liberalization in developing states. Scholars of international trade have focused on democratization and institutions, each of which is particularly relevant in the context of developing states.

Trade Liberalization in Developing States

Internal Influences on Economic Reform

Since 1974, a new wave of democracy has swept the globe (Huntington 1991). The introduction of newly democratic voices and new democratic institutions has increased the possibility of successful trade liberalization in many ways. Some scholars have argued that democratic countries may be less likely to pursue protectionist policies. Wintrobe (1998) claims that autocratic countries will be more rent seeking, and that protection is merely one form of rent seeking. Mansfield, Milner and Rosendorff (2000) demonstrate that democratic pairs of states tend to be less protectionist and more likely to sign trade liberalizing agreements than autocratic dyads. And several studies have shown that democratic dyads tend to have much more open trade relations than other dyads (Dixon and Moon 1992, Mansfield et al 2000). These influences of democracy on economic reform are the starting point for domestic theories of trade liberalization.

Broadly, those theories fall into two categories: they either focus on how domestic groups influence free trade, or they concentrate on how domestic institutions affect trade policy. In part, scholars have suggested that domestic political regimes, bureaucratic and political party institutions, and the interests of politicians, technocrats and key constituent groups all influence the implementation of economic reforms (liberalization) in developing states (Haggard and Webb 1994).

domestic groups

In a new democracy, three groups may affect economic reform. As before the transition, policymakers exert influence over the economy. Bauer, Pool and Dexter (1972) use surveys to show that constituents rarely have strong preferences about trade policy, and that even when they do they don't often communicate those preferences to their representatives. Instead, they find that trade policy mostly depends on the personal preferences and ideas of politicians. Similarly, Baldwin (1986) and Goldstein (1988) each argue that it is policymakers' ideas about trade policy that matter the most, and that ideational factors matter more than material ones for determining policymaker preferences. This work suggests that changes in policymakers' ideas with respect to trade policy may play a large role in explaining trade policy outcomes, including trade liberalization in developing states.

In addition to policymakers, newly democratic interest groups and individuals may be suddenly able to pressure the government. With respect to interest groups, scholars have looked at how particular characteristics make an industry not only more likely to desire protection but also more capable of inducing policymakers to provide it (Baldwin 1986, Caves 1976, Marvel and Ray 1983, Pincus 1975, Ray 1981, Trefler 1993). These analyses "tend to demonstrate that in advanced industrial countries low-skill, labor-intensive industries with high and rising import penetration are frequently associated with high protection" (Milner 2002:451). Many have also shown that export-oriented industries and multinationals tend to favor freer trade and be associated with less protectionism (e.g., Milner 1988). In states where these industries exert appreciable power, they may prevent trade liberalization. For democratizing countries, then, the ability to move beyond low-skill, labor-intensive industries—the ability to industrialize—could well be linked to trade liberalization.

Like interest groups, individuals in democratizing states suddenly find their political voices. Some scholars assume that individual voters derive preferences from their role as consumers. Since consumers gain from free trade (e.g., through more price competition), they should favor it (Grossman and Helpman 1994).

Institutions

Policymakers, groups and individuals likely have preferences about trade policy. In order for those preferences to influence policy, though, they must aggregate across individuals. Some scholars have observed that different institutions aggregate and constrain preferences differently, and used that to explain the emergence of distinct trade policies across space and time. In democratizing states, where democratic institutions are just being established, understanding how those institutions affect trade policy would allow us to understand a little better how and why states liberalize (or do not).

One possibility is that institutions affect which domestic groups have the most access and voice in policymaking. For example, different institutions may give different interest groups greater access to policymakers, making their demands harder to resist (Baldwin 1986; Destler 1986; Goldstein 1993; Haggard 1988). Alternatively, institutions that delegate authority may insulate policymakers from the demands of interest groups, giving them greater leeway to set policy according to their own preferences: "In every successful reform effort, politicians delegated decision making authority to units within the government that were insulated from routine bureaucratic processes, from legislative and interest group pressures, and even from executive pressure" (Haggard and Webb 1994: 13). Policymakers should be the most insulated in countries with large electoral districts and PR systems (Rogowski 1987).

In addition to understanding which institutions inhibit or promote liberalization, scholars have studied the effects of regime transitions for economic reform, including the role of democratization for trade liberalization. In this vein, Alesina (1994) finds that political transitions from authoritarian rule to democracy provide an opportunity for incoming and outgoing political elites to make informal bargains that minimize societal polarization and effective interest group pressures for new government spending. When it occurs, this situation is highly conducive to economic reform.

Haggard and Webb (1994) study the effect of the status quo on economic reforms in new democracies. They find that new democracies that inherit chaos macroeconomically (e.g., Poland) do best when they implement quick, radical economic reforms during their political honeymoons. They do less well when they institute gradual reforms, since opposing interests within the state can erode these away. On the other hand, new democracies that inherit an economy already undergoing reform (e.g., Chile) can best consolidate stabilization through political pacts between incoming and outgoing political elites. Inherited administrative capacity and insulated decision making increase the likelihood of successful reform. During the transition to a liberal economy, labor almost always finds itself at a disadvantage. Institutions and leaders that can control labor (e.g., by developing corporatist arrangements) can therefore facilitate economic reform. Ultimately, in order for reforms to succeed in the long run they must become popular with the interests that could otherwise oppose them, or be packaged with other, more popular programs.

External Influences on Economic Reform

Scholars have argued that institutions aggregate and constrain individual preferences. By the same logic, the international system aggregates and constrains state preferences. The collective outcomes that result are evident in both informal and formal international institutions. With respect to the former, hegemonic stability suggests that a unipolar system should encourage global free trade. With respect to the latter, neoliberal institutionalism argues that formal institutions facilitate free trade by reducing uncertainty and transaction costs.

With the end of the Cold War in 1989, the United States claimed its place as the sole, dominant world power. Neorealist scholars, who argue that systemic forces are crucial in shaping state behavior, see this unipolar environment as uniquely conductive to global free trade (Waltz 1979). Hegemonic stability theory suggests that free trade among states will be most likely when a hegemon is present and creates international military and economic stability (Gowa 1994; Gilpin 1987; Lake 1988; Krasner 1976).

Like neorealists, neoliberals have shed light on the impact that institutions can have on international trade (Mansfield and Milner 1999, Keohane 1984). Specifically, formal institutions impact state behavior by providing information, thereby reducing uncertainty and transaction costs (Keohane 1984). Recently, economists have also begun integrating international institutions into models of trade flows (e.g., Rose 2004). These ideas have support in practice: In the context of trade, institutional frameworks like GATT, the WTO and the World Bank are able to facilitate an open, multilateral trading system.

What's compelling?

As is so often the case, each explanation for trade liberalization has its weaknesses. However, critiques of domestic explanations are much more damaging than critiques of international influence. Theories of interest group influence, for example, have a number of weaknesses related to the idea of aggregation. They cannot explain how preferences are aggregated from various firms to the sector as a unit, or from the various industries to the policymakers. These theories are therefore silent if firms in an industry are divided over trade policy, or if some industries are in favor of liberalization and others support it. Broadly, theories of societal pressures are unable to address when and how political actors mitigate interest group pressures and preferences.

Domestic theories of economic reform have also been criticized on empirical grounds. As I noted above, individuals in democratizing states may pressure policymakers to adopt particular policies. Since individuals derive preferences from their roles as consumers, and since consumers gain from free trade, individual voters should favor free trade. However, some studies contradict this expectation, suggesting that voters may derive preferences from at least one other area. Mayer (1984) finds that trade policy follows from the median voter's preferences, which in turn follow from that voter's factor endowments. The more well endowed s/he is in the factor used intensively for producing import-competing goods, the more protectionist s/he will be (Mayer 1984). And Scheve and Slaughter (1998) argue that the preferences of individual voters depend on how trade affects their assets. In particular, individuals in regions with a high concentration of import-competing industries should be more favorable to protection, because as imports rise economic activity in the region falls, causing their housing assets to fall in value. Whether and why individual voters favor free trade, and how that matters for policy output, remains largely unelucidated.

Like domestic theories of reform, international theories are open to criticism. Unlike domestic theories, though, the weaknesses in the institutional research program may be more accurately portrayed as areas of developing research. The neorealist claim that hegemony is a necessary condition for global free trade has been challenged both theoretically and empirically (e.g., Keohane 1998; Lake 1993; Conybeare 1984). One particularly powerful criticism has been the challenge that a hegemon is neither necessary nor sufficient for an open trading system (e.g., Krasner 1976, Mansfield 1994). For example, Snidal (1985) argues that small numbers of powerful countries could maintain an open system as well as a single hegemon could. As Milner (2002:455) notes, "in light of these results, the theory has been modified as scholars examine more closely the dynamics of interaction among countries in the trading system" (e.g., Russett 1985, Strange 1987, Haggard 1995).

In the institutional context, then, scholars of economic reform have used existing weaknesses to raise and address new questions. By Lakatosian (1978) standards, this program is progressing. This is the first reason that I find international explanations for trade liberalization in developing states compelling. The second reason begins with the observation that democratization has swept the globe before, but that many international institutions are just beginning to grow teeth.

If we believe that there has been a third wave of democratization since 1974, we must also recognize the first two waves. If democratic transition provides the major impetus for trade liberalization, then we should have seen economic reform in the states that democratized in the early 1800s and just after World War II (Huntington 1991). Further, the process should have been similar to the reform in Latin America and elsewhere over the past thirty years. In terms of research design we might say that in the pseudo-experiment that is our world, the democratization treatment has been applied before: This time, the outcome is different.

On the other hand, the current state of international organizations is a totally new treatment condition. International explanations for trade liberalization point to some unique phenomena. As I noted in the introduction to this essay, for example, the successful 1994 conclusion of the Uruguay Round of trade negotiations under the GATT reduced tariffs and NTBs across the world. This promoted free trade and directly facilitated trade liberalization in the developing world. For these reasons—because they are well equipped to respond to criticism and because they can explain the novelty of economic reform in developing states since the 1970s—I find institutional explanations for trade liberalization in developing states most compelling. As international organizations continue to develop and strengthen, these theories expect the current liberalization trend to continue, and so do I.

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