A Little Bit Louder Now: Signaling, Interests, and the Liberal Peace.

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This dissertation examines competing explanations for the liberal peace (the idea that trade between states creates pacific relations between those states) and uses statistical analysis to argue that traditional "interest-based" explanations (Russett and Oneal, 2001) do not explain aspects of interstate conflicts as well as more recent "signaling-based" explanations (Fearon, 1994; Gartzke et. al., 2001). Specifically, the dissertation examines the probabilities for interstate conflict between states when a measure that examines whether one state is economically dependent on the other is included. The results suggest that more dependent states are more likely to both initiate interstate conflicts and use higher levels of hostility in ongoing conflicts than non-dependent states. These results, I argue, are consistent with signaling-based explanations for the liberal peace, and wholly inconsistent with interest-based explanations. Additionally, by examining the actions of dependent states, the dissertation looks at one set of boundary conditions (Mansfield and Pollins, 2001) in establishing the limits for the liberal peace. Next, the dissertation argues that the two dominant data sets used to study the liberal peace (Gleditsch, 2002; Barbieri, 2002) are both flawed: the Gleditsch data by biased GDP measures and the Barbieri by missing values. Finally, the existence of the liberal peace itself is called into question: results of empirical tests indicate that higher amounts of trade with specific trading partners lead states to greater probability of initiating interstate conflict and using higher levels of hostility in ensuing interstate conflict with that trading partner.

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PREFACE

This dissertation has taken a long time to finish. Among other things, that means the list of people I need to thank is quite long. Micheal Aleprete, S. Lauel Weldon, David Manel, and Crissy Carr all contributed thoughtful help and moral support along the way. Aaron Hoffman provided the crucial intellectual idea that allowed the project to get underway. He is also responsible for keeping alive the "cape" idea. My family stuck with me through this long road, and never once asked "how's the paper." You can't know how grateful I am for that. Dr. Tony Walters helped with some of the IPE theory, and navigating the University bureaucracy. Dr. Davis Bobrow asked the crucial questions at the end which whittled away much that was irrelevant, and allowed the focus of the dissertation to remain on the Liberal Peace. Dr. Joe Hagan read significant portions of other drafts and his enthusiasm for the project helped me work through some long nights. No dissertation is possible without a committed Chair, and that is certainly the case here. Dr. Chuck Gochman worked tirelessly to read and re-read drafts, and push the dissertation to be better and better. All the praise for this project should go to these people, while any errors or omissions are solely my own fault.

Special thanks must go out to Jamie Jacobs, who gave me not only intellectual and moral support, but carried me through those times I wanted to hang it up. I don't deserve her.

Finally, my father did not live to see the end of this. While he never edited any part, I always wrote with him in the back of my mind, trying to achieve the level of clarity and smoothness that seemed so effortless to him in his stories and columns. I don't think I achieved that, but it was always (and remains) a worthy goal. I would very much have liked to have put the finished dissertation in his hand.   I think he would have been proud. Thus, the dissertation is dedicated to Charles Newland McCorkle Hauser (1929 – 2005).

1.0 Chapter One: Introduction

1.1 Introduction

The study of economic ties and interstate conflict is filled with "common" lore. It is widely assumed that the expansion of trade and economic interdependence will affect political-military relations in a positive way, increasing cooperation through the operation of the liberal peace (the idea that economic interdependence brings pacific security relations). However, scholars have also argued that increased interaction between states creates more chances for friction, which can lead to more interstate conflict and less cooperation. The effect of economic ties on conflict is particularly important for an accurate understanding of interstate relations in the current era of globalization. While globalization is generally viewed as an economic phenomenon, there are security implications. Will increasing economic interactions among states, and greater economic dependence between states, promote conflict between those states - or reduce it? Given the trend toward greater globalization and increasing economic ties for many states in the twenty-first century, this is a central question for academics and policy-makers. It is also the central question of this dissertation. More precisely, the objective of this dissertation is to disentangle the competing logics that argue for the security benefits of economic interaction, using various data sets and measures of economic dependence.

Our understanding of why states engage in militarized conflict is limited. The results of theoretical and empirical examinations of the causes of conflicts and wars have been mixed, with some false starts and promising elements. Perhaps the most significant empirical finding has been that democratic states do not wage war on one another. Over the course of about a decade and a half of empirical studies, that finding has been

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replicated more often than not, to the point where most social scientists, and policy makers, are willing to believe in the validity of the finding. In turn, the field has moved into related areas of inquiry, the most prominent being the investigation of the so called "liberal peace," which has been interpreted as the idea that economic relations between states can have a independent and pacific effect, beyond the democratic peace. It is in this discussion about the liberal peace that conflict studies links itself to the larger debate on globalization.

There is an expanding body of research under the rubric of liberal peace studies that has theorized and empirically examined the relationships between economics and interstate conflict. While these studies are important first steps in an understanding of the relationship between international economics and conflict, they do not present convincing evidence of a cause and effect relationship that can be translated into policy actions. Mansfield and Pollins (2001) make this precise point, arguing that "[t]oo little attention has been focused on identifying the causal mechanisms underlying any such relationship." (Mansfield and Pollins, 2001, pg. 835).

This dissertation focuses primarily on that question, making a more specific theoretical and empirical test of the causal linkage between economic relations and interstate conflict. While previous research has generally found a correlational link between increased trade and decreased involvement by interstate dyads in interstate conflict (see Russett et al., 2001; Oneal et al., 2003 for the most recent of a number of empirical tests; see Barbieri, 2002, 1995 for a somewhat dissenting view), all the articles have centered around dyadic tests of the association of trade with interstate conflict. The specific causal linkages that connect increased trade to decreased conflict, while

hypothesized briefly in all the articles, have not been tested themselves. In short, we have some good evidence that trade accompanies decreased interstate conflict, but we have little idea why that occurs; the studies have given us strong correlational evidence, but no direct test of causation. This dissertation rectifies that missing link.

This is critical from both a policy perspective and a theoretical one. From a policy perspective, if we can see how the links are made from economics (trade being considered a proxy to general economic relations) to policy outputs (decreased interstate conflict), we can seek to reproduce and reinforce those specific linkages, rather than a more broad-based, shotgun approach of just seeking more trade for states. From a theoretical perspective, if our goal is to understand the path that leads to war (or paths that lead away from war), then knowing the specific link advances our understandings of interstate conflict, and may shed light on other debates on interstate conflict (e.g., the role of interests versus signaling in a general theory of interstate conflict).

1.2 The Liberal Peace

The liberal peace is a theory that has close ties to liberal economists over several centuries. The basic theory posits a positive relationship between states' economic ties[1] and a reduction of tensions, disputes, conflicts and wars between those states. The theoretical and philosophical basis for the liberal peace traces the same path as the democratic peace - back through Montesquieu's The Spirit of the Laws (1648) and Immanuel Kant and his argument about The Perpetual Peace (1795). Past those philosophical roots, the intellectual and historical heritage of the liberal peace traces a

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[1] Historically, researchers and theorists have generally discussed trade as the motive for the pacific relations between states. It is only recently, as financial relations between states have risen to the same volume as trade relations that theorists have begun to discuss dependence and interdependence in terms other than trade relations. This is discussed further in the section on research design in Chapter 4.

path through the early liberal internationalists writing in the United Kingdom (Cobden and Angell) to the capital-L Liberals following the First World War (Wilson) through to the free trade zones and discussions of globalization sweeping both politics and academic international relations today. While a more detailed literature review begins in Chapter 2, suffice it to say that there is a long and distinguished line of academics and politicians that has argued and implemented ideas related to the liberal peace.

The central argument (the "interest-based" logic) for the operation of the liberal peace is that increases in national wealth due to international trade make national decisions in favor of interstate conflict or war prohibitively expensive, as war or conflict leads inevitably to a decline in trade (and, hence, national wealth). If this is true, rational leaders should prefer nonmilitary means for resolving interstate disputes so as to avoid the loss of national economic gains from trade. Most research into the liberal peace assumes that decisions are the product of unitary rational actors, and results are presented with this assumption in mind. Hence, in the "interest-based" explanation of the liberal peace, trade restrains conflict. This was a simple, logical argument: trade benefited states, so it was in their interests to avoid actions that would end those benefits. This interest-based logic seemed so self-evident as the causal connection between trade and pacific relations that it went basically unchallenged until recently.

It is this model of the liberal peace that most researchers have used in the studies discussed in Chapter 3. Those studies have generally found a significant statistical connection between trade and pacific relations between states. Not all researchers have found this, but most have. However, none of those studies (whatever their results) has

attempted to directly test the unitary-rational actor, interest-based argument (that first appeared in Polachek, 1980).

As the critics discussed in the second half of Chapter 3 note, there are alternate theoretical paths by which interests can motivate states into the liberal peace. There exists an interest-group logic that argues that state decision-making is not the product of a unitary rational actor, but the consequence of competing domestic interests (Papayoanou, 1996; Jungblut, 1999). The state is merely an arena within which competing interests struggle, and overall state policy reflects the balance of power among these interests. Thus, state restraint with respect to interstate conflict is not a reflection of national well-being, but of the narrow interests of the dominant domestic group or coalition of groups pursing the same goals. Another alternative interest-based logic (not contradictory to interest-groups, and potentially complimentary to that theory) is the "trading state" (Rosecrance, 1986) hypothesis that argues that the economic relationship between any pair of states is not sufficient for an understanding of the political actions of either state. States have relationships (economic, political, etc.) with many states. When involved in potential or actual interstate conflict with another state, factors specific to that state-to-state relationship affect policy choice, but so do factors associated with the totality of a state's foreign policies. In other words, neither states nor pairs of states exist in a vacuum, and actions states take may be based more on overall state policy goals than specific relationship with another state. This argument is similar to the model in the studies discussed above (leaders choose based on state interests), but rather than leaders examining the potential loss due to interstate conflict with one specific economic partner, leaders look to the potential economic loss with most trading partners and the political

damage associated with that loss in trust as the motivating interest in pursuing a nonviolent conflict resolution strategy.

All of these competing interest-based theories of the liberal peace share a common root: economic interests, derived from economic exchange with a partner state, influence the foreign policy of the state towards more pacific relations. The theories do not agree upon the path by which those economic interests influence the security policy of the state, but this shared notion of interests unites these theories.

Recently, a significantly different explanation of the liberal peace has been put forth. The alternative explanation (Gartzke, et al., 1999) draws on rational choice literature (Fearon, 1994) for an explanation of the liberal peace. The key is signaling. Fearon argues that if states engaged in a dispute knew which disputant possessed greater capability and/or resolve (in other words, who would win if it came to a fight) there would always exist a bargaining outcome that all involved states would prefer to the uncertainties and costs of conflict. There is, of course, no way for states to actually know the objective truths – all they can do is signal back and forth to each other. Where states are better able (more credibly) to demonstrate their ability to prevail in an armed conflict, there should be less conflict. This is the trap, however, as it is clear that states have an incentive to bluff, and exaggerate in their signals the resolve or capability they do have in order to convince the opposing state to back away (and, hence, gain more). The opposing state knows this, however, and even signals that are intended to (truthfully) indicate superior capability or resolve in order to facilitate bargaining and avoid conflict are discounted. The misunderstanding that one state has (or both have) about the opponent's willingness and/or capability leads to a miscalculation that lets one state (or both) go

forward into war, where the miscalculation is revealed with high costs – and, often, terrible destruction.

This logical template has been used by rational choice theorists in the study of war to examine a number of different issues. Fearon (1995) discusses audience costs: states where leaders pay high audience costs for foreign policy actions (presumed to be democracies, but this is untested) have a more credible signal, and hence prevent wars. In a related vein, Schultz (1998) discusses signaling in terms of democracies and the democratic peace: democracies, because of their open structures, can more credibly signal, and hence have more success in preventing wars.

Gartzke, et. al. (1999) argues this same logic applies to trading relations, and argues that this signaling logic serves as the basis for the liberal peace. Trade creates valuable relationships, and actions by states that could result in the loss of these relationships serve as ways to make statements by that state more credible than in cases where states have no trading relations and are not risking the loss of anything valuable. Thus, a liberal peace exists because trading allows credible signals, and credible signals allow resolutions of disagreements before they become violent.

This is a significantly different basis for explanation of why trade results in more pacific relations than the philosophical and historical logic offered by most other theorists using the "interest-based" logic described above. Where the "interest-based" logic of the liberal peace sees states as willing to undertake interstate conflict (or war) if the balance of risk and reward are suitable, and the liberal peace operates by creating real (economic) incentives that push the states away from risky conflict (states avoid interstate conflicts because interstate conflicts reduce the benefits of economic exchange), the "signaling-

based" logic of the liberal peace assumes that states always want to avoid conflict, and that economic relations serve as ways to make the exchange of signals more credible, and increase the likelihood of avoiding interstate conflict. These are two very different explanations of conflict, and very different theories (applied to the liberal peace) of how economic relations restrain interstate conflict. The problem is that both theories explain the empirical results: in general, pairs of states that trade at higher volumes seem to experience fewer incidents of interstate conflict. The interest-based logic explains this result by arguing that trade generates mutual benefits for these states, and they avoid actions, like interstate conflicts, that would diminish these benefits. The signaling-based logic argues that trade creates opportunities for states to signal more credibly than states without trade, and hence trading states are able to avoid conflict more often than non-trading ones.

Therefore, while there remains some question of the existence of the liberal peace (Barbieri, 1995, 2002), an important issue that remains unanswered by all the empirical studies that support the liberal peace is an fundamental question: which theory of the liberal peace (interests or signaling) better describes the logic that drives states to avoid interstate conflict? What *causes* the liberal peace? The nature of the empirical evidence in favor of the liberal peace does not answer this question: it only provides evidence that there is a reduction in interstate violence between trading partners versus non-trading pairs of states. The empirical tests to date provide peace researchers no ability to discern whether states are driven by interest-based considerations or signaling-based ones. This is a critical omission in any attempt to understand the liberal peace.

1.3 Central and Auxiliary Questions of the Dissertation

A recent review of the liberal peace literature (Mansfield and Pollins, 2001) offers three fundamental criticisms of the efforts of researchers to this point:

First, there is a pressing need to improve the theoretical basis of claims about the effects of interdependence on the use of force. Although considerable progress has been made over the past 20 years in moving beyond broad speculation about whether interdependence influences conflict, too little attention has been focused on identifying the causal mechanisms underlying any such relationship. Second, more effort should be devoted to identifying the boundary conditions of the effects of economic exchange on belligerency…. We need a better understanding of how such factors condition the relationship between interdependence and hostilities. Third, too little attention has been paid to the definition and measurement of interdependence and conflict (Mansfield and Pollins, 2001, 835).

Their primary criticism is that the causal connection between trade and conflict is under-theorized and under-researched, as I argued above. The article goes on to list myriad possibilities of domestic interest groups, states, firms, and other actors that might be influenced by the benefits of international economic exchange and in turn influence the decision-making of a state (as I discussed above). Mansfield and Pollins (2001) have called for research to examine which of these interest-based explanations is more accurate, but have ignored the more fundamental question of which logic (interest versus signaling) is driving the liberal peace.

Therefore, while the Mansfield and Pollins (2001) quote is a good general summary of the auxiliary issues that the liberal peace has left unresolved, it is flawed because it leaves out the essential question of causality: interests or signaling? As I noted above, there is an alternative conception of the liberal peace divorced from interests that explains the same empirical phenomenon: the Fearon (1994)/Gartzke et al. (1999) signaling-based explanation. A necessary question before one looks to how interests are operating within the state is to ask if interests themselves are relevant. If interests are

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secondary (or even less important, as the signaling-logic would argue), and signals are more significant, then nuanced and detailed research to unpack the state's relation to internal economic and political actors is unnecessary and misleading, and will add only "noise" to our understanding of the liberal peace.

This is the fundamental question of the dissertation. The first step to answering this dominant criticism of the liberal peace research program (that we do not understand the causal connection between trade and pacific relations) is to try to determine which of the competing logics that support the liberal peace is more empirically accurate. This dissertation examines the two logics of the liberal peace and tests circumstances where the two logics predict different outcomes for the behavior of states, thus putting the two logics to a direct test against one another. While no attempt is made to directly measure signals (sent or received) or interests (at the individual, group, or state level), the circumstances of the tests look to evidence of how states act if they are governed by either the signaling-based logic or the interest-based logic of the liberal peace. Based on this evidence, the field can move forward to more detailed (nuanced) examinations of interests or signaling.

This dissertation has secondary goals: other aspects of the liberal peace, as Mansfield and Pollins (2001) note, remain unresolved questions. Overall, there are three related goals that the dissertation seeks to fulfill:

First, and primarily, it attempts to empirically discern which, if any, causal logic underlies the liberal peace. As explained above, this is critical to a better understanding of how the liberal peace operates. This is the primary goal of the dissertation.

Second, it explores the "boundary conditions" (Mansfield and Pollins, 2001, 2003) of the liberal peace by examining the degree to which an economic relationship of dependence (or independence) affects state actions with respect to conflict initiation and conflict hostility (how "conflictual" the ensuing conflict is). That is, if, as many empirical researchers have found, trade restrains states from engaging in interstate conflict, does it do so for all types of trading relations? Does the liberal peace operate for dependent states, as well as independent states? What happens to those trading states that (overcoming the odds) become involved in hostilities? In other words, is trade universally pacific, or are their certain types of states (or certain types of trading relationships) where trade either fails to limit conflict, or even increases the odds of interstate conflict (or makes for more violent forms of conflict).

Third, and finally, the dissertation tries to sort out the consequences of using different sources of data and, secondarily, alternative measures of trade-based interdependence. Almost all liberal peace researchers use data provided by Gleditsch (2002; updated several times) to study the effects of trade on interstate conflict. The Gleditsch data set provides information on trade and GDP for almost all states in the post-1950 era. Recently, however, Barbieri (2002) has argued that the Gleditsch data contain flaws, and offers an alternative data set that is significantly less complete (in terms of states and years covered) than the Gleditsch data but (according to Barbieri) more valid (in terms of accurately reporting the economic figures). In this dissertation, I utilize both sets of data, comparing the results to determine what difference the data sets make. Additionally, because quantitative liberal peace researchers disagree about the best way to measure dependence/interdependence, I utilize two different measures of

dependence to determine how sensitive the empirical results are to the specific measures employed. If both sets of data (and both measures) produce substantially similar results, we can have added confidence in the robustness of the relationship. If they do not, the dissertation will examine why the differences exist, and how researchers should approach the controversies. These tests of alternative data sources and alternative measures are not intended as a theoretical attempt to discern which of the various methods for constructing interdependence is best. Rather it is an opportunity to empirically explore how different measures and data sets affect the findings generated by the liberal peace research community, especially since differing results have cast doubt on the overall validity of the liberal peace. As such, this part of the dissertation addresses the third criticism of the liberal peace offered by Mansfield and Pollins (2001), namely that too little attention has been paid to issues of definition and measurement.

1.4 Map of the Dissertation

The eight chapters (and one appendix) that follow address the objectives outlined above. Chapter 2 provides an overview of the theoretical and historical literature that is the basis for the interest-based and signaling-based logics that underlie the liberal peace. The first part of Chapter 3 discusses and synthesizes the empirical results of quantitative tests of the liberal peace. The remainder of Chapter 3 discusses the criticisms of the empirical tests, and the liberal peace in general. I also discuss, in greater detail, how this dissertation fits into those criticisms.

Chapter 4 presents the research design for the dissertation, and explains the logic of the test used to differentiate the interest-based from the signaling-based explanation of the liberal peace. The test involves the examination of states with specific trading

partners (not pairs of states, as has been traditional in liberal peace studies), and looks at the correlation between a given state's balance/imbalance of trade with a partner state, and the likelihood of the given state initiating an interstate conflict (or using more hostile actions in an ongoing interstate conflict) with the partner state. The interest-based logic would argue that a dependent state would never take any action that would threaten harm to itself, and thus would avoid initiating any interstate conflict, and if one began would work to limit the hostility in the conflict in order to preserve the economic relationship that is critical to the state. The signaling-based logic would argue that the dependent relationship offers opportunities for credible signals, including initiating interstate conflicts and pursuing ongoing interstate conflicts at higher levels of hostility, in order to avoid war. Chapter 4 also involves extensive discussion of the data sources, methods of variable construction, and statistical test. In particular, it describes the differences between the Gleditsch (2002) and Barbieri (1995, 2002) data, and considers measures of interdependence beyond trade.

The results of the analysis are presented in Chapters 5 through 8. Chapter 5 describes the tests of trade imbalance on interstate conflict initiation. Chapter 6 parallels Chapter 5, but looks only at the effect of trade imbalance on interstate conflict hostility. Chapter 7 introduces alternative explanations of trade and interstate conflicts, and checks to see if the results from the previous two chapters hold up when alternative variables are introduced. Finally, Chapter 8 introduces measures of economic ties most often used in liberal peace studies (as opposed to the measures of balance/imbalance used in Chapters 5 through 7) and re-examines the results and the liberal peace in general. Chapter 9 reviews the results from the previous four chapters and offers some general conclusions

about the liberal peace. Appendix 1 contains some of the detailed analysis of the differences between the two data sets (discussed above) that influence the results in Chapters 5 through 8; this analysis is critical to understanding the results, but outside the central focus of the dissertation and has been moved to the appendix to streamline the explanations in the chapters.

1.5 A Brief Review of the Results

While there are many tests, over four chapters, the general conclusions are clear-cut: the results of this study support, by a significant preponderance of evidence, the signaling-logic over the interest-based logic as the causal explanation for the liberal peace, at least in the cold war and post-cold war periods. This seems especially clear in the tests of conflict hostility, if less so in tests of conflict initiation (where results from the 1950s alone seem to drive the entire 1950 – 2000 results). The evidence is compelling: *not a single test in the post-1950 era indicates support for the interest-based logic of the liberal peace.* While the evidence in favor of signaling may be equivocal, I find absolutely no evidence that interests drive the liberal peace in the post-war and post-cold war eras. The findings for the pre-1950 era are less clear, though the preponderance of evidence supports interest-based (rather than the signaling-based) explanation of the liberal peace prior to 1950. This result is perplexing: there is nothing about the interest-based or signaling-based explanations for the liberal peace that would argue for either of them to be more (or less) valid in different temporal spans.

With respect to the "boundary conditions," the empirical tests indicate that trade does not always serve as a constraint on conflict behavior. There are circumstances where a state engaging in more unreciprocated trade with a partner (and, hence,

generating a greater and greater imbalance in trade) will make itself more likely to engage in more hostile forms of interstate conflict. The greater the imbalance of trade, and the more important the trade, the more likely the more dependent state is to both initiate an interstate conflict, and use higher levels of hostility in an ongoing interstate conflict (at least in the post-1950 era). In the same way that democratic peace researchers found that only when democratic states are paired with other democratic states is there a pacific effect, the results here argue that only when states have balanced and interdependent trade are the most pacific effects of the liberal peace maximized.

Finally, the debate over the Gleditsch (2002) versus Barbieri (1995, 2002) data is likely to continue. The tests indicated that there are serious flaws when "adjusted" GDP (GDP data adjusted for purchasing power parity) and "unadjusted" trade data are used to create measures for dependence, as Gleditsch (2002) does. However, the results also show that the Barbieri (1995, 2002) data with its sizable "missing data" component produces substantially different statistical results than the "more comprehensive" Gleditsch data. Thus, both data sets (in different ways) affect the results.

1.6 Assumptions, Limitations, and Definitions

One thing clearly needs to be said at the onset: the impact of economic interaction on the foreign policies of states, or even the interstate conflict propensity, is merely one of many influences. This dissertation does not argue that economic factors are predominant, or even important, in many circumstances. The discussion of the impact of economic interaction on a state's conflict propensity must be seen in a comparative sense. Only by introducing other independent variables in the statistical tests can we gather evidence about the relative importance of economic interaction

compared with these other variables. Thus, I am not arguing in this dissertation that

economic considerations are dominant or important for states when they engage in or

avoid interstate conflict, only that they could be important, and are part of the basket of

factors, positive and negative, that states consider when faced with initiating or

responding to interstate conflict.

Additionally, this dissertation is decidedly not a test of the micro-foundations of

how states make decisions. No attempt is made here to determine the actual thoughts and

actions of decision-makers, their advisors, or other influential individuals. The results of

the empirical tests are correlative – suggesting explanations for state actions that are

consistent with the observed results.

Finally, throughout the dissertation I make a distinction between the words

"dispute" and "conflict."   By "dispute" or "international dispute," I mean disagreement

between two or more states with respect to any issue or material object. The disputatious

parties are defined as the states and the official governments that represent the states.

Disputes between states can either be resolved (by the states, or with

mediation/intervention by a third party) or can escalate to "conflicts." By "conflict" or

"interstate conflict," I mean a dispute that was unresolved before one side or the other (or

both) has threatened, displayed or used force in an attempt to end the dispute. Interstate

conflicts are defined identically to the operational definition used for the Militarized

Interstate Dispute (MID) data developed by the Correlates of War Project. It is assumed

that all conflicts are preceded by disputes, but not all disputes become conflicts

Another assumption I am making is that while disputes are not as serious as

conflicts, neither are they "safe." Given that conflicts must arise from disputes, and wars

must arise from conflicts (such is the assumption built into the construction of the MID data; see Jones, Bremer and Singer, 1992), disputes carry the implications that they could prove very serious and costly to states. For example, if conflicts are assumed to seriously threaten trade between partners (see Chapter 3 and 4 for a longer discussion), then disputes must be assumed to be somewhat threatening to a trading relationship, and hence should be avoided, according to the logic of the liberal peace.

1.7 Final Thoughts

Fundamental to a continuation of liberal peace research is an understanding of which of the two logics – interests or signaling – drives the empirical behavior that most research observes, and is the focus of this dissertation. If, as critics of the liberal peace note, we lack an understanding of the causal nature of how trade reduces interstate conflict, the signaling-based logic adds a new dimension of complexity to more traditional interest-based explanations. Before any research attempts to discern which of the many "interest-based" explanations serve (what effect do interest-groups have? (Jungblut, 1999); how do different economic interests interact within the government? (Kehr, 1932)) as the foundation for an explanation of the liberal peace, the first step is to determine which family of explanations, interests or signaling, serves as the logical basis for the liberal peace. Other empirical studies have uncovered correlation between trade and reduced incidents of interstate conflict, but offer nothing with respect to determination of underlying process.

In a strictly academic sense, however, discussion of the liberal peace provides a background for a broader and more theoretical discussion of interstate conflict and rational choice. The signaling-based explanations for interstate conflict advanced by

Fearon (1994) argue for a fundamentally different understanding of how conflicts and wars come about between states. This dissertation explicitly tests the use of signaling in a multi-stage process of conflict and war to determine the utility that signaling has in preventing or mitigating the escalation of interstate conflict and war. While the primary purpose of this dissertation is to more fully understand the connections between international economics and interstate conflict, the results suggest something about the empirical importance of signaling in interstate conflicts and crises; and by extension, the utility of rational choice approaches.

2.0 Chapter Two: Literature Review and Interests versus Signaling Logics

2.1 Introduction

The foundations of the liberal peace thesis are rooted in the writings of mid-eighteenth century political philosophers. The following chapter (Chapter 3) describes the modern political science studies of the liberal peace; this chapter concentrates on the logics (interests and signaling) that compete to explain the liberal peace and those that reject the connection. The chapter first concentrates on theorists who posited a connection between trade and peace, then looks at those who have argued against the connection.

The primary focus of this dissertation is empirical. I seek to determine which, if any, logic of the liberal peace is most consistent with the historical record. As such, there is no new theory being offered here. However, a review of the theoretical bases for the liberal peace is necessary for understanding how the empirical tests cast light on underlying logics. Additionally, a review of the theoretical objections to the liberal peace helps set the stage for empirical results reported in Chapters 5 and 6 of the dissertation that indicate little association between trade and conflict prior to 1950, and a positive association between trade and conflict since 1950. Thus, the primary purpose of this chapter is to present the theoretical foundations of the liberal peace in order to justify the empirical tests that will be undertaken and help the reader interpret the results of those analyses.

The arguments favoring peace have evolved into two distinct explanations of the liberal peace. The first and most prominent is that peace will prevail where the benefits of trade outweigh the benefits of conflict and war, given the costs and risks associated

with conflict and war. The second is that trade represents a pathway by which states can credibly signal truthful information to one another and seek solutions that avoid the costs and risks conflict and war.

Those who reject the liberal peace vision fall into two camps. One camp, following a traditional Realist point of view, argues that there is no connection between trade and decisions for war and peace. Trade is part of "low politics," while decisions for and against interstate conflict are "high politics." High politics trumps low politics, and decisions are made without regard to lesser political issues. The other camp argues that dependent trade relations create conditions where states lose their freedom of action and, in the process, peace prevails, but a malevolent peace. Dependent states are forced to acquiesce to the demands of less-dependent states for fear of economic ruin. While not traditionally Realist, this camp does accept a Realist view on the relationship between strong and weak states in the international system: the weak do what they must, while the strong do what they wish.

Given the focus of this dissertation on testing the competing logics of the liberal peace, it might seem that discussion of the arguments against the liberal peace is out of place. Thus, a discussion of the arguments against the liberal peace may be useful from a research design perspective and for understanding all the possible outcomes of the empirical tests. In fact, the empirical results in Chapters 5, 6 and 7 for the pre-1950 era are substantially (though not entirely) consistent with the predictions of the theorists who reject the liberal peace.

Moreover, while most of the empirical tests in the dissertation are not directly comparable to other liberal peace research (due to differences in cases and differences in

trade-based variables), Chapter 8 offers tests that are more comparable to previous liberal peace research and the results call into question some of the inferences drawn from earlier empirical research.[2] Hence, the arguments of those that do not believe in the liberal peace become more relevant. For this reason, it is worthwhile to review the theoretical objections to the liberal peace.

Early political thought on trade was sparse. As Baldwin (1985) notes in his review of economic statecraft in history, most of the classical literature neglected issues of foreign policy (Baldwin cites Aristotle as an exception), focusing instead on discussions of the ideal city-state – independent and self-sufficient. Plato, for example, notes that trade may be necessary to supply that in which the city-state was not self-sufficient, but this arrangement was not ideal (Plato's Republic, trans. by Grube, 1974, 41). Such early writing made no connection between trade and peace among political entities. Development of that theoretical linkage would wait until the birth of liberalism in the European renaissance. Discussions of the foundations of the liberal peace are first found in the writings of Montesquieu in the eighteenth century.

2.2 Theoretical Arguments in Favor of the Liberal Peace: Classic Interest-Based Logic

One of the first philosophers/political economists to discuss the connection between commerce and peace was Montesquieu (1748). While his historical view was restricted to observing the incessant conflict between England and France, rather than a more global vision, he nonetheless provides the foundation of a general liberal peace

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[2] As noted in Chapter 8, these results are not a direct refutation of previous liberal peace findings as the cases and empirical tests are not precisely the same. Nonetheless, the findings do cast some doubt onto the universal applicability of liberal peace findings of other researchers (Russett and Oneal, 2001). See Chapter 8 for a complete explanation.

theory. "Commerce is a cure for the most destructive prejudices; for it is almost a general rule, that wherever we find agreeable manners, there commerce flourishes; and wherever there is commerce, there we meet with agreeable manners…Peace is the natural effect of trade. Two nations who traffic with each other become reciprocally dependent; for if one has an interest in buying, the other has an interest in selling; and thus their union is founded on their mutual necessities" (Montesquieu, 1748 [1966], pg 316). While other theorists have added to the liberal peace debate, the logic that Montesquieu uses – states have interests in trading for the positive economic good it does them – has remained the core of the liberal peace program.

The idea that trade creates a motive to avoid conflict is a powerful one. Trade, according to liberal economists, is wealth creating[3]: I have what you want, you have what I want, we exchange and we both feel and are better off. These gains are possible because of the trade: autarky could not have produced at equivalent costs the benefits of international trade. However, gains that are realized through trade are dependent on the goodwill of the trading partner[4] – trade is a two-way street and needs two willing participants. Hence, states will refrain from actions that might end the goodwill of the partner state, terminate the trade between them, and cost the state gains from that trade. If trade is suppressed or ended between states involved in interstate conflict (there is some evidence this is untrue: see the discussion below), states that gain from trade will

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[3] Clearly not all forms of economics see trade as a positive good. Economic nationalists note that trade is sometimes good, sometimes bad – it depends on both the domestic and international situations for both trading partners. Thus, overall, trade is situationally contingent. Marxist economists, including dependency scholars, would point to the negative aspects of trade for some states in some dependent economic relations. See a longer discussion later in this chapter for more details of the alternative economic arguments that trade and conflict are unrelated or harmful.

[4] There could be an exception to this statement in dependent economic relations. States that are economically (or politically – like the former Warsaw Pact was to the Soviet Union) dependent may be forced into unwilling economic relations, and in that case the trade is not the result of goodwill on both parts. See the discussion below about interstate conflict and dependent economies.

refrain from war or actions that suppress trade. This was simple logic: wars are about

gains.[5] Trade is about gains. You cannot have both trade and war. The more gains from

trade, the less a country will be willing to gamble on war. Montesquieu was the first to

clearly explicate this connection, but not the only one. Over the centuries it has been

expanded, debated, criticized, and tested numerous times. Nevertheless, the debate

returns again and again because the fundamental logic – gains from trade create interests

for states to avoid conflict – seems so eminently plausible. This "interest-based" logic

for the liberal peace, defined many centuries ago, remains the core of the dominant

research program that examines the liberal peace.

Recognize the underlying explanation for interstate conflict implicit in the logic.

States choose war; they choose war when the gains from war are likely to be greater than

the gains from any policy short of war. War is a rational choice. All wars and conflicts

are risks, and different states with different convictions about the level of risk ("risk

averse" versus "risk acceptant") will choose differently with respect to war. Nonetheless,

the underlying model here is simple: wars (and conflicts) occur when states compare the

probable outcomes of various courses of action to the costs/benefits of those courses of

action, and decide that war (for all its risks) is a worthwhile gamble. Trade, by this logic,

operates by changing a state's decision calculus: by making the war/conflict option more

expensive (the state loses the benefits from trade when the conflict begins), states that

trade are less likely to choose a war or conflict when comparing their possible policies

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[5] Of course wars are not only, or even primarily, about material gains. Wars are fought for reasons of honor (Kagan, 1995), independence, autonomy, domestic politics, or some combination of any or all of these (or more, including material gain). That being said, while material consideration may not be primary or even necessarily high on the list of issues, the liberal peace logic argues that the loss of the material gains from trade can help influence a state into a more pacific or at least less belligerent action, due to the negative costs of losing the trade with the partner. My thanks to Prof. Tony Walters for pointing out this issue.

(and the costs associated with them). This model of conflict is implicit in Montesquieu's writing, but recognizable.

Our modern sensibilities might question Montesquieu for the somewhat clouded logic he uses – it is unclear whether "manners" bring commerce or commerce brings "manners", or if the two mutually reinforce each other (hence, in a sense, create each other). This is an important distinction, as one argument against the liberal peace (see Gowa, 1994, discussed below) is that friendly nations trade (and friendly nations, by definition, do not fight), not that trade creates friendly nations. Montesquieu is bluntly clear that commerce and trade are linked, but leaves unclear this "chicken and egg" question concerning the direction of causality.[6]

As an additional criticism, Montesquieu leaves unclear whether the source for the pacifying effects of trade lie with the individual citizen or with the state itself. "But if the spirit of commerce unites nations, it does not in the same manner unite individuals. We see that in countries – Holland – where the people move only by the spirit of commerce, they make a traffic of all the humane, all the moral virtues; the most trifling things, those which humanity would demand, are there done, or there given, only for money" (Montesquieu, 1748 [1966], pg 316 – 317). Here, the argument is that while states are united by commerce, this same force does not work on an individual level. Later, however, he notes: "Happy is it for men that they are in a situation in which, though their passions prompt them to be wicked, it is, nevertheless, to their interest to be humane and virtuous" (Montesquieu, 1748 [1966], pg 366). In contrast to the previous quotation, here

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[6] Questions of causality continue to be addressed in the larger liberal peace research program. Some of the more recent analysis (Reuveny and Kang, 1996; Reuveny, 2001) generally indicates that there is a causal arrow from trade to pacific relations. Thus, while Montesquieu is not concerned with the issue, it is something the wider liberal peace research program addresses.

Montesquieu seems to be saying exactly what others during his time were saying (see the discussion of Hirschman's "The Passions and the Interests", below), that the naked self interest of man to be rich can outweigh the "passions" of man to do wicked deeds, thus bringing peace (both in a domestic and international sense) through economic exchange and activity. While not a critical part of the overall argument for Montesquieu, this distinction between individual and state (as does the discussion of "manners" and trade) can be found in recent theorizing and tests of the liberal peace (see Papayoanou, 1996, and Jungblut, 1999, below). Overall, it is clear that Montesquieu believes and argues that commerce will bring peace between states, but it is not so clear why he believes this. That said, Montesquieu is worth the time devoted to reviewing his thought, as he establishes the fundamental logic of the liberal peace and presages two modern debates relating to the liberal peace.

While there are numerous other political philosophers and early political scientists who argued for the liberal peace, they remain united in the reasoning underlying their argument. All relied on the interests of trade to overwhelm any interests of the state to make war. John Stuart Mill wrote in support of the liberal peace. There was a substantial French school of thought that argued the same position. Bastiat, Dupuit, and Dameth (see Silberner, 1957), all French political economists, made much the same argument. Given the similarities of their logics, a review of all their work is redundant. They differ with respect to details – individuals or states as the focus of the argument, for example – but those considerations have not been important to the modern liberal peace debate that still questions the ability of trade to restrain interstate conflict, and have been left aside for the moment. The "interest-based" logic, defined by Montesquieu, remained

fundamental to understanding how commerce created pacific relations for all these authors. Only a few additional writers need be singled out.

The early philosopher's basis for the liberal peace – interests in wealth overcoming motivations for war – is explored in a more recent work that helps shed light on how the original logic for the liberal peace was created. Hirschman's *The Passions and the Interests* (1977) obliquely explores the connection between trade and interstate conflict through a philosophical examination of the early political economists. His goal in discussing Montesquieu, Hume, and others is to provide context for their thoughts, and help explain the questions they asked and the answers they found. He shows how these political philosophers saw that the naked self-interest of man could be used as a powerful counter-balance to the despotic or potentially despotic actions of the government and fellow man. The passions that drive man to war, conquest, and aggression (and despotism) need to be countered by some other force in order for mankind to be able to live together in communities and nations – else, the natural savagery of man would shine through and pull down civilization's advances. Hirschman argues that these political philosophers saw that the self-interest of man to economically gain more and more (become richer and richer) could be a powerful force to counter the destructive impulses of man. Political structures (institutions, laws, constitutions) that helped man become richer and richer were possible, and served to hold back the base passions of man. Thus, interests restrain passions. As many of the philosophers at the time noted, including Montesquieu and Hume, this predominantly domestic concern (maintaining a domestic political structure that would not gravitate towards despotism) had an international relations aspect: nations could be restrained from "passionate" warlike activity by their

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"interests" in greater economic wealth through trade. This reveals the early liberal peace thinkers as not so much concerned directly with interstate war, but instead grappling with domestic concerns such as despotism, anarchy, revolution, democracy, and government. The liberal peace came about almost as an afterthought.

Writing only a few years after Montesquieu, Kant (Perpetual Peace, 1795) more specifically linked economics to peace. "The spirit of trade cannot coexist with war, and sooner or later this spirit dominates every people" (1795(1983), 125). Moreover, Kant argues that the "mechanism of man's inclinations," in other words, his greed and acquisitiveness, will help nations maintain perpetual peace. People want goods and material possessions, and economic activity is the method that people use to achieve their desires. War prevents this, and thus people will place economics over war (the same argument that Hirschman uses above). To the degree that people have control over the government, commerce can bring peace.

In the debate over where to locate the liberal peace, Kant clearly decides for the people over the state. The "Kantian Peace" (Oneal and Russett, 1999c) argument that emerged in the democratic peace debate of the 1980s and 1990s, builds explicitly on the democratic and liberal peace arguments of these eighteenth century thinkers regarding the effects of democracy, trade, and membership in international organizations on relations among states. For Kant, commerce as a means to peace was only part of a system that included, as a necessary condition, a democratic or republican form of government that gave the people some say in the affairs of state. Kant is clearly more willing to link the peace that results from trade to the demands of individual citizens, but he supports Montesquieu's overall logic – peace results from the interests that trade creates.

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The theory of these political philosophers of the seventeenth and eighteenth centuries was advocated as policy in the next century. The "Manchester School" takes its name from the group of politically active merchants in the Manchester Chamber of Commerce that sought a continued economic advantage by expanding their economic trade beyond the borders of England to other parts of Europe in mid-nineteenth century Great Britain (Redford, 1934, 64). The English textile industry was industrially strong and had saturated the markets in England itself. The group of merchants sought additional markets for their products, and saw that the best way of achieving this was the advocation of free trade by all, including England. Beginning in 1820, the Manchester Chamber of Commerce lobbied the English Government to reduce duties and to see that other states reciprocate in order to increase trade (Redford, 1934, 139.) The Chamber of Commerce and the associated merchants were not seeking greater trade to promote peace in the world; their aim was to increase their share of the global market. Peace was an added benefit that figured into their political arguments for freer and greater trade.

Richard Cobden, an English politician active in the mid-1800s and one of the founders of the Manchester School, is widely known for being an advocate of free trade for economic, political, and social reasons. He also argued that free trade would lead to peace in the world. "I have been seeking to form arrangements by which these two countries shall be united together in mutual bonds of dependence, and, I hope, of future peace" (Hirst, 1903, 342). Cobden is the first to mention the idea of "mutual dependence," which today is the concept of "interdependence," that lies at the heart of the liberal peace debate. Trade can now be thought of in terms of creating "dependencies" and peace can be assured through economic channels. Beyond this, however, the

reasoning behind why trade brings peace remains firmly rooted in the logic of Montesquieu some hundred years before. States are restrained from war through the economic gains and dependence of trade, not because war is morally wrong: "In particular, international businessmen, prospering through free trade, would oppose war as damaging of commerce…The practical basis of this argument, emphasizing profit and prosperity, will be apparent. Cobden was himself a shrewd judge of the public mind. He knew that a practical approach was more likely to attract interest than one couched in terms of abstract high moral principles…Fortunately, God had contrived that peace-keeping could be made not only morally right but also materially advantageous. Men could be led via materialism to morality" (Read, 1968, 110). Thus, people's self-interests could be harnessed to lead the world to peace.

Perhaps the most widely known work that purports to support the trade-peace hypothesis is Norman Angell's "The Great Illusion." Written just before World War I (1911) and revised just before World War II (1933), Angell set out not to show that economic interaction between states would bring peace (as many have inaccurately credited him with advocating), but instead that there are no positive economic incentives for states to invade and occupy other states – war does not pay. The two points of view (trade brings peace versus war has no economic value) are related, but not identical. Angell argues that the extent of international trade is such that there are no longer economic motives for seizing another state's territory. Whether Germany or France owns Alsace-Lorraine is irrelevant (from an economic point of view), as the inhabitants will continue to produce products, and will continue to trade with the rest of the world. Angell's argument echoes a similar logic from the United States in the mid-1980s with

respect to Japanese companies buying American businesses (movie studios, record companies, Rockefeller Center, etc.). While some Americans were horrified at the perceived loss of American economic power, most economists noted that the change of nationality of ownership had little real effect on the output and economic activity of whatever was bought. Angell makes no claims that trade will bring peace to the world (never once in his book does he make the claim), in fact arguing that wars will continue for a number of reasons (wars, he argues, happen not because of bad men acting evilly, but good men acting in the belief they are right – Angell, 1933, pg. 13). It is clear, however, that Angell sees that increased international trade is necessary for economic motives for war to be reduced (if a coveted piece of land trades with no one, then there can be some economic motivation for a state to seize it, and begin to make economic use of it). Angell would seem to be sympathetic towards the trade-peace hypothesis without ever saying so. It is interesting, however, that his work has been so consistently misrepresented as a formulation of the liberal peace idea.

The theoretical bridge between the "classical" philosophers that saw commerce as producing manners and the modern political science development of the study of the "liberal peace" is the work of Keohane and Nye (1977). While other works of the 1960s and 1970s discussed and debated the relationship between interstate conflict and international economics, Keohane and Nye's Power and Interdependence pushed hard against the dominant Realist paradigm, particularly in security studies (long thought to be where Realism was best at explanation), arguing that military power was no longer at the top of the issues agenda for states.

The theory of complex interdependence argues that multiple channels connect states (linkages exist between governments, subnational actors, and international organizations), that there is no fixed hierarchy of interests (where issues of national security and survival, as a Realist would argue, are always at the top), and finally: "Military force is not used by governments towards other governments within the region, or on the issues, when complex interdependence prevails" (Keohane and Nye, 1977 [1989], pg. 25). Keohane and Nye explicitly argue that states where complex interdependence governs their relations will not engage in the resolution of differences through military means. Moreover, they explicitly point to the flows of "money, goods, people and messages across international boundaries" (Keohane and Nye, 1977 [1989], pg 9) as the important constitutive elements that create the mutual dependence that leads to complex interdependence. Clearly, for Keohane and Nye, economic interactions are an important and fundamental part of the complex interdependence that will keep states from interstate conflict.

The focus of Keohane and Nye is on the inter-relations of states and the creation of regimes that facilitate the operations of complex interdependence, not on the degree to which greater dependence specifically reduces the probabilities of conflict between states. However, the ideas of complex interdependence resonate strongly in all the more modern (statistical based) explorations of the liberal peace.

The modern exploration of the effects of economic interdependence on interstate conflict can be traced back to an article by Polachek (1980). In this article Polachek (citing Keohane and Nye, 1977) clearly links trade to reduced interstate conflict though an economic motivation that comes directly from the logic and explanations of the

classical philosophers discussed above. "The inverse relationship between trade and conflict is not merely asserted. Instead, a logical justification using economic theory is given. The basic model assumes what is standard economic theory, namely that countries maximize their own social welfare. Obtaining the highest levels of material well-being possible does this. Given cross-country differences in technology and factor (resource) endowments, any one country can raise its social welfare by specializing in domestic production of commodities for which it has comparative advantage, and trading for commodities produced relatively less efficiently. Thus, given different technologies and factor endowments across countries, trade patterns emerge. Conflict is assumed to affect the terms of trade. Specifically, greater levels of conflict make trade more difficult. Reasons include retaliatory tariffs, quotas, embargoes, and other trade prohibitions. Conflict thus raises the costs of trade, thereby making at least one of the countries worse off (in a welfare sense). The implicit price of being hostile is the diminution of welfare associated with potential trade losses" (Polachek, 1980, 60).

This statement is one of the first to clearly put into modern economic terms the simple logic by which the traditional liberal peace operates. Using elements of simple economics (differences in factor endowments lead to comparative advantage which leads to international trade) and linking it to simple liberal theory (states seek to maximize their own social welfare – states seek to be rich), Polachek creates a modern logical justification for the liberal peace: states will seek to avoid international conflict because it interferes with states' pursuit of wealth. Trading states have an economic interest in avoiding interstate conflict.[7]

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[7] While a logical assumption, the assertion that interstate conflict will destroy or at least reduce trade between adversaries has only recently been empirically tested. The findings are contradictory, with some

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Other researchers in subsequent studies of the trade/conflict nexus have added nuances and details to this statement, but none (until recently, see below) have rejected the logic put forward here. Interest-based explanations for the liberal peace continue to be the foundation for why trade should result in decreased interstate conflict. Polachek finished the article with an empirical analysis that gives credibility to his logic (although subsequent empirical studies have gone far beyond the work here), but the article continues to be cited and discussed today because of the theoretical foundation (updating the logic of the original political philosophers to modern economic terms) that he constructs for the liberal peace.

Like Montesquieu, Polachek has no explicit model of interstate conflict. Like Montesquieu, the implicit model has rational states making decisions for or against conflict based on the welfare maximizing results of that decision: when states gain from conflict, they will engage in it, when states see that conflict is costly, they will reject it. As Polachek notes, "Thus the price of being belligerent is an implicit price that increases with the level of trade. Ceteris paribus, the greater the amount of trade, the higher the price of conflict, and the less the amount of conflict that is demanded" (Polachek, 1980, pg. 61). Economic relations between states raise the cost for a state to engage in conflict with that state. Since war and conflict, by this implicit theory, are rational decisions based on gains and losses, the higher the cost of war the less states will choose war. This

studies finding that interstate conflict significantly reduces trade, while others find there is no connection between interstate conflict and subsequent trade. If trade between states does not decrease in the presence of conflict, Polachek's reasoning (as well as the logical basis for most liberal peace theory) evaporates, unless one argues that the actual change in trade (negative or no-effect) as a result of interstate conflict is irrelevant, and the important point is that state decision-makers believe that interstate conflict will reduce trade. If decision-makers have this perception, then physical changes in trade are irrelevant for the liberal peace to operate: as long as leaders believe interstate conflict reduces trade, they will act in ways to consistent with that belief and avoid interstate conflict. The function of leaders perceptions in the debate over conflicts effects on trade is generally missing from the literature (see Li and Sacko, 2002, for example). This point is expanded below.

implicit theory of conflict differs radically from the theory of conflict used in the signaling-based explanation of the liberal peace (described below).

Some researchers have gone beyond the simple interest-based explanation of the liberal peace to argue for greater effects of interdependence and trade. Rosecrance (1986), for example, offers a more sweeping argument about the pacific effects of trade on interstate conflict, contending that the nature of international politics is changing. Rosecrance argues that the world is in the process of changing from a "Political-Military" (a Realist) world into a "Trading" world. Why is this occurring? "Between 170 and 180 states exist, and the number is growing. If contemporary nationalist and ethnic separatist movements succeed, some states … may be further subdivided into new independent states or autonomous regions. These small and even weak states will scarcely be self-reliant: increasingly they will come to depend on others for economic and even military necessities, trading or sharing responsibilities with other nations. The age of the independent, self-sufficient state will be at an end. Among such states, the method of international development sustained by trade and exchange will begin to take precedence over the traditional method of territorial expansion and war" (Rosecrance, 1986, 15). States, according to Rosecrance, are driven to develop – to grow.

This is a similar logical foundation to the interest-based explanation that serves as the centerpiece of the liberal peace. Modern, small, weak states will not have the ability (even if they wanted) to be self-reliant, and thus if these states are to develop, they must band together in economic, military, and political systems. Where the liberal peace theory is explicitly about the existence of a single economic relationship restraining the political actions of those specific states involved in that relationship, Rosecrance is

expanding that logic and arguing that states that trade as a means of welfare gains will refrain from military conflict with all other states. States that have shifted away from the Politico-Military viewpoint of the world have an incentive, with respect to all their political relations, to be more pacific and avoid interstate conflicts that harm all their economic relations.

States still try to get ahead, as they did in the Realist "Political-Military" world, but they do so by different methods – primarily trading and economic interdependence. "While trading states try to improve their position and their own domestic allocation of resources, they do so within a context of accepted interdependence. They recognize that the attempt to provide every service and fulfill every function of statehood on an independent and autonomous basis is extremely inefficient, and they prefer a situation which provides for specialization and division of labor among nations. One nation's attempt to improve its own access to products and resources, therefore, does not conflict with another state's attempt to do the same. The incentive to wage war is absent in such a system for war disrupts trade and the interdependence on which trade is based. Trading states recognize that they can do better through internal economic development sustained by a worldwide market for their goods and services than by trying to conquer and assimilate large tracts of land" (Rosecrance, 1986, 16).

Another difference between the work of Polachek and Rosecrance, on the one hand, and that of earlier theorists relates to the agent taking action. Kant, Cobden, and other classical political economists of the nineteenth century argued for the liberal peace by showing that individual's personal motivations would be to support welfare-enhancing trade over wealth-destroying war. People would see the gains from trade, and thus not be

supportive of war. The classical theorists were not as concerned with modern precise theories that looked specifically at states as actors, or individuals, and it is somewhat of a generalization to declare that they all focus on the individual over the state. For the most part, however, it is true.

Like Polachek (1980), Rosecrance shifts focus away from the citizen to the state as primary actor. States seek to develop. States seek a division of labor. States seek to share military, political, and economic burdens because they are too weak to go it alone in the international system. Moreover, Rosecrance sees states turning away from war not because war realizes no gain for states, but because states' long-term and continued growth depends on international cooperation not only in the economic realm, but also in the political. War is short term and risky, while cooperation is longer term and less chancy.

This logic of the liberal peace has a history going back several centuries. Its basis is the simple and logical idea that the concrete gains from trade outweigh the risky potential gains and sure losses from interstate conflict, and, hence, states that trade will seek to avoid acts that put that trade at risk. This logic forms the basis for the dominant research program examining the liberal peace, though it has been modified and expanded (Rosecrance, 1986).

2.3 Alternative Logics for the Liberal Peace: Signaling

Recently, a logic that challenges the traditional interest-based theory has gained traction. The "signaling-based" logic accepts the liberal peace conclusions (trade suppresses interstate conflict), but rejects the two centuries worth of logic that underlies

the liberal peace – the idea that states forgo the risky gains from conflict in favor of the guaranteed gains from trade.

Rational choice theorists have presented an alternative explanation of why interstate conflict and war occur (and, by extension, the circumstances under which conflict can be avoided). This alternative explanation centers on the idea that decision-makers find it difficult to credibly signal their resolve or capabilities when engaged in interstate disputes, as well as their intentions to abide by agreements absent a threat of force (Fearon, 1995).

Fearon notes that traditional rational[8] explanations for war fall into five camps,[9] but that a single rational explanation for war dominates the discussions of war in the literature.[10] This is the argument that states deliberately choose war over peaceful conflict resolution because they estimate that the benefits accruing to them as a result of war exceed the expected costs from engaging in war. While Fearon (1995) does not mention the liberal peace in his article, the traditional "interest-based" logic of the liberal peace clearly uses this rational explanation of war as a theoretical basis for arguing for the pacifistic effects of trade. By the traditional logic of the liberal peace, trade and other forms of economic exchange are assumed to diminish or cease with the coming of war and so would the economic benefits to the state, thus raising the costs of war. Trade

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[8] By limiting the discussion to rationalist explanations for war, Fearon is deliberately leaving aside arguments that states participate in wars because of irrational leaders – those that cannot link means and ends or have "biases and pathologies that neglect the costs of wars" (Fearon, 1995, pg. 379). He also leaves aside arguments that wars will benefit leaders while harming states – that decisions for wars are rational for leaders (because they benefit) while simultaneously being irrational for the states (because they pay the costs). Thus, the discussion centers on arguments that leaders, acting rationally for their states, choose war as a deliberate policy option.

[9] These are: "(1) Anarchy; (2) expected benefits greater than expected costs; (3) rational preventative war; (4) rational miscalculation due to lack of information; and (5) rational miscalculation or disagreement about relative power" (Fearon, 1995, pg. 381).

[10] This argument is used formally in Bueno de Mesquita, *The War Trap*, 1985, and informally by Waltz, *Man, the State, and War*, 1959. It can be found in many other sources.

brings peace through making war more expensive. States will rationally calculate that

peaceful solutions to problems are less costly than war.

Fearon, however, directly rejects the logic behind this argument. By translating

the theory into formal logic, he proves that there is always a settlement range (a set of

negotiated settlements) that both sides would accept rather than go to war.[11]

This simple but important result is worth belaboring with some intuition.
Suppose that two people (or states) are bargaining over the division of $100 – if they can
agree on a split they can keep what they agree to. However, in contrast to the usual
economic scenarios, in this international relations example the players also have an
outside option. For a price of $20, they can go to war, in which case each player has a
50-percent chance of winning the whole $100. This implies that the expected value of
the war option is $30 ($(\langle 0.5 \times 100 \rangle + \langle 0.5 \times 0 \rangle - 20 = 30)$[12] for each side, so that if the
players are risk-neutral, then neither should be willing to accept less than $30 in the
bargaining. But notice that there is still a range of peaceful, bargained outcomes from
($31, $69) to ($69,31) that make both sides strictly better off than the war option. Risk
aversion will tend to increase the range yet further; indeed, even if the leaders pay no
costs for war, a set of agreements both sides prefer to a fight will still exist provided both
are risk-averse over the issues. In effect, the costs and risks of fighting open up a
'wedge' of bargained solutions that risk-neutral or risk-averse states will prefer to the
gamble of conflict. The existence of this *ex ante* bargaining range derives from the fact
that war is inefficient *ex post.* (Fearon, 1995, pg. 387 – 388)[13]

By rejecting this commonly used logic for why states go to war, Fearon must

show there is some rational reason that states choose war or else admit that wars happen

for irrational reasons. Rationally, the only reason left for states to fight is that they

disagree on the calculation of the expected value of conflict. There are three parts to the

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[11] See Fearon (1995) for the full argument and stated assumptions. The appendix in his article contains the
formal proof. The assumptions inherent in this argument are that decision-makers are unitary, rational, and
control policy implementation (assumptions, by the way, that the interest-based logic of the liberal peace
shares) and that decision-makers are risk averse or risk neutral (i.e., they do not prefer the risks inherent in
rolling the dice in conflict).
[12] Expected value is calculated by multiplying the probability of winning times the value of winning, plus
the probability of losing times the value of losing, and subtracting the costs of "playing" (participating in
the war) from that sum: (probability of winning $\times$ value of winning) + (probability of losing $\times$ value of
losing) – (costs of starting a war). In this example, the formula sums to $30.
[13] Though Fearon does not mention this, even risk-acceptant states may choose negotiations over conflict.
It would depend on the states degree of risk acceptance, the "cost" for war, and the payoffs. As Fearon
notes, risk-aversion will increase the bargaining range – risk-acceptance will decrease it. If the state is risk-
acceptant enough (highly risk-acceptant), there may be no range for negotiated settlement (perhaps this
might describe Hitler's Germany in late 1939).

calculation of expected value: the cost of the war option, the value of winning and losing, and the probability of those outcomes. The states could disagree on all, some, or none of those factors. For example, if both sides think the probability they would win a war increases further and further above 50-percent (while each side might believe the odds of winning are greater than 50%, this is not logically possible since the sum of the probabilities must be 100%), Fearon's bargaining range (payoffs each state would accept rather than go to war) shrinks to zero and war occurs between states. Look at Fearon's example with the $100, above. If you tell one side, privately, that for the $20 cost of war they would have a 90-percent chance of getting the $100, and you privately tell the other side that it has a 90% chance to get the $100 (the same information), there is no range over which a negotiated settlement acceptable to both sides exists. The expected value of the war option is now $70 (i.e., $\langle 0.9 \times 100 \rangle + \langle 0.1 \times 0 \rangle - 20 = 70$) for both sides (because of their private information), and no negotiated division of the $100 will be acceptable. As a result of the private information, both would rationally choose war. As in this example, it is irrelevant whether the private information is objectively accurate or truthful, just that each side believes it is and is willing to act on that information. While the example illustrates the effect of private information related to each side's chances of winning the conflict, the example can easily be recast using private information on the "cost" of conflict or the value of winning/losing for each side.[14] To translate this to the real world, Fearon argues that unless both states honestly and objectively know the reality of the expected value equations for both sides (i.e., no private information), the states

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[14] For example, if the issue over which the states are in conflict is "life-or-death" for one side (i.e., the cost of losing is prohibitively high) that narrows the bargaining range for that state. If the other side does not know this, it may assume an expected value for conflict far lower than the state facing the "life-or-death" issue, and through inaccurate information may offer bargaining positions that it thinks are acceptable and push the other state into war.

may end up in a situation where they believe they will gain more from conflict than negotiation, and war results.

Fearon, however, is subtler than this. He reasons (Fearon, 1995, pg. 393) that both sides have an incentive to reveal their private information to the other side so that both sides can know, accurately, the real payoffs of conflict.[15] Both sides of a conflict know that war is inefficient, but they cannot realistically bargain until a consensus exists publicly (at least between the two sides) as to the real payoffs of war for both sides. Given that both sides share that knowledge, we are back to the original argument: that there always exists a set of negotiated settlements that both sides would prefer to war. Yet, of course, war exists in the world. Thus, the real question for Fearon is that, given states have an incentive to share private information about their resolve and strength (so that all parties know the real chances of winning a war), why can they not do so in order to avoid war?

The answer is in the logic of the problem. If a state knows that the bargaining outcome between it and another state is conditioned on the private information it must reveal in order to find that bargaining solution, then it has a "strategic incentive" (Fearon, 1995, 400) to misrepresent its private information in order to gain a better negotiated outcome, and the other side knows that. In other words, both sides have incentives to bluff in order to get a better deal than the truth might get them, and both sides know this about the bargaining environment.[16]

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[15] Of course, one or both sides may choose to bluff. This is discussed below.

[16] The degree of risk-aversion or risk-acceptance of both the states determines the size (broad or narrow) of the acceptable bargaining range, but does not invalidate the model as a way of describing how states find themselves in armed conflict. As noted, the state or states may be so risk acceptant that there is no range of possible bargained solutions, and war is a guaranteed outcome.

Think about the $100 example again. The adversaries can engage in a war at a cost of $20, or they can negotiate any division of the $100 to which they both agree. Neither side knows what to correctly offer the other in order to peacefully distribute the $100 until they have reasonable knowledge about the value the other side attaches to winning and losing, and the probabilities of war as a method of distribution. Without that knowledge, they may make an offer that would result in less of the $100 than they "deserve" given the objective probability of winning a war. No state wants to deliberately short itself, and so a reasonably accurate knowledge of the payoff structure for the other side, including the probability of winning the war, is necessary for peaceful negotiations. Suppose we are omniscient beings with complete knowledge and we reveal to only one party that the (real) chance of winning the war is 50-percent. This side is responsible for telling the other what the chance of winning a war would be. If the party with the information says, "the chances of winning the war are 50-50", then the bargaining range for successful negotiations is as it was in the original example (anywhere between $31 and $69 is better than a war; see the full explanation above).

This example shifts, however, if the knowledgeable state decides to bluff. If the party with the information knowingly lies and says, "I have a 90-percent chance of winning, and you only have a 10-percent chance," then the ignorant side knows (assuming it accepts as credible that statement) that the bargaining range the knowledgeable side would accept has changed compared to the 50-50 chance of winning example (now the calculation gives $\langle 0.9 \times 100 \rangle + \langle 0.1 \times 0 \rangle - 20 = 70$ expected utility from war), and the knowledgeable side will not accept any offer less than $70 (leaving only $30 for the ignorant side). The knowledgeable side lied, and got itself a better outcome.

However, the ignorant side, knowing that the knowledgeable side has a strategic

incentive to misrepresent the actual chances of winning, will discount the information

received from the knowledgeable side. The ignorant side knows that they have likely

been lied to, because the other side had an incentive to do that, and is thus more likely to

reject agreements and choose war as a solution. Even if the knowledgeable side reveals

the "true" odds to the ignorant side, the logic of strategic misrepresentation holds, and the

ignorant side (having no way of knowing the information it has been given is accurate) is

likely to discount it. For Fearon, states may rationally find themselves in wars with other

states because they cannot credibly reveal private information that leads to mutually

advantageous negotiated settlements that avoid the costs of war.

According to this logic, if states could credibly (truthfully) signal their relative

strength and resolve (their private information that allows them to more accurately know

the real payoffs of conflict)[17], there would (almost[18]) always exist a nonviolent resolution

to a dispute that would avoid the costs of conflict and the unpredictable outcomes

inherent in war and other forms of interstate conflict.[19] If states could be shown the

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[17] For Fearon's logic to operate, it is not necessary for the states to possess objectively accurate (god-like omniscience) information about the probability of winning the war. In fact, it is impossible (even given all available private and public information) for states to be able to know with certainty what the chances of winning a conflict are: there are too many unmeasureable factors that influence the outcome. The logic still holds, however, if states possess private information that allows states to more accurately (as opposed to perfectly accurately) determine the probability of war.

[18] Again, highly risk-acceptant states might not accept any bargain and prefer the risk of war (and gains of successful war. It would depend on the degree of risk-acceptance, the costs for conflict, and the payoffs of winning and losing for each side. Even highly risk-acceptant states, however, are not likely to chance conflict without some knowledge of these factors (no matter how risk-acceptant a state/leader is, if the other side is facing a "life-or-death" issue such that they will not every compromise, then the leader may choose to avoid conflict).

[19] Fearon's argument is made explicitly about war, but the logic would also apply to militarized interstate disputes (MIDs) – which are conflicts between states involving the threat of force, display of force or use of force short of war. Wars are a costly way of settling something – cost creates an overall inefficiency. If MIDs have costs (Li and Sacko, 2002, show that trade decreases in the presence of MIDs as well as war, so MIDs entail some cost to a state), then the same logic can be extended. Engaging another state in a MID may entail a cost less than that for war, but because there is a cost, MIDS are inefficient vehicles for

"post-conflict" results where the relative capabilities (the probabilities of winning and losing) and resolve (the degree to which the states are willing to bear the costs of winning and losing) are known by who "won" the conflict, they would find a solution to the dispute that they would prefer, pre-conflict, given the costs associated with actually participating in the conflict. Absent credible signals, adversaries cannot discriminate between truth and bluff and, therefore, cannot precisely calculate what pre-conflict, non-violent resolution would be preferable. Thus, in this "signaling-based" explanation, military conflict is a product of uncertainty arising from the inability of adversaries to credibly communicate their resolve and/or capabilities. Any structures, procedures, or conditions that alleviate some of the uncertainty should reduce the likelihood of conflict.[20]

This logic was used by Fearon (1994b), at least in part, to demonstrate how the democratic peace (see Chan, 1997, for a review of the democratic peace literature) can operate. Fearon argues that one way for states to reduce the inability to credibly

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resolving disputes , and just as with wars, there exists some bargaining range within which states would prefer a peaceful outcome.

[20] Fearon's explanation notes how states can avoid war or international conflict short of war – by signaling "true" capabilities and resolve through available channels. In a sense, this is a theory of peace, not war. States can avoid war and international conflict through effective signaling. The theory says nothing about why states end up fighting, just that if states do fight, they have failed to credibly signal. Gartzke (1999; Morrow, 1999, has a similar conclusion, but tailored to the interest-based traditional liberal peace logic) points out this logic, noting that rationalist explanations for war and conflict will never be able to pinpoint specific causes of interstate conflict because information available ex ante to researchers about factors that drive states to wars are logically available pre-conflict to states to use in bargaining to avoid war. Arguments, theories, and evidence known by researchers about factors that are significant in causing conflict can also be seen and used by the states themselves to avoid war and interstate conflict. If peace research determines that factor "X" is significant in leading states to war, then states will know to pay attention to factor "X" in bargaining, which suddenly turns factor "X" into an irrelevant part of conflict (given that factor "X" determines interstate conflict, no state that lacks factor "X" would involve itself in potential interstate conflict with one that possesses factor "X"; and the predictive abilities of factor "X" are changed). Gartzke's argument is that, in the end, decisions for war may be only a stochastic process – given a set of determined and capable states, the factors that push some into war and some into peaceful settlement can never be precisely determined. Researchers can determine causes of peace (anything that improves signaling), but never causes of war. Gartzke's argument does not affect the application of rationalist logic with respect to the liberal peace, but is worth noting.

communicate is to use costly signals to demonstrate resolve, capabilities, and/or

intentions. Signals that are costly to send are more likely to be believed (and accepted as

truthful by the opposing state) because of the cost of sending them. Signals of resolve,

intentions, or capability that have no cost could easily be bluffs (or the truth – states have

no way of distinguishing between the two), while signals that are costly – signals that

might result in a loss of domestic political power by the leaders of the state, for example

– have a built in mechanism for providing at least *prima facia* credibility, and

believability. Thus, signals that have domestic political audience costs are more likely to

be believed by the other side, and reduce the likelihood of armed conflict or war arising

between states. Fearon assumes that political costs associated with displays of armed

force and other such actions are greater for leaders in democratic political systems than in

autocratic systems[21], and formally demonstrates that states can "learn" from costly

signals and seek methods of conflict resolution that are less likely to result in violence.

Thus, the democratic peace can be explained not with norms or institutions (see Russett,

1993, for an explanation), but by showing that democracies have more of an ability to

credibly demonstrate their resolve and/or capabilities to opposing states, thereby creating

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[21] The assumption by Fearon that democracies have higher domestic audience costs to leaders from
displays of military force compared to autocracies is never tested or explored. Clearly, democracies,
because of their more transparent nature, allow people to more clearly see what their leaders are doing and
punish those leaders (by removing them from office) for action that the people find inappropriate – hence,
leaders make costly signals by taking actions that might offend the voting public. I do not argue that
democracies do not have costly signals, but only that the assumption that democracies have more costly
signals than autocracies should be empirically tested before being accepted without question. Leaders of
autocracies certainly can operate (make signals) in an environment that is less public than leaders of
democracies. However, all leaders, autocratic or not, have some domestic political power base they depend
on to remain in power. Autocratic leaders making signals to other states may incur domestic political costs
with their power base just as readily, or more so, as democratic leaders do taking similar actions with the
general voting public. Moving troops to threaten a border can clearly be costly to leaders in a democracy.
The same action, in an autocracy, could also be costly, if it seems to threaten the economic interests of
oligarchs that keep the leader in power, for example. My disagreement with Fearon is not that audience
costs can make costly signals more credible; it is with the assumption that democracies by definition have
higher audience costs than autocracies. That assumption calls for more theorizing and empirical testing.

more opportunities than are available to non-democracies to avoid war. This logic has been subjected to empirical tests (Eyerman and Hart, 1996; Shultz, 1999; Leeds, 1999; Partell and Palmer, 1999) and found to be more effective than competing logics for explaining the democratic peace.

Using Fearon's arguments about peace being achieved through credible signals, an alternative explanation for the liberal peace might be that economic ties serve as conduits of credible information, about the intentions, resolve, and capabilities of other states, thereby reducing the possibility of militarized conflict and war. Additionally, as a consequence of interdependence, certain forms of behavior (such as threats of force) may be particularly costly (because an interstate conflict reduces trade, which causes economic harm to the states engaged in that trade) and therefore serve as credible signals that can restrain the escalation of violence.

This is the line of argument taken by Gartzke, Li, and Boermer (2001) in their discussion of the liberal peace. Drawing on the logic of Fearon, above, they argue that the empirical results of the liberal peace are better explained using Fearon's signaling-based logic than the traditional interest-based one.

Instead of deterring conflict, interdependence can convey credible signals, obviating the need for costly military contests. Actors' behaviors potentially inform observers about the value of strategic variables, dissipating private information. [Economically i]nterdependent states that endure opportunity costs in pursuit of political objectives differentiate themselves from other, less resolved, competitors. To the degree that nonviolent conflict allows observers to identify opponents, costly signaling also allows efficient *ex ante* bargaining. States seek to obtain settlements while competing for preferable terms. War is less often necessary when states possess nonviolent methods that credibly inform. (Gartzke, Li, and Boehmer, 2001, 401)

In other words, economic interdependence allows states to credibly inform opponents through costly signals of their relative capabilities or superior resolve. These

costly signals are read by the opposing state, which accepts them as credible, and the settlement offers between the opposing states have a much greater chance of allowing a non-violent resolution of the conflict than if the information were not available.[22] Simply, interdependent non-resolved (or non-capable) states will accept terms that end the dispute which interdependent resolved (or capable) states would reject (because of their increased capabilities or resolution). Following this logic, economic interdependence has no special significance beyond the signals it can send, but is only another conduit or source (similar to democratic political systems) of costly signaling that allows disputing states to find solutions short of war or other costly interstate conflict.

What Gartzke, Li, and Boehmer do not discuss is the nature of the signal. The signal must be costly, but does it have to be economic? Could it be some nonviolent, but nevertheless military display as a means of conveying the state's continued resolve or capability even in the face of the economic interdependence? Could it be an economic action of ending the economic interdependence (and not a military action) that is the costly signal? The authors never specify. "Unresolved states prefer interdependence and a demanding settlement to fighting. Resolved states … prefer to fight rather than to accept a poor settlement. Since states willing to fight expect to forfeit the benefit, these states are more willing to signal, destroying interdependence at the outset" (Gartzke, Li and Boehmer, 2001, 401). For what signal researchers (and policy officials involved in interstate disputes) must look is unclear, and could be any action that threatens the normal economic (and other?) relations between states. One of the additions this

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[22] Certainly, these "costly" signals, as with the costly signals by democratic states, can be just costly bluffs. There is nothing in Fearon or Gartzke that prevents states from this action. Their argument is that because of the cost of sending signals, they are less likely to be bluffs and that over many interactions the costly signals will lead to fewer interstate conflicts than costless talk between states.

dissertation makes to the overall liberal peace research program is to argue that the signals can be (but are not limited to) initiation and escalation of interstate conflicts (see below).

The signaling-based logic for the liberal peace is vague on the mechanism by which trade/economic ties allow for more credible signals between states. Trade seems to serve two complementary, but distinct, functions within these arguments. First, in a simple informative sense, trade channels are additional pathways by which states can gain information about other states. The signaling logic argues that states do not and cannot have complete information in order to peacefully resolve differences. Additional contacts between states serve as ways of gathering information and gaining a clearer picture of the opposing state. These can be formal channels, like official trade representatives exchanging official statements at arranged talks, or unofficial channels, as when businessmen are interviewed by intelligence services following a trip to another country. Absent an economic relationship, this additional information would not exist. Given this information, a state can get a clearer picture of the opposing state, and can (according to Fearon's logic) negotiate more effectively and accurately, preventing conflict. Hence, one part of the signaling explanation for the liberal peace is that economic ties serve as conduits of credible information about the intentions, resolve, and capabilities of other states, thereby reducing the possibility of militarized conflict and war.

Second, trade serves as a form of a tripwire that the opposing state can clearly see as evidence of resolve or hidden capability. Fearon's signaling based logic argues that states "fall" into conflict because of an inability to speak to each other in statements that

each knows to be "true" or credible. Economic relationships have value for states (what Polachek, 1982, in the interest-based logic, would call welfare gains for the state) and are clearly seen by both parties as valuable and worth retaining. Deliberate actions by states that imperil or end these relationships are "signals" of resolve or capability that have more credibility or "truth" than costless signals such as statements by deputy foreign ministers or other officials. Hence, actions by states that imperil real relationships of value - actions like continuing a dispute, initiating military hostilities, escalating a military dispute, etc. - serve as credible signals of intentions, resolve, or capability. These serve to give "truthful" information to opposing states, which in turn allows negotiations between the states to be more accurate and successful. It is this second mechanism for trade that is more often discussed in the signaling literature.

Thus, signaling works two ways. In a "latent" manner, trade and economic relationships serve as conduits for information that allow states to better understand opposing states and negotiate more accurately. In a more "active" manner, trade and economic relationships may increase the costs of signaling, serving to make actions more credible and create credible communication between states due to the threat of making that trade disappear. It is this second, "active", manner of signaling that is the focus of the empirical tests in this dissertation.[23]

Beyond the specific arguments of direct trade in the liberal peace, the signaling framework has been expanded in liberal peace studies to include an argument that

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[23] Differentiating between the two types of signaling is not attempted in this dissertation. "Active" signaling argues states will take actions to endanger the beneficial connection between the two states (thus credibly signaling to the opposing state); "passive" signaling is only seen through studies of specific decision-makers and their actions at specific decisions (knowledge gained through the interaction helps the decision-maker – or aides – communicate effectively with the opposing state). This dissertation concentrates on "active" signaling, leaving aside questions of "passive" signaling.

commercial institutions (international organizations devoted to international trade)

facilitate pacific relations between states in addition to the direct trade ties (Mansfield

and Pevehouse, 2000; Bearce, 2003). In the same way that trade provides channels for

signaling that reduce uncertainty, membership in international organizations that focus on

trade provides increased expectations that trade will bring future gains to the state (*a la*

Copeland, 1996), facilitates personal ties between leaders (increasing trust), and makes

available information about capabilities and resolve of opposing states that would be

unavailable without the international organization (Bearce, 2003, 349 - 350). These

effects exist independently of the benefits to pacific relations from direct trade between

states. In this way, the signaling argument is not unique to the direct trading relationship,

but can be linked to almost any economic, political or any other (cultural? religious?

ethnic?) tie between the states.

Overall, the signaling logic provides an alternative to the traditional interest-based

logic that has driven the liberal peace for centuries. However, this alternative logic is not

so far from the interest-based explanations as some of the proponents of signaling seem

to suggest – at least the "active" version of signaling discussed above. Economic

interdependence provides a channel through which information can be credibly sent to

the opposing state. The reason the information will be seen as credible by the opposing

state is directly related to the potential loss of economic gain (the "interests") should the

conflict turn violent and the economic relationship end.[24] In other words, this logic

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[24] Bearce (2003) sidesteps this argument by discussing signals sent through preferential trading agreements (PTAs) instead of trade directly. However, PTAs do not exist for much of the historical empirical record that the liberal peace has been tested against (limiting our ability to determine their effect on conflict), and are relatively small in number compared to the volume of trade and totality of trading partners for states in the world. Thus, while not all the signaling logic depends on the "interests" that trade generates, the signaling-logic described here is the dominant theory in the literature.

allows one state to inform the other that "we know it is in our economic interests to resolve this dispute before our trading relationship is damaged, but we are resolved on this issue, and the fact that we are willing to risk the economic relationship by continuing this dispute is a sign of our credibility that you should see and allow us to find a resolution that is acceptable to both our states." The signaling-based explanation for the liberal peace rejects the idea that economic interdependence creates interests in states that push them towards peace in favor of the idea that those economic interests create "stakes" that allow for costly signaling which, in turn, allows for resolution of interstate conflict. Fundamental to this logic is the idea that the economic relationship creates interests, that those interests have value, and the "wagering" of the economic value allows for more credible communication. The signaling-based logic described here cannot operate without at least implicitly accepting the idea that economic interdependence creates interests for states.[25] It is this shared use of "interests" that makes the signaling-logic part of the liberal peace, as opposed to a theory outside the liberal peace that coincidentally also discusses a relationship between trade and interstate conflict.[26]

2.4 Theoretical Arguments Opposed to the Liberal Peace

Given the nature of the empirical tests later in the dissertation, one of the possible outcomes of the tests is to find no association between measures of trade (as proxies for

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[25] To be clear, the "conduit" version of signaling (that trade opens pathways by which information flows back and forth between states and allows each state to more credibly understand the resolve and capabilities of the other, and hence reduce conflict) is not affected by the argument here. There is no part of "interests" that is relevant for the "conduit" version of the signaling logic.

[26] The signaling logic has been accepted by most liberal peace researchers as part of the overall debate on the liberal peace, and has not generally been seen as a outside the normal scope of the liberal peace. However, to the degree that the liberal peace can be narrowly defined as a theory whereby shared economic interests dampen the resort to force between economic partners through mutual benefit in a non-coercive manner, then the signaling-logic fails to meet that definition and can be considered not a part of the liberal peace directly. However, few, if any researchers have defined the liberal peace in such a narrow manner.

dependence or interdependence) and forms of interstate conflict.[27] The theoretical

explanation that best fits these results is found in the following pages where researchers

have detailed theoretical objections to the liberal peace.

Opponents of the liberal peace thesis have put forward two dissimilar arguments.

The clearest rejection comes from Realists (before the Realists, the Mercantilists had

similar arguments), who reject the idea that interstate economics affects state's decisions

with respect to national security issues. They argue that state's decisions on peace and

war are made under a decision calculus that looks to issues of power, security, and state

survival. Economic issues have some to do with that; certainly, to the degree that a

strong economy is important to overall state power, economic issues are a relevant

concern to Realists. However, economic issues are always subordinate to state

power/security interests. Under no circumstances should a state make decisions purely

on interstate conflict (for or against) based on economic criteria – only power and

security considerations are relevant. Hence, the liberal peace (which argues that states do

exactly that) is incorrect. States should try to grow economically, in the name of

increasing power, but not at the cost of increased risk – hence, trade can be a danger as

much as a benefit. Some theorists go so far as to argue that trade can cause conflict,

generating issues over which states fight.

The second school argues that trade can cause pacific relations, but only through

coercive economic relations that force states to become economically and politically

dependent. Dependent states are constrained from engaging in physical conflict with

those states upon whom they are dependent, and hence there will be peace among those

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[27] As noted, this is substantially the result in the pre-1950 era. Hence, the theory presented here explains
empirical results found later in the dissertation.

states, though it is not a peace based on mutual (shared) beneficial economic relations, but a forced peace. An individual held at gunpoint will (likely) not act aggressively towards the individual with the gun, but their relationship can hardly be called pacific or peaceful. The dependency school goes so far as to argue that these economic relations can cause conflict, as the dependent states can fight to remove the economic ties that hold them down, or the controlling state can use military force to make the dependent state politically as well as economically dependent.

Overall, these two groups of theorists argue that, at best, economic relations are irrelevant to our understanding of interstate conflict. At worst, they are a source of interstate conflict.

Preceding the modern Realists (Carr, 1939; Waltz, 1979) were the Mercantilists (generally placed from around 1500 to 1750, just preceding the birth of Liberalism), who (like the later Realists) also held that international politics was power politics, and that international economic issues must bend to the national security needs of the state. Trade, at least by this theory in this time, did not bring peace, since trade was directed by the state to increase power and security in preparation for war. "When the guiding principle of the state is mercantilism or totalitarianism, the power of the state becomes an end to itself, and all considerations of national economy and individual welfare are subordinated to the single purpose of developing the potentialities of the nation to prepare for war and to wage war…Almost three hundred years ago, Colbert epitomized the policy of the French monarchy of Louis XIV by saying that 'trade is the source of finance and finance is the vital nerve of war'" (Edward Mead Earle, 1943; in Paret, 1986, 217).

Even among the classical political economists there were detractors who did not see universal free trade as an end to armed interstate conflict. One such argument is that trade was just not particularly important to states, so that ending trade (through war) was not a sufficient threat to prevent war from occurring. "In the life of nations, trade does not play such an important role that, completely free and unhampered, it imposes upon them peace as a permanent and imperative need" (Dupont-White, 1851, 233; quoted in Silberner, 1972, 166).

This argument remains valid today. Among the talk of globalization is the simple fact that the economies of the world's largest states are not greatly interconnected. As Wade (1996, 66) notes, ninety percent of goods produced in the United States, Japan, and "single-unit" Europe (meaning the European Union) are consumed within those same states. Even given the passage of a decade, the percentages cannot have increased dramatically. This is not to say that trade is irrelevant, but does give some leverage to Dupont-White's argument that trade issues, even a hundred and fifty years later, are not so important as to be able to prevent or even significantly hamper impulses to war.

There were other detractors. Cunningham (1904), who began life as a Cobdenite-Free Trader, argued that in domestic economic circumstances we do not observe companies and individuals acting in peace and harmony – so why should nations? Domestic economic actors, operating in the domestic version of a free trade zone, do not evidence any social harmony, and could be said to be actively hostile to each other, trying to remove rivals and capture markets. If domestic economic activity does not produce domestic peace and solidarity, why should international trade generate international peace and solidarity?

The Mercantilists gave way, over time and with the advent of modern political science, to the Realists, whose position on the liberal peace was unchanged from that of previous Mercantilist theorists. States, in an anarchic world that requires self-help procedures, must rely on their own power to protect themselves and gain security. Thus, international conflict and war are inherent in state actions as an adjunct to displays of state power. The security of the state, gained through increased state power, is paramount. All other considerations, argue the Realists, are secondary. Economic issues are important to states and are an important factor in state power. However, if economic considerations ever come into conflict with state security and power considerations, national security is the primary goal, and economic issues must take a back seat. Thus, Realists reject the idea of the liberal peace. Economic ties between states should have little bearing on state decisions for or against international conflict.

This is the argument that Blainey (1973) uses. He reviews the period of history most often cited as being the most peaceful and most commercial – the "Hundred Years" of peace in Europe between Waterloo and the First World War. His conclusion is that whatever the cause of this "peace" (and it was not very peaceful, as there were numerous conflicts and wars among the European powers), it is not trade and associated other theories of close contact.

> Those living in the three generations after Waterloo had wondered at the long peace and sought explanations in events that were happening simultaneously. They noticed that international peace coincided with industrialism, steam engines, foreign travel, freer and stronger commerce and advancing knowledge. As they saw specific ways in which these changes could further peace, they concluded that the coincidence was causal. Their explanation, however, was based on one example or one period of peace. They ignored the earlier if shorter periods of peace experienced by a Europe which had no steam trains, few factories, widespread ignorance and restricted commerce…The ease with which

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ideas, people, and commodities flowed across international borders was very much an effect of peace though in turn it may have aided peace…. In one sense the Manchester theory of peace was like the mountebank's diagnosis that shepherds were healthy simple because they had ruddy cheeks: therefore the cure for a sick shepherd was to inflame his cheeks…. The conclusion seems unmistakable: the Manchester creed cannot be a vital part of a theory of war and peace. One cannot even be sure whether those influences which it emphasizes actually have promoted peace more than war. (Blainey, 1973, 29 – 31).

The connections between peace and trade were logical and coincidental, but not causal.

This view is reinforced by Buzan (1984), who argues that, while there has been a decline in the use of force during the cold war as well as an increase in interdependence, the two are not necessarily causally connected in the manner posted in the liberal peace. Liberal economic practices (free trade, growth) cause, argues Buzan, liberal political practices (relative peace and stability), but, in order to achieve the liberal economics, the system must be brought to the end results (peace and stability) through other (Realist) means. "The fact that a liberal economy depends, as a prior condition, on the effect it is supposed to cause does not undercut the arguments made about the liberal impact on the use of force…. It does, however, reinforce the claim to primacy of military and political factors as the major causes of decline in the use of force. At best, the liberal economic arguments can be read only as reinforcing a decline in the use of force that is already under way for other reasons" (Buzan, 1984, 607 – 608). In other words, states reduce the use of force for military and political reasons (stability of the bipolar system of the cold war, the presence of nuclear weapons, and so forth), and both trade and growth (liberal economic practices) spring up. The fact that both trade and peace occur is not a causal connection between the two, but is instead evidence of (an unspecified) change in international politics that allows both trade and peace to flourish. Whatever systemic

changes in international politics allow both trade and peace are (or should be) the center

of the debate, not a liberal peace debate about a fictitious relationship between trade and

peace. There is a connection between trade and peace, but it is a larger systemic change

in international politics that allows both.

One of Realism's most powerful supporters, Waltz, argues that close economic

connections between states can serve as causes of conflict rather than causes of peace.

"Many seem to believe that a growing closeness of interdependence improves the

chances of peace. But close interdependence means closeness of contact and raises the

prospect of occasional conflict. The fiercest civil wars and the bloodiest international

ones are fought within arenas populated by highly similar people whose affairs are tightly

knit. It is impossible to get a war going unless the potential participants are somehow

linked" (Waltz, 1979, 138). Contact or closeness is necessary for interstate conflict –

states do not fight states with which they have no relations – and trade or other economic

relations provide a vehicle for contact. The fewer ties between states – economic,

cultural, political, military, etc. – the less opportunity exists for interstate conflict to break

out. This is not an argument that says that reducing ties between states will end all

conflict – states are still motivated to acquire valuable territory and resources in a Realist

world, and to deny the same to opposing states – just one that argues that there will be

fewer conflicts if there are fewer issues over which states can fight.

Following on from Waltz, Uchitel (1993) uses cases of Nazi Germany and

militaristic Japan to show how states' dependence on strategic materials can cause

interstate conflict rather than prevent it. She argues that Germany and Japan chose to

pursue their expansionist military strategies in part because they saw a national need to

find permanent sources of supply for specific materials deemed critical to the survival of the nation. The states' dependence on external resources, rather than being a brake to potential conflict, caused the states to seek more secure sources of materials and choose military options to pursue those ends (other options – trade, political ties, treaties, etc. - were deemed not sufficiently dependable to assure state survival and, hence, only taking the materials by force was acceptable to Japan and Germany in those situations). Generic trade, in and of itself, does not cause or restrain interstate conflict, but it is a deeper question of the nature of that trade that must be answered before analysts can determine the beneficial or harmful effects of interstate economic interaction. If a state is dependent for strategic materials on outside sources, the state (following Uchitel's use of straight-forward Realism) can choose a military option to reduce its dependence and enhance its security. Uchitel admits, however, with the case of interwar Great Britain, that economic dependence can have a stabilizing effect by restraining states in just the fashion that traditional liberal peace authors would visualize.

The argument that there is no pacific connection between trade and interstate conflict is not unique to Realism, though the strongest proponents of that claim come from the Realist school. Morrow (1999) argues that there should be no connection between trade and conflict, rejecting the logic put forward by previous theorists. His argument is simple. If trade between states generates welfare gains for those states (and raises the economic cost of ending that trade – the interest-based logic of the liberal peace), that knowledge is available to both states of the pair – each knows the value of the trade. If the presence of the economic relationship makes it harder for either state to choose to resolve a dispute through some form of interstate conflict, the other state knows

that. Thus, an "initiating" state, knowing that the opposing state is less likely to take action to resolve a dispute that harms the welfare-producing trade, might deliberately provoke international conflict in order to intimidate the other state into conceding early (and avoiding the loss of welfare associated with a loss of trade). Knowing that trade induces more pacific behavior, a state might push another state into a choice between ending trade (beginning an interstate conflict) or conceding over a disputed issue. In this sense, trade does not reduce conflict. "Trade makes war less attractive to both parties, but the target's lower willingness to fight makes coercion of the target easier and more attractive to the initiator" (Morrow, 1999, pg. 487). Hence, trade decreases the chance of conflict between states (trade creates welfare gains that benefit all trading states) at the same time it increases the chance of conflict (knowing the value of trade, a predatory state might force a weaker state to give in to avoid losing the benefits of trade). Overall, Morrow argues, trade has an indeterminate effect on interstate conflict.[28] In this, Morrow's conclusion is the same as some of the Realists.

There is a more complex form of the same logic – trade and economic relations provide no pacific benefit between states because the presence of trade that allows more costly signals could be a form of bluff. This more "complex" form of the signaling-based

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[28] Two comments on Morrow's argument: First, it is a very small step from this logic to the logic of the signaling-based explanation for the liberal peace. Where Morrow sees the value of trade as a club the predatory state can use to threaten the weaker state, signaling theorists see the welfare enhancing value of trade as a channel for transmitting credible information. Neither is more "right" than the other: both are logical theories. One of the purposes of this dissertation is to test which is more accurate. Second, Morrow's argument foreshadows a key point of the dependency theorists (below) and one I will make in the research design. For Morrow, it seems an unstated assumption that the predatory state values the trade less than the weaker state (either the predatory state gains less from the trade than the weaker state or – if the predatory state gains equally or more from the trading relationship – it values those welfare gains less than the potential gain from the conflict it is fomenting. In either event, the trading relationship is viewed unevenly – one side is more dependent on the trade than the other). The significance of asymmetric trading relationships is not often discussed in the liberal peace (an exception is Barbieri, 1995, 1996a), but it is critical to the empirical test outlined below. Morrow's argument here implicitly acknowledges that uneven trade (or at least uneven valuation of trade) can have significant effects on how effectively the liberal peace operates.

explanation of the liberal peace argues that one can make no prediction with respect to interstate conflict. The difference between the Fearon/Gartzke signaling-based explanation for the liberal peace and this signaling-based explanation for no liberal peace hinges on the degree of credibility that the target attaches to the signal it receives (Morrow, 1999; Gartzke, 1999). In the "simple" liberal peace signaling argument, the more costly the signal, the more credible it is, and the more information it contains to be used to find a resolution to the conflict.

The "complex" signaling argument notes that costly signals are not necessarily credible. It could be a (costly) bluff. The signaler might reason that, to avoid the uncertainty and costs of war, the recipient will be willing to reach a nonviolent settlement. If the signaler can convince the recipient that the signaler has more capability or resolve than it actually has, a more favorable agreement to the signaler can be reached (i.e., the recipient will give up more in the bargain, as it believes that the signaler will make a conflict more costly). The recipient, however, knows that the signaler might think this. Hence, the recipient is unable to differentiate between a genuine costly signal that honestly reflects the signaler's level of capability or resolve and a signal that is a bargaining ploy – a bluff. [29] Hence, economic relationships may generate costly signals, but these costly signals are not necessarily credible, since the recipient cannot distinguish between truth and bluff. Consequently, the "complex" signaling argument suggests that

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[29] By this logic, any information passed from the signaler to the recipient can be considered by the recipient as a bluff – costly as it may be. The recipient cannot believe any information about the signaler, as even previous records of credibility and truthfulness can be seen in this logic as part of an attempt to bluff. If the signaling nation has a record of always doing what it says (never bluffing, always credible), the recipient knows this, but also knows that the signaler knows the recipient knows this, and thus the signaler could be counting on this to make a bluff (for the first time). Thus, no factor of the signaler, including previous actions, track record, or reputation, can serve as a means of conveying completely credible information to the recipient.

economic relationships have no effect on the probability of interstate conflict initiation or escalation. [30]

This logic has generated a response, which argues that states do not bluff. Fearon (1997) discusses costly signals and their effect on international crises. He breaks costly signals into two camps. "Sunk cost" signals require states to pay an up front cost to demonstrate resolve or capability. Examples of this are difficult to offer, but might include building arms or mobilizing troops (though only keeping them at home). These actions have an up-front cost to the state, but do not affect the "relative value of fighting versus acquiescing in a challenge" (Fearon, 1997, 70). In other words, the state pays an up-front cost to demonstrate its resolve or capability, and the cost of that demonstration has no effect on the subsequent decision by that state whether to settle the dispute or opt for fighting.[31] The other type of costly signal is a "tying hands" signal, where a state takes an action that increases the costs of backing down if challenged, but entails no or few costs if the state is unchallenged. Examples of this are easier to discuss; public statements of "we will not let this pass" (costless to say, costly if not backed up), harnessing a states' reputation through costless defense treaties with other states (costless to sign, costly when the state publicly fails to come to the aid of whomever they signed the treaty with), or putting a very small military force in a threatened region to serve as a trip-wire (low cost if challenged – a small group of soldiers is sacrificed, but if no larger

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[30] An additional possibility, not explored here, is that the generation of these costly (but ignored) signals could raise the stakes for the signaler, leading to an increased probability of conflict or war. Costly signals are, by definition, costly. The more a state "pays" to play, the greater the sunk costs, the more difficult a state might find it to withdraw from a dispute or conflict, both in terms of foreign costs (reputation, honor, etc.) and domestic costs (loss of office, change in domestic leadership, etc.). Thus, costly signals may be associated with increased likelihood of conflict in an environment where all signals are ignored as potential bluff.

[31] Fearon recognizes that sunk-cost signaling is not as prevalent in international relations as tying-hands signaling; and may even be impossible. However, since it is frequently used in economics research, which is where Fearon draws much of his logic, he discusses the type.

army follows, then a high cost is paid to reputation; see Fearon, 1997, for a full discussion). All these are relatively costless actions that only have a cost when a state is challenged and fails to respond. In this light, economic interdependence (not discussed by Fearon) would be a tying-hands signal rather than a sunk-cost signal. Threatening to reduce or sever an economic tie has no (actual) cost until the economic tie is, in fact, reduced or severed. The state would lose the gain from trade if challenged, but would not lose the gain from trade until it was challenged and backed down.

Fearon demonstrates that, in either case (sunk-costs or tying-hands), states will not bluff. For the sunk-costs signals, the argument is that the state could choose any range of costs to sink in order to demonstrate its credibility or run a bluff on the opposing state. The state could choose a very high sunk-cost option: one so high that it is only a logical choice if the state is certain to fight rather than settle the dispute. If the state fails to choose a sunk-cost signal in that range (high cost, hence certain-to-fight), then the opposing state can infer from this that the state is not willing to fight (that it is a bluff) and can press upon it to settle. Thus, a state that wants to run a bluff is forced into a position where it can only choose a sunk-cost signal that costs so much it would have to fight in order to make its bluff credible, but the cost of that is so high that it forces the state to fight anyway.

This same general logic works for the tying-hands signals. The state can choose any statement that involves *ex post* costs if the state backs down. Resolved states will choose signals that have large *ex post* costs, thus forcing them to fight rather than pay those costs. Opposing states know this, and any costly signal by states that are not costly enough to guarantee that the state will fight will be seen by the opposing state as an

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admission of lack of resolve and ignored as a bluff. Thus, states considering a bluff can only bluff by using a tying-hands signal that would prove so costly that they cannot back down and must fight, and hence, it is not a bluff. Logically, no bluff is possible with either type of signal.

Fearon (1997) notes that, empirically, states have bluffed, continue to bluff, and will likely do so in the future. His argument is better at explaining why bluffs fail (their costs are not high enough to convince the challenging state that the defending state is credibly signaling resolve), than why states bluff. The empirical section of the article is not systematic or entirely convincing, but it is suggestive.

There is one clear riposte to this argument. Fearon assumes that states are operating under conditions of uncertainty, which for him means neither state knows the value the other state places upon the outcome (winning or losing) of the dispute. If this is true, this uncertainty also means that neither state can compare the cost of the signal to the value of winning/losing, and thus the opposing state cannot determine if the costly (sunk or tying-hands) signal sent by the state is costly enough to not be a bluff. If states are uncertain about the actual value the other state places on the overall outcome (this is written into the assumptions that Fearon discusses), they are also unclear on the cost of the signal (how can the opposing state judge how costly an action is, when it, by assumption, does not know the overall value of winning and losing for the opposing state?). States certainly can see that some signals are costly (although it would be possible to misperceive some costly signals as costless, and costless signals as costly, but perceptions do not enter into Fearon's argument), but under incomplete information (they know their own values, but not the opponents) they should not know the actual cost of the

signal, and thus should be unable to know if the signals are maximally costly (and, hence, not a bluff), and so should not be able to determine if states are bluffing or not. The bottom line to this chain of logic is that states can clearly bluff – opposing states cannot know the value of the costly act, and must guess as to whether it is "maximally costly" (hence, credible) or not - and may choose to ignore all sources of information in order not to be taken in by a bluff.

A similar form of this logic has been used to challenge the entire signaling-based set of hypotheses (liberal peace, democratic peace, etc). Finel and Lord (1999) do not disagree that institutions and relationships[32] can serve as channels for states to more credibly communicate with other states – they argue, specifically, that democratic institutions can allow a more "transparent" view of capabilities, intentions, and resolve for outside states trying to gain knowledge about an opponent. However, Finel and Lord argue it is possible that outside states might become overwhelmed by the amount of information that a democracy makes available, and become even more confused about the democratic state's "real" capabilities or resolve in the face of contradictory pronouncements and press releases by different cabinet level officers, opposing party politicians, public opinion, and influential observers. This, they argue, is the "negative logic of transparency" – the idea that states can be transparent, but that opposing states may not be able to "see" the real level of capability or resolve in the face of massive amounts of information from a number of different quarters. Finel and Lord's case studies indicate that transparency can actually increase the potential for interstate conflict either by impeding behind-the-scenes negotiations or confusing opposing states regarding

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[32] Finel and Lord (1999) do not discuss economic relationships – trade – as a form of signaling, but the logic they use has clear application for this discussion.

which decision-makers are empowered to negotiate and make agreements (e.g., the private diplomacy involving a trip to Syria by Reverend Jessie Jackson on behalf of a downed U.S. pilot in 1983).

In a completely different direction from the signaling-based logic discussed above, other theorists offer explanations of how economic relationships can lead to a malevolent peace.

One of the most powerful theoretical arguments against the liberal peace was put forward by Hirschman (1945), observing the actions of the Nazi German government before the Second World War. He noted that trade between nations was not always equal, and this inequality of trade could be used to political advantage. "The necessary basis for the idea that the interdependence created by trade would or should lead to a peaceful collaboration between nations, is, indeed, the belief that the dependence of A on B is roughly the same as the dependence of B on A. Mill was one of the first to show that the material benefit derived from international trade is not necessarily divided equally between the various trading nations" (Hirschman, 1945 [1980], 10). Given that trade could sometimes – often – be unbalanced, what could be the political consequences of the economic difference?

"[E]very sovereign nation has some influence of this kind, since through the control of its frontiers and the power over its citizens it can at any time interrupt its own export and import trade, which is at the same time the import and export trade of some other countries. The stoppage of this trade obliges the other countries to find alternative markets and sources of supply and, should this prove impossible, it forces upon them economic adjustment and lasting impoverishment. True, the stoppage of trade will also

do harm to the economy of the country taking the initiative in bringing about the stoppage, but this is not unlike the harm an aggressive country can do to itself in making war on another. A country trying to make the most out of its strategic position with respect to its own trade will try precisely to create conditions which make the interruption of trade of much graver concern to its trading partners than to itself" (Hirschman, 1945 [1980], 15 – 16.)[33]   Trade can serve as a mechanism of political domination, similar in many respects to war. Different levels of trade between countries can be exploited by one country over another for the sake of political gain.

Hirschman goes on to demonstrate that Nazi Germany had deliberately and systematically made several states in Eastern Europe economically dependent on Germany before the war. The consequence of this action was that these countries were substantially restrained from politically or militarily opposing the Nazis because of their extreme economic dependence. Germany, for example, would sell to these countries raw materials at lower than market prices, and buy from them their raw materials at higher than market prices. Germany became their largest supplier and largest market. These policies could be employed by Germany because of the size of the German economy in comparison to the economies of the Eastern European states. While German trade with these countries was a significant percentage of their overall trade (and overall economic activity), Germany's trade with these countries was only a small percentage of Germany's overall trade, and an even smaller percentage of overall German economic activity. As Hirschman noted, ending this trade would cause an economic dislocation for Germany, although not substantial harm, but would devastate the economies of the dependent Eastern European countries. When Germany threatened to cease trading with

[33] This sounds a great deal like Morrow, 1999, above.

Eastern Europe, these countries had little choice but to accede to German political demands or face economic ruin. In a perverse sense, this is the liberal peace – trade creates conditions in states where the economic losses from ending trade are so high that states cannot consider the use of violence or force as a way of resolving political disputes. Realistically, this one-sided dependence can be used, as demonstrated by Hirschman, as a method of domination and political subjugation. Peace is achieved, but at the price of political independence.

Hirschman showed theoretically and, in at least one case, empirically that greater trade (and levels of trade between Germany and the Eastern European states had increased over the decade of the 1930s) could be associated with political and economic domination, not a mutual dependence that brought peace. Hirschman's study has been cited as one of the foundations for the later work on dependency (applied most commonly to Latin American economies), and has been held up as both a theoretical and empirical problem for liberal peace theorists.

More substantive empirical evidence to support Hirschman is found in Richardson and Kegley (1980). Examining states with dependent trading relations with the United States, Richardson and Kegley discover that states that have vulnerability dependence (as opposed to sensitivity dependence in the Keohane and Nye (1977, 1989) sense of the terms) are much more likely to have foreign policy positions and policies that are consistent with United States positions than those states without vulnerability dependence, but that sensitivity dependence has no effect on foreign policy compliance. In terms of the liberal peace, it can be assumed that where there is foreign policy compliance by one state to another, there is also a lack of militarized interstate conflict.

In this case, trade does create peace between states, but as a consequence creates a dependent relationship between dominant and subordinate states that speaks more to control than mutual economic advantage restraining political action, as the liberal peace theorists had envisioned. It is even possible that the dominant state does not exercise this control willingly – that the subordinate state changes its policies (and refrains from interstate conflict) without the direct request of the dominant state because the subordinate state recognizes its dependence and acts to minimize potential harm – trying to avoid even appearing on the dominant state's radar screen, as it were. The economic relationship might even be mutually beneficial for the states, though that does not change the coercive nature of the peace.

Additional empirical support for Hirschman's argument comes from Roeder (1985). Roeder examines the effect of Soviet economic and military aid, as well as intra-bloc (socialist to socialist) trade, on political decisions by Soviet client states. While the study is clearly biased in its selection of cases (concentrating entirely on states with military and economic ties to the Soviet Union) the results are consistent with both the liberal peace and Hirschman. Roeder finds that where trade dependence occurs (where states are dependent on the Soviet Union for either imports or exports in order to keep their economy functioning), there is a remarkably large amount of political compliance (in this case measured by votes consistent with the Soviet position in the United Nations) and no evidence of any interstate conflict between the Soviet Union and its client states. While there are other alternative explanations of Roeder's UN votes beyond the economic linkage between the Soviet Union and its client states, these results are consistent with the overall "dependency" perspective: peace is assured at the cost of one

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state being subordinate to another. This position is not inconsistent with the logic of the liberal peace, but it is clearly not a state of affairs that proponents of the liberal peace would view with approval.

While the authors above were responding directly to issues raised by the liberal peace, there are related theories of international relations that touch substantially on the same issues. Drawing at least some of the theoretical basis for their arguments from Marxist schools of thought, imperialist and dependency authors argue the international trading system was developed as a way of keeping the rich states rich, and preventing the poor states from getting rich. As such, the trading system has clear political effects on the states that traded depending on the state's position in the economic hierarchy. While these schools do not directly address the issue of the liberal peace, they do discuss interstate conflict.

Schumpeter (1951) and Hobson (1902) concentrate on the economic and political effects of a system of imperialism, and hence do not directly discuss the question of economic links causing or suppressing interstate conflict. Clearly, however, writers of this quasi-Marxist vein feel there is an interaction between economics and politics. The most fundamental difference between Schumpeter and Hobson and the liberal economists who founded the liberal peace theory is the effect of trade on economics. Liberal economists believe that trade is welfare enhancing, operating through comparative advantage – the idea that states can have specific resource, capital or labor advantages in production of specific goods. Thus, Portugal is better at producing wine than cotton, England better at producing cotton than wine, and both nations would be better off (increased overall general welfare) if they concentrated on producing that in which they

had an advantage (England in cotton, Portugal in wine) and traded for what they needed with the other. Thus, trade is welfare creating as it enables a state to gain more cotton and wine (or anything else) than that state could produce alone.

Hobson and Schumpeter, representative of other quasi-Marxist authors, dispute this notion of welfare creating trade and comparative advantage. Where other analysts see a system of free trade and commerce, these authors see an imperialist system: one not designed to serve the general welfare of the state through comparative advantage, but designed to serve the narrow interests of one domestic class using the state apparatus for their own economic gain:

> We have seen that it is motivated, not by the interests of the nation as a whole, but by those of certain classes, who impose the policy upon the nation for their own advantage…. Put into plain language, the theory is this, that any British subject choosing, for his own pleasure or profit, to venture his person or his property in the territory of a foreign State can call upon this nation to protect or avenge him in case he or his property is injured by the Government or by any inhabitant of this foreign state…. The recent habit of investing capital in a foreign country has now grown to such an extent that the well-to-do and politically powerful classes in Great Britain today derive a large and ever larger proportion of their incomes from capital invested outside the British Empire. This growing stake of our wealthy classes in countries over which they have no political control is a revolutionary force in modern politics; it means a constantly growing tendency to use their political power as citizens of this State to interfere with the political condition of those States where they have an industrial stake. (Hobson, 1902 [1978], 356-358.)

Economic interaction (including, but clearly not limited to, trade) creates vested interests within the wealthy state, and economic elites use the state itself to protect their economic interests and economic privilege. Where impending interstate conflict threatens to harm the interests of the dominant domestic economic faction, that faction discourages it. It is not a difficult stretch of logic to see that interstate conflict could be either hindered or encouraged (depending on the individual cases of states, domestic

factors, etc.) by economic ties with other states. Hence, trade can restrain conflict, but not in the mutually welfare enhancing manner of the liberal peace. However, where economic interests of the dominant class in the wealthy state are threatened in the other state, interstate conflict might be preferable as a way of ensuring continued economic access and ownership. Trade, in some cases, might even lead to conflict. This is a similar argument to Hirschman (above), but instead of states dominating states in their own interests, now we have interest groups within the states influencing (through their wealth and domestic political power) state actions, acting to dominate other states in the interest of their group. In either case, Hirschman's or Hobson's argument, there can be peace between states that have significant economic interaction (trade and economic ties beyond trade), but not the mutual peace envisioned by liberal peace theorists.

Schumpeter has a more complicated argument, but one that directly connects economics to interstate conflict. Schumpeter notes that full free trade – where "foreign raw materials and foodstuffs are as accessible to each nation as though they were within its own territory" (Schumpeter, 1951, 99) – is actually associated with less conflict. In this case, no state or domestic interest group has motive for expansion, and thus economics contribute to peace. However, Schumpeter argues, full free trade does not exist. There are always some sorts of protective tariffs that distort the completely free market, and it is through those distortions that economics can be a source of friction for states. Protectionist policies allow the formation of cartels and trusts (Schumpeter, 1951, 104), which change the nature of interests within the country. Cartels can only function behind a protectionist barrier, where they can charge a monopolist price for their goods. The most efficient, and most profitable, forms of enterprises take advantage of economies

of scale (larger production runs lower the price per item). However, according to

Schumpeter (1951, 105), the domestic (protected) market cannot support the high volume

of goods (necessary for economies of scale efficient production) at the monopolist price

(high, to ensure more profit). Thus, cartels are forced to sell the excess production

outside the domestic market, sometimes below cost (dumping – Schumpeter, 1951, 105).

This is the situation in many states – lots of cartels looking for external markets to shore

up their domestic monopolies. Each cartel in each state is forced to compete

internationally to dump the excess product in order to ensure profits. Moreover, trade

barriers to help the cartels based in those states sometimes protect the foreign markets,

much like the domestic one. These struggles among "dumped" products in the foreign

markets are critical for the survival of the cartels and, hence, the economic leaders of the

state (Schumpeter, 1951, 107).

> "In such a struggle among 'dumped' products and capitals, it is no longer a matter of indifference who builds a given railroad, who owns a mine or a colony. Now that the law of costs is no longer operative, it becomes necessary to fight over such properties with desperate effort and with every available means, including those that are not economic in character, such as diplomacy…. What matters is to gain a foothold of some kind and then to exploit this foothold as a base for the conquest of new markets…. Nevertheless, the situation that has just been described is really untenable both politically and economically. Economically, it amounts to a *reductio ad absurdum*. Politically, it unleashes storms of indignation among the exploited consumers at home and the threatened producers abroad. Thus the idea of military force readily suggests itself. Force may serve to break down foreign customs barriers and thus afford relief from the vicious circle of economic aggression. If that is not feasible, military conquest may at least secure control over markets in which heretofore one had to compete with the enemy" (Schumpeter, 1951, 107 – 109).

Schumpeter sees that trade and other economic channels can serve, as the realists argued,

as sources of conflict between states rather than any kind of restraining influence.

Economic relations for Hobson and Schumpeter are not envisioned as welfare-creating links built upon comparative advantage, but as chains that pull states into foreign entanglements – and possibly interstate conflicts – driven by powerful economic minority interests. Here, in this pessimistic view of international economics, economic relations serve as channels of state hostility, causing conflicts of interest between states and potentially leading to serious political conflict or war.

Building on the imperialism of Hobson and Schumpeter, dependency authors like Cardoso and Faletto (1979) argue that the explanation for the underdevelopment of peripheral economies, as well as the dominant/subordinate relationship of social classes within those peripheral economies, lies in global economic relations. Central economies create peripheral economies for their own benefit (see Wallerstein, 1974). Historically, these peripheral economies were colonies of the core, but have since achieved political independence. However, political independence does not equal economic independence, and these newly created dependent states find their political actions severely circumscribed by the central economies. Hence, trade and other economic ties between states serve as evidence of control or influence. Unlike Angell and others, who see trade as a welfare-creating device, Cardoso and Faletto see economic arrangements as means of control.

> The decision by local forces to rebel against colonialism and to create a nation implies an attempt to influence local history according to local values and interests. Economic links with external markets still impose limits to decisions and actions even after independence. The contradiction between the attempt to cope with the market situation in a political autonomous way and the de facto situation of dependency characterizes what is the specific ambiguity of nations where political sovereignty is expressed by the new state and where economic subordination is reinforced by the international division of labor and by

the economic control exerted by former or new imperialist centers. (Cardoso and Faletto, 1979, 21).

While Cardoso and Faletto do not directly discuss the linkage between international economics and interstate conflict, they clearly discuss interstate dominant and subordinate economic relations that affect the states both internally (social class arrangements) and externally. The focus of Cardoso and Faletto is the relationship between internal political institutions and development, but implicit in the argument is the idea that dependent states are constrained by economic subordination. Part of this constraint is the inability of dependent states to choose a course of action that would be damaging to the dominant partner, and if interstate conflict can be assumed to be economically damaging, then the logic that Cardoso and Faletto describe leads to the conclusion that dependent economic relationships could be more peaceful than independent ones. While the economic relationship that Cardoso and Faletto describe says nothing about restraining the potential for interstate conflict for the independent state, restraining half of the relationship from interstate conflict should reduce the number of conflicts overall. Once again, trade restrains interstate conflict, but it is through coercive, dependent economic ties between dominant and subordinate states.

While the authors that reject the liberal peace come from very different theoretical perspectives, they agree that the liberal peace does not function (or, at least, that it may not be liberal). Realists would argue that economic issues are subordinate to security and military concerns for states, and economic ties could serve as means of creating friction and disputes between states. Scholars drawing on Marx (imperialists and dependency authors) point to the inequalities in the international trading system and argue that trading relations create economic dependencies, which in turn create political dependency. For

this school, trade may create peace (it may also create conflict), but it is not a peace based on mutual shared benefits, as the liberal peace argues. This is a peace of unequal power arrangements, where one side controls the tenor of international politics and decisions for peace or war.

In a sense, those that reject the liberal peace are rejecting the interest-based logic that underlies the theory. Realists are arguing that while trade between states may create an interest in maintaining that relationship, other interests – security and power – take precedence (or, according to Hirschman, economic factors can be harnessed in pursuit of power and security), and economic interests are less relevant than other factors when investigating questions of peace and war. Marx-based scholars would agree with liberal peace proponents that economics drive political concerns, but they assert that international economics is competitive (not cooperative). State interests and/or domestic economic interest groups use economic ties to economically and politically dominate other states for their own benefit. Trade does bring peace, but only through subjugation.

2.5 Conclusions

Overall, both proponents and critics of the liberal peace base their arguments on solid logical ground. The interest-based arguments the proponents of the liberal peace use are a logical extension of liberal economic principles that have been accepted for centuries. The interests that critics point to in rejecting the liberal peace also have foundations in widely accepted theories (Realism, Marxism). From a theoretical foundation, the liberal peace is in good shape – perhaps too good. There are two theories (interests and signaling) that support the liberal peace, which can lead to some conceptual confusion. This is not a trivial issue. The economic gains that the interest-based logic of

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the liberal peace points towards are unique to economic relations between states, while the signaling-based logic argues any valuable (costly) relationship can be the basis for signaling to avoid conflict. If the signaling logic is correct, the liberal peace is one part of a more general theory (signaling) of interstate conflict, and economic ties are not unique in how they keep states from engaging in hostilities. Parsing these explanations is critical to the understanding of the liberal peace. Further, the illiberal logic of Hirschman and some of the other critics of the liberal peace is remarkably similar to the interest-based logic of the liberal peace. How can the coercive components of dependent economic relations be understood within the liberal peace framework?

The primary focus of this dissertation is to try and untangle, from an empirical perspective the signaling-based and interest-based explanation of the liberal peace. This dissertation makes no theoretical claims that one, or the other, of the logics is theoretically superior as an explanation for the overall liberal peace. Instead, the focus is on an empirical test to see if there is evidence that states act more like one logic than the other. It is for that reason that a review of each of the logics is necessary.

Beyond that primary purpose, the dissertation also seeks greater understanding of the nuances of the relationship between trade and conflict. As Mansfield and Pollins (2003) call it, finding the "boundary conditions" under which the general finding (trade reduces conflicts) begins to fray. One of the aspects of that investigation is an examination of the theories that reject the liberal peace: as a source for "boundary conditions", examining why some theorists reject the liberal peace is helpful. In this way, the Marxist and Dependency scholars' focus on dependent economic relations is critical to this dissertation, and helpful in a general sense of finding the "boundary

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conditions." Focus on unbalanced and dependent trade is both critical in the primary purpose of this dissertation (explained fully in Chapter 4, the research design) and an interesting investigation in its own right in terms of examining trade and conflict in narrow slices – "boundary conditions". Thus, discussions of the theoretical roots of the liberal peace itself are not out of place in the overall scheme of this dissertation.

Finally, as the introduction makes clear, this dissertation looks at two aspects of interstate conflict – conflict initiation and conflict hostility. In this chapter, I have ignored the distinction, and only discussed interstate conflict generally. This is in keeping with most of the literature on the liberal peace – both theoretical and empirical. However, the distinction between conflict initiation and hostility is important and one that I will address in subsequent chapters.

The next chapter, Chapter 3, examines recent empirical tests of the liberal peace. Where this chapter reviewed the theory, the next reviews the empirical attempts, mostly of the last 25 years, to test the empirical relationship between trade (and/or interdependence) and interstate conflict. In other words, given the theories described above, what is the general consensus of the peace research community on the veracity of the liberal peace?

3.0 Chapter Three: Modern Empirical Tests: Empirical Evidence in Support of the Liberal Peace, and Associated Criticism

3.1 Introduction

This chapter reviews the over twenty years of empirical analysis that seeks to directly test the causal effect of interstate economic relations on patters of interstate conflict. All authors in this section have used the liberal peace thesis outlined above as the theoretical basis for their empirical work (and most have used the interest-based logic as the conceptual hook). Some have added nuances to the theory (these are discussed below), but the authors here are not as concerned with detailing the liberal peace thesis, as testing it. For these authors (at least in the context of this chapter), their primary point is a coherent and logical research design that empirically tests the relationship between trade and interstate conflict, not debates over theories of the liberal peace. The first part of the chapter details the studies that have found evidence in support of the liberal peace (a summary is presented in Tables 3.1 and 3.2). The second section discusses specific criticisms of this line of research, as well as other empirical works that disagree with the conclusion that the liberal peace is operating.

3.2 Empirical Evidence in Favor of the Liberal Peace

While the liberal peace has been written about for centuries, specific empirical examinations of its veracity are relatively recent. Polachek's original (1980) article is generally considered the beginning of the modern debate on the liberal peace, though

there were tests of trade and interstate conflict before that.[34] Overall, the empirical tests

of the liberal peace support the proposition. To be precise, the higher the trade between a

pair of states in a given year the lower the likelihood of an interstate conflict between

those states in the subsequent year. These results are consistent across different research

designs, samples, and temporal domains. There are exceptions and points for debate,

discussed below, but the overall conclusions of the various studies are remarkably similar

(see Tables 3.1 and 3.2, below).

As noted in Chapter Two, Polachek provided a modern theoretical foundation for

examining the liberal peace. In the article most widely cited for its theoretical

contribution, Polachek (1980) examines the level of cooperation versus conflict between

NATO states and Warsaw Pact states during the cold war, and tries to empirically

determine if levels of trade across the Iron Curtain are correlated with periods of

cooperation and if conflict occurs during times of less economic interaction. Polachek

measures trade in simple dollar value (unadjusted for the size of the state's economy or

the state's total amount of trade).[35] His results support the liberal peace in this specific

setting. When the East and West traded, political relations were generally better. When

trading ebbed, politics became more hostile. There are questions of causality in

Polachek's study (did the improving political relations bring increased trade, or did trade

bring more cooperation – as the liberal peace would argue), but the results do support the

liberal peace thesis.

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[34] Richardson (1960) discussed aspects of the liberal peace, as did Russett (1967). Russett's original findings are that trade within regional blocs increases interstate conflict. Russett later goes on to be one of the strongest proponents of the liberal peace, and rejects his earlier conclusions.

[35] Almost all subsequent statistical studies have adjusted the gross value of trade, dividing it by either the total trade or the gross domestic product (GDP) of the state, creating a percentage variable – trade with a partner state expressed as a percentage of overall total trade or GDP. This allows comparison of trade across large and small states without the distortion of the overall economic size of the state affecting the variable of interest.

In a related study, Gasiorowski and Polachek (1982) again look at US/Warsaw Pact relations in light of trade between the blocs. They explicitly link complex interdependence (Keohane and Nye, 1977) to the liberal peace argument, pointing out that, given complex interdependence, states have incentives to work with hostile states to reduce their insecurities. "In empirical terms, we expect an inverse relationship to hold between interdependence and conflict. This inverse relationship occurs because incentives to reduce hostilities arise when a country is sensitive or vulnerable to the actions of another. These incentives follow from a country's need to reduce threats posed by sensitivities and vulnerabilities that may hamper the benefits anticipated under an interaction. In order to maintain these benefits, a country will seek improved relations with actors that can threaten them" (Gasiorowski and Polachek, 1982, 711). Empirically, they show that the greater the trade[36] between the two adversarial sides in the Cold War, the better the political relationship (at least, the better the relationship when defined by a lack of conflict).

For Polachek, this seems to be a step back in terms of clarity from his original (1980) work. By invoking the ideas of complex interdependence, the authors seek to provide an understanding of the connection between trade and interstate conflict that is rooted in political science terms, rather than economics (as is the case in the 1980 article). While the ideas of sensitivity and vulnerability do have resonance in the liberal peace debate, the authors do not sufficiently apply the concepts to give the argument intellectual

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[36] While Keohane and Nye (1977) discuss in detail the differences between sensitivity and vulnerability interdependence, Gasiorowski and Polachek make no attempt to empirically disentangle the two forms of interdependence, relying only on an overall measure of trade. While this is perhaps theoretically weak – why mention specifically the Keohane and Nye idea of complex interdependence if you are not going to attempt to empirically model it? – it is fairly irrelevant for the overall usefulness of the test for the liberal peace research program.

force. How does sensitivity differ from vulnerability in this case? Which interdependence makes for less conflict? Why? Moreover, the quotation above seems to imply that states seek better relations in order to avoid harm (a la Hirschman) rather than the logic put forward by Polachek (1980) where he argues states seek the improved welfare (that results from trade) and so avoid harmful actions. Is Polachek modifying his assumptions about the theoretical basis for trade bringing peace, or just being imprecise? It is unclear. Subsequent authors have reported on the empirical findings in this paper (which, overall, support the liberal peace conclusions) and have generally avoided the theoretical arguments.

Polachek (1992) goes on to expand on the original findings. The original study, as noted, used only levels of trade as a measure of the gains of trade. His theoretical model, however, noted that it is the potential loss of welfare gains from trade (not the trade itself, but the value gained to the state from the trade) that is the motive for states to avoid interstate conflict. Thus, measuring the levels of trade is only partially sufficient, and a better measure would be the actual state gains from the trade, not just the trade itself. This later article does just that, estimating welfare gains from trade through estimates of states' demand elasticities. The results are consistent with his previous study (1980) and also more sweeping. On a dyadic level, the greater the trade (again, measured through demand elasticities), the less the chance of conflict between pairs of states. Additionally, the relationship between welfare gains and chance of conflict holds at a monadic level – the greater the welfare gains from trade for any given state, the less the chance that state will engage in interstate conflict with any other state. Thus, trade seems to damp conflict not only with trade partners, but also in general.

In a subsequent paper, Polachek and McDonald (1992) break down trade between countries into imports, exports, and specific categories of commodities traded. Using only OECD (developed) countries (due to availability of data), they show that, indeed, consistent with previous work, the more trade, the less interstate conflict. Unlike, Polachek (1992), however, monads are not tested, so the results are applicable only to dyads.

Finally, consistent with Polachek's research program, Skaperdas and Syropoulos (1996) use slightly modified, traditional economic models (they add the ability of states to contest territory to an economic model of trade) to theoretically explore the effect of trade on conflict and vice versa. They hypothesize that there can be incentives for states to take territories (engage in interstate conflict) if they commit to not trading after the conflict, but that, *ex post*, interstate trading makes interstate conflict not economically feasible. This, as they note, is consistent with Polachek (1980), as it shows that states that already trade (and, therefore, are presumably open to continued trading) will be less inclined to pursue interstate conflict. Their work, however, remains a theoretical econometrics model without supporting empirical evidence.

Bruce Russett and John Oneal (with a variety of co-authors) have the longest ongoing research program with respect to the liberal peace. In a number of articles and books, the pair of researchers has added an impressive chapter to the field's understanding of the liberal peace. Theirs has been consistently the strongest research program supporting the pacific effects of trade on interstate conflict, and their recent research has expanded to link trade, democracy, and international organization

membership into a "Kantian Peace" with effects greater than any of its individual components.

Oneal, Oneal, Maoz and Russett (1996, 11 - 28) presents the liberal peace as part of the Kantian peace, noting "[e]conomic interdependence reinforces constitutional constraints and liberal norms by creating transnational ties that encourage accommodation rather than conflict" (Oneal et al, 1996, 12) Later in the article, they credit the liberal peace with increasing interest-based activities, arguing that interdependence in democracies may have a pronounced effect, as individuals can form groups to lobby for their interests which are served by maintaining the trade that exists. This line of logic is given a more pronounced place in a 1999 article by Oneal and Russett, which again argues that the liberal peace is part of the Kantian peace. International conflict is avoided because people seek prosperity; trade brings wealth that would not be available without international trade (through the economic idea of comparative advantage), so acting in their own self-interest, people will avoid international conflict that disrupts their prosperity/trade – logic that is not far removed from that articulated by Montesquieu two centuries ago.

Oneal and Russett (1997) serves as the foundation for much of the empirical debate about methods and research design appearing in articles at the end of the decade, and so deserves a closer look. Their theoretical reasoning does not stray far from traditional liberal peace logic. Citing Kant, Mitrany, and others, they note there has long been a tradition of thought behind the idea that trade will bring peace between nations.

The importance of this article is its research design and analysis. The template that Oneal and Russett lay out has been followed by most subsequent researchers, who

have either tested other independent variables in this model in attempts to find

intervening effects for the liberal peace, or argued about case selection and temporal

spans and the effects those have on the empirical results. In this article, Oneal and

Russett limit their analysis to the Cold War era (1950 – 1985), noting this time frame's

increasing trade, large numbers of democracies (they are interested in the "Kantian"

peace, and want to link interdependence and democracy), and existence of important

covariates (alliances, economic development). Unmentioned, but important, is the

existence of reliable trade data (available from the International Monetary Fund) for this

time frame.

Oneal and Russett limit the sample size to "contiguous pairs of states and dyads

that contain at least one state defined as a major power by the Correlates of War (COW)

project" (Oneal and Russett, 1997, 273). Thus, rather than test all possible relationships,

they focus on the subset that is most likely to produce conflict: (1) neighbors, because

they are more likely to have issues over which to have interstate disputes and conflict and

have the ability to reach one another militarily (see Bremer, 1992) and (2) major powers,

because they are the only states with significant potential to conduct military operations

beyond their immediate neighbors.[37] Oneal and Russett's focus on so-called "politically

relevant dyads" separates them from others (notably Barbieri, 1995, 1996 – see below)

who have used the universe of cases.[38]

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[37] This case selection argument closely resembles the opportunity and willingness logic found in Most and Starr (1989).
[38] Barbieri (1995, 1996) does not limit her study to politically relevant dyads, but includes all possible combinations of states. Russett and Oneal have tested both sets of cases (politically relevant and the universe of cases) and found no difference in results, and so have limited case selection in order to reduce the size of the data sets.

The use of "dyad-years," pairs of states observed each year (with each observation treated independently), is consistent with other researchers (Polachek, 1980; Pollins, 1989a, 1989b). The data are structured as pooled time-series cross-sectional data. Each dyad, or pair of states, for each year of the sample provides a separate, independent observation. Mexico-US in 1970 is treated as a distinct and independent observation from Mexico-US in 1971. The data are considered "pooled" because all observations from all times and areas are included in a single data set. The "time-series" descriptor comes from the fact that the observations, while being treated as independent, are part of a series of time-dependent (yearly) observations of a common unit – the dyadic pair of states. The "cross-sectional" descriptor reflects the fact that the sample looks across the selection of subjects (dyads). This detailed description of the form of the data is important, as it becomes the basis for a pair of methodological criticisms of the Oneal and Russett research program.

The dependent variable for Oneal and Russett is binary: the initiation or not of a militarized interstate dispute (MID) between a given pair of countries (the dyad) in a given year.[39] The independent variable of theoretical interest is derived from trade flows between pairs of states. For each state the total trade with a partner is divided by the state's GDP (adjusted for purchasing power parity). This numerical value is a gross measure of dependency: the higher the value, the more a state's entire economy (GDP) depends on trade with the partner state and, presumably, the more economically dependent is the state. The lower the value, the less the state is harmed by the reduction or ending of trade with the partner and, hence, is less dependent. This "dependency"

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[39] Militarized interstate disputes are defined as any state threatening, demonstrating, or using military force against any other state. The articles to refer to are Gochman and Maoz (1984) and Jones, Bremer, and Singer (1996).

measure is calculated for both states in the dyad, and Oneal and Russett use the lower of the states' two scores.[40] Additionally, a number of independent variables are included as covariates. These are measures of state and dyad characteristics that have been found to have significantly affected interstate conflict in previous empirical studies, and are included to see if trade interdependence has an effect that is independent of other characteristics. These variables are: democracy (of both states), lesser of the economic growth rates of the pair of states, alliance between the states, contiguity, capability ratio, and some indicators of political change towards or away from democracy within the dyad.[41]

The model is tested via logistic regression, a statistical approach that allows for categorical dependent variables. The results support the liberal peace: "Higher levels of economically important trade, as indicated by the bilateral trade-to-GDP ratio, are associated with lower incidence of militarized interstate disputes and war, even controlling for potentially confounding, theoretically interesting influences…" (Oneal and Russett, 1997, 288). This article, and the associated research design and methodology, with some revisions served as the basis for other empirical tests of the

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[40] The shortcoming of this approach is that it is impossible to take an inherently monadic concept (the level of dependence of a state) for two states and turn it into a single variable that covers the dyad. Oneal and Russett include the measures of dependency for both states in the dyad in the statistical equation under the "weak-link" hypothesis (see Dixon, 1994, for a complete discussion). The "weak-link" hypothesis is that for any pair of states, if there is to be an action taken (in this case, initiating an interstate conflict), it is the state with the smaller of the values for the variable that is the "weakest link" in the chain. In other words, if high levels of interdependence push states to avoid interstate conflict, it is the state with the lowest value for dependence that is more likely to initiate interstate conflict. As Oneal and Russett note, "…we assume that the less-constrained state has the greater influence on the likelihood of dyadic conflict. The less-dependent state should have greater freedom to initiate conflict because its economic costs would be less and the beneficial influence of trade as communication would be less" (Oneal and Russett, 1997, 275 – 276). The fault with this approach is that there is no dyadic component of the trade actually tested in the model (the same criticism can be made of the democracy score). In other words, this measure informs us of the level of dependence of the least dependent of the pair, but tells us nothing about the relative level of interdependence of the dyad (as a whole). While this does not make the measure, or the test, invalid, it does not allow an understanding of the effects of trade interdependence on interstate conflict.
[41] Most of this list is derived from Bremer (1992).

liberal peace (e.g, Gartzke et al 2001, Hegre 2000) and is consistent with the standard approach to testing not only liberal peace questions, but many other questions relating to interstate conflict as well.

Oneal and Russett (1999a) revisit the liberal peace, responding to specification and methodological issues raised by other researchers with respect to the previous article (Oneal and Russett, 1997). They note that, employing different samples and different methods, other researchers have purportedly found no pacific effect of trade on interstate conflict (Beck, Katz, and Tucker, 1998; Barbieri, 1998). Beck, Katz and Tucker (1998), for example, using a global sample and controlling for "fixed effects" – the authors argue that some dyads are more conflict prone than others, and control for this by including a measure of the length of time since the last interstate conflict for a pair of states – find the pacific relationship between trade and interstate relations disappears, leading to the conclusion that the liberal peace is an artifact of faulty research design in the Oneal and Russett (1997) article. Oneal and Russett respond to this criticism by replicating an empirical study of Barbieri (1998),[42] but differ from her analysis by replacing most missing trade data with zero.[43] This reduces the number of missing cases, and the pacific effects of trade on interstate conflict return. Additionally, Oneal and Russett modify the contiguity measure by including data on dependencies of states (territories, colonies,

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[42] Barbieri's work (1998) is an unpublished conference paper, looking at post-World War II dyads and employing a research design similar to Oneal and Russett's (1997). The important differences were in data – she defined a different set of dependency measures (discussed below). Her overall results were not favorable to the liberal peace.

[43] The authors cite a phone conversation with the International Monetary Fund (the source of the post-World War II trade data), in which the IMF official (Laveda) notes that IMF member states are required to report all imports and exports – if the figures are missing, it is because there was no trade between the states in question, and hence missing data can be replaced by zero trade between any pair of partners.

etc.).[44] Finally, they simplify Barbieri's measures of interdependence (salience and

significance, see below) by coding (using the weakest link assumption) the smallest of

any pair of states' trade-to-GDP ratios. They then include a measure of duration

dependence (recommended by Beck, Katz, and Tucker) via a variable that codes the

length of time since the last dispute.

The Beck, Katz and Tucker (1998) analysis argued that the specific form of the

data used in a previous Oneal and Russett article (1997), as well as that widely used in

large-N statistical conflict studies, suffers from temporally dependent observations.[45]

When these observations are treated as independent, errors occur in the statistical

results,[46] leading to incorrect inferences. The Beck *et al.* methodology to fix this problem

is to include a measure, for each case, of how long it has been since the last interstate

conflict between a given pair of states. Once this is done, their empirical analysis

demonstrates, the liberal peace vanishes. Oneal and Russett, while employing the Beck

et al. methodological correction, reject the logic behind it. Their argument is that trade is

likely to flourish only in the absence of serious disputes, and the longer the time since the

dispute, the greater the chance of increased trade. Thus, trade is likely to be correlated

with any temporal variable that looks at past political misbehavior – like a measure of

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[44] For example, the United States is directly contiguous with Canada and Mexico. However, through
dependencies, the US has been (at one time or another since 1950) contiguous with the Netherlands, Great
Britain, France, Japan, and New Zealand, in addition to Canada and Mexico. Oneal and Russett employ
this expanded definition of contiguity in the 1999 *Journal of Peace Research* article.

[45] A common methodology used in peace research is binary times series cross-sectional analysis (BTSCS).
This method takes every subject (whether a state or pair of states – a dyad) and treats each observation
(almost always a yearly observation) as independent from the subsequent and previous year's observation.
Thus, Mexico observed in 1965 is treated as an independent observation from (and a different data point
from) Mexico in 1964 and 1966.

[46] Temporal correlation in the variables, a consequence of nonindependent observations can reduce the size
of standard errors, meaning that variables may appear to be statistically significant when in reality (i.e.,
with appropriate specification to address temporal correlation) they are not. According to Beck *et al.*,
Oneal and Russett (1997) found significance for the pacific effects of trade as a result of temporal
correlation.

time since the last political conflict, which is just what Beck, Katz, and Tucker introduce. Hence, any analysis that includes a temporal correction variable (correlated to trade) is likely to reduce the statistical importance of trade within the model (as Beck *et al.* found). Oneal and Russett, nevertheless, include a temporal correction variable within their model and still find a pacific effect of trade on interstate conflict. Overall, Oneal and Russett (1999a) are able to reject the methodological and research design criticism of Beck *et al.* and Barbieri, and find renewed support for the liberal peace.

Oneal and Russett (1999c) again address the liberal peace in a 1999 *World Politics* article. Using the familiar research design they extend the temporal domain back to 1885. They also include a measure of joint intergovernmental organization membership (IGO), as well as systemic measures related to realist assumptions about the nature of interstate conflict. Finally, instead of coding the dependent variable as the presence or absence of the initiation of an interstate conflict, the authors code whether there is a dispute newly initiated or ongoing between the pair of states in the dyad in a given year. Thus, multi-year disputes are counted every year they go on, not just once (the initiation year), as was done before, thereby increasing the observed number of conflict occurrences. Oneal and Russett cite Blainey (1973), and offer his argument that the same causal logic that leads states to initiate/engage in a dispute is present in decisions to continue (or escalate) the dispute, and hence the inclusion of every year of multi-year disputes as an independent observation is valid.[47] Their results continue to

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[47] This specification of the dependent variable is problematic. As Beck, Katz, and Tucker (1998) point out in criticism of the previous Oneal and Russett model (1997), the presence of temporal dependence can distort standard errors in logistic regression and lead to false significance for variables. By including all years of interstate conflict as independent observations, Oneal and Russett would seem to be allowing the same criticism to be made, since the continuation of an interstate dispute is clearly temporally linked to its onset. Thus, while Oneal and Russett may have good logic for the inclusion of all years of interstate conflict, there are serious methodological questions about this specification of the variable. However, as

uphold the pacific effects of trade on interstate conflict, and not just in the post World

War II era.

A second round of methodological criticism was leveled at the Oneal and Russett

research program by Green, Kim, and Yoon (2001). Their argument is that the form of

analysis Oneal and Russett (and many other empirical researchers) have employed is

flawed. Pooled time-series cross-section analysis fails to account for the specific effects

of particular dyads ("fixed effects"). "For example, year after year, trade levels between

India and China fall below what one would expect on a regression model that takes into

account population size, gross domestic product (GDP), and shared borders [a standard

econometric model that accurately predicts trade]. Because such a model fails to take

note of the Himalayas, economic endowments, linguistic dissimilarities and diplomatic

relations, this model repeatedly overestimates bilateral trade between India and China,

just as it consistently underestimates trade between Belgium and Switzerland. Pooling

data implicitly assumes that the independent variables eliminate these persistent cross-

sectional differences, or render them uncorrelated with the predictors in the model. In

this example, the fact that India-China differ in unmeasured ways from Belgium-

Switzerland makes this assumption implausible" (Green, Kim, and Yoon, 2001, 442).

In other words, each dyad has specific characteristics that the independent

variables cannot measure. Forcing all the individual dyads to be part of a common

equation assumes that all the important differences between those dyads are accounted

for in the independent variables used in the equation. This, the authors argue (as the

above quotation describes) is not appropriate. While this criticism is similar to the

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Oneal and Russett report, two versions of the dependent variable were used in the analysis (initiation only
and initiation plus involvement) with no change in overall results. Thus, while the decision to use
involvement instead of initiation may be suspect methodologically, it made no substantive difference.

argument by Beck, Katz, and Tucker (1998), it goes beyond the limited arguments made there. Beck, Katz, and Tucker were arguing that the Oneal and Russett (1997) data omitted critical information by not including a reference to previous conflict, but that once this information was included, dyads were comparable (and the analysis could proceed). Green *et al.* argue that, even with this information, dyads contain unique features that make them inherently incomparable, and researchers must go further than taking into account past conflicts.

They specifically suggest employing dummy variables for each individual dyad in the time-series. This allows the unique aspects of each pair of states to be expressed and understood in the overall equation, and allows a better understanding of the independent variables. In practice, this means that each pair of states would have a separate intercept point, a different "baseline" with respect to the dependent variable. This, the authors note, is a "fixed-effect" model, and is used frequently in econometrics and other areas of political science. Using Oneal and Russett's data (1999c – the data updated to respond to the Beck, Katz, and Tucker dispute), Green *et al.* re-analyze the liberal peace using fixed effects and find no effect of democracy or trade on suppressing interstate conflict.

Oneal and Russett (2001) respond. They note that Green *et al.* limit their analysis to the post-war years (1950 – 1992), rather than a longer temporal span (1885 – 1992; the length of time used in the 1999c article). This seemingly minor difference is, in fact, crucial to the results that Green *et al.* derive, and when the entire temporal span is used, the democratic peace and liberal peace re-appear in the empirical analysis. The key to this change is in how "fixed effect" models operate. Fixed effect models assume variation on the dependent variable for each independent effect (in this case, for each pair

of states). Without that variation, each independent effect has no impact on estimating

the likelihood of the dependent variable – in practice, if there is no variation on the

dependent variable for an effect (an independent variable), the entire dyad is dropped

from the overall equation. For the liberal peace analysis of Green *et al.*, this means that

each pair of states that fails to have an interstate conflict contributes nothing to the

overall equation, which Oneal and Russett claim leads to the spurious results reported by

Green *et al*. By lengthening the temporal span, Oneal and Russett (2001) allow for more

pairs of state to experience an interstate conflict, and hence be included in the overall

empirical test. As Oneal and Russett note (2001, 8), Green *et al.* included 201 dyads in

the overall analysis, while Oneal and Russett's temporally longer domain has 388 dyads,

almost double that of Green *et al*. This simple change in model specification allows

Oneal and Russett to show again that trade has a substantial effect on interstate conflict.

Finally, Russett and Oneal (2001), in a book length treatment of the liberal peace

review their findings and report that, indeed, the liberal peace operates. "[T]he pacific

benefits of trade are not limited to the countries normally considered liberal – the

Western democracies – or relations among them. Countries that are interdependent

bilaterally or economically open to the global economy, whether democratic or not, have

an important basis for pacific relations and conflict resolution" (Russett and Oneal, 2001,

155). As an additional examination of the liberal peace, they include a Granger causality

test (2003) of their basic model (1999a, 2001), which statistically demonstrates that trade

causes both a reduction in militarized conflict and fatal militarized conflict (where there

was at least one military fatality).

Oneal and Russett, through a number of different research designs and articles, have consistently been the strongest proponents of the liberal peace. Time and again they have found a strong positive connection between trade and decreased interstate conflict, and have been quick to respond to methodological criticisms.

The most credible empirical test of the liberal peace that finds some evidence against the pacific effects of trade is Barbieri (1995, 1996). Barbieri expands the overall liberal peace debate by significantly pushing the data back in time (which was subsequently done by other researchers, see Oneal and Russett, 1999c, above) and adding a more nuanced interpretation of trade. The theoretical foundation of her work breaks no new ground. Drawing upon various theoretical paradigms, Barbieri posits competing predictions concerning the effect of trade on conflict: the realist view that trade increases interaction and, thereby, the possibilities for conflict; the liberal view that trade reduces conflict; and a Marxist/dependency view that the inequitable trade associated with dependency can result in a lack of conflict because dominant states have no interest in a conflict, and dependent states pay too high a cost in economic disruption in a conflict.

On the empirical front, however, Barbieri attempts to measure the level of interdependence or dependence of states in two ways: the significance of trade, and the salience of trade – an approach reminiscent of Keohane and Nye (1977). She theorizes that high levels of interdependence in a dyad will reduce the amount of conflict, as traditional liberal theorists have written. Unlike most other empirical tests of the liberal peace, Barbieri's findings are more equivocal. Very high and low levels of interdependence result in decreased conflict, but intermediate levels of interdependence

are associated with slightly increased levels of conflict. That is, interdependence and peace are related in an "inverted-U" shape.

Barbeiri's is one of the few, and the best, empirical study that does not find overall support for the liberal peace hypothesis. As such, it deserves a brief methodological discussion. As noted, the study creates a measure of interdependence by examining both the salience and significance of trade for a pair of states.[48] The measure, a combination of salience and significance, has some properties that require explanation. Barbieri has created a measure of the amount of interdependence (high or low) in a dyad from measures of the level of dependence of individual states. When her measure of dyadic interdependence is close to its theoretical maximum, each state in the dyad is dependent on the other. When the measure is near its theoretical minimum, its meaning is less clear. If one state has a measure of dependence near zero (by Barbieri's formulas), then any amount of dependence by the other state will result in a dyadic interdependence score near zero. Thus, a dyadic measure near zero only provides information that one of the pair is not dependent; it offers no information about the other state. Additionally, it is not clear what mid-levels of dyadic interdependence mean. Is it that each state is partially dependent? That one state is very dependent, while the other is not? The dyadic interdependence measure cannot distinguish the situation in which two states are moderately and comparably interdependent from many asymmetrically dependent

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[48] The study uses trade share (imports and exports from the partner divided by total trade) instead of trade levels (trade divided by GNP or GDP). There are strengths and weaknesses to either approach, which I will discuss below. The measure of salience multiplies the trade share of each state together and then takes the square root of that number (the resulting measure ranges between 0 and 1; when trade shares are high for both states it is close to 1, when low for both states it is close to 0). The measure of symmetry looks at the difference in trade shares between the states: 1 - |TradeShareStateA – TradeShareStateB|. The symmetry measure also ranges between 0 and 1, being close to 1 when both states have similar trade shares and 0 when they do not. Interdependence is defined in the study by multiplying salience and symmetry. I include this somewhat detailed description of the variables so that my discussion of the study, which hinges on the variables, is complete.

relationships (where one state is very dependent and the other is relatively independent). While Barbieri's attempt at a genuine measure of interdependence has these problematic properties, the attempt to move beyond interdependence and examine the effects of economic dependence is welcome. As discussed in the preceding chapter, trade dependence, as well as interdependence, may bring peace (see Hirschman's discussion of the Eastern European economies just before World War II), although not the kind of peace envisioned by liberal peace theorists. Barbieri's study explicitly attempts to model and analyze the effects of dependence versus interdependence on interstate conflict, and she should be commended for that.

The weight of empirical evidence, however, is against her conclusions. Subsequent studies have adopted Barbieri's temporal span and criticized her choice of the universe of cases (as opposed to politically relevant cases). Oneal and Russett (1999c) is in every respect the same research design as Barbieri, minus the salience/symmetry/interdependent component (preferring instead the weakest-link assumption) and selecting only politically relevant states. Their findings do not support Barbieri's conclusions, although subsequent researchers have not specifically tested to see if interdependence has a non-linear effect (as Barbieri's inverted-U result would indicate).

Barbieri has gone on to expand her basic research, and further investigate the liberal peace (2002). Her extended findings continue to support her original results: empirical support for the liberal peace does not exist. The later work extends her original data by testing a GDP based "economy dependence" term similar, if not identical, to the variable used in the Oneal and Russett (1999c, 2001) studies. Barbieri's findings are

consistent with her earlier work (1996), and exactly the opposite of Oneal and Russett (2001): higher levels of interdependence are correlated with higher probabilities of interstate conflicts. These results are consistent across both the pre-World War II period, and the Cold War era. Additionally, higher levels of trade (among states already involved in interstate conflicts) are associated with an increased chance of conflicts escalating to war. Evidence in favor of trade as a restraining influence in interstate conflict also exists: Barbieri finds that increased trade is correlated with a higher probability of negotiated settlements as well as lower numbers of battle fatalities by states already involved in some form of interstate conflict. Finally, she finds some support for the monadic effect of trade to reduce interstate conflict, with empirical results showing that states with a high measure of "trade dependence" (total trade divided by GDP) have a significantly lower probability of engaging in interstate conflicts. This monadic result is the best evidence Barbieri can muster in favor of the liberal peace. Her findings, while somewhat overtaken by other researchers, remain a single, troubling result for proponents of the liberal peace.

The preceding studies focus almost exclusively on dyads – pairs of states – as the unit of analysis. There are studies that have examined the effects of trade on interstate conflict for individual states (Does trading with other states, in general, make for a more pacific state?), as well as for the international system as a whole (Does more trade in the world create less interstate conflict in the world as a whole?). While not the focus of the empirical liberal peace research program, which tests primarily the effects of trade on pairs of states, these are clearly related empirical questions.

Mansfield's *Power, Trade and War* (1994) is a systemic level treatment of trade and war (not general interstate conflict, as in most of the empirical studies discussed above). While none of the original liberal theorists of the nineteenth century envisioned a systemic role for the liberal peace, some more recent international relations theorists argue that increased trade will reduce the number of interstate conflicts in the world by changing international norms (Haas, 1958; Rosecrance, 1986; see discussion above). Mansfield does not, himself, make this norms-based argument.

Mansfield finds very strong evidence for the years in his study (the nineteenth and twentieth century) that, at the systemic level, cycles of trade and peace coincide. Moreover, trade proves to be a significantly better predictor of peace than other alternative predictors such as polarity, power concentration, or open/closed economic trading system. The most serious failing of Mansfield's study is that the entire investigation is framed at the systemic level, meaning that his results are not necessarily translatable to individual state behavior. From Mansfield we know that as worldwide trade rises, international war (in general) falls, but we do not know if those states most involved in trading are the states most responsible for the (diminishing) number of wars. Mansfield's choice of level of analysis undermines our ability to answer this question.[49]

Domke (1988) is one of the few recent analysts of the relation between interstate economic activity and interstate conflict who works from a monadic perspective – trade for a state results in a more pacific state, overall. His reasoning follows almost directly

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[49] A troubling aspect of Mansfield's research is his choice of data on global trade from an East German economist at the height of the Cold War, raising questions about the data's validity. While this does not automatically make the data from Kuczynski (1980) invalid, some efforts should have been made to determine whether the data from a government sponsored East German economic institute had some bias or fault (either accidental or deliberate). Kuczynski's work has never been translated into English, thus for those who do not understand German it is difficult to judge the value of his methods and sources. Mansfield does not discuss this issue.

from the liberal peace theorists of the nineteenth century. "Because the hypothesis advanced here – that foreign trade produces a constraint on decision for war through the growth of international, domestic, and governmental forces with a stake in open and unfettered foreign dealings – the relevant indicator of foreign trade would measure an economy's involvement in trade" (Domke, 1988, 118). This is a fair, if short, restatement of the logic of the liberal peace. This logic follows from Rosecrance (1986), whose arguments about "trading states" forecast a wholesale change in international politics. Rosecrance argued for a monadic effect of trade on interstate conflict, and Domke's empirical study allows for some test to be made of Rosecrance's hypothesis.

Domke posits three ways in which economic interests are linked to interstate conflict. First, he employs a standard liberal peace argument that governments see the benefits to the state at large from an increase in trade, and will resist activities and policies that will damage that benefit. Second, the domestic interests engaged will act politically to keep their benefits (again, consistent with the liberal peace hypothesis, though it works through domestic actors rather than decision makers). Third, the nature of national security shifts as trade becomes a more important component of the national economy and the economy becomes a critical component of national security. The state will seek to manage the national economy in order to preserve national security and keep trade relations stable (consistent with the logic in Rosecrance, 1986, and the ideas of political realism presented in the previous chapter).

Domke's empirical test is not a traditional one, and deserves mention for that reason. The level of analysis is monadic – individual states – rather than dyadic, as is more common. He looks at the level of exports from each state (divided by GNP) in year

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T and examines whether the state opts for war in year T + 1, using data from 1870 to 1975. While his findings are not conclusive, he generally finds that the lower the level of a state's exports, the greater the chance of war the following year. Domke's finding provides some support for a monadic effect of trade on interstate conflict, consistent with Rosecrance (1986) logic. No other researchers have investigated this monadic effect. Domke's results suggest that it may be worth pursuing, a task partially done in this dissertation (see below).

The conclusions of these and other researchers who have examined the liberal peace through large-N statistical techniques are summarized in Tables 3.1 and 3.2. Each entry in the tables is a separate study of the liberal peace, including many not discussed above. There is no intent to slight omitted authors. Rather, for the sake of brevity, studies that were not significantly different with regard to theory or research design have not been discussed. Most of the additional studies test the liberal peace by incorporating independent or intervening variables that might affect the relationship between trade and conflict. The two tables are a complete list of published empirical tests of the liberal peace. Table 3.1 lists the temporal domain, level of analysis (almost always dyadic), type of interstate conflict (dependent variable), and type of trade or interdependence variable employed in each study. Table 3.2 lists, briefly, the overall conclusion of the study with respect to the liberal peace hypothesis. For purposes of readability, neither the type of empirical test employed (logit, probit, MLE, etc.) nor a list of control variables incorporated in the studies appears in Tables 3.1 or 3.2.[50]

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[50] See Barbieri and Schneider (1999) for a similar table that does include a specific list of covariates included in each analysis. The idea for these tables was taken from Barbieri and Schneider (1999), although I have changed the contents of the table entirely.

Table 3.1: Empirical Tests of the Liberal Peace – Geographic and Temporal Spans, and Source of Variables of Theoretical Interest

Author, Cite	Temporal Span	Level of Analysis	Dependent Variable	Economic Variable
Sullivan, 1974	1955 to 1957	Dyadic (Sample of 29)	Events Data (Rummel)	Trade/Total Trade
Gasiorsowski and Polachek, 1982	1967 to 1976	Dyadic (NATO/Warsaw Pact)	Events Data (COPDAB)	Trade
Polachek, 1980	1958 to 1967	Dyadic (Sample)	Events Data (COPDAB)	Trade
Gasiorsowski, 1986	1960 to 1977	Monadic	Events Data (COPDAB)	Trade/GDP
Domke, 1988	1870 to 1975	Monadic	War (COW)	Exports/GNP
Sayrs, 1989	1950 to 1975	Dyadic	Events Data (COPDAB)	Trade/GNP and Trade/World Trade
de Vries, 1990	1950 to 1960	Dyadic (US/Europe)	Events Data (Azar)	Trade/Total Trade
Polachek, 1992	1948 to 1978	Monadic, Dyadic	Events Data (COPDAB)	Modeled Gains from Trade
Mansfield, 1994	1855 to 1964	Systemic	War (COW, Levy)	Exports/Total Production
Barbieri; 1995, 1996	1870 to 1985	Dyadic	MID	Trade/Total Trade
Oneal, Oneal, Maoz, Russett, 1996	1950 to 1985	Dyadic (Politically Relevant)	MID	Trade/GDP
Reuveny and Kang, 1996	1960 to 1990+/-	Dyadic (Sample of 16)	Events Data (COPDAB/WEIS)	Trade/Total Trade
Polachek, 1997	1958 to 1978	Dyadic (30 Country Sample)	Events Data (COBDAB)	Exports
Reuveny and Kang, 1998	1960 to 1990+/-	Dyadic (Sample of 16)	Events Data (COPDAB/WEIS)	Trade/Total Trade
Gartzke, 1998	1950 to 1985	Dyadic	MID	Trade/GNP
Polachek, Robst, and Chang, 1999	1958 to 1967	Dyadic (30 Country Sample)	Events Data (COBDAB)	Exports, Imports
Heldt, 1999	1950 to 1990	Dyadic (Territorial Disputes)	MID	Exports/GNP
Oneal and Russett, 1999a	1950 to 1992	Dyadic (Politically Relevant)	MID	Trade/GDP
Oneal and Russett, 1999c	1885 to 1992	Dyadic	MID Involvement	Trade/GDP
Hegre, 2000	1950 to 1992	Dyadic (Relevant Dyads)	MID (Fatal Disputes)	Trade/Total Trade; Trade/GNP
Bennett and Stam, 2000	1885 to 1984	Directed Dyads and Dyads	MID	Trade/GDP
Mousseau, 2000	1950 to 1992	Dyadic (Sample and Full)	MID	Trade/GNP
Sherman, 2001 (APSA Paper)	1950 to 1990	Dyadic	MID	Trade/GNP with predicted conflict
Oneal and Russett, 2001	1885 to 1992	Dyadic (Politically Relevant)	MID (Onset and Involvment)	Trade/GDP
Barbieri, 2002	1870 to 1992	Dyadic, Monadic, Systemic	MID	Trade/Total Trade and Trade/GDP
Reuveny and Li, 2003	1950 to 1992	Dyadic (Politically Relevant)	MID	Trade/GDP
Jungblut and Stoll, 2002	1950 to 1978	Dyadic	MID and COBDAP	Trade/GDP
Hewitt, 2003	1918 to 1992	Dyadic	ICB and MID	Trade/GDP
McDonald, 2004	1960 to 2000	Dyadic	MID	Trade/GDP and Free Trade
Hegre, 2004	1951 – 1992	Directed-Dyads	MID	Trade/GDP and GDP Asymmertries
Kim and Rousseau, 2005	1960 – 1988	Directed-Dyads	Rousseau's Conflict Data	Trade/GDP

Table 3.2: Empirical Tests of the Liberal Peace: Comparison of Results

Author, Cite	Result
Sullivan, 1974	Increased Trade correlates with increasing cooperation.
Gasiorsowski and Polachek, 1982	Increased Trade correlates with decreased conflict.
Polachek, 1980	Increased Trade correlates with decreased conflict
Gasiorsowski, 1986	Increased Trade correlates with decreased conflict with trading partners as a group
Domke, 1988	Increased Exports correlates with decreased war
Sayrs, 1989	Increased Trade correlates with fewer conflictual actions; No effect when primary state has high volume of trade and target state has low volume of trade
de Vries, 1990	Increased Interdependence (trade and other interactions) correlates with more intense similar activity – trade and other peaceful actions
Polachek, 1992	Increased Trade correlates with fewer conflictual actions
Mansfield, 1994	Increased Trade correlates strongly with decreased incidence of war.
Barbieri; 1995, 1996	Mixed: Very high and very low levels of "interdependence" associated with lower interstate conflict; mid-levels of trade associated with increased conflict
Oneal, Oneal, Maoz, Russett, 1996	Increased Trade correlates with decreased interstate conflict.
Reuveny and Kang, 1996	Causality test: Sometimes trade causes a lack of conflict, sometimes a lack of conflict causes trade.
Polachek, 1997	Increased Exports correlates with increased cooperation; effect independent of democracy
Reuveny and Kang, 1998	Commodity specific trade and causality test: some commodities are more strongly associated with bringing pacific relations than other
Gartzke, 1998	Increased Trade correlates with less interstate conflict (modified version of Oneal data)
Polachek, Robst, and Chang, 1999	Increased Exports, Increased Imports correlate with increased cooperation; Foreign Aid and trading with a larger economy independently reduces conflict
Heldt, 1999	Increased Exports correlates with fewer military conflicts over disputed territory.
Oneal and Russett, 1999a	Increased Trade correlates with decreased interstate conflict; when all dyads tested, effect disappears.
Oneal and Russett, 1999c	Increased Trade correlates with decreased involvement in interstate conflict, including effects of IGO membership and systemic variables
Hegre, 2000	Increased Trade correlates with decreased interstate conflict, but only for developed economies.
Bennett and Stam, 2000	Increased Trade correlates with decreased interstate conflict initiation
Mousseau, 2000	Increased Trade correlates with decreased interstate conflict, but interaction of trade and development increases interstate conflict
Sherman, 2001 (APSA Paper)	Increased Modified Trade correlates with fewer MIDs
Oneal and Russett, 2001	Increased Trade correlates with decreased interstate conflict initiation; Kantian Peace
Barbieri, 2002	Increased Trade correlates with increased dyadic conflict but lowered monadic conflict
Reuveny and Li, 2003	Increased Trade correlates with decreased conflict in simultaneous equations
Jungblut and Stoll, 2002	Increased Trade Correlates with decreased conflict at quarter year intervals and controlling for conflict short of MIDs
Hewitt, 2002	Replicates Oneal and Russett (2001) with ICB crises data instead of MIDs, finds same result
McDonald, 2004	Free Trade better predictor of pacific relations than Trade/GDP
Hegre, 2004	While Trade decreases Conflict, Asymmetric Economic Size and Trade increase Conflict
Kim and Rousseau, 2005	When Trade and Conflict are tested simultaneously, Trade is not significantly related to Conflict

The purpose of Tables 3.1 and 3.2 is to clearly and briefly indicate several points.

First, the liberal peace has substantial empirical support. Only a few studies (notably

Barbieri, 1995, but also Reuveny and Kang, 1996, to some degree) find evidence refuting

the liberal peace.[51] Hence, while not universal, there is substantial evidence that trade

does reduce conflict. Moreover, this evidence exists across two temporal spans (a

lengthy historical period - 1885 to 1992 - as well as the Cold War period), multiple

versions of the dependent variable (events data and aggregated yearly counts

observations based on the MID data), and two versions of the independent variable (trade

measured as percent of total trade and as percent of total economy). It is this almost

unanimous result of the empirical tests that allows liberal peace proponents to argue that

the liberal peace exists as surely as the democratic peace.

Second, most quantitative analyses of the liberal peace rely on the same data

sources. While Tables 3.1 and 3.2 list upwards of twenty studies that support the liberal

peace, the data for the studies are drawn from a handful of data sets. Measurement of the

dependent variable (interstate conflict) derives primarily from two or three sources and

measurement of the independent variable relies heavily on one or two sources.[52] One

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[51] As discussed above, there are several articles (Beck *et al*, 1998; Green *et al*., 2001) that have
methodological critiques focused on the Oneal and Russett (1997, 1999) research program. These articles
find the liberal peace has no empirical support, and for that reason could be included in Tables IA and IB.
However, since these empirical tests add no new independent variables, and are rebutted by Oneal and
Russett (1999c, 2001), and in a sense rejected, they have not been included in the tables.

[52] For all but a few of the studies listed in Tables 3.1 and 3.2, the measurement of interstate conflict comes
from one of three research programs. The COPDAB (covering 1948 to 1978) and WEIS (1964 to 1994)
data are collected from news reports (public sources) of cooperative and conflictual actions taken by states
against other states. These are "events data" that record and categorize events in international politics
according to their degree of cooperation/conflict. While the two events-data sets use different coding rules,
they are far more similar than they are different. The MID data are collected primarily from historical
sources (though public news sources are used as well) and focuses specifically on militarized threats and
uses of force between states (no attempt is made to measure any kind of cooperative actions). Other than
some of the early liberal peace studies (Sullivan, 1974, for example), all subsequent empirical tests of the
liberal peace have used as the dependent variable one of these three sources. With respect to the
independent variable, the data are even more limited. All post-WWII trade data come from the

could argue that instead of twenty studies that support the liberal peace, there are

essentially two studies[53] repeated a number of times and conclude that it is premature to

close the book on the liberal peace. There are variations among the empirical studies

(different temporal spans, different combinations of covariates, different statistical tests,

for example), but fundamentally the same data sources for trade and interstate conflict

have been employed again and again. In that respect, the evidence in support of the

liberal peace is not so substantial.[54]

Third, the statistical empirical evidence to date runs counter to the realists'

argument (see above) that there is a minimal connection between trade and interstate

conflict. The realists' argument, like that of the interest-based dominant liberal peace

theorists, is based on interests. Liberal peace theorists argue that interests in the gains

from trade, when the trade is significant, can restrain state's actions. Realists counter that

lesser "interests," like economic considerations, do not affect state decisions for war and

peace, and hence there is little direct connection between trade and interstate conflict.

The evidence presented above suggests the realists are wrong. In the question of whether

economic interests are as significant (at times) as considerations of power and security,

the answer seems to be yes.

As noted in the introduction, what remains unanswered by the evidence above are

key questions of causality. Which theory of the liberal peace (interests or signaling)

better describes the logic that drives states to avoid interstate conflict? What *causes* the

International Monetary Fund (except for non-IMF members). Pre-WWII data come from the League of
Nations archives. All empirical tests use these data.
[53] One study correlates trade data with events data (COPDAB/WEIS); the other uses the same trade data
and tests them against MID data.
[54] Proponents of the liberal peace would rebut, not reject, this argument by noting the consistency, strength
and statistical significance of empirical findings.

liberal peace? In other words, given the correlational evidence that trade and interstate conflict are related, causal evidence of how trade creates motives and actions that work to reduce and end interstate conflict is necessary. It is the lack of empirical evidence that speaks directly to causal connections between trade and pacific relations that drives much of the general criticism of the liberal peace research program. Additional criticism focuses on the reliability and validity of indicators and measures employed in tests of the liberal peace thesis. While the weight of statistical evidence is in favor of the liberal peace, there are still questions about that association.

3.3 General Criticism of the Liberal Peace Research Program

Criticism of the empirical studies of the liberal peace breaks into two camps. Some criticize the specific methodology or statistical approach of those that find support for the liberal peace (discussed above); other critics argue that the research designs of the empirical studies that purportedly support the liberal peace fail to adequately test the logical and theoretical foundations of the liberal peace. Most of the statistical criticism was discussed above. This section of the chapter focuses on criticisms of the general research design of liberal peace studies.

It should be noted up front that this review of the criticisms of the liberal peace research program is general, and not all criticisms will be addressed in this dissertation. The attempt here is to outline the extent of the objections to the studies in the previous section, and discuss why so many liberal peace scholars find the statistical results incomplete or insufficient (of course, many find the results outlined in the previous section compelling). Some of the laundry list of objections are contradictory: objections that argue statistical methods are fundamentally insufficient (e.g., because large-N studies

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omit the domestic actors and interest groups that are fundamental to the liberal peace; Jungblut, 1999) are incompatible with objections that argue the statistical approaches need to use additional variables (e.g. alliance patterns, as Gowa, 1994, argues) to account for systemic-level influences. Or, more fundamentally, that large-N statistical studies are incapable of testing the links (within the states and among the decision-makers) that connect economic relations to conflict behavior versus the idea that only statistical studies offer sufficient scope to genuinely test the liberal peace. The body of criticisms discussed here is not an alternative research program, or even a coherent set of criticisms (as noted, some criticisms are contradictory). Reviewing the criticisms serves the larger purpose of showing how this dissertation advances, incrementally, the study of the liberal peace by showing which criticisms are discussed and tested here. This dissertation is not "revolutionary science" in the Kuhnian sense; the "normal" paradigm is not (in my opinion) in need of revolution or replacement (and there would be debate about what the traditional paradigm is: the liberal peace? All of conflict studies? All of political science?) in need of wholesale rejection. Using the criticisms already advanced by peace researchers of the liberal peace to build further tests of the relationships between economics and conflict is part of the evolutionary aspects of political science and peace research. An important goal in the scientific process is comparability – new studies must take into account the methods and practices of older studies, while moving the investigation forward, and incorporating relevant criticisms. This incremental approach argues against wholesale revisions of established practices (unless established practices fail to explain large parts of the empirical record and the field is ripe for a Kuhnian "revolution", which is not, I think, accurate here), and instead looks to the gradual

accumulation of knowledge that leads inevitably to greater knowledge. As Gartzke put it more poetically, "Science is a perpetual learning process, in which we gradually whittle away at uncertainty." (Gartzke, 2005) Attempting to remain consistent to previous liberal peace research argues against wholesale research design changes in response to critics (who, after all, are themselves rejected by the researchers of the previous section who find nothing wrong with their research designs). At the same time, this section is not simply a checklist of "fixes" offer by previous liberal peace critics that will be undertaken to "improve" the liberal peace findings. Rather, I discuss the arguments and objections of the critics, and incorporate some of them into the research design presented in Chapter 4. Thus, the research design for the dissertation (Chapter 4) is a compromise between past efforts (for comparability), unique and new methods (needed to answer the specific questions that are the focus of this dissertation), and responses to valid criticisms.

The critique that Sayrs (1989) offers, while more than a decade old, still provides a good outline for a more general discussion of alternatives to the empirical approaches discussed above and generates questions about how extensive our knowledge of the liberal peace genuinely is. Sayrs offers a direct criticism of the assumptions Polachek (1980) uses that serve as the foundation for the most recent research on the liberal peace. Sayrs does not deny that trade can create interests – social welfare gains that states can see in increased consumption and general public goods within the state – but she argues that the connection between trade and interstate conflict should be examined by a more nuanced model that includes politics. "In the absence of an explicitly modeled social welfare function that includes the risk and uncertainty inherent in politics, the precise form of political action that arises from trade is difficult to discern. Deterrence,

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compellence, dominance and compliance become blurred while the trade dilemmas which give rise to these behaviors become blended rightly or wrongly with public goods (Conybeare, 1984). The result is underspecification, misspecification, and incorrect predictions about the way in which trade inhibits or enhances social welfare derived from international conflict and cooperation" (Sayrs, 1989, 170).

In other words, because trade generates all sorts of questions about social welfare, domestic economic growth, jobs, security concerns and other political questions, a more precise model of the political aspects of trade and how states in general confront the trade/conflict/foreign policy nexus is needed before our understanding of the liberal peace can be known. This model and associated understanding are missing from work on the liberal peace.

Specifically, Sayrs (1989, 156) notes that: (1) a greater understanding is needed of the actions of subnational actors (firms, bureaucracies, individuals, etc.) who are actively involved in the trade between economic partners; (2) interstate conflict is a process, and political decisions are made in a context of information about past actions and conduct; (3) trading and economic decisions are part of an overall foreign policy and not made in a political vacuum; and (4) conflict and cooperation are not mutually exclusive. These four points serve as a detailed condemnation of the liberal peace research, though some of the points have been addressed by subsequent literature.

Sayrs' own statistical work indicates that while, for her sample, overall trade reduces overall interstate conflict, a more detailed examination shows that when a high volume trading state is paired in a dyad with a low volume trading state, there is no effect of trade on interstate conflict (i.e., no liberal peace effect). While her empirical analysis

cannot explain these results, she points to her discussions of the limits of our understanding of the how trade affects politics as a starting point for an explanation.

Sayrs' four objections to liberal peace research serve as a good outline for discussion of other theoretical and empirical examinations of the nexus of trade and interstate conflict. She is not alone in finding the theoretical development of the liberal peace research program lacking. Thus, these four criticisms serve as a framework by which to discuss the criticisms of other authors.

The largest body of criticism is focused on the first objection – that researchers have failed to understand the causal linkage between domestic and international politics. The argument that foreign policy and decisions for or against war are influenced by domestic political considerations is not new. Those who propound this position argue that we need to move beyond simple specifications such as "trade brings conflict" or "trade restrains conflict" in favor of more complicated, but historically accurate, examinations of states' particular political structures and political party relationships to determine how and why states make foreign policy choices. In other words, while trade creates welfare gains (as Polachek, 1980, argued), it does not necessarily do so evenly for all individuals or interest groups throughout the state. If the gains from trade are uneven within the state, then the linkage between trade and foreign policy that defines the interest-based logic of the liberal peace needs to be more precisely traced from those who gain from the trade to those that make the decisions about foreign policy.

One of the earliest examples of attempts to connect domestic political/economic actors to state's foreign policies can be found in the work of Ekhard Kehr (1965), who wrote during the Weimar Republic era (he died in 1933) of the workings, policies, and

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politics of Germany up to and during the First World War. "It goes without saying that foreign policy may determine the attitude of parties and classes…. But for the classes and parties the influence of foreign policy is always secondary. Much more intensive in their case is the exploitation of the external situation for internal political, social and economic ends. In making their decisions, the parties proceed from the objectives of their domestic and economic policies. Foreign policy is in their eyes only a means to their domestic ends" (Kehr, 1970, 24).

Kehr focuses on explaining German naval policy pre-World War I not through conventional Realist balance-of-power discussions, but instead on the relative political power, strength, and core constituency of the German political parties and their internal political struggles to unite the country, win support, and retain power. While the modern debate about the liberal peace did not exist at the time of his writing, Kehr would reject the liberal peace theory that trade "creates" domestic support for continued good relations and would instead argues that, if a state pursues a pro-trading policy, that reflects domestic political realities and strategies, not a decision taken on the basis of foreign policy goals. Hence, trade may be correlated with a more pacific foreign policy, but that reflects the political ascendance of groups that favor trade (as a means to domestic economic success) and hence seek foreign relations to support their domestic and foreign economic agenda. Trade does not bring peace; pro-trade domestic economic groups promote both peace and trade as a means to wealth. Kehr demonstrates this argument using the case study of German foreign policy before the First World War.

Schattschneider (1935) implicitly agrees, at least with the idea that economic interests have a tremendous amount of influence or even control over political policies.

Schattschneider's review of the American tariff reforms of the early 1930s reveals a profound influence by business interests in government policy. The interest group activity that surrounded the policy making process was intense, and business groups were able to promote legislation privileging their own interests over the interests of consumers or even a "state" interest that would serve the country as a whole. "Influence is the possession of those who have established their supremacy in the invisible empires outside of what is ordinarily known as government. From this point of view the function of pressure politics is to reconcile formal political democracy and economic autocracy. If the overlords of business are not masters of the state, they seem at least to negotiate with it as equals" (Schattschneider, 1935, 287).

Even if the state itself fails to recognize economic gains from international commerce, business interests which directly benefit from those economic ties have the political power to force the state to reconsider its position, if not outright force the state to adhere to positions that benefit the business interests. While Schattschneider did not discuss interstate conflict or even state foreign policy in general, his ideas are consistent with the notion that the liberal peace operates through domestic economic interests rather than black-boxed welfare-maximizing state decision-makers.

These authors would reject Polachek's (1980) theoretical basis that states recognize the welfare-creating trade and gain an interest in avoiding actions (interstate conflict) that endanger those gains. Schattschneider and Kehr would argue that states do not respond directly to foreign economic ties, but rather foreign economic ties influence domestic economic interests, to which the state responds. With respect to trade and interstate conflict, increased trade creates (or enhances) the fortunes of those economic

actors who profit from foreign trade and those actors in turn influence the government in an attempt to retain or continue to profit from their trade. There are, presumably, domestic actors that are harmed by foreign trade, who organize to oppose it (Gerschenkron, 1966). Official state foreign policy reflects the winner of internal political conflicts between domestic interest groups in favor of and opposed to trade. It is this balance of power among domestic groups that determines whether the liberal peace operates. When interest groups that benefit from trade are able to control government policy, the liberal peace will operate, no matter what the level of trade for a given state (high or low). If domestic interest groups that oppose trade have control or superior influence (again, no matter what the overall level of trade), the liberal peace cannot function.

Moreover, complicating this still further is the potential effect of "perceptions" on the relationship between economics, politics and conflict. In other words, whatever the economic realities of trade (beneficial to the state, harmful to a faction, etc.), if the perceptions of the relevant actors (again, whatever the model of domestic politics is being argued: state decision-makers, interest groups, business groups, etc.) is that trade is beneficial, then there can be a connection between trade and interstate conflict that exists outside the empirical realities of whether trade is legitimately helping (or hurting) the states or relevant domestic actors. Actors with political power that believe that trade is beneficial (whether objectively true or not) will push the state in the direction of acting like the liberal peace predicts; if the same actors believe that trade is harmful to them or their interests, they will push the state in the opposite direction. While international relations scholars have theorized and examined how perceptions impinge on foreign

policy decision making (see, for example, Jervis, 1976), liberal peace researchers have not examined specifically the relation between the perceptions of state decision makers (or of individuals within interest groups) regarding trade and decisions to engage in interstate conflict. This is in addition to the general neglect of sub-state actors and their influence in questions of trade and conflict. The issue of perceptions adds a whole other layer of complexity to the question.

Other researchers have followed in Kehr's and Schattschneider's footsteps in a more explicit manner and have directly "unpacked" the state in an attempt to theorize how sub-state structures (interest groups) have an intervening effect on trade and interstate conflict. Papayoanou (1996) links economic interdependence to domestic political groups that are pro- and anti-trade, as Kehr discussed above. When anti-trade actors (subnational actors that have a vested economic interest that is harmed by increased trade) have control of government, economic ties between states do not restrain state actions (and the economic ties themselves are likely to be strained). On the other hand, when pro-trade actors (subnational actors who economically benefit from trade) have political control, those actors see the interests of the state in continued economic trade, and are restrained from political actions that might harm trade. Papayoanou uses this logic to explain how war emerged in 1914 between two actors with strong economic ties: Britain and Germany. Papayoanou's empirical evidence shows that in Britain, pro-trade groups had more influence in government, and Britain was in fact restrained from threatening or provoking Germany for fear of the damage that would do to the trade between the states. In Germany, however, an anti-trading bloc (the famous "iron & rye" coalition of Gerschenkron, 1966) held the reigns of power and trade between Britain and

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Germany was not a pacifist check on Germany's expansionist policies. By this logic, the

constraining effects of interdependence require influential internationalist internal

political factions. If isolationist factions hold power, trade or other economic ties have

little restraining influence on political actions.[55]

Jungblut (1999) expands directly on Papayoanou's understanding of the domestic

links of the liberal peace. It is only an assumption that leaders of states respond to

economic welfare gains from trade by avoiding political actions that will harm trade. Not

all leaders are motivated by generalized gains and losses. While, in the aggregate, states

may benefit from trade, Jungblut argues that it is individual economic actors (firms and

individuals) who both benefit from or are harmed by trade with foreign countries. The

liberal peace requires a direct causal link between those that will be economically harmed

by ending trade and the individuals that make the political decisions. Jungblut

demonstrates in the specific cases of pre-World War I France and Germany with respect

to Moroccan trade disputes that domestic economic interests advocated a peaceful

resolution of French/German disputes in the name of commercial harmony, but that

economic interests in both states, who would benefit from a reduction of trade between

the two states, pushed their respective governments to take a harder line.

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[55] Papayoanou (1997) expands this model, and brings in a discussion of non-status quo great powers. The revised argument is that, when status quo great powers have extensive economic interdependence with other status quo great powers, they are more likely to take firm, but nonaggresive balancing actions against revisionist powers with which they do not have extensive economic ties. Under all other conditions (economic ties more extensive with the revisionist state than with other status quo powers, for example), the status quo power is constrained by those economic ties from balancing (threatening) against revisionist states. The revisionist state, rejecting the system and seeking a change in status and rules, values change more than economic ties. It is unconstrained by the economic ties and is more likely to begin aggressive action that could lead to military conflict. The basis for these hypotheses is the logic of the domestic economic interest groups discussed in the 1996 article. This article does not add any new understanding to the basic "domestic politics matters" position, but does add an interesting addition in the comparison of economic ties between states (status quo versus revisionist) and the effects they can have on great power actions.

This is a step beyond Papayoanou's argument. Instead of the state being hijacked by either pro- or anti-trading factions that would run the government in their interests, the state can be seen as a semi-neutral actor with interests in both directions (depending on other domestic political considerations), responding to the balance of influence among domestic interest groups. From this perspective, the liberal peace requires not only significant trade between states, but also domestic pro-trading factions with sufficient political influence to restrain government policy. As Jungblut notes, this argument places a significant burden on the researcher in devising empirical tests. Domestic political factions must be identified and tied to specific economic interests; those economic interests must be evaluated in terms of "winning" or "losing" if trade between the state and a specific partner is disrupted; and a determination of which political factions have greater decision-making authority within the state must be made. This must be done for each state, and each state's trading partners, to determine if the pro-trade groups have sufficient motive and authority with government policy to have the liberal peace operate for each state. Any break in the causal chain (the pro-trade economic faction has no link to a political faction, or has a link but the political faction is out of power, to use two examples) and the liberal peace cannot operate no matter what level of trade exists between states.

This is a daunting research challenge, and has made progress in sub-state examinations of the liberal peace difficult. For example, Reuveny (2001) presents a model of trade/conflict/cooperation that includes some sub-state actors. In the model, the governments "control" the extent of conflict/cooperation between pairs of states and independent import/export domestic factions "control" the extent of trade volume that

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flows between pairs of states.[56] These actors have an indirect effect on each other (the government can help its own import/export sector by offering cooperation to the other government, which can result in increased trade), but according to how the model is structured there is no direct interaction between domestic actors (unrealistically, the government has no direct contact with its own import/export actors), and hence no domestic politics of any substantive kind. Moreover, the model lacks domestic economic actors opposed to further trade (protectionist factions) as part of the model of domestic politics. Reuveny's (2001) model is a start at bringing sub-state actors into questions of how the liberal peace operates, but it lacks a serious model of domestic politics.[57]

McDonald (2004) focuses on domestic actors, but sidesteps a direct model of domestic politics. Building on the domestic interest group logic, McDonald argues that a traditional measure of trade (imports and exports divided by GDP) ignores the issue of pro- and anti-trade factions within the state. It is not the pure volume of trade that is important in creating peace, but policies of free trade that are critical in determining the activities of the liberal peace. Logically, free trade means that the domestic groups that benefit from trade have significant political power, and thus a direct chain exists from trade to pacific relations (the interest groups that benefit are also significant in the government, creating the interests to avoid conflict). However, even though McDonald refers directly to interest groups in the theory behind his test, empirically all he measures

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[56] Reuveny's model is dyadic, thus resulting in four actors (a governments and a domestic import/export faction for each state). The domestic actors control what their state does (the government offers conflict/cooperation which the other government sees and responds to as one part of the simultaneous equation model; the import/export faction offers to sell and buy goods, to which the other import/export actor responds in the other part of the simultaneous equation model).

[57] It is also worth noting that, for the simplicity of Reuveny's (2001) model (four actors, no domestic political interaction), he is only able to gather data for four states (US, USSR, Japan, Germany), thus clearly indicating the difficulties of gathering sufficient data for an extensive, statistical examination of the sub-state aspect of the liberal peace.

is tariffs collected (technically he uses the ratio of tariffs collected to value of imports; the higher the ratio the more protection and the less free trade) and the degree of variation between the actual value of trade between states and the maximum predicted by a gravity model borrowed from economic theory (the higher the difference between real trade and predicted trade, the more protection and the less free trade). While his empirical results back his theory (the greater the free trade, as he measures it, the fewer Militarized Interstate Disputes between states within dyads; when the "traditional" measure of dependence – trade divided by GDP – is included, it is insignificant), he has not directly measured domestic interest groups or domestic politics. Nonetheless, McDonald's (2004) study begins the process of trying to explicitly move domestic politics out of the "black box" and into direct consideration with regard to the liberal peace.

The criticism that the study of the liberal peace has neglected sub-state actors and interests is not unique to Sayrs (1989). Barbieri and Schneider (1999), in their brief but cogent review of the liberal peace research, explicitly call for disaggregating the state – doing away with unitary rational actor assumptions – in order to create a better "micro-foundation" for the theoretical side of the research program. States are clearly not single actors, and this may be especially the case involving economic issues, which are the driving force behind much of domestic politics in democratic and authoritarian systems alike. To base the liberal peace research on an assumption that is clearly untrue, Barbieri and Schneider argue, does the academic study a disservice.

Other authors have attempted to provide a causal chain for the liberal peace without "unpacking" the state. Weede (1995) argues that trade does lead to peace, but not directly. He posits an indirect link: trade leads to prosperity, which leads to

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democracy, which leads to peace. Given that this causal chain has several links, trade has little direct and immediate impact on interstate conflict, and the effects of trade are seen some time later, rather than immediately, as is common in most empirical tests of the liberal peace. Weede asserts there is little disagreement in the economics literature that increased trade leads to increased prosperity for states, rich and poor alike. He reviews the separate comparative politics literature on the link between prosperity and democracy, again showing that it is well substantiated and generally accepted in the field. Finally, looking to political science, he shows that there is a strong empirical claim that democracy leads to peace. He has no specific empirical test of this chain, but relies on empirical work done by a number of researchers in different fields and disciplines for evidence.

There is additional evidence in favor of an indirect relation between trade and interstate conflict that does not depend on domestic political factions. Hegre (2000) looks to see what effect economic development has on the liberal peace hypothesis and finds that the liberal peace works only for developed – economically wealthy – states. Poor states have a somewhat lower incidence of militarized conflict when they trade, but the real effect of the liberal peace happens between rich trading states. Hegre notes that when level of economic development is left out of an empirical test, there is general support for the liberal peace (as other studies, like Oneal and Russett, 1997, have found), but by including a measure of economic development, the direct impact of trade on interstate conflict is unclear. The liberal peace operates, but primarily for richer states where the interruption of interstate economic relations would cause more serious harm than for poorer, less developed states. This study provides some direct support for

Weede's lengthy, but plausible, chain of logic that connects trade through development

and democracy to less interstate conflict, and is an issue that proponents of the liberal

peace have not adequately addressed.[58]

I noted earlier that previous liberal peace studies have generally neglected the idea

of "perceptions" in the liberal peace. While no researcher has introduced perceptions into

sub-state analysis of economics and conflict, one researcher has kept the "black box" of

the state, but used perceptions. Copeland (1996) argues that it is not the present gains

from trade that motivate states to avoid international conflict (in order to protect the

economic relationship), but rather it is the shadow of future economic gains that

motivates states to avoid interstate conflict. Copeland contends that expectations of

future trade, not present trading levels, drive states and leaders to choose to avoid

conflicts with present and future trading partners. Thus, states that have a large amount

of current trade with a partner may not be restrained by the potential loss of that trade

when considering the use of force or war, if they foresee that the trade is likely to decline

in any event – the shadow of the future reduces the import of current trade in the decision

calculus. By the same token, a state with little current trade with another state may be

restrained from initiating a conflict if it expects a substantial trading relationship to

emerge where none now exists – the shadow of the future motivates the state to avoid an

act that would harm future gains. Copeland uses Germany before World War I and

World War II as brief case studies to demonstrate that the levels of trade in existence

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[58] Russett and Oneal (2001), in their book-length review of the liberal peace program test for the effect of economic growth on the liberal peace, finding neither high nor low growth rates have any effect on the liberal peace. They do not test level of economic development, though they do acknowledge Hegre (2000) in a footnote, noting that economically less developed states may not see any pacific effect of trade. Considering the number of economically less-developed states in the world, this seems a cavalier dismissal of a potentially important finding in the trade/conflict debate.

before the conflicts (relatively high) were not good indicators of future expectations (relatively low) and, thus, not a good predictor of leaders' actions at the time (low expectations minimized the value of trade for the leaders, so trade had no pacifying effect).

Other researchers have not systematically tested Copeland's argument, although there is additional case study evidence. Long (1996) defines and discusses "economic incentives" and their success in modifying state behavior and cooperation. He finds, anecdotally, that states that offer economic incentives resulting in increased trade are able to influence recipient states to be more cooperative (and presumably less conflictual) in return for future economic gains. While Long's research has no direct bearing on the liberal peace, his results make Copeland's case more difficult to dismiss. There is, however, a major empirical problem in discussing economic expectations and interstate conflict. The difficulty in accurately determining leaders expectations of future trading relationships is an obvious impediment to a more thorough test of the thesis and one likely to be insurmountable for large-N analyses. Both Long and Copeland rely on case studies,[59] which makes the empirical evidence in favor of this argument limited. That said, the theoretical argument is plausible, and deserving of a more general test. It cannot be simply dismissed.

Overall, these researchers argue that present theories of how the liberal peace operates are not adequately developed to explain the connection between trade and foreign policy. Politics is more complicated than the interest-based liberal peace logic (Polachek, 1980) would make it, and only by adopting a more realistic (and more

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[59] In Long's case, he selects on the dependent variable. Long chooses three cases where economic incentives resulted in policy changes in the receiving state, and no cases where there was no result or a negative (more conflictual) result.

complicated) model of how trade influences states to adopt peaceful behavior will a true understanding of the liberal peace be achieved. Present research, in general, does not do this.

With respect to this dissertation, I make no attempts to "unpack" the state; the focus (as noted in Chapter 1) is on a test to see which "family" of explanations (interests or signaling) is more consistent with the empirical record. The criticisms above outline how broad and deep an interest-based explanation of the liberal peace is likely to be once the state is unpacked, and a wide variety of actors (political and economic) are introduced. The signaling-based logic of the liberal peace is a more recent literature, and has not suffered the criticisms that have been heaped on the interest-based explanation. That being said, the literature that describes the signaling logic (Gartzke, *et al.*, 2001, Fearon, 1994) contains no "microfoundations" that clearly and explicitly outline the connections between economic factors and interstate conflict within the state that lead to a signaling-based liberal peace, and is just as open to the same criticisms as the interest-based explanations. This dissertation does not seek to provide (or test) microfoundations for either theory of the liberal peace. The literature described in this section clearly argues that those microfoundations are necessary for a complete understanding of the liberal peace. However, given that interests and signaling describe very different ways by which the liberal peace operates, the microfoundations of either theory would look completely different (different actors and different processes). Thus, the results of this dissertation are meant to be used to guide future research on uncovering those microfoundations by suggesting which larger family (interests or signaling) is worthy of more detailed investigation. More bluntly, there is little use in developing an extensive

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set of theories that explain the microfoundations of signaling if the liberal peace operates by some form of interests, and vice versa. This dissertation does not develop new theory, but tries to close off dead end avenues of research so that the field can concentrate on the most likely explanations and develop the (more accurate) microfoundations.

The pitfall in this approach, however, is that by neglecting a specific microfoundation for either interests or signaling (by not explicitly adopting a theory of how economic relations connect through the state and significant sub-state actors to conflict decisions) the measures used to test interests versus signaling might not be valid indicators (which would invalidate not only this research, but all other research on the liberal peace). By assuming that other researchers' measures are valid (who also neglect the microfoundations of either theory of the liberal peace) and using those measures to test interests versus signaling, this dissertation runs the risk of failing to accurately measure interests or signaling, and thus coming to conclusions that are false (i.e., that signaling is more empirically accurate than interests, when, in fact, that is not true).

This is a chicken-and-egg problem. If we had better theories of the liberal peace (ones that were more specific about their microfoundations), then our measures of interests and signals would be more valid. However, in order to create those more accurate theories, we would need a great deal more theorizing and testing of competing theories of different forms of interests and signaling – and which theory (interests or signaling) should be more closely examined? Without the test in this dissertation, we do not know which family of theories of the liberal peace is more accurate, but without the microfoundations that this dissertation will point researchers to develop, we do not know if the measures used are completely accurate.

This dissertation argues that the understandings of interests and signaling, while without specific microfoundations, are reasonable enough to allow for a valid test, and that the results of those tests will help us describe those microfoundations. As such, a complete theory of the microfoundations of the liberal peace is a natural progression that would follow the research and results presented here.

In the Sayrs (1989) framework, all of the above discussion falls under a combination of her first (i.e., subnational actors are relevant to issues of trade and conflict), her second (i.e., past political action affects present political decisions) and third (i.e., other factors beyond trade affect interstate politics) points. While those three points generate many lines of criticism, they can be bluntly summarized into: an introduction of factors below the level of the state (opening the "black box") is necessary for a complete understanding of the microfoundations of any theory (interest based or signaling based) of the liberal peace. Criticism related to Sayrs (1989) fourth point (i.e., that conflict and cooperation are not mutually exclusive) is focused around a line of argument made by Gowa (1994).

Gowa (1994) offers a different perspective on the relationship between trade and war, specifically questioning the causal arrow from trade to peace. Gowa highlights the chicken-and-egg nature of the relationship (as Montesquieu did, discussing "manners"). Trade and a lack of interstate conflict may be related, but not necessarily because trade leads states to prefer peaceful relations. Rather, peaceful relations come first, followed by trade. Gowa offers a Realist argument, noting that states elevate security issues over economic ones. States determine their economic partners (and international economic policy) based on security externalities. If trade creates economic benefits for states, the

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choice for any state is to decide whether those benefits will flow to allies or adversaries. For Gowa, the only answer is allies – and those pairs of states are least likely to engage in international conflict or war. There is no liberal peace; there is trade between friendly states.

Further buttressing this argument, empirical work by Mansfield and Bronson (1997) shows that if a trading partner is a member of a select group of states (a political ally, a member of a preferential trading arrangement or a major power), dyadic trade is likely to be greater than normally expected. This is consistent with Gowa's argument: allies are on good political terms; states are not likely to make permanent favored trading arrangements with those with whom they have negative political relations. The inclusion of major powers in the study is irrelevant in a discussion of the liberal peace and the finding that major powers are important states to be on the good side of is consistent with Gowa's Realist bias. States trade with those with whom they have existing good political relations, which are precisely the states they are least likely to engage in interstate conflict.

Reuveny and Kang (1996) specifically examine the issue of causality in the liberal peace – does trade reduce conflict, or does political cooperation between states result in greater trade? Their empirical test, using a limited sample (16 dyads) and limited temporal domain (1960 to 1990), produces mixed evidence. Employing Granger causality tests, they find in a third of the cases that political cooperation causes increased trade (not the liberal peace), in another third that trade results in increased cooperation (consistent with the liberal peace), and in the final third no relationship.[60] Long (2003)

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[60] Reuveny and Kang (1998) employ substantially the same research design in their 1996 article, but break dyadic trade down by commodity, examining the hypothesis that strategic goods (Baldwin, 1985) have a

disaggregates types of allies, finding that defense pacts (defense treaties that require the partner state to come to the military aid of its ally in times of crisis or war) are associated with a significantly higher volume of trade among partners than alliances that only require neutrality or non-aggression. In those latter cases, trade among allies was not significantly different than between friends and enemies. Again, we find mixed empirical evidence with regard to Gowa's argument.

As straightforward as Gowa's logic is, Morrow (1997) shows that it is not complete. Formally modeling the gains from trade using rational choice theory, Morrow shows that even in the face of relative gains fears, rational states will continue to allow trade with adversaries if the (required) additional allocations of resources to military spending (to offset the gains the opposing state gets from the trade) are less than the gains from trade received from the economic exchange. In other words, if the price a state has to pay in terms of a higher military budget is less than the gains accrued from trading with an adversary, the rational states should continue to trade. Thus, from a rational choice perspective, there is no security externality from trade and the issue is irrelevant. Morrow, Siverson, and Taberes (1998) empirically test this proposition. They examine three reasons why politics should affect international trade (political relations, security concerns, and joint democracy). Their empirical results, based upon analysis of great powers from 1907 to 1990, indicate that trade is significantly affected by political relations and joint democracy, but not by security concerns. Thus, Gowa's (1994) theory,

different effect on the causality question than non-strategic goods or overall trade figures. The results of this study are substantially the same, in that a mixed result is reported. Some trade in goods (in general) cause lessened conflict (foodstuffs, tobacco), while lessened interstate conflict causes increased trade in other goods (minerals, fuels, some technology – clearly more strategic commodities). These results are substantially in agreement with the previous study (1996), in that support for the liberal peace is mixed – not all trade causes peace; some trade causes peace, some cooperative political relations cause trade.

at least as tested by Morrow, Siverson, and Tabares (1998), fails to find empirical backing.

Werner (1997) makes a similar argument against Gowa, noting that states are embedded in an international system and are not just concerned with a single dyadic relation or even a large number of independent dyadic relations. States' relations are interconnected and, under Realist assumptions about power and security, it would be logical for states to trade with "enemies" – to trade with a dyadic partner where the state would lose in a relative gains sense compared to that partner – if the state's overall security situation relative to all interstate relations was improved. In other words, states should be willing to enter into economic partnerships with individual (or small numbers) of opposing states if the results of that economic partnership elevate their relative power over the entire range of their interstate security concerns even though those economic links may bring greater relative gains to the specific partner(s) than themselves. States should be willing to "lose" on a dyadic basis if, as a result of that economic transaction, they "win" in an overall sense. Thus, trade is possible between states that are not on good political terms, and Gowa's argument is incorrect.

Finally, Barnett and Levy (1991) argue that the determination of alliance partners itself is not a wholly external political determination, but instead is based on a combination of external security concerns (Realism), domestic political-economy issues, and internal political concerns. States are forced to confront a number of issues when determining alliances, and security externalities are only one of these. In sum, alliances are possible between states that are not on the best of terms, and so is trade.

Smith and Eyerman (1999) examine just this issue in an empirical test. They specifically examine both Realist and liberal hypotheses about the trading partners of states. Their results support the liberal logic (Pollins, 1989a 1989b) that trading partners are chosen by individual economic actors based on a "risk versus reward" strategy (i.e., partners are chosen by firms after they balance the gains they can receive through the economic relationship against the calculated risk of political conflict between the states interfering in the economic relationship between the firms in the two states) rather than determined by the political allies and adversaries of the state itself. In other words, economic actors weight the benefits of trade with other economic actors against the risks of those relationships being severed for them by conflict between the states; firms do not choose their economic partners based solely on the political alliances of their parent government (as Gowa would argue). Thus, contrary to Gowa's Realist logic, there is empirical evidence and logic that trade is independent of state political alliances, and can have an independent pacifying effect on interstate conflict.

Benson (2004) specifically examines the effect of economic and security ties between partnered states (for dyads) versus economic and security ties to the prevailing international order (in an argument similar to Gowa's, 1994). In other words, do economic and security ties between pairs of states have a greater effect on suppressing conflict than do ties between those states and the system hegemon? Using traditional liberal peace measures of trade between states and alliances between states of a dyad during the cold war era, Benson also includes a measure of the dyad's ties to the US (the dominant state of the international order). This allows direct comparison of the effect of ties between partners (interstate) versus ties to the international order. Her findings are

that the interstate ties are significantly more important in terms of limiting escalations up the Militarized Interstate Dispute (MID) hostility scale (she uses all five levels, not a binary conflict/no-conflict measure as do most other studies), but that ties to the international order (the US) did dampen conflict between states within a dyad. In this way, Gowa's argument (and others that argue that states actions are not focused solely on a single other state when issues of interstate conflict are considered) is correct – states are imbedded in a larger web of economic, security and political concerns. However, as Benson (2004) shows, relations between potential combatants have significantly more effect on conflict propensity than "systemic" issues and relations.

Clearly, states actions and decisions are not made in a political vacuum, it is important for researchers to take account of the greater political and economic context of states when testing for the liberal peace. This has been done to some degree: recent statistical tests include as covariates measures ranging from the balance/imbalance of capabilities (a crude measure of state power) between the states, to the level of alliance between the states, to how many intergovernmental organizations they are jointly members of, to the length of time since their last interstate conflict. Sayrs' objections are valid, though researchers can always find additional complexities to include and the tests have included the measures that previous research (both within the liberal peace and the general peace studies community) has determined are significant. This dissertation makes its own incremental advancement in this direction by including a measure of the balance/imbalance of trade between states and (in a later test) a measure to look to the overall economic openness of the state (to look at the effect that might have on the possibilities of conflict).

However, the questions about the causality of trade and conflict are troubling and not easily refuted by empirical studies. Reuveny and Kang's (1996, 1998) tests of causality for a small sample of states indicate that the relationship between trade and conflict may not accord with the simple liberal peace "trade deters conflict" logic that proponents expound. But there is also evidence that Gowa's assertion that security concerns drive trade is not correct. There is no unequivocal evidence what the direction of causality is. This is one of the most troubling results for proponents of the liberal peace.

Continuing with a discussion of Sayrs' (1989) fourth point (conflict and cooperation are not mutually exclusive), a debate has emerged regarding the extent to which trade is disrupted by militarized conflict between states. The assumption that trade is reduced or eliminated between adversarial states is a key point of the liberal peace debate. The original liberal peace theorists (Montesquieu, Mill, Kant, Cobden, Angell) as well as current day researchers (Polachek, Oneal and Russett) have uniformly posited that there is economic value to trade between states and that trade relationships are damaged or destroyed by interstate conflict and war. The trade-disrupting or trade-destroying assumption associated with militarized conflict seems reasonable. It is, however, a crucial assumption. If it could be shown that militarized conflict has no effect upon trade - that trade continues at pre-conflict levels during and after militarized confrontations between trading partners – there would be no motive, no reason, for a liberal peace. Trade would no longer have any logical reason to provide a pacifying effect on state behavior. If trade or other economic relations do not decline in the face of interstate

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conflict, states would no longer have reason to avoid conflict.[61] Thus, what appears to be a logical assumption (i.e., trade declines or ceases in the face of interstate conflict) is, in fact, a crucial empirical question.

It is, thus, surprising that there has been little empirical examination of the subject. The first empirical test of this proposition (Barbieri and Levy, 1999) finds that, in a limited sample (seven dyads), the presence of war significantly decreases the dyadic trade in one dyad (the other six all decline, but not significantly), and in all seven cases the long term effects of interstate conflict are negligible (trade returns to its pre-war levels quickly) or "positive" (post-war trade levels rise above pre-war levels). Neither author argues that war "causes" an increase in trade – there are other political dynamics at work if trade levels rise above pre-war levels. Their purpose is to empirically test the assumption that war brings about a decrease in trade. The study looks only at the effect of war on trade, and ignores interstate conflicts short of war. If war between states only disrupts trade to a minor degree, the effect of interstate conflict short of war may be even less significant, making suspect many of the works supporting the liberal peace.[62]

Morrow, Siverson, and Taberes (1998) test hypotheses on what types of political variables affect international trade, and examine whether the presence of interstate conflict causes a significant reduction in trade. They report no effect of interstate conflict

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[61] There is one exception to this statement. As Copeland (1996) argues, it may not be the actual level of trade that makes states act more pacific, but rather the expectations of (presumably) significant trade in the future [see the more detailed discussion of Copeland (1996) above]. In this sense, if leaders expect or believe that trade will decline in the presence of interstate conflict, they will be restrained from interstate conflict, no matter what (empirically) happens to trade. In other words, if states' leaders think that the assumption underlying the liberal peace is true (i.e., trade will be harmed when interstate conflict occurs), they will act on that belief, no matter what the empirical reality is. In that sense, the test of whether trade declines in the face of interstate conflict is not crucial to the liberal peace, but the crucial test then becomes whether leaders believe that conflict reduces trade.

[62] Anderton and Carter (2001) expand Barbieri and Levy's sample (looking specifically at major powers versus minor powers) and refine the statistical method, finding that trade is disrupted by war. Their conclusion rejects the Barbieri and Levy findings.

on trade, but test only for a simultaneous effect – using interstate conflict and trade

variables recorded for the same year. They do not examine subsequent years (to see if

there is a delayed effect of conflict on trade) nor look to the level of conflict to see if

more violent conflict (war, for example) has a pronounced effect on trade. Thus, their

results, while suggestive, are not sufficient to allow any conclusions. Reuveny (2001)

finds, in a very limited sample, that increased conflict and increased trade occur

simultaneously, contrary to the liberal peace assumptions. This pair of empirical studies

cast further doubt on this crucial assumption, suggesting that trade is insulated from

political interstate conflict.

Finally, Li and Sacko (2002) offer what they claim is the definitive study of the

effect of interstate conflict on trade. Their research design is similar to Morrow, Siverson

and Taberes (1998), but with improvements. Their primary theoretical argument is that

unexpected interstate conflicts are more likely to result in trade disruption than expected

interstate conflicts. States do not trade, but firms do.[63] Firms, being rational economic

actors, pay attention to the (political) world around them and are cognizant of the level of

hostility between states. Firms that see, *ex ante*, an increase in hostility (but not, yet, an

interstate conflict) will anticipate a future interstate conflict and reduce trade with firms

in the other state to avoid the coming disruption. The more unexpected the interstate

dispute, the less firms realize the coming political problems and the more trade there is to

disrupt. Thus, Li and Sacko compare the level of dyadic trade in a given year to the level

of dyadic trade from a previous year (avoiding the problem of simultaneous effects

inherent in Morrow, Siverson and Taberes (1998)) and test whether militarized interstate

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[63] While there are states that act as their own economic actors, such action is increasingly rare and is not incorporated in the Li and Sacko (2002) model.

disputes affect the level of trade. They show that the presence of a militarized interstate dispute significantly lowers trade in the subsequent year, and the longer and more hostile the dispute the more trade is suppressed. They then test whether an "unexpected" MID results in decreased trade by calculating for each case (each dyad-year) the expected probability of a MID using the most common liberal peace equation (Oneal and Russett, 2001). If a dyad has a smaller than 20% chance of a MID occurring (according to the Oneal and Russett equation) and a MID occurs, it is considered unexpected.[64] Unexpected MIDs result in statistically greater reduction in trade than all MIDs, leading the authors to conclude that interstate conflict reduces the overall trade between states and that economic actors (firms) anticipate the likelihood of conflict.[65] Thus, while the study supports the general assumption of the liberal peace, it complicates the nature of the trade-conflict relationship.

3.4 Conclusions

This chapter demonstrates two contradictory results: there are substantial empirical tests by a wide variety of researchers that demonstrate time and again that interdependence (measured through trade) brings more pacific relations, and that there are substantial criticisms of the ongoing research program's choice of data, method and assumptions that call those conclusions into doubt.

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[64] The decision to use 20% as the cutoff for an unexpected MID is arbitrary. Also tested were a 10% cutoff and a complicated model that codes the probability of a MID occurring for only those cases where a MID actually occurs. For both alternative models, similar results to the discussed outcome were found.
[65] While this study is the most thorough to date and is substantial evidence that militarized conflict does cause a reduction in trade, it is not conclusive. The study only examines the shift in trade over a pair of consecutive years and does not examine trends in trade over several years preceding and following any militarized interstate dispute. Expanding the temporal scope to several years around any militarized dispute would greatly increase the power of these tests.

This dissertation will not attempt to address all of the criticisms, and this chapter is only intended to serve as a categorization of the forms of the criticisms. The most substantial criticism of the liberal peace findings is the argument that the foundations of the interest-based logic that underlies almost all the empirical tests is significantly under specified – exactly how within the state do trading interests cause political interest to refrain from interstate conflict? Critics do not seem to accept the "black box" of the state argument with respect to the liberal peace.

Without rejecting this criticism, I argue that this criticism is premature. Given the challenge of the signaling-based logic of the liberal peace, a more fundamental question than the "black box" of the state is whether interests or signals drive the empirical connections between trade and interstate conflict. This is a critical question, superseding the importance of the domestic politics criticism. If, in fact, states are driven by interests (as the original liberal peace theorists, and the bulk of modern researchers, believe), then examinations of interests groups, economic interests, and economic policy's connection to foreign policy and security policy within multiple state structures are warranted. This is precisely what the critics discussed above are demanding.

However, if it can be demonstrated that states are acting as the signaling logic argues, then an examination of economic interests and trade policy is less important in understanding the microfoundations of the liberal peace than an examination of decision-making, decision-makers, signals (both receiving and sending) and channels (again, receiving and sending).

I do not reject the valid criticisms of the lack of explanation for the foundations of the liberal peace, but only argue that if liberal peace researchers are going to turn to

examinations of sub-state political actors and interests, they do so using an accurate theory of the liberal peace. Thus, the question of interests versus signaling is paramount (at least with respect to this criticism of the liberal peace).

This is the primary focus of the dissertation. The research design described in the next chapter makes no attempt to break open the "black box" of the state, but instead uses indirect measures based on outcomes of conflicts for specific sorts of economic relationships (unbalanced trade) to argue that some preliminary evidence in favor of either interests or signaling can be gained. This is clearly not the end of the story: using this research, future liberal peace scholars concerned about the microfoundations of how the liberal peace operates can have a better understanding of whether interests or signaling drives state actions, and tailor research designs to examine the actions of actors within the state.

This chapter also demonstrates that while there have been a large number of empirical tests of trade and interstate conflict, liberal peace scholars have not looked closely at the "boundary conditions" (Mansfield and Pollins, 2001, 2003) where the liberal peace breaks down, or at least does not behave in a straight forward trade-brings-peace manner. Tables 3.1 and 3.2, while lengthy, indicate that while some tests have begun to examine the behavior of some kind of states (politically relevant, rich versus poor), there has been little examination of kinds of trade (unbalanced, commodities). Examining the "boundary conditions" of the liberal peace has not occupied many researchers, and adding to that store of knowledge remains a secondary task of this dissertation.

4.0 Chapter Four: Research Design – Two Distinct Theories of How Trade Brings Peace

4.1 Introduction

The previous chapters discussed the theoretical basis for two competing classes of theories (interest-based and signaling-based) that explain the findings of most liberal peace research (increased trade between economic partners reduces the incidence of militarized conflict between them). This chapter presents a research design for determining which logic, if any, is operative in the liberal peace. As the previous chapter demonstrated, few researchers[66] have attempted to discern which of the competing explanations is more accurate.

This chapter describes the specific tests, variables, data, and logic that will be used to empirically test the two competing logics of the liberal peace. Both logics argue that there is a liberal peace, and most previous studies have found empirical evidence consistent with a liberal peace. Previous empirical tests, however, have been unable to test for the different liberal peace logics because they were not designed to do so. This chapter discusses a specific series of tests that are designed to examine the competing logics of the liberal peace. These empirical tests are important for two primary reasons.

First, as was argued in the Introduction (Chapter 1) and at the end of Chapter 3, the question of whether interests or signaling drive a liberal peace is a critical first step to understanding the direct causal connection between economic issues and security issues that leads to the liberal peace. While there are a host of researchers and authors that have proposed theories as to how interests operate within the state (from Polachek's 1980

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[66] The exception is Gartzke, 2002, unpublished. See below for a further discussion.

"black box" of government that acts according to welfare concerns through domestic

interests groups as Jungblut's 1999 presentation argues to Kehr's 1920s connection of

competing domestic economic factions seeking supremacy within a government), all of

these theorists of the liberal peace are assuming that interests govern the liberal peace.

Given that the signaling explanation challenges the interest-based explanation of the

liberal peace, before any exploration of domestic economic groups and domestic political

factions begins, it is necessary to examine whether interests, in fact, are governing how

the liberal peace operates.

Beyond the direct questions relating to interests and signaling, these tests will

provide a better understanding of the limits of the liberal peace. For example, while the

two competing explanations offer logics and supporting empirical evidence that trade

reduces interstate conflict in general[67], we do not know if this is true for all dyads. Are

their limiting conditions under which, for a certain sub-set of states, increased trade leads

to an increased likelihood of conflict? This question is intimately related to an

understanding of the different logics of the liberal peace. The interest-based explanation

(based on Polachek, 1980) of the liberal peace assumes trade is always pacific. There

are no limiting conditions. The signaling-based logic allows for the possibility that

increased trade may not reduce, in fact may be associated with an increased likelihood of,

interstate conflict. If the ultimate goal of academic research on war and peace is to

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[67] Some studies restrict themselves to "politically relevant" dyads, reducing the size of the population to be tested from all possible dyads to only those that have a substantial possibility of engaging in military confrontation. Thus, researchers examine whether Chad and Libya fight, and the conditions that influence their potential conflict, but ignore the possibility of Chad and Ecuador fighting as neither possesses the physical capability to engage in conflict with the other. In practice, "politically relevant" dyads pair each state with its neighbors and with major powers (states which have a global reach).

inform the policy community as to what actions reduce war and suffering, an accurate understanding of the conditions that produce war and peace is critical.

Second, interest-based and signaling-based logics are not unique to the liberal peace. Both provide generalizable logics of when and how conflict and war come about in the world. To the extent that empirical evidence relating to the liberal peace casts light on the more general debate regarding the nature of interstate conflict, the research undertaken here may contribute to the cumulative effort to understand war. Thus, while this dissertation addresses the narrower issue of the liberal peace, it may provide insight into the general merits of interest-based versus signaling-based explanations of interstate conflict.

4.2 Basis for Theory: Unbalanced Trade

The theoretical question explored in this chapter is: do both theories of the liberal peace agree in the more limiting case of unbalanced trade? As we shall see, they do not. It is this difference that allows us to empirically test which logic of the liberal peace is more accurate. Unbalanced trade has figured in previous theoretical discussions of the liberal peace. Hirschman (1980) looks at Balkan state economic dependence on Nazi Germany before and during World War II as explanation for the unwillingness of the Balkan states to offer anything but token resistance to German political (and later military) domination. The nature of the unbalanced trade allowed Germany to dictate to and control much smaller economies. However, beyond the theoretical debates, states with unbalanced trade compared to the partner trading states (the state the primary state trades with) are an important sub-set of the universe of cases. Using some "back of the envelope" statistics, over twenty percent of the cases since 1948 show unbalanced trade

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between partners, and over ten percent of the cases show significant imbalance.[68]

Unequal trading relations between economic partners are not uncommon. The empirical

effect of unequal trading on interstate conflict has not been examined, and represents an

important sub-set of cases in the world.

No previous work on the liberal peace has specifically examined the effects of

unequal trade on pacific relations. Barbieri's early work on the liberal peace (1995, 1996)

created a measure of "symmetry" for a pair of trading states that reflected how evenly

balanced members of a dyad were in terms of volume of trade. Russett and Oneal (2001)

calculate the importance of trade for each member of a dyad and employ the score for the

less dependent state, a process they call the "weakest link" approach, for the purpose of

their analysis. In both cases, the unit of analysis is the dyad. The premise for focusing

on the dyad is that interstate conflict requires at least two states. The problem with this

approach is that "dyads" do not take action or make decisions. Dyads can experience

conflict, but dyads do not make decisions for war and peace. States and governments do.

Most empirical tests of the liberal peace have used interstate dyads as the unit of

analysis. In examining economic interdependence, researchers (with the exception of

Barbieri, 1995, and in a partial way, the Russett and Oneal "weakest link" approach – see

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[68] Of the approximately 1,005,000 cases of directed dyads since 1948, about 240,000 show an imbalance of
trade dependence between the two states of over 2 to 1 (in other words, dyadic trade divided by GDP for
one of the states was more than twice as large as the dyadic trade divided by the GDP of the partner state).
I call this unbalanced trade. There were approximately 130,000 cases of trade where the balances are
greater than 10 to 1 (dyadic trade divided by GDP for one state was ten times as large as dyadic trade
divided by the other states GDP), which I label as significantly unbalanced. There is no basis for the
designation of trade as "unbalanced" or "significantly unbalanced" in these descriptions. While other
researchers might use different cut-points, the general point that there is a great deal of unbalanced trade
(however it is defined) is clear. Moreover, while there are relatively few states where trade with the partner
state is a significant portion of the overall economic activity (arbitrarily defined as states where trade with
the partner state accounts for 5% of the entire GDP; there are only approximately 31,000 directed dyads
where this is true), fully 20% of those directed-dyads are significantly unbalanced (over 6,000 of the 31,000
directed dyads had trade dependence ratios greater than 10 to 1). Thus, if a state is significantly
economically tied by trade to a partner, there is great likelihood that the trade is unbalanced. The data are
from Gleditsch, 2002.

above, and footnote 7 in Chapter 3) have not distinguished between the different states within the dyad. Dyads are treated as if Country A is as dependent on Country B as Country B is on Country A. This assumption is unrealistic. France is not as dependent on Monaco as Monaco is on France. A measure of the aggregate economic interdependence of France and Monaco does not capture the potentially different costs of the disruption of the economic relationship. The solution to this problem is to incorporate the notion of "directed dyads". By examining, for each pair of states, A's dependence on B and B's dependence on A separately, one can better explore the relationship between economics and interstate conflict.

4.3 Unbalanced Trade and Interest-Based Logic

The interest-based theory of the liberal peace uses a traditional conception of state decision-making that argues states weigh the costs and benefits of interstate conflict; when costs of conflict are higher than benefits, states rationally choose to avoid hostility. It is this framework that undergirds the interest-based liberal peace hypotheses. Accordingly, states with high levels of economic dependence on a specific economic partner should be hesitant to pay the costs of disrupting those relations by pursuing interstate conflicts.[69]

To the extent that trade produces significant economic benefits, it should generate interests in pacific relations (Oneal and Russett, 1999). Hence, trade creates peace and, *ceteris paribus*, more trade reduces the potential for interstate conflict even more. By this

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[69] This discussion ignores the idea that states will be concerned with trading relations beyond the specific partners they trade with. In other words, states worry about important trading relations with other specific states, not whether trade is important in general. The idea that states will act according to a general trading impulse is equivalent to Rosecrance's (1986) trading states argument (discussed in Chapter 3). While Rosecrance's theoretical argument is ignored here, it is empirically tested in Chapter 7.

"interest-based" logic for the liberal peace, trade is universally a positive good in mitigating interstate conflict, as it can never produce interstate conflict.

The interest-based logic makes no distinction between balanced trade and unbalanced or dependent trade - all trade offers states a motive to avoid interstate conflict and the harm to their economic gains that would ensue from an end to trade – and the depth of the motive is directly correlated to the size of the trade. In balanced economic relationships, both states have more or less identical motives to avoid actions to harm their interests, and both states are individually motivated to avoid conflictual actions. In the case of unbalanced trade, one state has greater economic motivation to avoid conflictual actions than the other because the potential economic costs to it are greater than for the other. States with dependent economic relations will, by the interest-based logic, be particularly sensitive to the potential costs of conflict. States that are not dependent will not have this constraint on their actions, and will choose to act or not based on other interests. In unbalanced economic relationships – where one state is dependent while the other is not – the dependent state will be significantly less likely to initiate or intensify interstate conflicts when compared to the less-dependent state, in that it will have more to lose.[70]

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[70] This dissertation, like all other interest-based arguments of the liberal peace, lacks a specific theory of how interstate conflict occurs. The implicit theory of interstate conflict adopted by the liberal peace theorists (discussed in Chapter 2) argues that states weight the risks of gains and losses of interstate conflict, and choose conflict when the gains outweigh the losses (and trade increases the losses, as the benefit-producing trade would cease when the conflict begins). The liberal peace argues that trade reduces interstate conflict, but avoids specific questions of how conflict (in general) does occur (the signaling-based logic, on the other hand, does contain a specific theory as to how states enter into interstate conflict: see the discussion in Chapter 3). This lack of a "conflict theory" is deliberate: the interest-based logic of the liberal peace argues that whatever the reason that states become involved in interstate conflicts, trade creates incentives for states to avoid interstate conflict. Thus, whatever process leads to interstate conflict, trade between the states potentially involved acts to reduce the likelihood of conflict because it creates an economic harm if a conflict ensues. Hence, there are interests to avoid interstate conflict.

4.4 Unbalanced Trade and Signaling-Based Logic

As discussed in previous chapters, there is an alternative explanation for the liberal peace that does not rely on "interests" generated by trade but still argues for the pacific effects of trading relationships in interstate conflict. Fearon (1995) has argued that if states could credibly signal their relative strength and resolve, there would always exist a nonviolent resolution to a dispute that would avoid the costs of conflict and the unpredictable outcomes inherent in war. If states could be shown the "post-conflict" results (where the relative capabilities and resolve are known by who "won" the conflict), they would find a solution to the dispute that they would prefer, pre-conflict, given the costs associated with actually participating in the conflict. Absent credible signals, adversaries cannot discriminate between truth and bluff and, therefore, cannot adequately calculate what pre-conflict, non-violent resolution would be preferable. Thus, in this "signaling-based" explanation, military conflict is a product of uncertainty arising from the inability of adversaries to credibly communicate their resolve, capabilities, and intentions. Structures, procedures, or conditions that alleviate the uncertainty should reduce the likelihood of conflict.

The signaling-based logic assumes a different underlying conflict process model than the interest-based argument with respect to the liberal peace. The interest-based hypothesis contends that the presence of an economic relationship changes the overall balance of states' interests, leading states to choose the benefits of peaceful economic exchange over the benefits of interstate conflict. The signaling logic posits that the economic relationship allows states, already predisposed to avoiding interstate conflict, to find ways to credibly communicate and avoid conflict. Hence, both interest-based and

signaling-based models are consistent with the liberal peace thesis, but have different

mechanisms by which economic relations discourage interstate conflict.

4.5 Theoretical Basis for Focus on Unbalanced Trade and Interstate Conflict

While it is not immediately apparent, the logic of the signaling-based hypothesis

allows that there are situations in which the existence of economic ties can increase the

chances of interstate conflict. The signaling logic operates by using the economic

relationship between states to provide both sets of decision makers a clearer

understanding of the intentions, resolve, and capabilities of other parties, and conflicts of

interest are less likely to become militarized, and militarized disputes less likely to

escalate to war (Gartzke *et al*., 1999; Gartzke *et al*, 2001). As was discussed in Chapter

Two, trade serves both as an additional pathway through which information about the

opposing state can be gathered and, when trade relations are deliberately threatened, as a

credible signal of resolve or capability. As such, trade can operate to reduce interstate

conflict by allowing credible communication between trading partners. What I will argue

here is that, in an unbalanced trading relationship, both of these channels are distorted

and the signaling logic operates to push the more dependent state into taking more

conflictual actions (initiating an interstate conflict and/or making more hostile an ongoing

interstate conflict).

Part of the signaling logic operates from the ability of states to "prove" through

costly actions that they are speaking truthfully, thus facilitating a resolution. As Morrow

(1999) notes, "…higher trade flows could create a way to avoid escalation to war. Crises

provide states with an opportunity to signal to one another credibly about their

unobservable resolve through costly signals. States with a greater ability to impose costs

on themselves through their actions provide more credible signals of their unobservable resolve, and so crises between such states are more likely to reach a peaceful conclusion." (Morrow, 1999, pg. 487).

The greater the gains a state receives from an economic relationship, the more costly it is for the state to threaten or end the relationship. It is this costly action that allows the state to demonstrate its resolve/capability to the other side. States with high levels of trade can credibly communicate high levels of resolve by imposing upon themselves actions that have high costs. In asymmetric economic relationships (unbalanced trading between partners), the unbalanced economic ties lead to unbalanced signaling. The more dependent state can signal more credibly because of the economic costs it can impose upon itself (the less dependent state cannot credibly signal, as it is not threatening to sever any costly economic relation). However, the more credible signals can only mitigate interstate conflict if they are received and understood by the less dependent partner (the second part of the signaling logic).

To the degree that the economic relationship is asymmetric (i.e., one state has more need for the economic relationship or would be more economically harmed by disruption of the relationship), the less dependent state is likely to be less informed and less likely to observe signals sent by the other party (this is the "tripwire" or informational part of the signaling-based logic of the liberal peace). This argument requires a reasonable, but debatable, assumption: the more significant the economic relationship, the greater attention is paid to the information and "tripwire" signals. In other words, the more dependent the state, the more effective the economic relation serves as a channel for credible signaling: dependent states "listen really hard" (because

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of their dependence), while non- or independent states do not "listen really hard" or at all (because their independence makes the economic relation less important). For unequal trade, the more dependent state should be more informed and more aware of the resolve/capability of the more independent state, while the more independent state is less informed.

Here is where the other part of the signaling-based liberal peace logic operates, and leads to the empirical tests that anchor this dissertation. According to the signaling logic, states avoid conflict by being able to credibly signal resolve and/or capability to the other state, thus facilitating the bargaining process and allowing a peaceful resolution that is mutually agreeable. In unequal trading relationships, the more dependent state is able to signal more credibly (because of the dependence), but the less dependent state is not as informed (for the reasons argued in the previous page) and is not paying attention as clearly (because the economic relationship is less important). States have multiple links and channels to other states, and will give attention to whatever relationship and signals seem most important at a given time. Inattention to the signals from the dependent states may make these signals indistinguishable from background noise.[71] This is a classic case of irony: the more dependent state is able to signal more clearly because the dependency creates higher costs and thus stronger signals, but the less dependent (or more independent) state may fail to observe these signals because of the limited relevance of

[71] This argument is, in a sense, similar to the "negative transparency" argument of Finel and Lord (1999), discussed in Chapter Two (pg. 44). They rebut the "democracies are more peaceful because they are transparent" argument by noting that there is so much talk, activity and debate that foreign observers are unable to clearly perceive what signals the democracy is sending. My argument here is that in a complex world of multiple connections between states, it is difficult for states to identify the right channels for correct information. In relations between states, there is always a significant amount of formal and informal talk, bluff, actions, and statements – "noise", in other words. Finding the right channel, and interpreting the communication is difficult, especially if the economic relationship is small to irrelevant. Finel and Lord make an argument specific to democracy; I am expanding that logic to all relationships (political, economic, etc.) that provide channels of communication.

this specific economic relationship to the more independent state. Hence, dependent states (who can more credibly signal but, conversely, are not being observed as closely by less dependent economic partners) must take more and more forceful actions to "punch" their signals through the noise of information, bluffs, statements and actions that the less dependent state receives on an daily (or hourly) basis. Those forceful actions can rise to the levels of initiating interstate conflicts, or using more hostile actions in an ongoing interstate conflict.

In sum, the greater the importance of an economic relationship to a state, the more attention it pays to that channel for signals. For a pair of interdependent states, the significant economic relationship for both allows two-way signaling that serves to reduce the chance of conflict (thus, the liberal peace). The less important the economic relationship, the harder it is for signals to penetrate and the clearer and louder the signals must be to stand out and be recognized.[72] *These costly signals can include initiating or pushing interstate conflicts to higher levels of intensity.* This, then, is the key to the empirical tests in this dissertation: the interest-based logic of the liberal peace argues that trade always reduces interstate conflict, while the argument here is that the signaling-based logic of the liberal peace allows for interstate conflict to occur or escalate in some cases (as part of the signaling process adopted by dependent states).

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[72] There is a caveat here. While the more independent state is, *ceterus paribus*, less informed because the economic relationship is less important, it is possible that different levels of state resources can offset the information asymmetry. In other words, the larger state cares less about a trading relationship with a smaller state, giving the smaller state incentives to have more information about the larger state (the smaller state cares more about the economic relationship), but the larger state potentially has more resources to devote to gathering political information (it might have, for example, a CIA, a State Department, military intelligence, and other agencies), and so the gap in perception due to importance of the trading relationship could be partially filled by the greater resources of a larger state. While this argument negates the informational aspects of the signaling argument, it allows states to signal through the "tripwire" aspects of threatening to harms ones own state to make statements more credible.

Given this interpretation of the signaling logic, one could question whether this is really a theory of liberal peace. Unlike the interest-based arguments, where economic relations constrain the actions of states, the signaling-based arguments outlined above put forward the notion that states will undertake hostile actions with economic partners in order to avoid even more violent interstate conflict. In other words, this is not a theory of peace, but one of potentially hostile communication that has the possibility of peace.

There are two responses to this. First, one could grant that criticism and argue that the semantics of whether the signaling-based logic is or is not a theory of peace are mostly irrelevant to the more central question of the relationship between economic ties and interstate conflict. The fact that the signaling logic does not operate through constraining states' actions is no reason to dismiss it. Second, the discussion above outlined the possibility of conflict when there is unbalanced trade between partners. For states where there is little trade on both sides, both theories argue that trade is irrelevant to questions of interstate conflict. In cases where there is interdependence, the situation outlined above will not occur. Both states would be informed and attentive to the other, and resolution of differences should never rise to the level of threats to sever the economic relationship. The presence of the interdependent economic relationship allows the states to communicate more effectively, allowing the states to reach an agreed bargaining solution without costly actions. In this case, the signaling logic is a theory of peace.

4.6 Theoretical Summary

It is not clear that increasing economic ties will reduce the likelihood of military conflict between states. Interdependent states should, by both the interest-based and

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signaling-based logic, observe a reduced likelihood of interstate conflict – the liberal peace. However, for economic partners where there is an imbalance in the relationship, the two logics behind the liberal peace argue for different outcomes. The interest-based logic argues that a more dependent state is more constrained through the dependence than an independent partner, and would be less likely than the partner to engage in interstate conflict or make ongoing conflict more hostile. The signaling-based logic argues that dependent states use their dependence to prove their credible statements to distracted independent states in order to achieve a mutually acceptable bargain – and some form of interstate conflict is possible as a method of demonstrating credibility. The fact that the two underlying logics of the liberal peace argue that economically dependent states should act in demonstrably different ways points to the key empirical tests of this dissertation: what are the effects of economic dependence on the actions of states with respect to initiating interstate conflict or using more hostility in an ongoing interstate conflict. The results of this empirical test will tell us something about the underlying logic of the liberal peace. Previous research on the liberal peace has not focused on this specific question. Large-N studies to date have generally been of a probabilistic nature, seeking to determine what characteristics of states have been most significantly associated with the initiation of interstate conflict. Intensity of interstate conflict is not generally studied. The table below distills the different logics that potentially describe the relationship between economic interdependence and interstate conflict.

Table 4.1: Predictions of Liberal Peace Logics

| Liberal Peace Logics | Trading Relationship | |
	Balanced Trade Between Economic Partners	Unbalanced Trade Between Economic Partners
Interest-Based	**Less Conflict for All**. Increased trade works to reduce interstate conflict for both states.	**The Dependent State is more Pacific**. Economically dependent states are restrained from taking conflictual action. Non-dependent states are unrestrained.
Signaling-Based	**Less Conflict for All**. Trading relationships between economic partners create signaling channels that allow both states to avoid interstate conflict.	**The Dependent State is more Belligerent.** Dependent states are more likely to initiate conflict and use more hostility in conflict in an effort to communicate.

By empirically testing the degree of economic imbalance between partners against the states' conflict propensity, we can advance the study of the liberal peace in two ways. First, we can directly test the two underlying logics of the liberal peace against each other to see if one proves more empirically accurate. If so, we can begin to understand the causal mechanism by which the liberal peace itself operates. Second, by examining the effects of economically dependence on states, we can begin to find out if there are any "boundary conditions" where the liberal peace fails to operate. Additionally, by examining the little-studied issue of interstate dispute intensity, a different sort of "boundary condition" test is performed.

4.7 Research Design

The empirical component of this dissertation examines the actions of states with respect to conflict initiation and conflict intensity or hostility. As the table above

indicates, the key question is the effect of the degree of economic dependence on the propensity for interstate conflict for states. If more dependent states are more hostile and are willing to use more intense forms of conflict than less dependent states, that is evidence in favor of signaling-based explanations of the liberal peace. Just as important, regardless of the signaling versus interest debate, this is evidence that there are limits to the liberal peace - that asymmetric trade can increase the probability of conflict and also lead to higher levels of hostility in a resulting interstate conflict. If dependent states are less likely to take aggressive actions, this is evidence in favor of interest-based explanations of the liberal peace. The central question, then, is: What are the effects of economic dependence on the actions of states with respect to initiating or intensifying interstate conflicts? Given this question, the remainder of the chapter is a discussion of how that single question is turned into an empirical test.

Our concern is the relationship between economic dependence and the initiation and intensification of interstate conflict. We want to test empirically how consistently these factors covary (by lagging independent variables, this will be argued to be causality). To do this, we need to translate the concepts of economic dependence and conflict initiation and intensity into measurable variables (i.e., indicators) and construct generalized equations that express the relationship among the indicators. Assuming the auxiliary theory (Blalock, 1968) linking our indicators to the original concepts is compelling, we can, from our empirical analysis, draw conclusions about the "true" relationships among the concepts.

We need to construct a data set that organizes information in terms of comparable cases. Most of the large-N empirical analysis of the liberal peace have used interstate

dyad-years as the unit of analysis. As described in a previous chapter, this

methodological approach starts from the assumption that a pair of states can engage in

some form of interstate conflict[73] in any given year. The use of annual intervals is

primarily a matter of convenience - many statistics regarding states are gathered on a

yearly basis. For each year, we note whether a given pair of states has high or low levels

of trade or engaged in some form of interstate conflict. We then record the same

information for the same pair of states in the next year, and then the year after, and so on.

We do this for every pair of states, for every year of available information. The result is a

data set constructed in terms of interstate dyad-years in which discrete data points

become part of a continuous time series. For example, the nature of relations between

Cuba and the United States from 1901 to 2000 can be broken down into 100 observations

that record whether or not, in any given year, the United States and Cuba engaged in

interstate conflict. By collecting data about the dyad in all those years, researchers can

use statistical techniques to determine the correlations between independent and

dependent variables. As noted, the use of dyad-years as the unit of analysis has been the

approach most frequently used in large-N studies of the liberal peace.

The problem with this approach is that dyads do not make decisions with regard

to interstate peace or war - only states (or, to be more precise, decision-makers within the

states) do that. By focusing on dyads, liberal peace researchers are unable to point

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[73] While there are many different definitions of "interstate conflict" with different data sets underlying each
definition, a general definition is all that is necessary here. By "interstate conflict" this dissertation is
referring to hostile political actions by one state against another that involve some aspect of military threat
or action. States have disagreements with other states on an almost daily basis, but only when those
disagreements rise to the level of one state overtly threatening or employing military force to resolve the
dispute is this an interstate conflict. Thus, purely political disagreements (threats to end a treaty, threats to
sever relations, threats to end trade, etc.) are not interstate conflicts. This definition is mostly consistent
with the Militarized Interstate Dispute research program (see Gochman and Maoz, 1984 for a discussion),
but is not inconsistent with the underlying logic of the Interstate Crisis Behavior (ICB) data set by Brecher
and Wilkinson (1989) or other conflict data.

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directly to actions by states that are pacific or hostile in nature - only actions by dyads. Moreover, the focus of this project - comparing the effects of varying degrees of dependence on the actions of states with respect to interstate conflict - requires looking specifically at states, not dyads. Dyads cannot be dependent or independent, only states can be. Dyads cannot choose to initiate or escalate interstate conflicts, only states (i.e., the decision-makers within) can.

There is another unit similar to dyads that have been used in peace studies that allow the researcher to retain the focus on states. That unit, the "directed-dyad", takes each dyad and divides it in two. A pair of cases is created from each dyad. The US-Cuba dyad can be broken down into the US-Cuba directed-dyad and the Cuba-US directed-dyad: US actions directed at Cuba, US trade directed at Cuba, US political statements directed at Cuba versus Cuban actions directed at the US, Cuban trade directed at the US, Cuban political statements directed at the US. Thus, two directed-dyads are created for every interstate dyad. For the remainder of the dissertation, I define the *primary* state in the directed-dyad as the state of primary focus in the directed-dyad. The *partner* state is the other member of the dyad. In the US-Cuba directed-dyad, the US is the primary state and Cuba is the partner state. In the Cuba-US directed-dyad, Cuba is the primary state and the US is the partner state. In sum, by creating directed dyads, we can identify asymmetric relations and isolate the behavior of each state within a dyad toward the other. This approach allows a focus on the actions of specific states, but with reference to specific partner states.

The advantages of this approach are clear. With respect to interstate conflict, use of directed-dyads allows the data to reflect the actions of each state in the interstate

conflict. For example, it allows us to know which state initiated a dispute and which state used higher levels of hostility within the conflict. With respect to questions of economic dependence, use of directed-dyads means the relative level of dependence by the primary state on the partner state can be precisely specified. By using directed-dyads as cases we can investigate the actions of economically dependent states in conflict initiation and intensity of conflict. While directed-dyads have not generally been the unit of analysis in large-N studies of the liberal peace, they are particularly appropriate given the focus of this dissertation. Similarly, previous work on the liberal peace has tended to focus on the question of conflict initiation. Given the logic of the theoretical arguments presented in previous chapters and summarized in Table 4.1, we need to also look at willingness of dependent states involved in conflict to use more intense forms of conflict.

4.8 Previous Research: The Liberal Peace and Directed-Dyads

While few studies have used directed-dyads (as opposed to dyads) as the cases studied, what articles that do have all been relatively recent. This argues that more and more liberal peace researchers are recognizing the utility of directed-dyads in questions of trade and interdependence.

The first article that used directed-dyads in a liberal peace context was Bennett and Stam (2000a). They provide a partial test of the competing liberal peace logics with respect to the initiation of interstate conflict. The primary objective of the article is to discuss how the choice of research design, methodology and empirical procedure influences the outcome of empirical analyses. They use liberal peace studies as their example. As part of their exercise, they conduct a directed-dyad test of interstate conflict initiation using standard liberal peace independent variables and cases (Oneal and

Russett, 1999c; Oneal and Russett, 2001). They find that the more a state trades with a partner the less likely that state is to initiate an interstate conflict with the partner, though the effect is less significant than "traditional" (non-directed dyad) tests. While this outcome is consistent with the interest-based logic of the liberal peace (states seek to avoid interstate conflict in order to avoid the harm that occurs from disrupting the economic relationship), the analysis is not intended to be a definitive test of logics underlying the liberal peace. Bennett and Stam make no attempt to determine the relative level of interdependence/dependence between economic partners, looking only at relative levels of trade. Such a determination is essential for testing the signaling based logic. Thus, while Bennett and Stam add empirical weight to the plausibility of interest-based arguments of the liberal peace, the validity of the signaling-based logic remains an unresolved issue.

Kim and Rousseau (2005) also used directed dyads in a test of the liberal peace. Their focus, like Bennett and Stam (2000a), is methodological, not theoretical: they argue that all liberal peace studies have neglected the simultaneity of trade and conflict. It is not, they argue, that trade makes for pacific relations, it is that trade and conflict mutually affect each other (greater trade causes more peace, which leads to greater trade, etc.). Thus, all previous liberal peace studies (including Reuveny, 2001 and Reuveny and Li, 2003 which both specifically address simultaneity, though not with the method that Kim and Rousseau use) are invalid, or at least flawed. While it is beyond the scope of this dissertation to argue the methodological points, their use of directed-dyads parallels Bennett and Stam (2000a) and this dissertation. Their results, however, back their own theory: that when a simultaneous account of trade and conflict is made, there is no liberal

peace. However, as a baseline study they run a single-stage directed-dyad model (similar to what Bennett and Stam, 2000a, do) and find that higher trade dependence (trade/GDP) significantly correlates with lower incidents of interstate conflict (not MIDs; they use an alternative measure of interstate conflict derived by Rousseau, and thus far used exclusively by him). In this sense, previous research using directed-dyads has found similar results to the main liberal peace studies (Russett and Oneal, 2001).

The most relevant of the directed-dyad articles is Hegre (2004). Hegre argues specifically that asymmetric trading relations and asymmetric power relations do not necessarily offer the same motives for trade bringing peace as do symmetric trade and power relations. Hegre points specifically to Hirschman's (1980, discussed above) discussion that asymmetric trade relations may not generate pacific relations when economic dependence is used in an attempt to coerce political concessions. He uses a derivation of Dorussen's (1999) complex mathematical simultaneous equations model that relates trade, power and interstate conflict. Hegre's goals differ from this dissertation: he seeks to explore how asymmetric trade and asymmetric economic size affects the conflict propensity of states. While that is a part of what this dissertation does, I go beyond that and argue that the behavior of states in asymmetric trading relationships is the key test of the underlying logics of the liberal peace (I also mostly ignore questions of asymmetric economic size between trading partners). Moreover, he uses Cox regression (hazard modeling) as the method of his analysis (this is necessary given the simultaneous equations model he uses that relates trade, power and conflict). His results support the assertion that asymmetric trade differs from symmetric trade in terms of interstate conflict propensity. Specifically, he finds that greater trade brings more pacific

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relations (a standard liberal peace results), but also that as the economic size asymmetry between two partner states increases (as one state becomes economically much larger than the other) there is a larger probability of interstate conflict (regardless of the relative trade between the two states). Moreover, and most significantly, these two effects interact, so that the most pacific relations are found between trading states of comparable economic size. Trade between states of different economic sizes significantly increases the chances of interstate conflict (even compared to states of different economic sizes that do not trade). Hegre (2004) looks at issues of size and trade asymmetries, while this dissertation focuses almost exclusively on only trade asymmetries. Nonetheless, his results argue that cases built from directed-dyads, especially in testing the effects of trade on different types of economic relationships, are likely to be fundamental to any research that seeks an understanding of the relationship between trade and interstate conflict.

4.9 Previous Research: Liberal Peace and Interstate Conflict Intensity

There is little previous empirical work that directly looks at issues of trade dependence and the intensity of interstate conflict. Barbieri (1995, 1996) looks at what she calls "conflict intensity"- the highest levels of conflict recorded between pairs of states involved in interstate conflicts - and, using ordered logistic regression techniques, tries to determine whether the level of trade within a dyad is correlated with the intensity of conflict between states.

She breaks down the results by temporal period. She finds that prior to World War Two (1870 – 1938), there is no effect of interdependence on the intensity of interstate conflict. After World War Two (1950 – 1985), some of her measures of interdependence are positively related to the likelihood of intense conflict between states.

Statistical results for the entire temporal domain (1870 – 1985) reveal no statistical

relationship between economic (i.e., trade) interdependence and conflict intensity. Thus,

what statistical evidence exists in this little-examined area of liberal peace studies runs

counter to the preponderance of findings regarding conflict initiation, namely, that

increased trade dampens conflict initiation.[74]

Of more direct relevance is Gartzke's (2002, unpublished) argument and analysis

of conflict escalation. He recognizes the fundamental differences between the interest-

based and signaling-based logics for the liberal peace and argues (as does this

dissertation) that one can construct an empirical test to determine which logic drives

states to make decisions with respect to interstate conflict. However, he argues that

increased escalatory behavior by a state would be evidence in favor of an interest-based

logic and de-escalatory behavior would support signaling-based logic.[75] Why are

Gartzke's arguments different?

Gartzke's conception of the interest and signaling logic is the same as presented

here. What differs is the research design used to translate the theory into empirical tests.

Gartzke selects as cases for his analysis all dyads where a dispute has escalated in

severity to "diplomatic-economic hostile action"[76] and potentially higher levels. He

argues that this is the threshold level for signaling: at this level of severity, states have

damaged their economic relations, which makes their statements of resolve/capability

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[74] Barbieri (2002) limits herself to an examination of the effect of economic dependence on the probability of a militarized dispute escalating to a war. She finds a correlation between greater trade dependence and increased incidence of war, consistent with her previous work described here.

[75] This is the opposite of what I argued in Table 1, above.

[76] The source of data for Gartzke is events data - a daily log of friendly, neutral, or hostile actions by one state against another - specifically the COPDAB data set. COPDAB categorizes actions on a 15-point scale (low numbers represent cooperation, high numbers conflict). Gartzke selects all dyads that engage in conflict coded at level eleven ("diplomatic-economic hostile action) and higher. See below for a further discussion of events data (COPDAB) versus aggregated data for the dependent variable.

more credible, and hence have made a settlement more likely. His empirical test examines the correlation between levels of trade and further escalatory actions into military conflict (to COPDAB levels thirteen or higher). He shows that, for dyads involved in political conflict (having escalated to damaging economic relations but not any form of militarized conflict), the greater the economic relationship, the less likely is the conflict to escalate to higher levels of hostility (some form of military conflict). For Gartzke, once the conflict reaches a level where economic relations are damaged, then signaling has already occurred, and the absence of further escalation is proof that the signaling was successful. Conversely, finding that dyads with lower levels of trade engage in higher levels of escalation, suggests that the absence of an ability to credibly signal led the states to engage in more hostile actions.

Why does Gartzke not argue that the interest-based logic for the liberal peace constrains states from escalating, as I have argued here? Gartzke's conception of how interdependence creates interests in avoiding conflict is similar to the argument here, but not identical. He argues that once states have escalated a dispute across a threshold then all trade ceases, and states have already lost the benefits they gained from trade. In other words, beyond a certain level of hostility in interstate conflict, all trade has ceased between economic partners, and there is no longer any motive for states to restrain their hostile actions. He argues this "threshold" level corresponds to the "diplomatic-economic hostile action" level in the COPDAB data (though does not empirically demonstrate this). Hence, for all of Gartzke's cases, he claims there is no trade and no interests operating to constrain states' actions. In other words, he has removed the

constraining effect of interests in his sample (by assumption), and he only tests to see if signaling is operating.

Overall, there are several difficulties with Gartzke's reasoning. First, the trade data that exist do not allow researchers to point to the moment when trade between states ceases or diminishes as a result of actual or impending interstate conflict. To assume, as Gartzke does, that the benefits of trade (and the interests and motivations they produce) end at the threshold asserted by Gartzke is a guess, and one that is not consistent with previous research. Li and Sacko (2002, see Chapter 3, pg. 129 in this dissertation for a discussion) show that trade only diminishes in the presence of a militarized dispute (a more severe conflict than Gartzke uses as his threshold), and does not end. Even this result may overstate the trade disruption, as Barbieri and Levy (1999) show some cases of trade continuing even between warring states.

Moreover, logically, it makes even less sense to assume that trade is an on/off relationship, ending suddenly as interstate conflictual relations pass a certain threshold. Given the large numbers of actors (firms, state-enterprises, individuals, etc.) involved on both sides of a bilateral relationship, some may continue to trade far longer than others (more risk-averse economic actors). A more reasonable assumption is that, as the risk of war increases (as states escalate to higher levels of economic, political, and militarized interstate conflict), trade falls, bottoming out at some minimum level (which could be zero or not) as states go to war with each other. Gartzke's assumption that trade ceases at his set threshold level of escalation and, hence, has no motivating effect for interest-driven states is difficult to test, but does not seem the most reasonable assumption. If, instead, trade reduces as states escalate a conflict, then states will continue to have an

interest in avoiding further escalation at any stage of an interstate conflict, and empirical evidence showing that dyads at higher levels of trade are less likely to escalate (as Gartzke's empirical tests show) could be interpreted as demonstrating that dyads that have the greatest motive to avoid damage (those states with the most trade) behave in accordance with the interest-based logic.

Second, Gartzke's empirical test makes no argument about the effect of economic dependence or independence on state actions. He focuses on the actions of the dyad, not the component states. I argue in this dissertation that the signaling-based logic implies that economically dependent states should be more likely than less or non-dependent states to escalate disputes in order to signal greater resolve/capability. The key to a test of interest-based logic versus signaling-based logic is to examine the actions of states, not dyads, with respect to the state's, not the dyad's, economic dependence on the partner. Gartzke misses this crucial point.

Finally, he must find some logical reason why interests are not operating for the cases he examines, else both logics of the liberal peace will point to the same conclusions. Gartzke argues that the level of hostility reached for his cases is sufficient to damage economic relations, and serves as a signal to enable de-escalation. If interests were operating, they too would constrain states and enable de-escalation. He reasons that the economic relationship between the states is over, and hence interests have ended. However, if interests affect state actions, it may be reasonable to assume that states (or domestic economic actors) may be concerned about loss of future benefits and, therefore, calculate these costs even as conflict escalates. Thus, the hope that as conflict de-escalates trade may resume serves as a constraint on state behavior. Gartzke assumes that

when trade disappears, the benefits and expectations of future benefits also disappear, and once a state has crossed that threshold there are no further economic motivations that restrain state actions. This is only true if the state in question loses the trade completely (debatable, see above) and the state does not expect the trade to ever (or at least in the foreseeable future) resume. While this may be possible in the event of war between states, where costly damage and death can give states and their populations a clear motive to dislike or hate a former antagonist, it is more reasonable to assume that states in the midst of an interstate dispute can expect to resume normal political relations at some point in the future, and economic relations can resume as well. If the state can reasonably expect the lost trade to resume, the state can continue to have an interest-based motive to avoid escalatory behavior. Gartzke's empirical tests that show the greater the trade in the dyad, the less likely the dyad is to escalate an ongoing political dispute to a militarized level should be interpreted as evidence in favor of the interest-based logic of the liberal peace, as trading partners seek to reduce conflict to recover lost economic benefits and encourage the political leaders to reduce political conflict to facilitate economic benefits.

Overall, while Gartzke's paper examines issues relating to competing theories of the liberal peace, his research design and discussion of the logics are sufficiently different from the ones presented here to warrant further examination of this subject. This dissertation takes a different approach with regard to the logic of liberal peace arguments and the use of directed dyads, and has the potential for improved results. The remaining sections of this chapter will discuss the specifics of the dependent variables, the economic dependence variables, and the alternative independent variables.

4.10 Research Design: General Discussion

The following sections outline the specific operationalization of the variables used to empirically test the relationship between interstate conflict and unbalanced economic relations. As discussed above, the dissertation will examine both interstate conflict initiation and interstate conflict intensity. Two separate sets of tests are envisioned: a test of the effect of economic dependence on conflict initiation, and a test of the effect of economic dependence on conflict intensity. The tests will parallel each other as much as possible in order to facilitate comparison. However, there will obviously be differences: primarily the dependent variable (initiation versus hostility) and the cases examined (there are far fewer cases of ongoing militarized disputes than potentially initiated disputes).

As was outlined above, the key to both sets of tests is the focus on dependent economic relations. For the test of conflict initiation, I use the universe of directed-dyads. For the test of conflict intensity, I examine only those directed-dyads where there is an ongoing militarized dispute. In both cases, the independent variable of interest is a measure (described below) that records the level of salience and significance[77] of the dyadic trade for the primary state in the directed-dyad. When this measure is high, the trade between the primary state and the partner state is high *and* unbalanced against the primary state; when close to zero, trade in the dyad is close to zero or balanced; when negative, trade is unbalanced against the partner state. For the test of conflict initiation, the dependent variable records whether the primary state initiates an interstate dispute

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[77] While Barbieri (1995, 1996) uses the terms "salience" and "significance" to refer to specific measures she develops, I do not use the terms as references to her measures. For purposes of the dissertation, I construct a single measure reflecting the salience (importance) and significance (relevance) of trade for states.

against the partner state. For the test of conflict hostility, the dependent variable records the level of hostility used by the primary state against the partner state.

4.11 Research Design: Dependent Variables

This dissertation relies on the Militarized Interstate Dispute (MID) database associated with the Correlates of War (COW) project to determine which states participated in interstate conflict. There are approximately 2600 militarized disputes between recognized states from 1870 to 2000.[78] The MID data record which states were involved in militarized interstate conflict, noting the beginning/ending dates of involvement, action, resolution, and other factors. For disputes that involve three or more states (multiparty disputes), the MID data only record for each state involved their own actions – not which of the other states involved in the dispute the actions were aimed at. An evolution of the MID data corrects this problem and records the militarized dispute actions for every pair of states in both single and multiparty disputes. It is this version of the MID data[79] that is used in the dissertation.

There are alternative sources of data for interstate disputes, but the MID data are superior for two reasons: completeness and compatibility.

In general, interstate conflict data come in two forms: events data and aggregated data. Both forms of data provide counts of conflict escalation and initiation, but in different formats. Events data record actions of one state with another over a specified

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[78] While the MID data record militarized disputes as far back as 1816, economic data availability limits the temporal span. See Section 4.12, below.

[79] The Maoz Dyadic Militarized Interstate Disputes Dataset, version 1.1, via Eugene, version 3.040. For the purposes of this dissertation, the use of the Maoz data means all multi-party MIDs have been reduced to their dyadic (state versus state) components. Thus, multi-party MIDs are treated as independent cases. The Maoz data is a reformulation of the MID 2.1 data (Jones, Bremer and Singer, 1996). A new version of the MID data (3.0) has recently been introduced (Ghosn, Palmer and Bremer, 2004), though that version is not dyadic. Thus, this dissertation retains the Maoz data. Given the limitations of the economic data (only current through 2000), little is sacrificed by using slightly older conflict data.

time scale (daily, weekly, monthly, etc.), and categorizes those actions in terms of

varieties of hostile, friendly or neutral acts. Aggregated data record the actions of states

involved in conflicts as a summarized account of the entire interstate conflict (length of

time, most hostile action, etc.). The most well-known events data project is GEDS

(Global Event Data System, an heir to the earlier Conflict and Peace Data Bank

(COPDAB) - project). The most commonly used aggregated conflict data are the

Militarized Interstate Dispute (MID) data.

In terms of compatibility, the economic data underlying measures of dependence

come from aggregated trade and national economy figures, most of which are not

available for intervals shorter than a year. Similarly, other covariates to be employed in

the analysis are also available only as annual aggregates. Thus, while events data may be

a theoretically preferable basis for constructing an escalatory dependent variable (though

not the initiation variable), the daily recording of conflictive and cooperative interactions

among states is irrelevant, given the absence of comparable measures for the other

variables in the analysis.

In terms of completeness, the MID data have a more extensive spatial and

temporal range than either the GEDS or COPDAB events data sets. The spatial-temporal

domain for the GEDS project currently encompasses only the Middle East and the

developed world for the period 1979 to 2000. In contrast, the MID conflict data include

all militarized interstate disputes from 1816 to 2001[80]. Thus, the aggregated MID data

will allow inclusion of significantly more cases of conflict, and conflict from times not

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[80] While the dyadic (Maoz) MID data are current through 2001, the economic data used here only record
cases through 1992 (for the Barbieri data) or 2000 (for the Gleditsch data). Thus, the limiting issue with
respect to cases is the economic data, not the conflict data. See later in the chapter for a more extensive
discussion of the differences between the Barbieri and Gleditsch data.

dominated by the Cold War. Given the paucity of interstate conflicts in general (if there are only 2600 militarized disputes, for example, from 1870 to 2000 – the span of available economic data – then there is a limit of only 2600 initiations that can be tested; given that there are, in the case of the Gleditsch data, almost a million directed dyads just in the 1950 to 2000 range), the more cases of conflict that can be included in the empirical test, the more variation in the dependent variable is included in the statistical analysis, and the more confident we can be of those results. The gain derived from the more extensive temporal domain substantially exceeds the limitations introduced by the aggregated form of the data.

In order to fully test the theories outlined above, in addition to measuring the propensity of states to initiate conflict, we would ideally measure the propensity of states involved in conflict to escalate to more hostile forms of conflict. By the signaling-based logic, dependent states should be more willing to escalate to higher levels of conflict intensity than non-dependent states. The use of events data (as Gartzke, 2002, used), which record the back-and-forth actions of states in interstate conflicts, would seem a good approach for measuring escalation. As outlined above, however, for reasons of completeness and compatibility the aggregated MID data are a better measure than the events data available. What is clear, however, is that the MID data, which only record the highest levels of hostility or intensity used by one state against another during a single militarized dispute, do not directly record escalatory actions (which, according to the logic presented earlier, are signals).

While direct evidence of escalation would be ideal for this empirical test, it is not necessary. In order for the dependent state to effectively convey to the non-dependent

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state its credible signal of resolve/capability (in the signaling-based explanations), it will need to escalate to a higher hostility level than states obeying the logic of the interest-based explanation (who want to avoid escalation or conflict in order to avoid any damage to the trading relationship). It is not just the rapidity of escalation that differentiates the two theories, but the end-points as well. Signaling-based explanations should see dependent states go to higher levels of overall hostility (compared to non-dependent states) in attempts to demonstrate their resolve/capability in comparison to states driven by interest-based explanations. Hence, examining the overall highest level of hostility a state reaches during an interstate conflict is a sufficient empirical test of interest-based versus signaling-based liberal peace theories. The aggregated conflict (MID) data allow for such a test, and the more "precise" information of the events data is not needed. So long as one accepts the premise in the MID data that the ordinal categories are accurately "ordered" (i.e., that threats to use force are less hostile than displays of force, which are less hostile than uses of force, which in turn are less hostile than outright war), the MID data are sufficient.

It is also worth noting that this approach has been used before. Others researchers have used the MID data in this fashion. Senese (1997) uses a similar conception of the MID hostility codes as conflict escalation (though he uses dyads rather than directed-dyads) and argues that specific evidence of threats coming before displays, and displays before uses of force, is not necessary to utilize the data in this fashion. All that is necessary, according to Senese, is that the ordinal categories show increasing hostility in order to be assumed to be escalatory actions, which the MID codes (threats, displays, uses) clearly do. Additionally, Barbieri (1995) employs this precise use of the MID

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hostility levels. In her later work (2002), she examines the effect of economic relations on the propensity of states involved in MIDs to "escalate" to the highest hostility level (war), which is again using MID hostility codes as proxies for evidence of escalation. The use of the MID hostility data is an acceptable measure that allows us to examine the influence of economic relations on the actions of states involved in interstate conflict.

It is worth precisely describing the MID data, as the categories the data record become relevant in discussing the empirical results of the tests. The MID data set records the highest level of militarized conflict in a dispute on a five point scale ranging from no action through threat of force, display of force, use of force and war. The difference between use of force and war is a difference of degree (the number of deaths in battle) rather than a difference in kind (as in the difference between verbal threat and display of force). The MID data record the highest level of militarized hostility or intensity undertaken by each state involved in a dispute and there are often differences among disputants within the same conflict. For example, one state may use force against another, while the other only threatens force. For the test of conflict intensity, for each dependent directed-dyad, I record the highest level of hostility used by the primary state against the partner in a conflict. I also record the highest level of hostility used by the partner against the primary state, as well, for use as an independent variable.

The most important difference between Maoz' Dyadic MID data and the data used in this dissertation is how each case is used. When creating the directed-dyads, each state in each dispute is the "primary" state once, and the "partner" state once (as opposed to dyads, when each state appears only once). The Maoz data, by assigning states to be either "A" or "B" ("A"s initiate, while "B"s are targets), only allows half the states to be

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the "primary" state, and the other half the "partner" state. When using directed-dyads

each state is the "primary" once and the "partner" once in each dispute. These are the

data used in the dissertation.

Table 4.2, below, describes the cross-tabulation of the hostility levels for the

Primary and Partner states.

Table 4.2: Crosstabs of MID Hostility Levels[81]

(Source: Maoz Dyadic MID data, version 1.1, Modified for Directed-Dyads; data based on the "restrictive"
set of conflicts.)[82]

	Highest	**Hostility**	**Level**	**By**	**Partner**	**State**	
Highest		No Action	Threat	Display	Use	War	Totals
Hostility	NoAction	2	110	427	660	0	1199
Level	Threat	130	12	38	31	4	215
By	Display	478	39	326	220	5	1068
Primary	Use	750	39	246	1320	11	2366
State	War	0	4	7	11	405	427
	Totals	1360	204	1044	2242	425	5275

For tests of conflict intensity, the sample of cases is all directed-dyads where a

MID has been initiated, and the dependent variable is the highest hostility level that the

primary state uses against the partner state in the course of the conflict. For tests of

conflict initiation, the sample of cases is all directed-dyads, and the dependent variable is

whether the primary state initiated a MID against the partner.[83] The latter is a very

similar measure to the one used by almost all liberal peace studies (modeled on Oneal

and Russett, 2001), only rather than record whether there was a militarized dispute within

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[81] This table covers the entire temporal span (1816 – 2000), while economic data availability issues reduce
the possible cases to only those conflicts from 1870 (or 1885, depending on data source) to present.

[82] "Restrictive" and "Broad" are described on the next page, and refer to how "joiners" (states that join an
ongoing interstate conflict) are treated in the data.

[83] For both the test of conflict initiation and conflict hostility the independent variables are measured in the
year prior to the measure of initiation or hostility. This is a standard practice in quantitative conflict studies
to ensure causality (by measuring the independent variables a year prior to the dependent variables we
know the conflict doesn't "cause" any effect on the independent variables). This is discussed more fully in
Section 4.13, below.

the dyad, the use of directed-dyads allows us to record which state of the pair actually initiated the dispute.

Additionally, there is a complicating issue of how to treat "joiners" in interstate conflict, i.e., states that choose to join either the initiating or target side in an ongoing interstate dispute. There are two approaches. First, one can argue that any state that becomes involved in an ongoing interstate dispute (on either side) is not making an independent choice to initiate interstate conflict (the state is drawn in by treaty obligations, new threats to its security, et cetera), and because of the lack of an independent decision, should not be part of the analysis. This is the "restrictive" set of cases, and by this count there are approximately 2270 cases of interstate conflict initiation from 1870 to 2000. The "broad" set of cases uses the argument that trade (and the other independent variables) influence state decision-making whether there is an ongoing interstate conflict between other states (partners, allies, enemies, et cetera) or not, and hence these cases should be included in the analysis. In short, every decision to engage in a MID (even if it is joining an ongoing MID) is a case of conflict initiation. By this count, there are approximately 3400 cases of interstate conflict initiation from 1870 to 2000. Both sets of initiations have theoretical validity, and both are used in this analysis. Where the two different sets of conflicts produce similar empirical findings, only one set of results is reported. Where they differ, the difference is noted. For the tests of conflict hostility, this difference between "restrictive" and "broad" initiations is irrelevant; the dependent variable records the level of hostility the primary state uses against the partner, and questions of initiation are irrelevant. Thus, instances of conflict hostility are much larger than either the "restrictive" or "broad" instances of initiation (because both the

actor and victim are included), though the total number of cases tested in the tests of initiation are significantly higher (because large numbers of "non-initiations" are also tested).

4.12 Research Design: Independent Variable - Economic Dependence

Empirical tests of the liberal peace have examined only the effects of trade on interstate relations, and have left aside discussions of other forms of economic interaction between states (loans of money, grants of money, direct investment by one state into another's economy, portfolio investment, et cetera). Theorists have not ignored the economic interaction beyond trade, but empirical measures that can reliably and accurately reflect flows of money do not exist.[84] Hence, empirical tests of the liberal peace have talked about measuring interdependence, but this has mostly been defined as trade.

The early theorists who discussed trade and international conflict spoke primarily of the idea that trade alone could pacify relations between states.[85] The common thread, until the recent academic discussions, was not that trade itself provided the motive for states to avoid conflict, but that trade could sometimes promote additional ties (Montesquieu's "manners") that could reinforce the pacific effects of trade. The logic here is straightforward and draws entirely from the "interest-based" theory of the liberal peace: trade provides a real (economic) reason to avoid conflict, but ties of personal feeling cannot be created quickly and can only be created and grow once trade has begun

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[84] The International Monetary Fund, which supplies the raw trade data that all liberal peace researchers use, does not collect figures on the amount of money that passes between states, nor does any other international organization collect the figures.
[85] "The spirit of trade cannot coexist with war, and sooner or later this spirit dominates every people." (Humphrey, 1983, pg 125, quoting Kant, 1795).

and flourished between states. Interdependence, in the modern sense of the term, was not considered.

While interdependence was not discussed or defined by the early theorists, it has come to be connected with the liberal peace debate in present times. It is likely the 1970s work by Keohane and Nye (1989[1977]; additionally Rosecrance, 1977)[86] that brought the ideas of interdependence to prominence in peace research.[87] Interdependence has been used primarily in the discussion of Realism and Neoliberal Institutionalism, as part of a much broader theoretical debate on the nature of international relations (Keohane, 1986; Baldwin, 1993; Grieco, 1988).

The idea of interdependence has been brought into the liberal peace debate slowly. Polachek, in his earliest work, uses the term "mutual dependence" (Polachek, 1980, pg. 56), noting that there are likely many different kinds of dependence, and his focus is on the economic kind. He then moves quickly to declare that he will take trade between states as a proxy for economic mutual dependence. In later work (Polachek and Gasiorowski, 1982), the operational term is "trade interdependence" and trade itself is portrayed as conveying both sensitivity and vulnerability[88] in the Keohane and Nye (1989[1977]) sense. Implied is the idea that trade is a significant (if not dominant) component of interdependence, and there is no need for additional data for other economic ties between states to create an overall measure of economic interdependence.

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[86] While there was research on interdependence before Keohane and Nye, the proximity of Keohane and Nye's influential *Power and Interdependence* (1977) to the modern foundational work on the liberal peace (Polachek's 1980 article) makes the earlier authors less important to the study of the liberal peace than those authors (primarily Keohane and Nye, but also Rosecrance, 1977) that were writing in the 1970s.
[87] Polachek (1980), in particular, refers to Keohane and Nye (1977) often, and the ideas of sensitivity, vulnerability, and complex interdependence figure prominently in the article.
[88] Keohane and Nye (1977) refer to interdependence in two forms: sensitivity and vulnerability. Sensitivity is defined as the degree to which changes in one country, transmitted by mutual interaction, cause changes in another. Vulnerability can be defined as the inability of a country to adopt alternatives to changes in the relations between the countries (definitions from Polachek and Gasiorowski, 1982, pg. 711).

Gasiorowski (1986) imports into his study the Keohane and Nye (1977) understanding of interdependence, discussing sensitivity, vulnerability, and interconnectedness (Gasiorowski, 1986, pg. 25 – 26). Trade is explicitly taken as economic interdependence: "Interdependence refers here to a particular kind of international relationship that emerges when countries are linked by interactions that can be both costly and beneficial. Countries trade, for example, in order to reap the 'gains from trade', a euphemism for the greater efficiency trade affords in the use of a country's productive resources…Unregulated trade can complicate domestic macroeconomic planning and create serious dependencies that expose countries to embargoes or blockades. International economic interactions thus simultaneously can have both costly and beneficial aspects, greatly complicating relations among the countries involved." (Gasiorowski, 1986, pg. 25).

Gasiorowski is typical of those who conflate trade and interdependence. The liberal peace was originally grounded solely in the idea of trade being the bridge for peace between states; interdependence (economic ties more extensive than trade alone) has been attached to the overall debate. While there is good theoretical reason for doing this (economic interaction beyond trade accounts for an increasingly large part of international economic relationships), there are empirical difficulties.

The logical link to expand the liberal peace discussion from trade only to include economic interdependence is clear: if trade has welfare enhancing properties, then more extensive economic ties (financial flows, economic agreements, foreign aid, private investment, etc.) create even more economic welfare for a state and even greater incentive to act to avoid ending the benefits of the relationship. Additionally, as

alternative economic ties grow in importance (Foreign Direct Investment as a percentage of the overall economy, Foreign Portfolio Investment as a percentage of the overall economy, et cetera), the relative importance of trade declines (other forms of investment grow, the importance of trade as a percentage of overall economic activity falls). The greater the potential economic cost to a state from ending an economic relationship due to interstate conflict, the greater the incentive to avoid the interstate conflict and the more economic ties can be seen as having pacific effects. Hence, if trade can bring peace, then other forms of economic links between states will function as trade does to restrain conflictual acts. In modern liberal peace studies, interdependence is discussed as being the theoretical basis for creating peace between states and a proxy measure for interdependence is trade between pairs of states.

One obvious objection to this stance is that trade is not necessarily a good proxy for overall economic interdependence; in other words, trade relations do not accurately parallel (or mirror) the more extensive economic ties that exist between states, especially modern states. As Keohane and Nye (1989) define it, "interdependence in world politics refers to situations characterized by reciprocal effects among countries or among actors in different countries. These effects often result from international transactions – flows of money, goods, people and messages across international boundaries." (Keohane and Nye, 1989, pg. 8 – 9). Economic interdependence is much larger than just the flow of goods, so looking just at trade as a measure may not accurately reflect the level of interdependence between states. However, attempts to move beyond trade have not been empirically successful.

Rosecrance *et al.* (1977) examines interdependence between states by looking at shifting factor prices in different regions on the assumption that if there are multiple economic channels between actors, then a reflection of that interdependence would be seen in the changing price of important goods (in other words, if they are interdependent, then the prices of important goods should move more or less similarly across the different states). Tetreault (1980) uses the same procedure to look at the interdependence of states within regions (Europe and the Middle East) and among a control group of random states from around the world. She finds, by examining factor prices, that interdependence among European states is high and approximately the same as interdependence in the Middle East at the time of her study (1974 – 1975) and only slightly higher than interdependence among the control group of random states (which should not be interdependent, as they come from different regions). As she notes: "[O]ne might reasonably expect to see a greater variety of relationships than 'very interdependent' and 'very, very interdependent', such as the correlation results suggest" (Tetreault, 1980, pg. 436). Her results cast doubt on the Rosecrance *et al.* strategy of measuring interdependence, which is more sophisticated than looking only at trade figure (as the liberal peace does).

The primary problem with finding a more extensive measure for interdependence is that, empirically, only trade data exist in sufficient quantities for use in statistical explorations of the liberal peace. Various international organizations have collected international trade data[89], but for several reasons data on other forms of international

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[89] The League of Nations, precursor to the United Nations, has international trade data going back to the nineteenth century, though it is not without errors and omissions. Subsequent to the Second World War, the International Monetary Fund collected data on international trade that remains the basis of a number of

economic exchange remain unavailable. First, until recently, international economic exchange was primarily trade – flows of money or other economic ties were relatively insignificant, especially for underdeveloped states (see Wade, 1996, for a discussion). It was not until recent decades that financial flows arising from deregulated financial markets, floating exchange rates, increased trade, and other economic factors, increased to the point where they were important considerations. Hence, the importance of non-trade economic factors in overall interdependence questions is debatable for anything but the most recent decades. Second, the data are more difficult to collect. Where trade is an accounting of the movement of physical goods (which are harder to hide and simpler to count), flows of money are more difficult to track and categorize. Given the proliferation of corporations doing business in multiple countries, financial flows may be unrelated to either the originating or receiving state.[90] Further, states sometimes have a political interest in concealing financial information. Military aid, a failing economy, lack of foreign exchange reserves, and other economic factors may encourage states to avoid accurately reporting the movement and stock of money for political (either domestic or foreign) reasons. Hence, little data are available, and the data that are available are substantially more suspect than trade data.

    In an ideal world, the non-trade dimensions of interdependence would be easily found and data for them widely available. This is not the case. Given that the use of only trade data arguably does not tell the complete story of interdependence, the central

recent studies. See Barbieri (1995) for a discussion of the IMF data and Russett (2001) for a recent use of IMF data.
[90] For example, if Mexico lends money to Finland, that could be seen as an economic tie between those two states. In fact, it may be that a Ford subsidiary in Mexico is investing in a new factory being built by Getrag (a German transmission manufacturer), who in turn is building the plant in Finland. How does one record this transaction? The money, in a sense, moved from Mexico to Finland, but did so because of agreements made in the US and Germany. It is not a simple set of circumstances.

question that arises is whether trade, as a proxy variable for all the forms of interdependence, is acceptable. In other words, does trade rise and fall for states approximately when other forms of interdependence (financial flows, investment, portfolios, foreign aid, etc.) rise and fall? A preliminary study, done for this dissertation (below), seems to argue that trade is reasonably correlated with foreign direct investment, and thus (by assumption) with other economic ties, and that FDI is substantially smaller than trade (thus reducing the relevance of FDI and making its omission less important).

The United Nations Conference on Trade and Development (UNCTAD) has collected limited Foreign Direct Investment (FDI) data. The data are only available from 1970 to 2003, and only report the total dollar value (in millions of dollars) of all investments in a country and by that country.[91] No attempt is made to record the flow of money from a specific state to another specific state: total amounts per state per year are the only recorded data. This is in contrast to the trade data that does record the trade between specific states. It is this specific state-to-state trade data that makes statistical investigations of the liberal peace possible. In this way, the UNCTAD FDI data are unsuitable for use any liberal peace study.

However, the FDI data can be compared with similar (state total) trade data to get some approximation of how closely the FDI flows from states match the trade flows from states. Table 4.3, below, gives the correlation between FDI and trade, and then gives the correlation for FDI and trade by decade.

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[91] Four categories of flows for each state are recorded: dollar amount of foreign investment in the state, foreign investment redeemed (sold/removed) from the state, dollar amount invested in other states by the state and dollar amount redeemed from investments and brought home.

Table 4.3: Correlation between FDI and Trade by Decade
(UNCTAD and Gleditsch data)

	1970 – 2000 (N=4875)	1970 – 1979 (N=1412)	1980 – 1989 (N=1551)	1990 – 2000 (N=1912)
Correlation	0.755	0.402	0.665	0.778

Table 4.3, above, indicates that trade and FDI (for the limited temporal span that both are available for comparison) are fairly highly correlated. Overall, the two variables correlate at a 0.75 level, high enough that if they were used together in OLS or logistic regression there would be potential collinearity problems and errors. Moreover, the decade by decade analysis clearly shows that over this four decade period, trade and FDI have become more tightly connected, rendering FDI even less important for analysis in the post-Cold War era.

Figure 4.1:   Average Global Trade and Foreign Direct Investment by Year
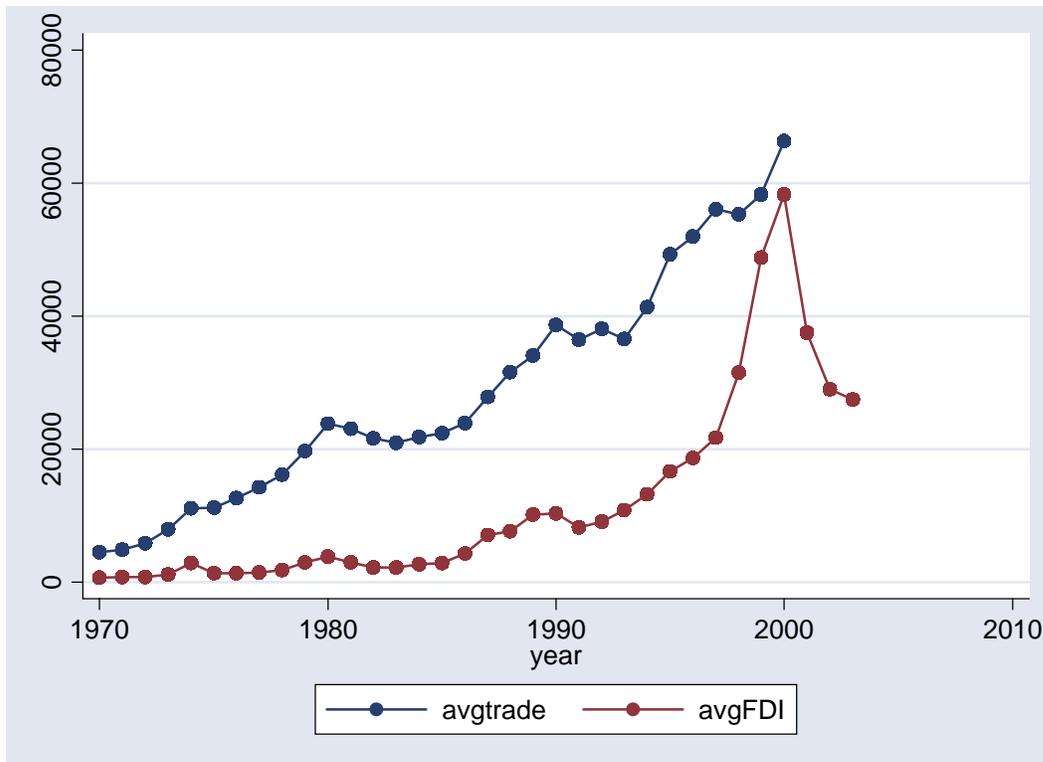
The top line in Figure 4.1 indicates the average value of trade for all countries in a given year (the x-axis), while the bottom line indicates the average value of FDI for all countries in the same year. Recognize that trade and FDI, while not identical, do follow a somewhat similar upward path. More importantly, the value of FDI is fairly minimal compared to the value of trade: average FDI is close to zero until 1986. The "value-added" of including FDI is minimal until FDI itself becomes important only in the last two decades.

This somewhat crude analysis is not meant to argue that FDI should be ignored, only that (a) given the form in which the data are available (no state-to-state data, like trade exist), (b) the high statistical correlation with trade data, (c) the general unavailability of data on FDI prior to 1960 (rendering it even less useful for analysis pre-1970 and arguably entirely irrelevant pre-World War II), and (d) the fact that the value of international FDI is significantly smaller than the value of international trade, all collectively suggests that the omission of FDI from an analysis of the liberal peace should not fundamentally threaten the validity of conclusions drawn from the analysis. Trade remains a good proxy for an overall measure of interdependence – not perfect, but reasonable.

When researchers have examined economic links in a more sophisticated manner, it generally has been done in the context of case studies rather than large-N statistical analysis. Papayoanou (1996) and Jungblut (1999) both argue that domestic politics is the key to understanding the link between economic interests and pacific foreign relations (both follow the "interest-based" theory of the liberal peace). Both researchers delve into the detailed history of specific cases to discuss not only trade relations between states, but

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also the more complex financial and political factors that influence overall state

decisions. This deeper understanding of the levels of interdependence makes the

connections between economics and politics more complete, but this method is clearly

not suitable for a general test of interdependence and the liberal peace.

One recent attempt worth noting is Gartzke *et al*. (2001) in which the authors

employ measures of mutual economic dependence that go beyond trade. The authors

develop measures of "pegging" that indicate whether the pairs of states have directly tied

their currencies together[92], and measures of the openness of the members of a dyad to

outside capital investment.[93] Both sets of variables prove to be statistically significant

and have the empirical effect of reducing interstate conflict, which is consistent with the

idea that greater interdependence reduces interstate conflict beyond the effects of trade

alone. However, the "pegging" variables do not provide a great depth of information

(they are binary) and the capital investment variables are not truly dyadic.[94] Nonetheless,

this is a noteworthy attempt to introduce measures of economic interdependence beyond

trade into the liberal peace debate.

While there is theoretical reason for researchers to think that states that are

broadly economically interdependent (not just tied via trade) are even more likely to

avoid interstate conflicts as a way of resolving differences, there remain significant

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[92] Specifically, two measures of pegging are included. The authors code whether one of a pair of states has pegged its currency to the other state (as Argentina had done with the US dollar) and a separate measure that codes if a pair of states together are pegged to a third currency.

[93] Two separate measures of capital openness are used. The first examines both states of a dyad and ranks them on eight indicators of openness to capital investment. Using the "weakest link" assumption (see Russett, Oneal and Davis, 1998), the score of the lower of the two states is employed. The second measure divides the overall value of direct foreign investment in a state (lagged by a year) by its GDP (resulting in a ratio). The lower value for the pair of states (the 'weakest link" assumption again) is employed.

[94] The capital investment variables measure the openness and amount of capital invested in a given year in a state not how much flow of money moved between the pair of states or the levels of overall stock investment by one state in the other.

(perhaps insurmountable) empirical challenges to developing a measure of dependence/interdependence other than trade. Moreover, what preliminary evidence exists seems to indicate that trade and (at least) foreign direct investment are reasonably parallel: that when trade rises, so does FDI. Hence, while trade does not account for all of a state's economic interdependence, it can serve as a proxy for all the other forms of economic interaction. For that reason, the focus in this dissertation will remain on trade.

Given a focus on trade as a basis for creating a measure of economic dependence, two central issues must be addressed. Oneal and Russett (2001) construct trade-based variables that look to the entire economy of a state to judge the level of economic dependence. Their approach has been widely followed by other liberal peace theorists. On the other hand, Barbieri (1995, 1996, 2002) constructs measures that focus on the total volume of trade by a state. Additionally, while the sources for trade data have been identical for all researchers (IMF data for 1948 and on, League of Nations data for pre-1948), analysts of the liberal peace have created different applications of the same trade data. There are sound reasons for employing each. Rather than adopt one approach or data source, I employ a comprehensive perspective and rely on two separate constructions of trade data (Gleditsch, 2002; Barbieri, 1995, 2002) to create multiple indicators of economic dependence. If the findings relating initiation or intensity to increased or decreased economic dependence are consistent across the different theoretical constructions of trade dependence we can have greater confidence in the reliability and validity of the results. If there are differences, further discussion of the data and operationalizations is warranted.

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These different approaches to measuring economic dependence stem from a common problem: the raw dollar value of trade is not an appropriate measure of economic dependence or interdependence among trade partners. Both the interest-based and signaling-based logics of the liberal peace depend on trade being significant in some sense to the economies of the states for there to be a liberal peace effect. Almost all empirical tests of the liberal peace draw on official International Monetary Fund data of the value (in current dollars) of trade between countries. This data series begins in 1948 and covers most trading countries in the world. Because the raw series makes no allowances for the different economic size and circumstances of countries, it is necessary to adjust the series to address the theoretical concerns of the liberal peace.

The most commonly used indicator of the significance of trade is the measure I (and others) call "trade dependence." This measure takes the dollar value of trade for a member of the dyad in a given year and divides it by that member's gross domestic product (GDP), creating a percentage measure of the overall dependence on trade for that dyad member. The greater the percentage, the more the overall economic activity of that state is tied to its partner state and the more economically dependent it is on its partner. This measure is the most widely employed indicator of trade dependence in large-N studies of the liberal peace.

An alternative theoretical approach employed by Barbieri (1995, 1996, 2002) divides the dollar value of trade for each member of a dyad by the value of that member's total trade, creating a percentage measure of the overall importance of trade with the partner state in relation to the total trade portfolio of the primary state. This is a measure

of "trade concentration"; how dependent a state is on a particular trade partner in comparison to its entire trade portfolio.

Barbieri's trade concentration measure has not been widely used within the liberal peace community. Russett and Oneal (2001, pg 133-134) offer a credible justification of why a GDP-based trade dependence measure is superior to a trade concentration measure. Using GDP as the denominator provides an indication of how important trade with the dyadic partner is for a state's entire economy; total trade as the denominator only provides an indication of how important trade with this partner is with respect to the import/export aspects of the state's economy.

While the trade dependence measure provides a clearer sense of how important trade is to the overall economy, there are still reasons to include Barbieri's trade concentration measure in an analysis of the liberal peace. The trade concentration measure provides information on the diversity of trade partners for a given state. It also sidesteps one potential shortcoming of the trade dependence measure. As Reuveny and Kang (1998, pg. 589) note, the use of trade/GDP ("trade dependence") can be misleading, as GDP can change while trade remains constant, leading to a faulty inference that trade has increased or declined, when in fact it has not. The inference can be avoided by careful examination of individual data points – something that is difficult in large-N studies. Because trade dependence and trade concentration reflect different aspects of economic ties that may have implications for the liberal peace, both measures will be used in this dissertation.[95]

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[95] Gartzke and Li (2003) argue that Barbieri's "trade share" (their term for what I have labeled "trade concentration") is inversely correlated with overall "trade openness" (a measure of the total trade for a state divided by its GDP), which itself is correlated with the Oneal and Russett "trade dependence" variable. Hence, according to Gartzke and Li, the different results of Oneal and Russett (and all others who use their

In sum, two types of trade variables are used in this study. Trade concentration measures what proportion of a state's total trade is with a particular partner. Trade dependence measures the proportion of a state's trade with the partner as a percentage of the state's overall economic activity. While both will be used in the dissertation, given the focus on dependence, the "trade dependence" measure is a better theoretical fit. However, the trade dependence measure alone is not sufficient, as it does not provide any sense of information about the number of trading partners and how narrowly (in the sense of how many other economic partners a state has) or broadly a state is tied into the overall global economy.

Given the theoretical importance of directed-dyads in this dissertation, the measures of trade dependence and concentration found in Oneal and Russett, Barbieri, and Gleditsch are not sufficient to test the arguments presented. For these ends, additional measures relating to the equality (balance or imbalance) of trade dependence and the concentration of trade for pairs of states were constructed.

In the liberal peace research program, the standard measure of trade dependence is the value of trade for a member of a dyad divided by that member's gross domestic product. This creates a measure of the extent of a state's economy that is dependent on trade with its partner state. Separate scores for trade dependence can be calculated for each member of the dyad. The relative trade dependence of each dyad member can be computed as a ratio of the dependence of the primary state to that of the partner state:

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data, or some part of it) and Barbieri (the most significant opponent to empirical findings in favor of the liberal peace) amount to a difference in variable specification. Gartzke and Li demonstrate mathematically that Barbieri's measure moves inversely to the measure of "openness," and conclude that her results are due to variable issues, not real empirical findings. Barbieri and Peters (2003) reject this critique and Gartzke and Li (2003b), in turn, reject the rejection. Since neither side admits their measure is in error, I continue to use both.

$$TradeDepen\,denceRatio\,(primarysta\,te) = \ln\left(\frac{TradeDepen\,dence\,(primarysta\,te) + 0.000001}{TradeDepen\,dence\,(partnersta\,te) + 0.000001}\right)$$

This formula for the trade dependence ratio incorporates the concept of a directed-dyad.[96] The higher the ratio the greater the trade dependence of the primary state compared to the partner state. When both states are equally dependent, the indicator equals zero (the natural logarithm of 1 is 0). Values less than zero indicate that the primary state depends less on the economic relationship than the partner state.

The trade dependence ratio provides information on the relative dependence of the members of the dyad, but not on whether this dependence is particularly important. State A can be more dependent on State B than B is on A, but the asymmetry may not be very meaningful if A's trade with B only represents a small portion of A's economy. To get at this, I create the Weighted Trade Dependence Index:

$$WeightedDependenceIndex = TradeDependence(primary) \times TradeDependenceRatio(primary)$$

The Weighted Trade Dependence Index tells us not simply whether a trade relationship is asymmetric, but whether that asymmetry is likely to be important. The Weighted Index of Trade Dependence is more valuable than the simple ratio, as it incorporates a degree of the importance of the trade as well as the level of balance or

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[96] Adding a fraction (0.00001) to the ratio is done for mathematical purposes - if the trade dependence of the partner state is zero, the ratio is zero and the term has no mathematical meaning. Adding a fraction should not change the results. Taking the natural log of the ratio is necessary (adding the fraction to the top and bottom eliminated the possibility of dividing by zero, which was a mathematical necessity), but does reduce the power of outliers to affect the results. In other words, some of the ratios were huge (greater than 100,000), and to reduce the ability of the small number of those huge ratios to affect the overall relationship in the equations, I decided to take the natural log of the ratio. Most importantly, the natural log function is zero when the ratio is one; it is positive when the ratio is greater than one (meaning the primary state has a higher trade dependence on the dyadic trade than the partner), negative when the ratio is less than one (meaning the primary state has a lower trade dependence on the dyadic trade than the partner), and zero when the trade is balanced. Thus, the measure accurately conveys a sense of "balance" in the economic relationship.

imbalance. As such, the Trade Dependence Ratio will not, itself, be used in the statistical tests, but the Weighted Index of Trade Dependence will be.

Given the complicated nature of these measures, Table 4.4, below, indicates how the Trade Dependence Ratio interacts with the Trade Dependence measure for a variety of combinations of both variables to create the Weighted Index of Trade Dependence.

Table 4.4: Examples of Weighted Index of Trade Dependence for Various Values of Trade Dependence and the Ratio of Trade Dependence

Trade Dependence Ratio	(Across) (→)	5 to 1	2 to 1	1 to 1	1 to 2	1 to 5
Natural Log of Dependence Ratio	(Across) (→)	1.6	0.07	0	-0.7	-1.6
Trade	0.20	0.32	0.14	0.00	-0.14	-0.32
Dependence	0.10	0.16	0.07	0.00	-0.07	-0.16
(Down)	0.01	0.02	0.01	0.00	-0.01	-0.02
	0.00	0.00	0.00	0.00	0.00	0.00

Down the side are possible values of the Trade Dependence measure, ranging from 0.00 (no trade) to 0.20 (20% of the GDP of the primary state is in involved in trade with the partner state).

Across the top are various ratios of trade (primary state to partner state). The "5 to 1" column indicates that the primary state is five times more "dependent" (trade with the partner as a percentage of GDP is five times as great as the same ratio – dyadic trade divided by GDP – for the partner) on the partner state than the partner state is on the primary state. Moving from left to right across the columns, the primary state becomes less and less dependent on the partner state until the middle column ("1 to 1") where the partner and primary state are equally dependent on each other. At the right, the dependency is reversed; "1 to 5" indicates that the partner state is now five times as dependent on the primary state as the primary state is on the partner.

This table numerically illustrates several important points about the Weighted Index of Trade Dependence. Notice that the "1 to 1" column is equivalent to no trade (the last row): both cases lead to all zero values of the Weighted Index of Trade Dependence. In other words, for this measure equal trade is equivalent to no trade. The measure is only significant (positive or negative) when there is some form of unbalanced trade between the partners (in one direction or the other). Values close to zero could result either from states with low values of trade (not much trade) or from states (perhaps with large trade volumes) with almost equivalent Trade Dependence Ratios. The Weighted Index of Trade Dependence is not efficient at telling the difference between these two cases. Moreover, highly interdependent states and mildly interdependent states (i.e., pairs of states where the ratio is 1 to 1, but with widely varying Trade Dependence) are equivalent (both would have a zero value for the Weighted Index), and are themselves equivalent to states that do not trade. The bottom line is that the Weighted Index of Trade Dependence only measures the imbalance of trade (the relative dependence of one state on another) and nothing else.

The Weighted Index of Trade Dependence is not a perfect measure of the importance and asymmetry of the trading relationship: since two measures are multiplied together, an equivalent Weighted Index of Trade Dependence could result from several different discreet combinations of Trade Dependence and the Trade Dependence Ratio. However, the concern of the dissertation is a focus on dependent states actions. High positive values of the Weighted Index can only come from significantly asymmetric trading relations when the primary state is significantly dependent on the partner state.

Hence, while the measure is not perfect, it is a valid measure for the cases we are most theoretically interested in.

As I have argued above, while most liberal peace studies have used only measures of trade dependence (dyadic trade divided by GDP), I will include measures of trade concentration (dyadic trade divided by total trade) both because the trade concentration measures have been used in other empirical tests and because the measures provide information not found in the dependence measures: an indication of how isolated a state is with regard to trade partners. To tap this, I construct a measure of trade concentration parallel to that I constructed for trade dependence:

$$TradeConce\,ntrationRa\,tio(\,primarysta\,te) = \ln\left(\frac{TradeConce\,ntration\,(\,primarysta\,te) + 0.000001}{Tradeconce\,ntration\,(\,partnersta\,te) + 0.000001}\right)$$

$$WeightedConcentrationIndex = TradeConcentration(\,primary) \times TradeConcentrationRatio(\,primary)$$

As was noted in the discussion of the trade dependence measures, the weighted measure is a better indicator of both the importance and balance of the trade than the ratio itself. The weighted concentration ratio is the variable used in the statistical tests, not the ratio itself. Recognize that the limitations of the Weighted Index of Trade Concentration are equivalent to the limitations of the Weighted Index of Trade Dependence (Table 4, above).

The data for constructing these measures have proven contentious. Russett and Oneal (1999, 2001, 2003) use trade data from the League of Nations and International Monetary Fund and GDP data from the Penn World Tables (1991) to construct their trade dependence measure. Barbieri (1995) uses the same League of Nations and IMF data to generate trade concentration scores, though later work (Barbieri, 2002) uses the World

Bank (1995) for GDP data to create the trade dependence scores. The IMF and League

of Nations data sets contain problems with missing values, with the League data being

the least complete[97]. Gleditsch (2002),[98] concentrating only on the post-World War Two

era filled in the missing trade and GDP series by interpolating missing data,[99] using

outsides sources to fill gaps,[100] and cross-checking missing data.[101] Consequently,

Gleditsch has generated what is perhaps the most complete trade data series, but only for

the post – 1945 years.

The differences between the Gleditsch GDP data (taken from the Penn World

Tables) and the Barbieri GDP data (taken from the World Bank) have also generated

controversy. While everyone agrees that the IMF trade data are the most reliable, valid,

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[97] Oneal and Russett (2001) approach the problem of missing trade and GDP data in a somewhat similar fashion to Gleditsch (see immediately below) rather than Barbieri: they assume some missing values are zeros (pg. 139, footnote 5) and interpolate missing values where missing values are obviously non-zero (pg. 140, footnote 6). However, two things differentiate the Oneal and Russett manipulation of data from the Gleditsch program. First, Oneal and Russett (1996, 2001) check the validity of their manipulations by dropping them from the analysis and comparing statistical tests with both sets of data (they find no difference, so continue using the manipulated data). Second, compared to the extent of the Gleditsch data manipulations, the Oneal and Russett program is relatively small potatoes. Oneal and Russett only assume some zero data points (where original data is missing), do some interpolation (where there are non-missing data and missing in-between), and use outside sources. The Gleditsch program goes beyond this. It should also be noted that once the Gleditsch data became publicly available (2002; post "Triangulating Peace", by Russett and Oneal, 2001), Russett and Oneal adopted the Gleditsch data for their own research. This would indicate that they are satisfied with Gleditsch's manipulations, and have no philosophical or empirical objections to the more heavily manipulated Gleditsch data. That being said, only the Barbieri data is fully unmanipulated: both the Gleditsch (more so) and the Oneal and Russett data (less so) manipulate the raw data to fill in missing data.

[98] The version of the Gleditsch data used in this dissertation is v4.0 (spring 2004). The significant changes to the updated data are an extension of the temporal span from the original 1997 to 2000, the addition of new countries that have entered the Correlates of War system membership listing, and some additional cleaning of data.

[99] Gleditsch uses a standard statistical technique of inserting the average value of bracketing data points when interpolating. For example, if the trade between the United States and Canada is known for 1950 and 1952, but not for 1951, Gleditsch averages the values of 1950 and 1952 and inserts it as the value for trade in 1951.

[100] Socialist bloc states generally did not report trade statistics to the IMF or GDP statistics to the World Bank. Gleditsch has used CIA World Factbook and other sources to gather data for the Soviet Union and its allies.

[101] IMF trade data depend on states self-reporting. Gleditsch was able to fill in some missing data by comparing reports from trade partners. For example, if the US did not report exports to Canada for 1951, it was possible that Canada reported imports from the United States for that year.

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and only source of post-World War II trade statistics available, there are multiple sources

of GDP data. Gleditsch (and Russett and Oneal) use the GDP data from the Penn World

Tables (PWT) project. The PWT economic data are a widely used source of economic

data for the vast majority of post-World War II countries and years (it makes no attempt

to cover the pre-World War II era). However, the PWT approach to GDP calculation

uses a "Purchasing Power Parity" formula for calculation of GDP. Each state's GDP is

adjusted to reflect the varying costs of living in different countries. For example, if one

state has a GDP that is twice another state's, but the cost of living in the first state is

twice as high, then the PWT would report both states' (adjusted) GDPs as being equal.

There is nothing wrong with this approach, but since the IMF trade data are not adjusted

for cost-of-living differences while the GDP figures are, there is some question about the

validity of figures when both sets of data are used together. Barbieri uses GDP measures

from the World Bank, which are not adjusted for cost-of-living differences between

countries. The World Bank GDP data are only available beginning in 1950, so Barbieri's

measures of trade dependence can be computed only for the Cold War era.[102] The Oneal

and Russett data (2001, used for all the analysis pre-1950) generated their measures of

GDP and trade primarily from League of Nations data, supplemented by various

academic studies (see immediately below).

The last issue with respect to the trade measures is one of temporal span. The

change in system structure from the multipolar, Europe-dominated world of the first half

of the twentieth century to the bipolar, Cold War-dominated world of the second half of

the twentieth century suggests that there may be a change in the effect of trade on

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[102] The issue of the "adjusted" PWT-GDP data versus the "unadjusted" World Bank-GDP data becomes significant when the Gleditsch and Barbieri data are analyzed together (see the end of Chapter 5, the end Chapter 6, and all of Appendix 1). Thus, the differences are touched on here.

interstate conflict as a result of systemic changes. However, the fact that both GDP-

series (the Gleditsch PWT GDP data and the Barbieri World Bank GDP data) begin in

1950 means that there is no source of pre-1950 GDP data to use to generate values of the

Weighted Indices. Oneal and Russett (2001) have generated values for Trade

Dependence in the pre-World War II era, but there are limitations on the data (discussed

below).

Most liberal peace researchers (notably Oneal and Russett, 2003) have adopted

the Gleditsch data for all post-World War Two analysis, using Oneal and Russett's

(1999a, 2001) original data only for the pre-World War Two period. It makes little sense

to include the Oneal and Russett data in post-1950 analysis (the Gleditsch data are more

complete, and are formulated exactly the same way). For the analysis in this dissertation,

I draw upon all three data sets in their appropriate temporal spans. Table 4.5 summarizes

the data available from these three sources.

Table 4.5: Summaries of Sources and Temporal Spans for Trade-Based Independent Variables

Source	Variable	Temporal Span
Barbieri (2002)	Trade Concentration	1870 – 1992
	Trade Dependence	1950 – 1992
Russett and Oneal (2001[103])	Trade Dependence and Trade Concentration	1885 - 1949
Gleditsch (2002/2004[104])	Trade Dependence and Trade Concentration	1950 - 2000

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[103] As noted, the Russett and Oneal data used in this dissertation come from their book project *Triangulating Peace* (2001). While Oneal and Russett have produced a more recent article relating to the liberal peace question (Oneal, *et. al*., 2003), the publicly available data from that article are not in a form utilizable for this dissertation. While all data from all original sources (Gleditsch, Barbieri and Oneal and Russett) are created with the dyad-year as the case (the norm in this area of peace research), Barbieri and Gleditsch make public the raw data (actual trade volumes and GDPs in dollars) for each state and dyad. Using those data, I can construct the directed-dyad cases used in this analysis. Oneal and Russett (2003c) do not provide the raw data, but only the data sets they use to create their results. The data needed to create

4.13 Research Design: Alternative Independent Variables

As with most large-N studies of the liberal peace, alternative independent variables (beyond measures of economic interdependence) are included in the analyses reported in the subsequent chapters. These variables have proven to be significant predictors of interstate conflict in other tests of the liberal peace (e.g., Bremer, 1992). Failure to include these could lead to false positive results – the economic dependence variables could be statistically significant when tested alone, but are in fact correlated with other independent variables – producing missing variable bias.

The other independent variables are taken from what has come to be a standard basket of covariates with respect to quantitative conflict studies, and have been used in previous empirical studies of the liberal peace (with such redundancy they have become known as the Oneal-Russett model). By using them here, we are able to more directly compare our empirical findings with those of other scholars. For each state, the level of democracy[105] is recorded, as is relative capability (which state is more "powerful"),[106]

the directed-dyad cases are available in *Triangulating Peace* (2001). Since the data from *Triangulating Peace* (2001) are limited to a sample of politically relevant dyads, the data from Oneal and Russett used here are also only that for politically relevant dyads. Thus, the Oneal and Russett data used here have a much smaller N than those of Gleditsch or Barbieri. The data are included, even given this limitation, as only Oneal and Russett have measures of trade dependence (and openness, see Chapter 7) for the pre-1950 era. Dropping the Oneal and Russett data make it impossible to examine the pre-1950 era.

[104] The article that describes the Gleditsch data is from 2002. Gleditsch has continued to update his data past the publication date of that article. The version used in the dissertation is from the Spring 2004.

[105] Drawn from the Polity III (2000) data set, the democracy levels of the primary and partner state are used, ranging from –10 (least democratic) to 10 (most democratic). The data were acquired through Eugene (v.3.040).

[106] Relative capability is a long-standing Correlates of War variable that attempts to code the latent power of each state by examining underlying military, economic, and demographic variables that are combined into a score comparable across states in a given year. Dividing the "power" of the primary state by the "power" of the partner state, and then taking the natural log of that number creates the ratio (taking the natural log is useful, though not necessary, to smooth out the curve and reduce the effects of outliers).

and major/minor power status[107]. Additionally, for each dyad, geographic distance,[108] alliance status,[109] and joint IGO membership[110] are recorded.

4.14 Research Design: Statistical Test

Because of the categorical nature of the dependent variable and ratio measures of many of the independent variables, some form of limited dependent variable regression is appropriate. For the empirical test of conflict initiation, logistic regression is used. In these tests, the dependent variable codes whether or not the primary state initiated a militarized interstate dispute (according the description in Section 4.10, above) in the year following the year for which the independent variables are recorded. Lagged dependent variables are common in quantitative conflict studies (e.g., Oneal and Russett, 2003). The procedure is employed to increase our confidence regarding the direction of causality.

Higher numbers indicate the primary state is more "powerful," while lower numbers indicate the partner is more powerful. Data are from the COW archives, through EUGene (v.3.040, Bennett and Stam, 2000)

[107] Power status of the states is a binary variable coded 1 if a state is a major power and 0 otherwise. Power status is derived from the COW system membership list (Small and Singer, 1982), by way of EUGene (v.3.040, Bennett and Stam, 2000).

[108] Distance between the primary and partner state is in miles, and uses major cities of both countries as a way of marking the distance if the two states are not land contiguous. The data come from EUGene (v3.040, Bennett and Stam, 2000), and were originally generated by Bennett and Stam.

[109] The alliance variable codes the type of alliance, if any, between the primary and partner states. This categorical variable ranges from a score of 1 indicating the highest level of alliance (defense pact) to a score of 4 indicating no alliance between the state and its partner, with intermediate values indicating intermediate agreements (2=neutrality, 3=entente). The data come from Bennett, 1997, by way of EUGene (v.3.040, Bennett and Stam, 2000).

[110] The intergovernmental organizations variable is taken from Pevehouse et al. (2004) recent publication and corresponding data site. The variable indicates the number of intergovernmental organizations to which both states jointly belong in a given year minus the average number for all dyads in that same year, divided by the standard deviation for all dyads in that year. This variable has replaced previous IGO variables that just used the number of joint IGOs because the number of possible IGOs available to join has increased dramatically in the last decades. The raw number of joint IGO memberships was thus biased towards more recent dyad years (where a relatively autarkic state in the last decade would be a member of a greater number of IGOs than Great Britain would be a member of in 1850). This new measure reduces that problem.

For the empirical test of conflict intensity, the MID data record five categories of hostile action. Ordered logistic regression is used to keep the knowledge of the ordered categories of the dependent variable and allows us to estimate the effects of the independent variables on each ordered category of the dependent variable. Again, to reduce concerns about the direction of causality, the dependent variable records the highest level of hostility used by the primary state against the partner state in the conflict following the year in which the independent variables are measured. For the small minority of militarized disputes that last more than one year, the independent variables are measured in the last year prior to the dispute.

These statistical approaches provide probabilistic results, rather than standard regression coefficients. For example, the statistical tests on hostility will report the standard (baseline) probability of the primary state moving to a higher level of hostility in an ongoing interstate conflict, or (in the tests of initiation) the probability of the primary state being the initiator of an interstate conflict. For all tests, the key is the effect on the probability of intensity or initiation as the independent variables change values. This approach will explicitly describe the effects of increased economic dependence or concentration (or any of the other variables described above) on the probability of conflict initiation or intensity for directed-dyads – precisely the empirical result needed to understand whether interests or signals motivate state actions with respect to the liberal peace.

4.15 Research Design: The Order of the Analyses

With the significant number of different tests (initiation and intensity), measures of dependence (trade dependence and trade concentration), and data sets (Oneal and

Russett, Barbieri, and Gleditsch), it will enhance the clarity of the analysis in the following chapters if the order of the tests is made explicit.

The hypotheses that underlie this dissertation focus heavily on the arguments that "balanced versus unbalanced trade" creates the conditions to examine the validity of signaling or interests as the logic that underpins the liberal peace. Thus, while the standard liberal peace variables (trade dependence and trade concentration) have been used by every other empirical examination of the liberal peace, they will not initially be used in these empirical tests.[111]

Chapter 5 will focus on conflict initiation. Three related but separate tests are undertaken. The first, using only the Gleditsch data for the post-World War Two era (1950 to 2000), compares the effects of the weighted indices of trade dependence and trade concentration on the propensity of directed-dyads to initiate militarized interstate conflict.

This test does ignore the Barbieri data, deliberately focusing on a single data set in order to explicitly compare the effects of trade dependence derived measures versus trade concentration derived measures. As was noted above, the Gleditsch data have been adopted by Oneal and Russett in their analysis, and are used by almost every liberal peace researcher. While some researchers find fault with Gleditsch's choice to interpolate data, they have become the standard data by which the liberal peace is tested.

The second test of the next chapter will introduce the Barbieri data, and examine the results that the two major differences between the data sets (interpolation/extrapolation to create more cases and the use of "adjusted" versus

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[111] The trade dependence and trade concentration variables will be used in a later analysis to examine the liberal peace directly. See Chapter 8.

"unadjusted" GDPs to create the trade dependence data) produce. The focus of this section of the chapter is to get an initial determination as to whether the differences produce significantly different empirical results.

The third section of the next chapter will use the Oneal and Russett data to examine the relationships between unbalanced trade and interstate conflict propensity in the pre-1950 era. The differences in the data sets restricts our ability to compare results across temporal spans, but it is only through using these data that we can examine the possibility that there is a temporal shift in the effects of trade on interstate conflict (something Barbieri, 1995, finds).

The final section of Chapter 5 will introduce alternative ways of "cutting" the data, isolating specific sub-groups of states (by region, by rich/poor states, et cetera) and testing whether the general relationships found with the larger population of directed-dyads holds for other types of samples below the level of the population.

Chapter 6 will focus on the relationship between trade and conflict intensity. Again, replicating the sections from the analysis of initiation, the first analysis will use the Gleditsch data to compute weighted indices to test the effect of economic unbalance on the propensity of states to use greater or lesser degrees of hostility with their partners in directed-dyads. The second section will bring in the Barbieri data to examine the differences between the Gleditsch and Barbieri methods of creating variables. The third section will analyze, using the Oneal and Russett data, the effects of unbalanced trade on the propensity for conflict hostility for the pre-1950 temporal span. The final section will look at sub-groups of states to see if the relationship between the economic and conflict

variables changes when the analysis moves from the universe of all conflict directed-dyads to samples of conflicts by categories of states.

Chapter Seven expands on the results of Chapters Five and Six, and tests the results by including additional trade based measures of dependence/interdependence to see how robust the conclusions from the previous two chapters are. Chapter Eight brings back in the measures of trade dependence and trade concentration to examine their effects on directed-dyad cases.

4.16 Research Design: Conclusion

Overall, this chapter describes the specific empirical test of interstate conflict intensity and initiation where the interest-based argument for the liberal peace predicts in one direction (the more economically dependent a state, the less likely it is to initiate or use higher levels of hostility within a conflict) and the signaling-based argument in the other (the more economically dependent a state, the more likely it is to initiate and use higher levels of hostility within a conflict). The cases for analysis are described, and previous research that informs this analysis is reviewed. The debates among the liberal peace researchers with respect to the dependent and important independent variables are reviewed, and the specifics of the variables selected for this dissertation are described.

The next chapters review the results of the tests described here and begins to analyze those results to determine if there is any empirical evidence in favor of either of the liberal peace logics. Additional tests that suggest themselves based on those results are discussed in following chapters. All additional tests use the same format and variables as the ones discussed here.

5.0 Chapter 5: Empirical Analysis – Effect of Trade-Based Measures of Interdependence on Interstate Conflict Initiation.

5.1 Introduction

The empirical results from this chapter are mixed, but generally support the conclusion that there is evidence that greater unbalanced trade is correlated with increased conflict initiation in the post-1950 era – evidence in favor of the signaling based explanation of the liberal peace. However, a conservative reading of the test results presented later in the chapter (using additional analysis) is that no empirical support is found for either interests or signaling as the underlying logic for the liberal peace. Were one forced to decide between the two logics, the results here provide equivocal support for the signaling-based logic as driving state actions, and little to no support for the interest-based logic.

What is clear is that there are instances in unbalanced trading relationships where a dependent state is likely to initiate an interstate conflict with a less dependent partner. Greater and more unbalanced trade, in some conditions, increases the likelihood of interstate conflict. This is a troubling finding for the argument that trade is pacific in all conditions. These results are not a rejection of previous research on the liberal peace: previous research (Russett and Oneal 2001, Oneal and Russett, 2003) examines the conflict propensity of dyads, finding the more trade within a dyad, the less chance of interstate conflict within that dyad. The results discussed here are for directed-dyads, not the dyads alone used in other research. It is theoretically possible that both sets of empirical results are correct: dyads that trade have less conflict, but states with unbalanced trading relations are more likely to initiate interstate conflict. However, the

results here are troubling for the general liberal peace hypothesis that trade, in all forms, is associated with peace.

The reason the results discussed here are equivocal is that, as was described in Chapter 4, different measures of dependence are employed, and the results are not always consistent between all the different measures. Where there is general agreement between all the different permutations, fewer results are reported. Where there is disagreement, more detail is provided. The conclusions are more a synthesis of the results than a specific description of a single test or set of tests.

In general, because of the large number of statistical tests, I have limited the presentation of the statistical results. Where there are a large number of tests that vary only by which trade-based independent variable is employed, I have first presented only the results for the trade variables, leaving aside discussion of the alternative independent variables. In addition, I have most often presented the Z-score (variable coefficient from the statistical test divided by the standard error of the coefficient) and the statistical significance of the coefficient (the probability that the coefficient for the variable is zero; the smaller that probability, the greater out confidence that the independent variable has an effect on the dependent variable), rather than the raw coefficients from the statistical tests. Unlike traditional OLS regression, where the coefficients are readily interpretable (a coefficient of 10, for example, in OLS regression would indicate that for every one unit change in the independent variable, the dependent variable would change by 10 units), the coefficients in logistic regressions and ordered logistic regressions represent the effect on the probability of change in the dependent variable. However, coefficients from logistic regressions are not immediately interpretable (a coefficient of 2, for

example, does not mean that a one unit change in the independent variable makes change in the dependent variable twice as likely to occur) and the distance between coefficients is not linear. Thus, the Z-score results are best used as a way to compare the relative significance of the independent variables against each other, rather than a reflection of how much effect a variable has on the dependent variable. The Z-scores allow simple and easy comparison of the relative effect on the probability of change in the dependent variable: the higher the Z-score, the more the independent variable influences the change in probability in the dependent variable.

5.2 Test of Initiation (Gleditsch Data)

As was noted in Chapter 4, the Gleditsch data have become the standard data used to test the liberal peace. While Gleditsch has been criticized for interpolating and extrapolating missing data, the data set offers the least amount of missing data, allowing the most complete test of the competing logics of the liberal peace theory. Any analysis is, however, restricted to 1950 and beyond, as that is when the Penn World Tables data begin.[112]

If higher levels of the weighted indices of dependence or concentration are correlated with increased probability of interstate conflict initiation, this will be evidence in favor of the signaling-based logic that explains the liberal peace (see Chapter 4 for a complete discussion). If higher levels of the weighted indices of trade concentration and dependence in the sample correlate with decreased probability of interstate conflict

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[112] The first year of trade data in the Gleditsch data set is 1948 (when the International Monetary Fund begins its series). However, to construct trade dependence measures, GDP is necessary, so no analysis involving trade dependence is possible before 1950.

initiation, this is evidence in favor of the traditional interest-based explanations of the liberal peace.

Overall, the results from the first set of tables offer support for the signaling theory, and no support for the interest-based theory. Table 5.1, below, presents only the trade-based weighted index variables across both the "restrictive" and "broad" set of conflict initiations among the population of directed-dyads that the Gleditsch data set contains. As noted, only one data set is used to generate the data for this test. Thus, I am (in effect) holding constant the different data generation and definitions that are used across the multiple sources of data that have been used to test the liberal peace. This test, then, looks at the different definitions of "dependence" that different researches have offered as ways of measuring the economic relationship between partners. Trade Concentration Weighted Index (column 1) is a measure championed by Barbieri (1995, 1996, 2002), while the Trade Dependence Weighted Index (column 2) is the most widely used measure, and advocated for by Oneal and Russett (1999, 2001, 2003). The weighted indices are the measures developed in this dissertation to specifically examine the effect of both volume of trade and inequality of trade (see Chapter 4).

Table 5.1: Effect of Trade-Based Variables on Probability of Conflict Initiation for the Population of Directed-Dyads: Gleditsch Data, 1950 – 2000 (Z-score and significance reported).

	Trade Concentration Weighted Index (N=817912)	Trade Dependence Weighted Index (N=808227)
"Restrictive" set of interstate conflicts	**3.37** **(0.00)**	**3.09** **(0.00)**
"Broad" set of interstate conflicts	0.99 (0.32)	**3.66** **(0.00)**

Three of the four tests offer results that are statistically significant. All three significant tests offer evidence that sizeable unbalanced trade is associated with increased

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probability of interstate conflict – evidence that supports the signaling logic over the interest-based logic. Thus, the conclusion is not that larger volumes of trade are associated with increased conflict, but that large volumes of trade when the trading relationship is unbalanced against the primary state (i.e., the primary state is more dependent on the partner state) are associated with increases in the likelihood of interstate conflict.

These initial results are not definitive – they do not clearly demonstrate that the signaling-logic is what is followed by states. However, employing what I argue are the theoretically best variables (the indices) for measuring the effect of unbalanced trade, most of the empirical tests indicate that the greater the combined volume and imbalance, the higher the probability of that state (the primary) initiating an interstate conflict against the partner state. This is consistent with the logic of signaling (that a dependent state would initiate in order to credibly signal its resolve or hidden capabilities), and completely inconsistent with the interest-based logic (where the interests of the dependent state should force it to avoid any action that would harm the economic ties it is dependent on).

Table 5.2 (below) presents typical results for the other independent variables as well as one trade-based variable in a format that converts the logistic regression results into more easily interpretable probabilities. Column 1 presents the Z-scores (and statistical significance) of the independent variables. Columns 2 through 5 present the probabilities of a state initiating an interstate conflict for different values of the independent variables, while all other independent variables are held at their mean values. By looking across the rows, you can examine the change in probability of an interstate

conflict being initiated as that independent variable moves from its minimum value, through one standard deviation below the mean value, to one standard deviation above the mean, to its maximum value.[113] Interstate conflict initiations are rare events (in the population of cases from 1950 to 2000, there are only 2270 instances of "restrictive" conflict initiation out of over a 800,000 cases), and the base probability of any initiation for any directed-dyad is correspondingly small (0.00027925). The final two columns provide two measures of the percentage change in the probability of conflict initiation: one (Column 6) as the independent variables changes from one standard deviation below its mean value to one standard deviation above its mean value and the other (Column 7) as the independent variable changes from its minimum to maximum values.

The bottom row in Table 5.2 presents the relevant information on coefficients and probabilities for the weighted index of trade dependence (earlier reported in abbreviated form in the upper right-hand cell of Table 1). In comparison to other independent variables in the range of values between a standard deviation above and below their means, the weighted index of trade dependence does not greatly affect the probabilities of interstate conflict: moving across that range only causes the probability of interstate conflict initiation to rise by 7%. Compared to other independent variables (democracy of the primary state causes a 14% reduction in the probability of interstate conflict, ratio of resources a 58% increase in the probability of initiation), the effect of the trade measure is statistically significant but small.

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[113] In cases where the mean value and the maximum or minimum values were close or identical, this comparison is dropped. For example, for contiguity (max value 6, mean value 5.8) and alliance (max value 4, mean value 3.8), the mean values were close enough to the maximums (and the standard deviations small: less than one) that it makes no sense to talk about a change in those variables across standard deviations. Additionally, for major/minor power status there are only two values (major or minor).

Table 5.2: Effect of Independent Variables on Probability of Conflict Initiation for the Population of Directed-Dyads: Gleditsch Data – 1950 – 2000. (Base Probability of Interstate Conflict Initiation: 0.00027925)[114]

Independent Variables	Column 1: Z-Score of Independent Variable (Statistical Significance)	Column2: Probability of Initiation at Minimum Value	Column 3: Probability of Initiation at One Standard Deviation below Mean	Column 4: Probability of Initiation at One Standard Deviation above Mean	Column 5: Probability of Initiation at Maximum Value	Column 6: Percent Change in Probability: One Stnd Dev below to One Stnd Dev. Above.	Column 7: Percent change in probability from minimum value to maximum value
Is Primary State a Major Power?	13.25 (0.00)	0.00027925 (0: minor power)	(n/a)	(n/a)	0.00092325 (1: major power)	(n/a)	230%
Is Partner State a Major Power?	15.67 (0.00)	0.00027925 (0: minor power)	(n/a)	(n/a)	0.00129818 (1: major power)	(n/a)	361%
Level of Democracy of Primary State	-2.30 (0.02)	0.00030920 (-10: autocratic)	0.00030152 (-7.6)	0.00025847 (7.1)	0.00025074 (10: democratic)	-14%	-19%
Level of Democracy of Partner State	5.69 (0.00)	0.00021906 (-10: autocratic)	0.00023258 (-7.6)	0.00033570 (7.1)	0.00036090 (10: democratic)	44%	65%
Level of Alliance between States	4.02 (0.00)	0.00019752 (1: strong alliance)	(n/a)	(n/a)	0.00027925 (4: no alliance)	(n/a)	41%
Distance between States	-11.69 (0.00)	0.00105819 (0)	0.00060303 (2000)	0.00012835 (7500)	0.00003618 (12000)	-79%	-97%
Contiguity Measure between States	-30.13 (0.00)	0.00543557 (1: Contiguous)	(n/a)	(n/a)	0.00027925 (6: not contiguous)	(n/a)	-95%
Ratio of Power between States	4.96 (0.00)	0.00011222 (-11.9: Partner is stronger)	0.00022191 (-3)	0.00035138 (3)	0.00069470 (11.9: Primary is stronger)	58%	519%
Relative number of Joint IGOs	2.88 (0.00)	0.00005431 (-20: no joint IGOs)	0.00025730 (-1)	0.00030307 (1)	0.00045634 (6: many joint IGOs)	18%	740%
Weighted Index of Trade Dependence	3.09 (0.00)	0.00023856 (-0.52: unbalanced in favor of Primary)	0.00027022 (-0.10)	0.00028859 (0.11)	0.71140985 (30.5: unbalanced in favor of Partner)	7%	298110%

Where the effect of trade becomes pronounced is in the final column, which records the percentage change in the probability of conflict initiation as the independent variables change from their minimum to maximum values. The effect of the weighted index of trade dependence is immense compared to other independent variables. This is

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[114] This table presents the results of the logistic regression equation that uses the "restrictive" set of initiations and the Weighted Index created using the Trade Dependence data. Of the four cells in Table 5.1, this table is the complete results for the upper right-hand box. Table 5.2 presents results for all independent variables, not only the trade-based independent variables. The results presented here do not differ substantially, with the exception of the trade-based independent variable, from the results in the other logit equations represented by the other cells in Table 5.1. In other words, these results are typical for the non-trade independent variables in the other seven boxes of Table 5.1.

somewhat inflated. The mean value of the dependence index is very close to 0 (0.005),

with a standard deviation of only 0.11. Hence, values for the dependence index that

approach the maximum in the data (30.4) are very rare: of the 802,000+ directed-dyads,

only 817 had values of the dependence index above 1 (10 times the standard deviation),

and only 19 had trade dependence above 10 (100 times the standard deviation). Of the

817 cases with trade dependence values above 1, 4 are cases where a conflict initiation

occurs (4/817 = 0.0048), which is not significantly greater than that for the entire sample

(2270/820000 = 0.003). However, of the 19 cases where trade dependence is measured

above 10, 1 case has an initiated conflict (1/19 = 0.053), which is a much higher

percentage than average. Does this make the result invalid? No, but it does indicate the

power of outliers (at least when making percentages). While highly unlikely, very high

values of the trade dependence index indicate states much more likely to initiate interstate

conflicts than others.[115]

Examination of the other rows of Table 5.2 finds few surprises. Being a major

power, being geographically close or contiguous, and having an advantage in physical

capabilities all increase the odds of initiating a militarized dispute, while being

democratic and being allied reduce the odds. These results are consistent with other

research. What is unusual is the effect of joint intergovernmental organizations on

conflict initiation and the partner state's level of democracy. Previous research (Russett

and Oneal, 2001; Oneal and Russett, 2003) has associated joint membership in

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[115] The nineteen directed-dyad cases where the dependence index is higher than 10 are: St. Kitts unevenly
dependent on the United States in 1987, Liberia unevenly dependent on Japan in 1993, the Bahamas
unevenly dependent on the United States from 1974 through 1984 (eleven years), and North Korea
unevenly dependent on Japan from 1952 through 1957 (six years). The single recorded MID in these cases
is a case of North Korea using force (MID level 4) against Japan in 1955 (Japan did not reciprocate (MID
level 1) – it did not respond to the North Korea use of force).

intergovernmental bodies with a reduction in probability to engage in interstate conflict.

Those tests used dyads (rather than directed-dyads), so these results are not a direct

refutation, though they are troubling for that leg of the Kantian peace (Russett and Oneal,

2001). The results for the partner states level of democracy are also troubling for that

research program. The greater the democracy of the partner state, the more likely the

primary state is to initiate. This result is particularly troubling as the change in

probability of initiation is high even within the bounds of the two standard deviation

range (a 44% increase in probability as the partners level of democracy moves from one

standard deviation below to one standard deviation above). These results are not directly

related to the questions this dissertation asks, and are only mentioned because they are

inconsistent with previous research in this area.

Results for the Barbieri data for the 1950 and on temporal span are presented in

Table 5.3. Barbieri's results are generally consistent with Table 5.1 (she finds the same

positive associations for both the weighted indices that the Gleditsch data, Table 5.1, also

finds).

Table 5.3: Effect of Trade-Based Variables on Probability of Conflict Initiation for the Population of Directed-Dyads: Barbieri Data, 1950 – 1992 (Z-score and significance reported).

	Trade Concentration Weighted Index (N=258552)	Trade Dependence Weighted Index (N=254671)
"Restrictive" Initiations	**5.57** **(0.00)**	**3.36** **(0.00)**
"Broad" Initiations	**3.78** **(0.00)**	**2.45** **(0.01)**

These results are almost completely consistent with the Gleditsch test of the same

temporal span. The only difference is that the magnitude of the Z-scores is not identical

between the two tests, but a direct comparison is impossible as the two sets of data use widely different sets of cases (the Gleditsch set contains three to four times the number of directed-dyad cases as the Barbieri data set). An exploration of the differences between the two data sets is the subject of the next section of this chapter.

Overall, this initial empirical test provides evidence that the greater the unbalanced trade for the primary state, the larger the increased risk of initiating interstate conflict for that state. Using both measures of interdependence (weighted indices of trade dependence and trade concentration) and both competing sets of data, all the tests find statistically significant results that support the signaling theory of the liberal peace. This is not direct evidence in favor of signaling – no attempt was made to measure or record actual signals between states or examine state decision-makers and ask if they thought in terms of signals or interests. This is only indirect evidence, though it is significant.

5.3 Comparison of Gleditsch and Barbieri Data

While the empirical results of the analyses in the last section using the Gleditsch and Barbieri data sets were substantially the same, the specific differences between the two data sets are important, and will remain an issue throughout the dissertation. There are two key differences between the Gleditsch and Barbieri data: first, the Gleditsch data interpolates, extrapolates and uses outside (beyond IMF and Penn World Tables) sources to fill in the data missing in the original data tables; second, Gleditsch uses "adjusted" GDP data from the Penn World Tables[116] to create his measures of trade dependence, while Barbieri relies on unadjusted World Bank GDP data to create her measures of trade

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[116] See Chapter 4 for a discussion. Briefly, "adjusted" GDP data has had the total gross domestic product of a state multiplied by an amount to adjust for the different cost of goods in different parts of the world ($1 worth of oil is not the same quantity in Saudi Arabia as it is in America), while "unadjusted" GDP makes no attempt to account for these differences across states.

dependence. The IMF supplied trade data that both academics use is in "unadjusted" values – the question is whether mixing the "adjusted" GDP and "unadjusted" trade data (as Gleditsch does) introduces errors in the data that using both sets of "unadjusted" figures (as Barbieri does) keeps out.

Given that the Gleditsch data set is larger (and more complete) than the Barbieri data set, the larger Gleditsch data set can be limited and tested with the same set of directed-dyads as the smaller data set. In other words, one can replicate the Barbieri tests using the same cases (directed-dyads), but substituting the Gleditsch data for the Barbieri data. This reduces the differences in the data sets by one degree: the only difference between the data sets once the Gleditsch data is substituted into the Barbieri cases is the "adjusted" versus "unadjusted" issue, since the same set of cases would be used.

This allows a direct test of whether the above results are due to differences in cases tested, or differences in data between the two sets. If the substituted data produce results that are similar to the full sets of data (i.e., if substituting Gleditsch data into Barbieri cases produces similar results to the tests using the full Gleditsch data, not to the results using the Barbieri data), then differences in results between data sets are due to actual differences of data in the cases. If the substituted data produce results similar to the data for which they are substituted (i.e., if substituting Gleditsch data into Barbieri cases produces results similar to the Barbieri data, not to the full Gleditsch data), then differences in results between data sets are due to the different cases being tested, not any change in the data themselves.

Table 5.4, below, includes results that derive from tests of directed-dyads where both Gleditsch data and Barbieri data exist. Rows one and five replicate results from

Tables 5.3 and 5.1 earlier in the chapter, and are included for comparison. Row 2 displays the results of the logistic regressions where the Barbieri data are used, and the cases are limited to those directed-dyads with valid Barbieri and Gleditsch data; Row 3 displays the results of the logistic regressions where the Gleditsch data were used with valid Gleditsch and Barbieri data (other than that change, the logistic regression equations for Rows 2 and 3 are identical). Since the Gleditsch data cover years that the Barbieri data do not (1993 to 2000), Row 4 uses the universe of Gleditsch cases and data, but limits it to the same years that the Barbieri data are limited to.

Table 5.4: Effect of Trade-Based Variables on Probability of Conflict Initiation for Limited Directed-Dyads: Data, 1950 – 1992 (Z-score and significance reported).

	Trade Concentration Weighted Index "Restricted" Initiations	Trade Concentration Weighted Index "Broad" Initiations	Trade Dependence Weighted Index "Restricted" Initiations	Trade Dependence Weighted Index "Broad" Initiations
Barbieri Data (From Table 3, Chapter 5)	**5.57** **(0.00)** (N=258552)	**3.78** **(0.00)** (N=258552)	**3.36** **(0.00)** (N=254671)	**2.45** **(0.01)** (N=254671)
Barbieri data substituted into valid Barbieri and valid Gleditsch cases	**5.56** **(0.00)** (N=257828)	**3.75** **(0.00)** (N=257828)	**3.35** **(0.00)** (N=253945)	**2.43** **(0.02)** (N=253945)
Gleditsch data substituted into valid Barbieri and valid Gleditsch cases	**5.56** **(0.00)** (N=257828)	**4.24** **(0.00)** (N=257828)	**2.80** **(0.01)** (N=253945)	**5.44** **(0.00)** (N=253945)
Gleditsch data (1950 – 1992)	**3.16** **(0.00)** (N=641085)	0.84 (0.40) (N=641085)	**3.43** **(0.00)** (N=641085)	**3.96** **(0.00)** (N=641085)
Gleditsch data (1950 – 2000; from Table 1, Chapter 5)	**3.37** **(0.00)** (N=817912)	0.99 (0.32) (N=817912)	**3.09** **(0.00)** (N=808227)	**3.66** **(0.00)** (N=808227)

The key comparison is between Rows 2 and 3: these two sets of tests are the same except that the data for Row 2 are Barbieri data where Row 3 uses Gleditsch data.

Having exactly the same cases means any difference between the two is solely the result of differences in the data themselves.

The results seem clear: substituting the Gleditsch data into the Barbieri data gives substantially (though not identically) the same results. The largest divergence between the two sets of data comes in the tests of the weighted index of trade dependence: even when the same cases are used, the Gleditsch results continue to differ from the Barbieri results (though both are positive and statistically significant). Moreover, the difference is not consistent: the "restricted" tests result in a Barbieri value for the weighted index of trade dependence substantially higher than the Gleditsch; when the "broad" initiations are used, the Gleditsch weighted index returns a Z-score substantially higher than the Barbieri.

Overall, this test makes clear that there are differences between the Gleditsch and Barbieri data beyond issues of interpolation and extrapolation; the data themselves differ. The difference at this time looks to be trivial; both sets of data return results in the empirical tests that are comparable and consistent. Until a clear difference between the two sets of data is shown to lead to significantly different results, the Gleditsch data (with its larger number of valid cases) is more useful. The importance of the Gleditsch techniques is in expanding the available data and creating more cases that are valid. While this test will be replicated with the discussion of hostility, this initial test seems to indicate that there are only trivial differences between "adjusted" and "unadjusted" GDPs when used in tests of the liberal peace.

5.4 Tests from the Pre-1950 Era

The next test to is the test of conflict initiation for the pre-1950 era.[117] As was

discussed above, the Gleditsch data are the most widely used, but they only begin in

1950. For researchers trying to test the effect of trade and trade-derived measures of

dependence, both Barbieri and Oneal and Russett provide data for the earlier temporal

span. Table 5.5, below, replicates Table 5.1, using the pre-1950 data. The results are

uniformly not statistically significant.

Table 5.5: Effect of Trade-Based Variables on Probability of Conflict Initiation for the Population of Directed-Dyads: Barbieri and Oneal and Russett Data, 1870 – 1949.

	Trade Concentration Weighted Index	Trade Dependence Weighted Index
Barbieri: "Restrictive" set of interstate conflicts (1870 – 1949)	-0.90 (0.36) (N=39421)	(n/a)
Barbieri: "Broad" set of interstate conflicts (1870 – 1949)	-1.13 (0.26) (N=39421)	(n/a)
Oneal and Russett: "Restrictive" set of interstate conflicts (1885 – 1949)	-0.76 (0.44) (N=22671)	-0.10 (0.91) (N=23015)
Oneal and Russett: "Broad" set of interstate conflicts (1885 – 1949)	-0.61 (0.54) (N=22671)	-0.92 (0.36) (N=23015)

These results provide little additional information to support or refute the previous

conclusions. While there is no additional support for the signaling-based hypothesis (no

positive correlation between any trade-derived measure of dependence and increased

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[117] As was mentioned in Chapter 4, technically Gleditsch's trade data begin in 1948 (when the IMF begins its trade data series). However, the source Gleditsch uses for most of his GDP data (the Penn World Tables) begins in 1950. Hence, the trade dependence derived measures (which require GDP) cannot be created before 1950 (while the trade concentration measures can, as they only require the value of the state's total trade). I have chosen to use the Gleditsch data from 1950 on for simplicity.

initiation), neither is there much support for the interest-based hypothesis (a statistically significant negative correlation between the same measures). Bluntly, unbalanced trade is irrelevant in influencing states to initiate interstate conflicts with trading partners before 1950. This conclusion should not be read as opposing previous research (Oneal and Russett, 2001, Oneal and Russett, 2003) that has found a beneficial connection between trade and peace in this temporal period: comparing these tests is comparing apples and oranges. Where the overwhelming choice of cases for other researchers examining the liberal peace is the dyad (i.e., whether pairs of states that trade are less likely to experience interstate conflict), the cases examined here are directed-dyads (single states actions with respect to partner states). It is clearly possible for both conclusions to be true: dyads that trade are more pacific and that trade has no effect on which of the states in the dyad is the one that initiates any dispute that does occur.

The stark contrast between the pre-1950 and post-1950 results demands additional analysis. Breaking the data into two temporal spans (1950 to 2000, 1870/1885 to 1949) is done for data driven reasons (the Gleditsch data do not begin until 1950; Barbieri's GDP data do not begin until 1950 as well), not for any theoretical reason. One can analyze the test of initiation in smaller temporal pieces than 50 (post-1950) or 75-year (pre-1950) slices. This smaller temporal parsing of the data provides "snapshots" of the effects of trade-based dependence on conflict initiation, rather than the long-term results already reported. These smaller slices of time are neither more nor less valid than the longer spans already discussed. A longer span removes the effects of outliers (the larger samples mean less chance for a few or singular result to make the entire sample significant) and allows conclusions over longer periods, but removes any ability to

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discern trends over time. Smaller spans allow investigation of trends over time, but lack

the ability to reach conclusions about long stretches of history (and are more prone to

manipulation by outliers).

Table 5.6, below, reports the Z-scores (and significance and number of cases) for

the concentration and dependence index variables for all three data sets for 1870 to 2000

by five-year intervals. Breaking the long post-war span (1950 – 2000) into these smaller

five-year pieces allows analysis of whether the general trends in the post-1950 era (some

support for signaling, in that most of the measures of unbalanced trade were positively

correlated with increased initiation) for the whole period masked some times when

measures of dependence were correlated with lower incidents of initiation.   It also allows

examination of whether the long-term results are true overall, or just present in some

years. A five-year interval allows more points of comparison than ten-year intervals

(there would be only five ten-year spans to examine), but less than single-year tests

(which sometimes contain too few cases to test).[118]

Another flaw worth mentioning is that the choice of beginning and ending years

for each block of five years can mask a potential trend. If a spike in conflicts occurs from

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[118] Test of the effects of the trade concentration index and trade dependence index on interstate conflict
initiation for each year (1950 to 2000; to 1992 for the Barbieri data) were performed. The trade
concentration index was positively associated with more conflict initiation in 1954, 1958, 1974, and 1977
(using the Gleditsch data); the concentration index was positively associated with conflict initiation in
1951, 1952, 1954, 1958, 1961, 1970, 1974, 1981, and 1988 (using the Barbieri data). The trade
concentration index was never negatively associated with interstate conflict initiation. The trade
dependence index was positively associated with more conflict initiation in 1977, 1981, and 1985 (using
the Gleditsch data); the dependence index was positively associated with conflict initiation in 1952, 1958,
1962, 1964, and 1974 (using the Barbieri data). The trade dependence index was negatively associated
with conflict initiation only once (1999, using the Gleditsch data). These results do not suggest any sort of
firm trend or conclusions about a single year (no year was significant for both index measures and both sets
of data). The only tentative conclusions from these tests are that the 1950s and the mid-1970s look to be
times of increased conflict, but there is no consistent result (multiple years consecutively significant with
either index) that supports that hypothesis. Hence, the year-by-year analysis was not pursued in favor of
the five-year analysis.

1947 to 1953, but I examine 1945 – 1950 and 1950 – 1955, the analysis may miss the

trend because of the choice of years in each block of time. There is no solution to this

problem, though it is a limitation worth mentioning.

Table 5.6: Effect of Trade Concentration Index and Trade Dependence Index on Probability of Conflict Initiation for all Directed-Dyads, by Five-Year Temporal Spans, Multiple Data Sources (Z Scores and Probability of Significance reported).

	KSG Conc. Index	KSG Dep. Index	Barb. Conc. Index	Barb. Dep. Index	Oneal and Russett Conc. Index	Oneal and Russett Dep. Index
1870-1875	(n/a)	(n/a)	-0.68(0.50)*	(n/a)	(n/a)	(n/a)
1876-1880	(n/a)	(n/a)	-0.64(0.52)*	(n/a)	(n/a)	(n/a)
1881-1885	(n/a)	(n/a)	-1.05(0.29)*	(n/a)	(n/a)	(n/a)
1886-1890	(n/a)	(n/a)	-0.21 (0.84)	(n/a)	-0.81 (0.42)	-0.56 (0.58)
1891-1895	(n/a)	(n/a)	-0.12(0.90)*	(n/a)	-0.50(0.62)*	-0.21(0.84)*
1896-1900	(n/a)	(n/a)	-0.78 (0.43)	(n/a)	-0.24 (0.81)	-0.91 (0.37)
1901-1905	(n/a)	(n/a)	0.10 (0.92)	(n/a)	0.94 (0.35)	-0.37 (0.71)
1906-1910	(n/a)	(n/a)	-0.89 (0.38)	(n/a)	-1.16 (0.25)	-1.43 (0.15)
1911-1915	(n/a)	(n/a)	**2.17 (0.03)**	(n/a)	**2.77 (0.01)**	**3.45 (0.00)**
1916-1920	(n/a)	(n/a)	0.01 (0.99)	(n/a)	-0.33(0.74)*	-0.33(0.74)*
1921-1925	(n/a)	(n/a)	0.53 (0.60)	(n/a)	0.83 (0.41)	-0.73 (0.46)
1926-1930	(n/a)	(n/a)	**-2.63 (0.01)**	(n/a)	-0.82(0.41)*	-0.21 (0.83)
1931-1935	(n/a)	(n/a)	-0.92 (0.36)	(n/a)	-0.62 (0.53)	-0.33 (0.74)
1936-1940	(n/a)	(n/a)	-1.60 (0.11)	(n/a)	-1.40 (0.16)	-0.71 (0.48)
1941-1945	(n/a)	(n/a)	(n/a)	(n/a)	(n/a)	(n/a)
1946-1950	(n/a)	(n/a)	0.02 (0.99)	(n/a)	(n/a)	(n/a)
1951-1955	0.85 (0.40)	**3.17 (0.00)**	**4.79 (0.00)**	**2.61 (0.01)**	**3.63 (0.00)**	1.38 (0.17)
1956-1960	**2.18 (0.03)**	0.60 (0.56)	**3.45(0.00)**	**2.98(0.00)**	**3.32(0.00)**	1.87(0.06)
1961-1965	1.02 (0.30)	-0.96 (0.34)	1.59 (0.11)	**3.18 (0.01)**	0.90 (0.37)	-0.14 (0.89)
1966-1970	-0.45(0.65)	-1.29 (0.20)	1.73 (0.08)	0.46 (0.65)	-1.06 (0.29)	-1.36 (0.18)
1971-1975	**3.02 (0.00)**	0.06 (0.96)	**2.42 (0.02)**	0.83 (0.41)	-0.08 (0.94)	0.14 (0.89)
1976-1980	1.00 (0.32)	0.41 (0.68)	0.99 (0.32)	-0.09 (0.93)	-0.61 (0.54)	-0.99 (0.32)
1981-1985	-0.18(0.86)	1.52 (0.13)	0.65 (0.51)	-0.68 (0.50)	-1.87 (0.06)	-1.82 (0.07)
1986-1990	0.05 (0.96)	-0.12 (0.90)	0.94 (0.35)	0.42 (0.68)	-0.66 (0.51)	-0.34 (0.73)
1991-1995	0.49 (0.63)	-0.74 (0.46)	0.88 (0.38) (1991, 1992)	0.10 (0.92) (1991, 1992)	0.33 (0.74)* (1991, 1992)	-0.35 (0.73)* (1991, 1992)
1996-2000	-0.81(0.42)	-0.55 (0.58)	(n/a)	(n/a)	(n/a)	(n/a)

(NOTE: Cells with an (*) indicate results were obtained from the logistic regression, but there were problems with the equation: colinearity, dropped variables due to perfect predictions, or missing data that prevented a complete result. Treat these results with caution.)

These tests were run with only the "restricted" set of initiations, as the table would be too complicated to analyze and explain if the "broad" set was tested as well. The results of this analysis indicate that there are "clumps" of years where the trade-based weighted indices (concentration or dependence) rise to significance, but overall unbalanced trade has little consistent effect on interstate conflict initiation. Clearly, however, the 1950s were a time where unbalanced trade had a significant effect on conflict initiation as all three data sets find significance over this decade. Additionally, 1911 to 1915 (before the Gleditsch data begin) may be another set of years where unbalanced trade and conflict are significantly associated (similar to the 1950 results). Lastly, the early 1970s are similar (at least according to the Gleditsch and Barbieri data). In only one period (1926 – 1930 with the Barbieri data) did unbalanced trade dampen interstate conflict initiation to a significant degree.

The addition of the short-span analysis to the above long-term analysis indicates that there are some critical years for the post-1950 era. While using both Barbieri and Gleditsch data, I find the weighted indices positive and significant for the post-1950 era (meaning larger imbalances of trade between partners is associated with the more dependent partner having a higher probability of initiating an interstate conflict; Tables 5.1 and 5.3). However, re-running the analysis for only post-1960 (i.e., dropping 1950 to 1959, which removes less than 10% of the overall cases, as fewer countries/dyads existed in the world then) causes three of the four coefficients to drop to insignificance (only the Barbieri-based weighted trade concentration index remains significant; the Barbieri-based weighted trade dependence index, and both the Gleditsch-based weighted trade dependence and weighted trade concentration indices remain positive, but drop below the

0.05 accepted level of significance). The only reasonable conclusion is that results for the entire Cold War post-1950 span are driven by a decade (the 1950s) where there was significant correlation between unbalanced trade and interstate conflict, while the rest of the period had little to no association between trade and conflict (as is the case in the pre-1950 results).

5.5 "Cutting" The Data in Additional Ways

The additional understanding that the detailed analysis of the temporal spans provided for the general understanding of the effects of unbalanced trade on interstate conflict encourages further exploration of the data. By using the other independent variables, we can examine whether different sub-groups of states behave differently with respect to the effects of trade on interstate conflict than the mass of unbalanced cases taken as a whole.

For example, while the results in Table 5.2 (Section 5.1, above) indicate that a major power has a significantly greater likelihood of initiating an interstate conflict than a minor power (this was true across both the data sets and different trade-based independent variables), this does not tell us whether major powers have a different relationship between trade and interstate conflict than minor powers. By restricting the analysis to major powers, it is possible to determine the effect of trade on interstate conflict for only that group of states. In the same way that the more detailed analysis of temporal spans revealed that the 1950s spike influenced the analysis for the entire post-1950 period (removing the 1950s decade removed many of the significant results), examining a host of types of primary states might reveal more about what kind of

directed-dyads are affected by the relationship between unbalanced trade and interstate conflict.

In point of fact, being a major or minor power makes no difference to the relationship of trade to interstate conflict. Major powers, when tested alone, displayed the same relationship in the weighted indices to interstate conflict as the entire sample did. Minor powers were the same, as well. This was true for both pre- and post-1950.[119]

Dividing the states into regions is another way of taking a cut at examining whether the relation between trade and interstate conflict changes depending on the cases examined. Examining the effects of the weighted indices on probability of conflict initiation across North & South America, Europe (broadly defined, includes Russia), Africa, the Middle East (ends at Iran) and Asia (begins at Afghanistan/Pakistan) using the Gleditsch data in the post-1950 period[120] produces results that are not consistent across regions.[121] Table 7, below, summarizes the results.

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[119] The Gleditsch and Barbieri data were used for a test of the post-1950 era, while the Oneal and Russett data (and the Barbieri measure of the weighted index of trade concentration) were used in the pre-1950 span.   Results for each sub-group (major powers as potential initiators and then minor powers as potential initiators) revealed no significant difference for the trade related variables compared to the tests that used all cases (major and minor powers together). These results, given their insignificance, are not reported in a table.

[120] Given the findings above, it would be unnecessary to test using the Barbieri data, as previous analysis indicated that any differences between data sets were likely driven by differences in cases tested, not actual differences in the data themselves. Thus, the Gleditsch data only are used.

[121] The test of regional difference in the pre-1950 era using the Oneal and Russett data found no region had a significant correlation between either weighted index and conflict initiation. This result is consistent with the test of all cases (the global population, Table 5.5, above).

Table 5.7: Summary of Effects of Weighted Indices on Interstate Conflict Initiation by Region, Post-1950: Gleditsch Data)

("Restricted" refers to the narrow set of initiations, while "Broad" refers to the more expansive set: see Chapter 4 for a complete explanation. In the body of the table, a "+" indicates a positive and significant relationship between the trade variable and interstate conflict initiation, a "-" indicates a negative relationship, and a blank cell indicates no significant relationship. Data for the "Entire World" row is taken from Table 1 for the Gleditsch Data and is included for comparison.)

	Dependence Index Restricted set of Initiations	Dependence Index Broad set of Initiations	Concentration Index Restricted set of Initiations	Concentration Index Broad set of Initiations
America				+
Europe		-		
Middle East	+			
Africa	+	+		
Asia				
Entire World (Table 1, Chapter 5)	+	+	+	

Unlike the results in Sections 5.2 and 5.3 (where the Gleditsch and Barbieri data substantially agreed across all the variables and initiations), this set of results has no consistency. Using the Gleditsch data, I find a significant positive relationship between the weighted index of trade dependence and conflict initiation (narrowly defined) for Africa and the Middle East. Using the "broad" definition of initiation, the relationship is positive and significant only for Africa. The relationship between the weighted index of trade concentration and conflict initiation (narrowly defined) is not statistically significant for any region, but is positive and statistically significant for the Americas when a broad definition of conflict initiation is employed. The only negative association between trade and conflict is found in Europe when using the trade dependence index and the broad definition of conflict initiation. For comparison, the data taken from Table 5.1 are presented as the last line of Table 5.7.

The analysis presented in Table 5.7 does little to simplify the overall picture thus far, and, in fact, complicates already existing conclusions. Where the findings reported in

Table 5.1 (and reproduced in the last line of Table 5.7) reveal that, using the Gleditsch data, the weighed indices of trade dependence and trade concentration are positively and significantly related to conflict initiation for the sample of all directed-dyads in three of the four statistical tests (all but the "broad" initiations tested with the weighted index of trade concentration), there is little consistency in the findings at the regional level. Changing between "broad" and "restricted" definitions of conflict initiation causes regions to fall in and out of significance, and moving between the dependence and concentration index does the same. There is no consistent pattern.

One clear result does emerge from the analysis in Table 5.7. Africa is the only region to show a positive and significant relationship between greater dependence and increased probability of initiating interstate conflict. An obvious question is whether the global results (Table 5.1; or the last row of Table 5.7) are being driven by this single region. The answer is "yes". Removing Africa from the entire Gleditsch sample (i.e., testing America, Europe, the Middle East and Asia only) causes the weighted index of trade dependence to become insignificant. This leads to the reasonable conclusion that it is something about that single region which, when it is included into a global sample, is causing the overall (Table 5.1) positive and significant results. It is hard, if not impossible, to avoid the conclusion that the general results (Table 5.1) are an artifact of the behavior of states within this one region of the world.

Moreover, the results from the regional analysis and the temporal span analysis are clearly not complimentary.[122] Removing Africa and the Middle East from the entire

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[122] I focus mostly on the column in Table 7 labeled "Dependence Index: Restricted set of Initiations" for the following paragraphs. The difficulty in rectifying this column with previous analysis is similar to the problems for the other columns (other weighted indices and different "restricted" or "broad" sets of initiations). Rather than discuss all the issues for all the variables, I have focused on this one column for

Gleditsch sample (i.e., testing America, Europe and Asia only) causes the weighted index of trade dependence to become insignificant. This leads to the reasonable conclusion that it is something about the African and Middle East regions that, when they are included in a global sample, is causing the overall (Table 5.1) positive and significant results. However, the temporal analysis above showed that removing the 1950s caused this same weighted index to change from significant to insignificant. Given these two empirical conclusions (without Africa and the Middle East the global sample has no significant result and, separately, without the 1950s the global sample has no significant result), it would seem reasonable to think that removing both Africa and the Middle East in the 1950s from all the other data would cause the same shift (weighted index of trade dependence to change from positive and significant to insignificant). This turns out not to be the case. Testing the sample without those two regions (Africa and the Middle East) in the 1950s still leaves the variable (weighed index of trade dependence) significant.

   This showcases the danger of parsing the data into smaller pieces for examination. The results from the analysis of the temporal spans seemed clear: the 1950s were a spike in conflict far above results for later in the temporal span. The results of the regional analysis were just as clear: without Africa and the Middle East, there was no correlation between trade and interstate conflict initiation (at least with respect to this one weighted index with the "restricted" initiations). Those two conclusions combined implied that there was something special about the 1950s in those two specific regions. However, when that very small sample (less than 10,000 cases out of over 800,000) was removed

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the discussion. There are equally serious problems in rectifying the other columns with previous analysis, but to save space I have highlighted just one variable.

from the data, there continued to be a positive and significant association between the weighted index of trade dependence and increased interstate conflict. These analyses of fragments of the overall data cannot be combined into some "meta-analysis" that allows a clearer picture. It is true that the connection between unbalanced trade and conflict in the 1950s is very strong. It is true that the connection between unbalanced trade and conflict in Africa and the Middle East is strong. It is not true that the 1950s in Africa and the Middle East were the key to the larger (entire world and longer time span) analysis.

One final, brief example from a different column: the weighted index of trade concentration (for "restricted" initiations) is never significant in any region, yet it is positive and significant when tested in the overall sample. Again, if the analysis of the different regions was additive, then at least one of those regions should have returned a significant association between the weighted index of trade concentration and interstate conflict, since there is a significant association for the entire (global) sample. Since none of the regions alone is significant, the utility of these regional analyses to allow further understanding of the entire question is limited.

Moreover, there is a danger of "data mining." Empirical examinations using statistical methods that search out significant associations between independent and dependent variables without preceding theorizing are dangerous procedures. This dissertation carefully treads the line between theory-driven empirical examination and "data mining" in how the temporal spans are tested. The published sources for GDP data (used to create the trade dependence and thus the weighted index of trade dependence for both sets of data) begin in 1950. This makes the Gleditsch data (see Chapter 4) the most complete. Thus, the data are analyzed in two groups: post-1950 (when the available GDP

data allow more precision and reliability in the results), and pre-1950 (when the only source of the trade dependence or weighted index of trade dependence is the much smaller sample of politically relevant dyads from the Oneal and Russett data). The choice of 1950 as a break-point for the analysis is not driven by theory, but by practical considerations. It just so happens that 1950 marks the approximate beginning of the Cold War, though why this should matter for the analysis is unclear. The signaling logic should remain unchanged by system-level changes in hegemony or polarity. The interest-based logic should also remain unchanged. Nothing in the theoretical underpinnings of either theory would argue for a change in behavior by the states as a result of the changes in the world introduced by the Cold War. This is not to say that the Cold War (or the change to the Post-Cold War environment) has no effect – to the degree that trade itself changes in terms of states adding trading partners or deepening their involvement with already existing partners as a result of the Cold War waxing or waning then there will be changes in the amount of interstate conflict in the world (assuming the already accepted Oneal and Russett findings about the relationship of trade and interstate conflict). What should not change, theoretically, is the relationship between unbalanced trade and conflict. The first part of this chapter skirts this "data mining" line by noting that the decision to break the data into pre- and post-1950 periods is a decision based on data availability, not theory. I make no claims that the evidence of some positive relationship between trade and conflict in the post-1950 era (see Table 5.1 and Table 5.3) and the lack of one in the pre-1950 era (see Table 5) are due to the onset of the Cold War at the system-level.

The addition of the analysis that looks at the 5-year spans, the actions of major powers versus minor powers, and the regional analysis all drift uncomfortably close to mining the data. I have no theory (unlike some of the analysis introduced below) for arguing that different regions, different times, or different status states should behave any differently with respect to the signaling-based or interest-based logics of the liberal peace. Having done the analysis, it is difficult to reject the findings: removing the 1950s from the modern temporal span removes many of the significant findings from the entire Cold War and Post Cold War analysis. By the same token, removing Africa and the Middle East from the post-1950 analysis removes almost all of the significant findings. A theory driven explanation of why this should be the case is unknown, as no theory was used to generate the analysis. I cannot explain why the 1950s were a time where unbalanced trade was particularly associated with more interstate conflicts, nor why Africa and the Middle East were places where unbalanced trade was more associated with increased interstate conflict: given those results, however, it is impossible to make any sweeping claims that there is any significant association between unbalanced trade and increased probability of interstate conflict as a general statement for the world (clearly, the analysis in this section shows that not to be the case). In simpler terms, there is little to no evidence that signaling or interests governs how states act with respect to trade and initiating conflict once one takes into account the non-theory driven analysis of this section.

There are ways of parsing the data that are driven by theory, and have the potential to fit into the overall goals of the analysis. While there is ongoing debate about the nature of both a "democratic peace" and an "autocratic peace" (see Peterson, 2004,

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for an overview) it is clear that democracy can affect the propensity of states to engage in interstate conflict. Similarly, some additional liberal peace research has looked at the nature of which states are more likely to trade (since trading affects conflict; see Hegre, 2000). Since one of the fundamental arguments in the democratic peace literature is that democracies are more responsive to the needs/interests of their citizens (Maoz and Russett, 1993), it is possible that democratic states will react differently to trading partners than non-democracies. This is different from simply including democracy as an independent variable in the analysis (which is done; its effects are described above); this argues that there is an interactive effect of democracy and trade. A democracy with a trading partner is likely to react differently to potential conflict than an autocracy or anocracy (a state that is not a democracy or an autocracy, but has some characteristics of both). This is empirically testable, in the same way that a test for regions or power status was – break the overall sample into smaller groups that represent states with those characteristics (democracies, autocracies, anocracies) and see if they have a different relationship between the weighted indices and conflict initiations (different both from each other and different from the overall global sample).

Beyond this additional analysis, other authors have argued for interactive effects between trade-based measures of interdependence, other explanatory variables, and interstate conflict. Two theories in particular lend themselves to this analysis. Benson (2004) examines the argument that both security and economic ties have independent effects on interstate conflict; her results indicate that states that tie themselves through both channels are less likely to engage in interstate conflict than states that do not. This is another interactive effect that can be tested with the mechanism described above:

alliance status between dyadic partners is a variable included in the analysis, and it is

simple to divide the population of cases into sub-groups by alliance type, and investigate

whether those groups differ in the relationship of trade to conflict. Finally, Hegre (2000)

argues that the level of wealth of the state also has an influence on the extent of trade

(wealthy states trade more than poor ones) and level of democracy (more likely to

respond to citizens; more open to interests). Once again, the population of directed-dyads

can be divided into groups of rich, poor and middle-income states and the relationship of

the weighted indices to conflict propensity can be tested.

These three tests (discussed below) differ from the test of region, power status,

and temporal span in that there is previous literature, related to the liberal peace, which

gives us initial hypotheses about how we expect groups of states with certain

characteristics to behave. As with the tests of region, power status, and temporal span,

the research design here is to run separate logistic regressions for each category of states

in order to discover what the effects are of unbalanced economic relations for specific

types of states (rich, allied, democratic).

Rich/Poor: Wealthy states should be less likely to initiate conflict than poor ones.
Alliance: States tied together through security alliances should be less likely to initiate conflict with trading partners than states that do not have security ties.
Democracy: Democracies should be less likely to initiate conflict against trading partners than other forms of government.

These hypotheses above can be translated into specific tests. The alliance

variable (see Chapter 4) codes formal security relations between states into four

categories (from lowest numbered in the data to highest): a defense pact (an alliance that

requires military assistance to the partner), a neutrality agreement (an arrangement that

commits a state to remain neutral if its partner engages in armed conflict with others), an

entente (the two states agree to consult if armed conflict or crises are threatening), or no alliance at all. Of the four types, the most prevalent is no alliance (93% of all directed-dyad cases from 1870 to 2000), followed by defense pact (6.28%), entente (0.48%) and neutrality (0.24%). The hypothesis specifically looks to the tightest of security ties, so the entente and neutrality cases can be lumped in with the no alliance types.

With respect to the democracy hypothesis, the norm in the conflict literature seems to have settled on using the combined democracy/autocracy measure from the Polity data, and classifying any state above a +6 on the scale (which runs from –10 to 10) as a democracy, any below –6 as an autocracy, and anything in between as an anocracy (neither a democracy or an autocracy; see Jaggers and Gurr, 1995). The empirical test here will divide the population of cases into those groups.

The test of state wealth divides the sample into three groups of states: poor, middle, and rich. Using the Gleditsch GDP data and population figures from the Penn World Tables (the source of the Gleditsch GDP data) allows each state's GDP per capita to be calculated per year. Using World Bank-derived cut-off points of GNP per capita, the dividing lines between each of these groups in each year since 1950 can be estimated. Each state, for each year, can be assigned to one of the three groups of states. Each group can then be tested independently to see if unbalanced trade among those types of states has any effect on interstate conflict. It should be noted, however, that the use of the Gleditsch GDP data created problems in the classification of states, and those problems were traceable to the "adjusted" GDP figures. The use of "adjusted" GDP data and "unadjusted" trade data to create the trade dependence and weighted index of trade dependence measures (as using the Gleditsch data does) creates a clear bias in the data.

As a result of mixing "adjusted" GDP and "unadjusted" trade data, poorer states have

significantly inflated GDPs (a function of the adjusting for purchasing power parity),

which in turn significantly reduces their measures of trade dependence and weighted

index of trade dependence (a higher GDP, compared to an unadjusted GDP as Barbieri

uses) means a comparatively lower trade dependence measure (Trade dependence is trade

divided by GDP; "adjusted" GDPs are larger than their equivalent "unadjusted" GDPs for

poorer states). This, in turn, means trade dependence measures get smaller – dividing the

same trade figure by a larger "adjusted" (versus "unadjusted") GDP produces a smaller

measure). This bias seems to have no effect on the results below, but is the first

significant finding that indicates an empirical difference between the Gleditsch and

Barbieri data (Tables 5.1 and 5.3, above, found no significant difference between the data

sets). See Appendix 1 for a complete discussion.[123]

Table 5.8, below, details the effects of the weighted index of trade dependence

and the weighted index of trade concentration on the odds of interstate conflict initiation

for the three different hypotheses described above. Given that other theoretical and

empirical work relating wealth, alliances, and democracy to both conflict and the liberal

peace exists, there are hypotheses projecting how "cutting" the data in these ways is

expected to affect the relationship of the weighted indices to interstate conflict

propensity. Interestingly, few of those hypotheses are bourn out.

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[123] I have moved the complete explanation of the bias in GDP figures to the Appendix in order to facilitate the flow of the narrative and description of the analysis.

Table 5.8: Effect of Trade-based Weighted Indices on Probability of Interstate Conflict Initiation.

(Gleditsch data only, 1950 – 2000: Z-Scores and statistical significance reported)

		Gleditsch Weighted Index of Trade Dependence (Z-Score, Significance given)		Gleditsch Weighted Index of Trade Concentration (Z-Score, Significance given)	
		"Restricted" Initiations	"Broad" Initiations	"Restricted" Initiations	"Broad" Initiations
Wealth	Poor States	-1.09 (0.27)	-0.77 (0.44)	1.38 (0.17)	0.77 (0.44)
	Middle-Income States	-1.08 (0.28)	**-2.71 (0.01)**	**2.72 (0.01)**	-0.18 (0.86)
	Rich States	**5.62 (0.00)**	**7.06 (0.00)**	1.67 (0.10)	1.82 (0.07)
Alliances[124]	Security Alliance	0.34 (0.73)	-0.57 (0.56)	**4.64 (0.00)**	**3.24 (0.00)**
	No Security Alliance	**3.65 (0.00)**	**5.03 (0.00)**	1.34 (0.18)	-0.16 (0.87)
Democracy	Democracy	0.53 (0.60)	-0.59 (0.55)	1.91 (0.06)	0.46 (0.65)
	Anocracy	0.28 (0.78)	-0.40 (0.69)	**2.65 (0.01)**	1.46 (0.15)
	Autocracy	**4.15 (0.00)**	**4.69 (0.00)**	1.48 (0.14)	-0.14 (0.89)

The original hypothesis with respect to democracy argued that the presence of democracy would increase the likelihood of trade being a pacific force in dyadic relations. That turns out not to be true. With democracy there is no empirical pacific effect: a democracy with unbalanced trade behaves no differently than an anocracy with unbalanced trade, as the degree of imbalance in trade increases. However, the absence of democracy is more likely to produce interstate conflict. In other words, being a democracy (scoring 7 to 10 on the democracy measure) does not mean that unbalanced trade decreases the likelihood of an interstate conflict between partnered states, but being an autocracy (scoring –7 to –10 on the democracy measure) with unbalanced trade dependence with a partner does increase the likelihood of an interstate conflict. These results are not replicated in the test of the weighted index of trade concentration, where only a very limited result that anocracies (neither democracies nor autocracies) with unbalanced trade concentration with a partner are more likely to initiate interstate

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[124] These logistic regressions were run without the alliance variable. Hence, the model is slightly different than the other tests. However, it was not possible to leave the variable in the equation, as it would not vary when testing those cases that have security alliances.

conflicts (only true for the "restricted" set of conflicts, not for the "broad"). These results are generally consistent with Table 5.2, above, where the independent effect of democracy was to reduce the likelihood of interstate conflict, as democracy scores got higher. The finding here that unbalanced trade dependence increases the odds of autocracies engaging in interstate conflict deepens the already established understanding that trade, democracy and interstate conflict share a complex relationship.

The results of the tests of security alliances are somewhat confusing. The original hypothesis was that states with security alliances would be more restrained in dealing with trade partners than states that lacked such security alliances. Security alliances should already lead to pacific relations, and trade ties should magnify that effect. Using the trade dependence based weighted index, the empirical results indicate not that trade enhances peace for those states already tied by a security treaty, but that those states that do not share a security alliance are more likely to engage in interstate conflict when faced with unbalanced trading.

What are even more confusing are the results of the tests of the weighted index of trade concentration. Here the results (for both sets of initiations) indicate that unbalanced trade concentration with a partner state where a security alliance exists is more likely to lead to interstate conflict (the opposite result from the test of the trade dependence index discussed in the previous paragraph). This outcome is clearly not consistent with the hypothesis, or previous empirical work. When the alliance variable is tested as an independent variable for the Gleditsch sample (see Table 5.2), it is positive[125] – arguing that as alliances are tighter (closer to a security alliances), the probability of conflict is

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[125] Table 5.2, above, only gives the coefficients for the other (non-trade) independent variables with the results for the Gleditsch test of trade dependence. When the Gleditsch weighted index of trade concentration is tested, the coefficient for the alliance variable is similar to that given in Table 5.2.

reduced. That finding was partially confirmed by the tests discussed in the previous

paragraph (the index of trade dependence increased the probability of conflict with non-

security alliance states). The finding that unbalanced trade concentration among alliance

partners increases the probability of conflict initiation is not consistent with any previous

results involving alliances, though it is robust (high Z-Scores).

Finally, the test of rich and poor states produces results that are clearly

inconsistent with the working hypothesis. While the original hypothesis argued that the

combination of rich states and trading states would be more pacific than either

characteristic alone, these results indicate that rich states in unbalanced trading relations

are more likely to initiate interstate conflict than middle-income or poor states. Given

that the original hypothesis was couched in the language of trade dependence/trade

concentration, and these tests used the weighted indices (which measures unbalanced

trade, not directly the level of dependence/concentration), these results do not directly

contradict the hypothesis, but do raise some troubling questions.

5.6 Conclusions

The early tests in this chapter (Tables 5.1 and 5.3) argued that greater imbalance

in trade in the post-1950 era are significantly correlated with increased probability of

initiation of interstate conflict: of the eight tests (four with Gleditsch data, four with

Barbieri data) that used two different operationalizations of initiation (restricted versus

broad definitions), seven of eight tests found positive and significant relationships

between both measures of unbalanced trade (the weighted index of trade dependence and

weighted index of trade concentration) and increased interstate conflict initiation. The

eighth test was positive, but not statistically significant. No evidence of any relationship

between either form of unbalanced trade and interstate conflict initiation was found in the pre-1950 era (Table 5.5).

The post-1950 results seemed to find a preponderance of evidence that unbalanced trade was associated with higher probabilities of the initiation of interstate conflict. Moreover, no test of a weighted index (with either the Barbieri or Gleditsch data) found evidence that unbalanced trade was associated with more pacific relations – with a reduction in the likelihood of interstate conflict. Given the hypothesis of the dissertation – that a positive association between more unbalanced trade and interstate conflict was consistent with signaling-based explanations of the liberal peace, while a negative association would be consistent with an interest-based explanation – this would seem to be some preliminary evidence in favor of finding for signaling.

However, there are several critical caveats to that statement that remove a great deal of its validity. First, these results do not "prove" that the signaling-based logic is the one that states use. These empirical tests do not directly measure the signals or attempts to generate or receive signals between states, and thus are not a direct test of signaling or interests. What these results indicate is a correlation between states in positions to send certain signals (dependent states in unbalanced economic relationships) and the results of those expected signals (an increased likelihood of initiation of interstate conflict).

Second, the additional analysis of these last two section calls into question the general statements about the relationships between unbalanced trade and interstate conflict. While the findings reported in Tables 5.1 and 5.3 (the general results in the post-1950 era) are supportive, it is just as true that the 1950s provide most of the association between unbalanced trade and conflict in the entire post-1950 era. Moreover,

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Africa and the Middle East are critical to the association (removing those regions removes any significant correlation). Thus, making a general observation about the empirical results supporting the signaling logic is problematic. The strongest claim relating to the core of the dissertation that can be made is that there is no evidence that supports the interest-based logic of the liberal peace. There is some evidence that is consistent with the signaling-based logic of the liberal peace. A synthesis of the results in this chapter would seem to suggest there is evidence that states act in the manner best described by the signaling-based logic, while granting that a more detailed examination of the temporal spans casts doubt on the general conclusions. This is not to say that there is no correlative evidence in favor of signaling in the post-1960 span: if those same eight tests of the weighted indices are conducted with the 1950s decade excluded (and are, thus, tests of 1960 and later), then three tests find a positive association (evidence in favor of signaling), five find no association, and none find a negative association (evidence in favor of interests). This is weaker evidence in favor of signaling, but still evidence in favor of signaling. One thing is absolutely certain: the overwhelming evidence in the pre-1950 span is that there is no association between trade (however measured) and interstate conflict initiation.

There are several secondary research questions that the results in this chapter cast light on. In particular, the chapter offers evidence of the "boundary conditions" (Mansfield and Pollins, 2001) where the limits of the liberal peace are reached. However one views the results in this chapter with respect to the signaling versus interests debate, it is clear that unbalanced trade can lead to increased probabilities of interstate conflict. More simply, trade is not always pacific. For the liberal peace to operate most

effectively, trade should be both with a large number of partners (reflecting the high

correlation between the weighted index of trade concentration and increased probability

of interstate conflict initiation) and be equally balanced and significant to both states in

the economic partnership (reflecting the high correlation between the weighted index of

trade dependence and interstate conflict). Additionally (echoing the results of Table 8),

rich states tend to be more susceptible to unbalanced trade increasing the likelihood of

initiating interstate conflicts (contradicting Hegre, 2005), as do states that are not in

security alliances with the partner state (echoing results found in Benson, 2004), as do

autocracies (which is a result that backs the "Kantian Peace" ideas of Russett and Oneal,

2001). Trade is not equal, or equally pacific to all states. These "boundary conditions"

argue that a policy of trying to blindly promote trade for any and all states around the

world will not necessarily lead to a more peaceful world.

Another secondary consideration of this dissertation is to examine the differences

between the competing data sets of Gleditsch (2002) and Barbieri (2002). Given the

results in this chapter there seems little difference between the Gleditsch approach to data

(use non-IMF sources, and interpolate/extrapolate to fill in missing data, using "adjusted"

GDP data with "unadjusted" trade data) and the Barbieri approach (no

interpolation/extrapolation, no non-IMF data, no "adjusted" data at all). Differences

between results were traced mostly to the cases each data set contained, and only to a

small degree the differences in the actual data between the two sources. This is not a

final result, however. As the results of the analysis in the last section (and Appendix 1)

show, there are significant differences between the data created with "adjusted" versus

"unadjusted" GDP. Those differences do not seem to have made a significant impact on these results, thus far, but the analysis in the future chapters will need to be cautious.

Overall, while no definitive answers with respect to interests or signaling (beyond no association of any kind, pre-1950) were found, this chapter does begin to shed light on the limits of the liberal peace, and the differences between the major data sets used to explore the liberal peace. While no firm conclusions can be made from the results of this chapter, the tests here are only part of the overall project.

With these results in mind, we turn to the test of hostility to see if any of the confusion can be cleared up.

6.0 Chapter Six: Empirical Analysis - Effect of Trade-Based Measures of Interdependence on Interstate Conflict Hostility.

6.1 Introduction

This chapter takes up the second of the two sets of tests that directly bear on the questions raised early in the dissertation: does the signaling-based logic or the interest-based logic drive the liberal peace. The two theories of the liberal peace argue that states in unbalanced trading relations should, as in the test of initiation, act differently with respect to hostility in ongoing interstate conflict. The interest-based logic argues that trade creates economic benefits, and that dependent states in unbalanced trading relationships will do much to avoid damaging the political relationship in order to avoid economic harm (if the trade ends, and a significant conflict would end the trade), and hence will not initiate interstate conflicts or use higher levels of hostility in ongoing ones. The signaling-based logic argues that trade creates pathways for signals between states, and ways to credibly send reliable information. Dependent states might need to initiate interstate conflicts or use higher hostility in ongoing conflicts in order to make the less-dependent partner pay attention or "prove" they (i.e., the dependent states) are serious about the issue underlying the conflict.[126]

While Chapter 5 examined the question by looking at the relationship between measures of unbalanced trade and the propensity of states to initiate interstate conflict, this chapter focuses on measures of unbalanced trade and the propensity of states in interstate conflicts to use higher levels of force against their partner state. The results

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[126] See Chapter 4 for a complete discussion of the signaling-based and interest-based logics, as well as their underlying assumptions.

from Chapter 5 were mixed, at best, with some empirical evidence pointing towards states behaving as the signaling-based logic would predict.

The empirical tests in this section differ significantly from the previous chapter. Where the dependent variable in the test of initiation was binary (initiation or not), the dependent variable here is categorical: in the Militarized Interstate Dispute data used here, states' actions in interstate conflicts are categorized into one of five responses (in increasing order of hostility/intensity): no action, threat of force, display of force, use of force, war. Thus, while the previous chapter used logistic regression as the primary tool of empirical analysis, this chapter uses ordered logistic regression. Ordered logistic regression allows for multiple categories of the dependent variable (as long as they are ordered categories, meaning they can be rank ordered), but returns similar output to standard logistic regression. In this way, the results here are similar, but not identical, to the previous chapter.[127] The presentation of results will mimic the previous chapter, as well.

This chapter will follow the previous in terms of it structure and organization. Following this introduction, there are four separate sections. The first section reports the results of order logistic regressions testing the effect of unbalanced trade on the

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[127] One of the underlying assumptions of ordered logistic regression is that the independent variables have a fixed effect across all the categories, and the categories are differentiated by constants. In other words, each independent variable has only a single coefficient representing its effect on the dependent variable. The effect of the single coefficient for each independent variable does not mean, however, that each independent variable coefficient has a fixed linear effect on the probability; the transition from the odds-ratio of ordered logistic regression to the actual probabilities is a non-linear equation, and as the constants change from category to category, the overall equation changes and the effect of any single coefficient (independent variable) is not constant. This is a long way of saying that if, for example, democracy is associated with a negative (and significant) effect on hostility, the effect of democracy is not the same across all the categories of hostility. It may reduce the probability of a state using force by 20%, but reduce the probability of displaying force by only 10%. The coefficient for democracy is the same in both cases, but the transition from the odds-ratio (returned by ordered logistic regression) to the actual probabilities uses the constants and a non-linear equation, so the effects of any independent variable on probabilities is not constant.

propensity of states already engaged in interstate conflict to use higher levels of hostility. This section will use only the Gleditsch data, and is limited to post-1950. The next section will introduce the Barbieri (post-1950) data, and directly compare the Gleditsch and Barbieri results to see if the difference in how the two data sets were constructed creates differences in empirical results. The same test in the previous chapter (Chapter 5) found little difference, though later exploration revealed that the use of "adjusted" GDP data and "unadjusted" trade data might distort the Gleditsch trade dependence-based measures. Thus, the test is repeated for this chapter, to see if the difference discerned in the previous chapter has an effect on the empirical results of the respective data sets. The third section will use the Oneal and Russett data to examine the effects of unbalanced trade on conflict hostility in the pre-1950 era and compare these results with the Gleditsch and Barbieri post-1950 results. The fourth and final section will "cut" the data employing the same categories as in the preceding chapter to see if the relationships between measures of unbalanced trade and conflict hostility are consistent across different kinds of directed-dyads.

It should also be noted that the number of cases tested falls dramatically in this chapter. While the number of directed-dyads in the previous chapter was high (as many as 800,000+ for the Gleditsch data), the numbers here are only in the neighborhood of 3000 (for the Gleditsch data; less for Barbieri). This is because the universe of possible cases in the previous chapter's tests was all possible permutations of each state paired with every other state in every year (since each relationship represented a potential opportunity for conflict initiation), while the set of tests in this chapter focuses on ongoing conflicts. Given that the MID data identifies only about 3000 conflicts from

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1816 on, and missing data prevent all of them from being examined, the number of possible directed-dyad cases that can be used drops significantly compared to the previous chapter. This in no way makes the results here less valid (like the previous chapter these tests also attempt to use the universe of all possible cases), though given the huge difference in numbers it is worth noting. One additional note: the discussion of "restricted" versus "broad" sets of initiations also falls away in this chapter. The distinction between the two groups of initiations was focused on whether to count as an initiated interstate conflict instances where one state joined an ongoing interstate conflict after the initial day of initiation. The "restricted" set of initiations did not count those as initiations, as the decisions for the state to join were likely caught up in the ongoing conflict and the sides already chosen, while the "broad" set did count those as initiations (hence, the "broad" set was larger than the "restricted" set of initiations). With respect to questions of hostility, the issues of whether the state initiated the conflict or not are irrelevant. Thus, for the tests in this chapter, the cases are directed-dyad years during which a pair of states are engaged in a MID. The value assigned to the dependent variable in each case is the highest level of hostility used in the interstate conflict by the primary state in the dyad, whether that state is an initiator, joiner, or victim. As in the preceding chapter, the "primary state" is the state from whose point of view the dyadic trade variables are computed (in terms of the balance/imbalance of trade), and are described more fully in Chapter 4.

6.2 Empirical Test of Conflict Hostility

Overall, the results from the test of conflict hostility provide additional evidence in favor of the signaling-based logic of the liberal peace, but still no definitive proof.

Table 6.1, below, presents only the results for the trade-based weighted indices from the ordered logistic regressions using only the Gleditsch data in the post-1950 period. Unlike the results from the test of conflict initiation, there is a stark difference in results depending on the type of trade-based variable used in the tests. The trade concentration weighted index is highly significant and positively correlated with hostility, while the trade dependence weighted index is not. In other words, the greater the imbalance of concentration (one state is more concentrated in its trade with the partner than the other) the more likely that state is to use higher levels of hostility against that partner in the interstate conflict.

Table 6.1: Effect of Trade-Based Variables on Probability of Conflict Hostility for all Conflict Directed-Dyads: Results of Ordered Logit Analysis for Trade Based Variables.
(Gleditsch Data; 1950+)

	Trade Concentration Weighted Index	Trade Dependence Weighted Index
Gleditsch (1950 – 2000)	**3.01** **(0.00)** **(N=2970)**	1.28 (0.20) (N=2887)

The results for the weighted index of trade concentration are not replicated by the weighted index of trade dependence. No support in any test is found for the interest-based explanations of the liberal peace.

These results, as in the tests of conflict initiation, offer qualified support for the signaling-based explanations of the liberal peace. As was argued in Chapter 4, the concentration-based measures are not widely used in the literature, but do offer information about trade and trading relationships that the GDP-based dependence measures cannot. While the GDP-based dependence measures offer a better understanding of how influential trade with the partner is to a state's overall economy,

the concentration-based measures offer an understanding of the degree of isolation of a state – the degree to which, however much trade there is and however important to the overall economy, the state economically interacts with only the partner. While it is accurate to describe the GDP derived measures as more representative of a genuine measure of dependence, the trade concentration measures are not without merit. The signaling logic argues that more dependent states already have information and receive good signals (because of that dependence). It is the less dependent partner states that fail to understand what the more dependent state is signaling, which causes the more dependent state to take action (like initiating an interstate conflict or pushing one to higher hostility levels) as a signal of resolve/capability. States with high values of trade concentration (and especially where the high trade concentration is unreciprocated by the partner, which is what the trade concentration weighted index measures) are also likely to be in the same position – more informed than the (less concentrated) partner, and hence more likely to take actions (initiations and higher hostility in conflicts) to make the partner more aware. States with fewer trading partners are in a similar position to economically dependent states with respect to the logic of signaling – they must take actions to convince the less concentrated partner state to pay attention to them. These empirical results (Table 6.1) support this logic.

What is troubling for the signaling-based logic is the lack of significant positive correlation for the trade dependence-based variable. In terms of measuring the dependence of one state on another (the focus of this dissertation), the GDP-based trade variables are conceptually superior to the trade concentration variables, and statistically significant results (either positive or negative) with these variables are important for

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attempts to examine the signaling and interest-based logics. Using the Gleditsch data, the most widely used and accepted, does not produce statistically significant empirical results (positive or negative) when used in the creation of the weighted index of trade dependence – though the Gleditsch data were significant and positive for the test of conflict initiation. We will further examine this issue in the section (below, and Appendix 1) that discusses the Barbieri results.

Ordered logistic regression is an unusual technique, and a graphical display of the results will maker more clear the effect of the weighted index of trade concentration on the level of hostility in interstate conflict. The assumptions of ordered logistic regression are that there are multiple categories of the dependent variable, and that each independent variable has a fixed effect on the probability of the outcome for each category. In other words, the equation returns a single coefficient for each independent variable, and calculates a constant for each category of the dependent variable. Figure 6.1 graphically shows the effect of the weighted index of trade concentration on the probabilities of each category of hostile action as the value of weighted index moves from its minimum to its maximum value.

Figure 6.1: Probabilities for Highest Level of Hostility for Different Values of Weighted Index of Trade Concentration (Taken From Table 1, above).
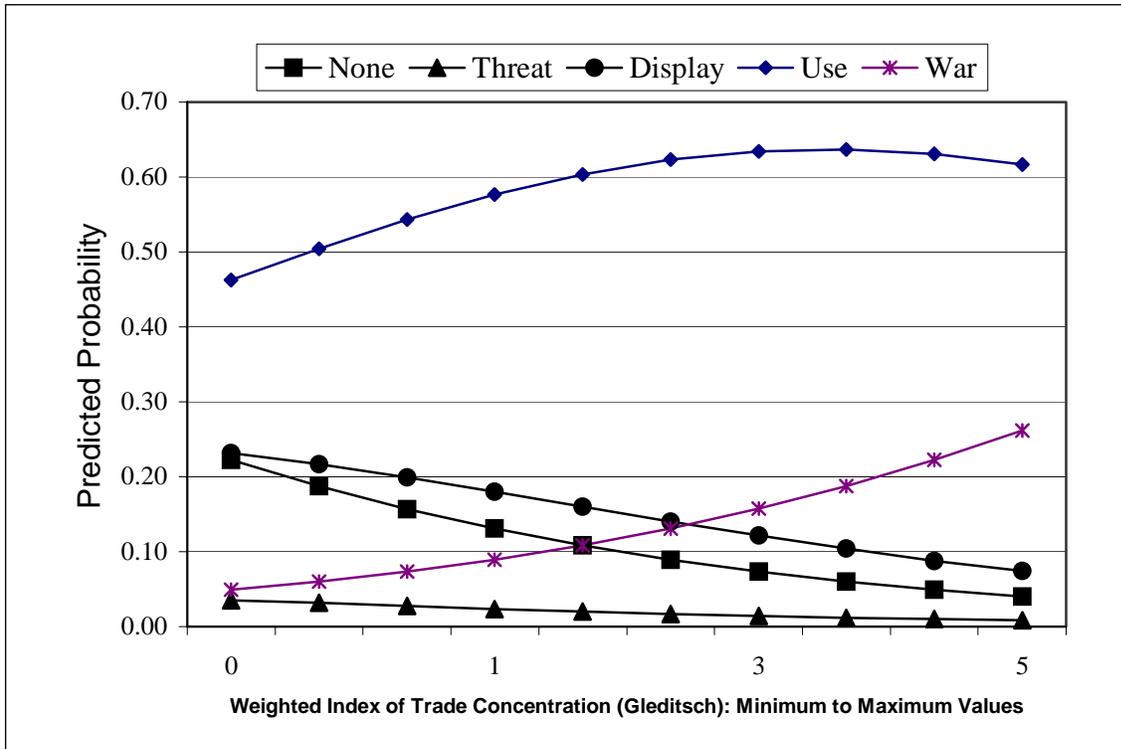
This figure graphically demonstrates the probability of each level of interstate conflict hostility for various values of the weighted index of trade concentration. Overall, the "use of force" (MID category 4) is the level of hostility most likely to emerge in an interstate conflict. In general, as the weighted index of trade concentration climbs from low to high values (which indicates that the primary state's trade is becoming a larger proportion of its total trade), the probability of "use of force" and war climbs, and the probability that the highest level of hostility will be either no response, a threat, or a demonstration of force decline. States with a fairly large number of trade partners (and relatively equal trade with all partners), if they engage in an interstate conflict, are (estimating the percentages at the left hand side of the figure) about 45% likely to use

force, about 20% likely to do nothing, about 20% likely to display military force, and about 5% likely to either issue a threat or go to war. States that have few trading partners (or a single one) and are heavily concentrated with them are (estimating the percentages from the right hand side of the figure) about 60% likely to use force (up 15%) in the interstate conflict, 25% likely to go to war (up 20%), 10% likely to display force (down 10%), and 5% likely to either do nothing or threaten force (down 15% for do nothing; no real change for threaten). In other words, increasing trade concentration is associated with an increase in the probability of the interstate conflict being one that involves more hostile actions. To the degree that trade concentration is a valid measure of the dependence of a state (as noted, this is disputed), this is evidence in favor of the signaling-based logic. Even if one rejects the validity of the measures of trade concentration, there is no evidence here in favor of the interest-based logic.

Table 6.2, below, reports the results for all independent variables for the two weighted indices reported in Table 6.1. Examining whether independent variables change sign and/or significance when the two different weighted indices are used in what are otherwise identical empirical tests allows us both to determine what the effects are of the independent variables on conflict hostility (if there is no change in sign and/or significance) and note which independent variables do radically change in the presence of the two different weighted indices. This comparison will add little to our understanding of interests and signaling, but is revealing in showcasing the differences between the sources of trade data.

Table 6.2: Effect of Independent Variables on Probability of Conflict Hostility for Conflict Directed-Dyads: Gleditsch Data, 1950 – 2000.

Independent Variables	Column1: Gleditsch Weighted Index of Trade Dependence Z-Score of Independent Variable (Statistical Significance) (1950 – 2000) (N=2887)	Column 2: Gleditsch Weighted Index of Trade Concentration Z-Score of Independent Variable (Statistical Significance) (1950 – 2000) (N=2970)	Column 3: Overall Effect of Independent Variable on Level of Hostility in Interstate Conflict: Is their agreement between the two tests about the effect of the independent variable?
Hostility Level of Partner State in Dispute	0.98 (0.33)	**2.90** **(0.00)**	Indeterminate
Is Primary State a Major Power?	**-3.55** **(0.00)**	**-4.32** **(0.00)**	Negative: If the primary state is a major power, that reduces the hostility the state uses in the interstate conflict.
Is Partner State a Major Power?	-0.01 (0.99)	-0.86 (0.39)	No Effect: Power Status of the partner state has no effect on the level of hostility used by primary state
Level of Democracy of Primary State	**-5.70** **(0.00)**	**-5.02** **(0.00)**	Negative: The more democratic the primary state, the lower levels of hostility that state uses in interstate conflicts.
Level of Democracy of Partner State	-0.22 (0.83)	-0.09 (0.93)	No Effect: The level of democracy of the partner state has no effect on the level of hostility used by the primary state.
Level of Alliance between States	0.27 (0.79)	0.26 (0.80)	No Effect: The degree of alliance between the two states has no effect on the level of hostility in the interstate conflict.
Distance between States	**5.32** **(0.00)**	**6.49** **(0.00)**	Positive: The greater the distance between the states, the higher the level of hostility used by the primary state.
Contiguity Measure between States	**-6.00** **(0.00)**	**-5.96** **(0.00)**	Negative: Contiguity correlates with lower hostility levels used by the primary state in an interstate conflict
Ratio of Power between States	**3.38** **(0.00)**	**3.90** **(0.00)**	Positive: The greater the imbalance of physical capabilities in favor of the primary state, the greater the hostility used by the primary state.
Relative number of Joint IGOs	**-2.49** **(0.01)**	**-4.10** **(0.00)**	Negative: The greater the number of Intergovernmental Organizations in which the pair of states both belong, the lower the level of hostility used by the primary state.
Weighted Index of Trade Concentration or Trade Dependence (see top of column)	1.28 (0.20)	**3.01** **(0.00)**	Indeterminate

In comparing the results in columns one and two, the coefficients for only two

variables – the level of hostility employed by the partner state and the weighted index of

trade – differ substantially.   In column one, where the weighted index of trade

dependence is used, the coefficients for neither the partner state's action nor the weighted

index is significant; in column two where the weighted index of trade concentration is

used, the coefficients for both the partner state's action and the weighted index are both

positive and statistically significant. The different trade-based variables are not directly

related to the other independent variables nor does changing the trade-based variable

substantially alter the number of dyads included in the analysis (the N for the weighted

index of trade concentration is 83 cases greater than for the test using the weighted index

of trade dependence). The results of the analysis reported in Table 6.2 are, therefore,

curious. The result using the weighted index of trade concentration makes more sense

than that for trade dependence. We would expect the actions of the partner state to have a

strong impact on the actions of the primary state. High levels of hostility are likely to be

met with high levels of hostility, low with low. Thus, the results using Gleditsch's

measure of the weighted index of trade dependence seem odd, and require further

study.[128]

Of the eleven independent variables, nine independent variables show agreement

in significance and direction when tested with both the weighted indices. Of those, six

have statistically significant results: (1) the level of democracy of the primary state, (2)

the distance between the states, (3) the degree of contiguity between the states, (4) the

ratio of physical capabilities between the states, and (5) the power status of the primary

state, and (6) the number of intergovernmental organizations to which both states belong.

Three independent variables were not statistically significant in both tests: (1) the power

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[128] As was discussed in the previous chapter, there are questions about the use of "adjusted" and
"unadjusted" data used together, as the Gleditsch weighted index of trade dependence does. This issue will
be investigated further in the next Section of this chapter.

status (major or minor power) of the partner state, (2) the level of democracy of the partner state, and (3) the type of military alliance between the states. The fact that there is general agreement across different sets of cases (based on availability of trade data) is reassuring in terms of the validity of these results.

Leaving aside the relevance of these other results for the tests of signaling and interests, the effect of these other independent variables on the level of hostility used in interstate conflict is unusual. The more democratic a state, the lower levels of hostility it is likely to use. While this is not inconsistent with previous research in either the democratic peace or the liberal peace, results from those studies generally argue for a jointly democratic effect: pairs of democratic states experience less interstate conflict. Here, democracy itself reduces the overall hostility of a conflict, regardless of how democratic the partner state is. Of course, if both states are democratic, then there remains a joint democracy effect in the sense that both the primary and partner state will use less hostile forms of interstate conflict. Distance and contiguity both point to the same result: the farther apart the pair of states, the higher the hostility used in the interstate conflict. For tests of conflict initiation, distance reduces the likelihood of a conflict. The results here show that once a conflict has begun, distance is no barrier to more violent forms of conflict. This likely comes from the higher than average number of major powers within this data set, as well as the overall larger number of disputes at higher levels of hostility. One of the definitions of a major power is a state that has the capability of reaching around the globe to project power, and major powers are not likely to project power in a limited fashion: if it is worth the expense of resources and power to display force at a distance, then the state is likely to be willing to use it as well.

However, that logic is rejected given the results of the power status variable: if the primary state is a major power, that reduces the hostility used by the primary state in the conflict. Thus, we are left with the perplexing result that minor powers (which are less likely than major powers to initiate MIDs) use more hostility against distant enemies, than major powers do against neighboring states. Again, this is peripheral to the focus of the dissertation, and will not be explored further, but is an interesting result. Next, the ratio of physical capabilities, when the balance favors the primary state, pushes the conflict to higher levels of hostility. Again, this result seem intuitively clear (when states have the force advantage, states will use it), though it is not a result that previous research has illuminated. Finally, in a result that supports the work of Oneal and Russett (2001), the greater the number of intergovernmental organizations of which both states are members, the less hostility the primary state will use versus the partner.

Overall, the results of the analysis of interstate conflict levels of hostility finds additional qualified support for the signaling-based logic of the liberal peace in the post-1950 era. This support is limited, as only the weighted index of trade concentration acted as the signaling-logic predicted. Thus, like the tests of initiation, there is support for the idea that states are willing to use higher levels of hostility in interstate conflicts especially when the balance of trade is against them: when they are, in some form, more dependent on the partner state than the partner is on them. This is consistent with the signaling logic that dependent states use that dependency as a signal to convey a credible message of resolve or hidden capability in order to bargain with the partner state more successfully. There is no evidence, even qualified, that interests, which should force dependent states to be more peaceful and less hostile, have any effect on state actions with regard to

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interstate conflict. Given these limited tests, the overall results are not strong enough to warrant any final conclusion. Moreover, when the Barbieri data are introduced and compared to the Gleditsch results, there is even less uniformity in results.

6.3 Comparing the Barbieri and Gleditsch Data in Tests of Conflict Hostility

As in the previous chapter, a critical theoretical and empirical question relates to the comparability of the Gleditsch and Barbieri data. The Gleditsch data remain the standard used by most liberal peace researchers, though others have raised objections. The analysis in Chapter 5 indicated that, while there were differences between the data (especially with regard to the "adjusted" GDP data that Gleditsch uses), the two data sets produced remarkably similar results when tested using the same sets of cases. Thus, on initial inspection, the debate about data seems much ado about nothing.

This section of the chapter will parallel the section in the previous chapter. We will replicate Table 6.1, above, except with the Barbieri data. If there are differences, then the Gleditsch data can be substituted into the Barbieri cases (as was done in the previous chapter) to see if the different empirical results derive from differences in case selection, or differences in the data themselves.

Table 6.3, below, is identical to Table 6.1, above, except that the Barbieri data are used instead of the Gleditsch data. The results are somewhat different than the Gleditsch (Table 6.1) results.

Table 6.3: Effect of Trade-Based Variables on Probability of Conflict Hostility: Results of Ordered Logit Analysis for Trade Based Variables, 1950+ (Barbieri Data)

	Trade Concentration Weighted Index	Trade Dependence Weighted Index
Barbieri (1950 – 1992)	**4.48** **(0.00)** **(N=1491)**	**3.24** **(0.00)** **(N=1442)**

While both Barbieri and Gleditsch agree with respect to the test of weighted index of trade concentration (there is a statistically significant positive relationship between trade concentration and levels of hostility), they differ with respect to the weighted index of trade dependence (the relationship using the Gleditsch data is statistically insignificant; the relationship is statistically significant and positive when using the Barbieri data).

Given this discrepancy, the next step is to limit the Gleditsch data to the Barbieri cases and see if the differences between Table 6.1 and 6.3 are due to differences in case selection, or differences in the data.

Table 6.4, below, replicates the table in the previous chapter in that the more complete Gleditsch data are limited to the valid Barbieri cases. Any change in results, compared to the original coefficients (taken from Tables 6.1 and 6.3, above), helps resolve the ongoing debate about the divergent results that occur from similar research designs, but different data source. When the Gleditsch data were substituted into the Barbieri cases in the previous chapter, there were no substantial differences. The same claim cannot be made about this substitution.

Table 6.4: Effect of Trade-Based Variables on Probability of Conflict Hostility for Conflict Directed-Dyads: Results of Ordered Logit Analysis for Trade Based Variables

(Gleditsch and Barbieri data, 1950+; Z-Scores, significance, and Ns reported)

	Trade Concentration Weighted Index	Trade Dependence Weighted Index
Row 1: Barbieri (1950 – 1992) (From Table 3)	**4.48** **(0.00)** **(N=1491)**	**3.24** **(0.00)** **(N=1442)**
Row 2: Barbieri data substituted into cases with valid Barbieri and Gleditsch data.	**4.59** **(0.00)** **(N=1480)**	**3.33** **(0.00)** **(N=1431)**
Row 3: Gleditsch data substituted into cases with valid Barbieri and Gleditsch data.	**4.18** **(0.00)** **(N=1480)**	1.04 (0.30) (N=1431)
Row 4: Gleditsch (1950 – 1992)	**2.57** **(0.01)** **(N=2353)**	0.92 (0.36) (N=2353)
Row 5: Gleditsch (1950 – 2000) (From Table 1)	**3.01** **(0.00)** **(N=2971)**	1.28 (0.19) (N=2887)

The key comparison is between the second and third row, where the only difference between the empirical test is the source of data (the cases are exactly the same). The results are clear. Only with the Barbieri data (Rows 1 and 2) does one find a statistically significant association between the weighted index of trade dependence and increased hostility in interstate conflicts. The original Gleditsch data (Row 5) does not; when the Gleditsch data are limited to the same years (1950 – 1992) as the Barbieri data (Row 4), there is again no significant association; finally, when the Gleditsch data are limited to only the Barbieri cases (row 3), there still is no positive and significant association between the weighted index and increased hostility. This clearly shows that the differences in data between the Gleditsch and Barbieri data sets are behind this significant difference.

Moreover, this is an important result. The Barbieri data produce results that find a positive and statistically significant association that supports the signaling-logic of the liberal peace. The Gleditsch data produce insignificant results. It will be difficult to find any substantive conclusion for the test of interests versus signaling unless these divergent empirical results can be rectified. This was not an issue in Chapter 5 (the tests of initiation), as results from both sets of data substantially agreed. Here, as opposed to the results from the test of initiation, we find an important difference that can be traced to differences in data, not case selection.

As has been discussed before, Barbieri and Gleditsch have the same trade data (as evidenced by the fact that when Gleditsch's trade concentration data are substituted into Barbieri's cases there is no change in results; the trade concentration variables are created using only trade data). Where Barbieri and Gleditsch differ is in GDP data: Barbieri uses GDP values from the World bank, while Gleditsch uses GDP data from the Penn World Tables (See Chapter 4 for a complete discussion; some further discussion can be found at the end of Chapter 5 and Appendix 1). The World Bank GDP figures are unadjusted for cost-of-living variations between countries (they are unadjusted for Purchasing Power Parity, PPP), as are the IMF trade figures. The Penn World Tables GDP figures, on the other hand, are adjusted for PPP.

In order for the Barbieri measure of the weighted index of trade dependence to reach the level of statistical significance when tested (where the Gleditsch measure does not), the Barbieri index should exceed the Gleditsch index more often at higher levels of hostility, or exceed the Gleditsch index by larger amounts at higher levels of hostility (or both). In other words, as we look across the five categories of hostility, we can explain

the significant Barbieri results (and insignificant Gleditsch results) if the value of the

Barbieri index is greater than the value of the Gleditsch index more often at higher levels

of hostility than lower ones[129], or if the value of the Barbieri index is many times the

value of the Gleditsch index at high levels of hostility (and the ratio is much smaller at

low levels of hostility).

That this is true is demonstrable. The detailed analysis is carried out in Section

10.3 of Appendix 1 (it is long, and is not necessary for the purposes of this chapter).

Section 10.3 of Appendix 1 demonstrates that there is a clear correlation between the

"adjusted " GDPs and the level of wealth of the state; poorer states have higher

"adjusted" GDPs than richer ones (this was discussed in Chapter 5, and demonstrated in

Section 10.2 of Appendix 1). This should be uncontroversial: it is expected that the

"adjusted" GDPs would be higher for poorer states. What is unexpected is that relatively

poorer states that are engaged in Militarized Interstate Disputes (MIDs) do so at higher

levels of hostility than other (richer) states. Thus, when measures of trade dependence

(and the weighted index of trade dependence) are created with the "adjusted" GDP data

(as the Gleditsch data provide), there is an unintended correlation between lower values

of trade dependence (since the GDPs of poorer states are inflated, while the trade figures

are unchanged, the overall result is that the measure of trade dependence falls) and more

hostile interstate disputes. When the measures of trade dependence created with

"adjusted" GDP data are used in analysis (Gleditsch data; Table 6.1), no statistically

significant relationship is found between the weighted index of trade dependence and

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[129] If the values of the Gleditsch index exceed the values of the Barbieri index at low levels of hostility, but the values of the Barbieri index exceed the values of the Gleditsch index at high levels of hostility, then that pattern would create positive and significant coefficients in the ordered logistic regression for the measure created with the Barbieri data – which is exactly what Table 6.3 shows.

increased conflict hostility. When "unadjusted" measures of trade dependence are used

in analysis (Barbieri data; Table 6.3), a statistically significant relationship is observed.

The reason the Gleditsch-based weighted index of trade dependence fails to find

statistical significance and the Barbieri-based weighted index of trade dependence finds

statistical significance is due to the use of "adjusted" GDP in the Gleditsch data, and

"unadjusted" GDP in the Barbieri data.[130]

Table 6.4 indicated that only Barbieri's data found a positive and significant

association between the weighted index of trade dependence and greater hostility,

meaning that unbalanced trade is positively correlated with greater levels of violence in

interstate conflicts. This result was not found with the Gleditsch data, even when the

exact same cases as Barbieri used were tested. Now, however, it is clear why the

Barbieri results differ from the Gleditsch results: Gleditsch's use of "adjusted" GDP data

artificially increased the GDP measures for poor states, which in turn depressed the

weighted index of trade dependence for those poorer states, which coincidentally

happened to use higher levels of hostility in conflict. Using the Gleditsch data, no

significant relationship is found; using the Barbieri data and the same variables, there is a

positive and significant association.

This means, I argue, that there is support for the conclusion that unbalanced trade

is associated with higher levels of interstate conflict. Three of the four tests in this

section (both Barbieri's trade dependence and trade concentration weighted index as well

as Gleditsch's trade concentration index) found support for that assertion. The single test

that found no association between unbalanced trade and increased conflict hostility

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[130] See Appendix 1, Section 10.3 for the full analysis.

(Gleditsch's weighted index of trade dependence) can be explained away through the discussion of the bias in the data used to create the measure.

This is not a trivial result. Assuming one accepts the assumptions that went into the set of tests in Appendix 1 (the results are described above), this demonstrates that the Gleditsch GDP data create potentially serious complications when used in empirical international relations studies that also contain the Militarized Interstate Dispute data (since a correlation exists between the "adjusted" GDP data and levels of hostility used by poorer states). The comparison of the Barbieri and Gleditsch data in the previous chapter found that Gleditsch's use of interpolation/extrapolation did not create any problems, or any problems from the use of the "adjusted" GDP data. This chapter, however, clearly indicates that "adjusted" GDP figures have a flaw that can affect the results of some empirical tests. It is likely that had the interstate conflict sample of cases that make up the core of the data in this chapter contained fewer "poorer" states at those high levels of hostility, the discovery of the effects of "adjusted" versus "unadjusted" GDP data would have been missed: it is only because the data used in this chapter contained a relatively large number of those cases that the issues with the data were discovered. However, it is worth noting that almost all liberal peace studies use both the Gleditsch GDP data and the MID data.

I hesitate to call these results "profound", but certainly they suggest that other peace researchers who have used the Gleditsch data should revisit their analyses to discover whether the "adjusted" GDP problems have affected their overall results. As this dissertation is not directly examining or questioning the central findings that a liberal peace exists, the results in this section are not sufficient evidence (or, in fact, any

evidence at all) that there is no liberal peace. However, the problems with the Gleditsch

data - which is the dominant data used by almost all liberal peace researchers – clearly

call for a re-examination of the claims of the liberal peace.[131]

Focusing on the questions this dissertation asks, compared to the previous

chapter's tests of conflict initiation, there is substantially more support for the signaling

logic in the tests of interstate conflict hostility than there was in the tests of initiation.

Where the tests of initiation produced mixed results, here all the tests showed evidence

that states act according to signals (if one discounts the single Gleditsch test, as argued

above). Moreover, there was no evidence in favor of the interest-based logic of the

liberal peace.

This is not proof-positive that the signaling logic is the answer. Again, as was

discussed in the conclusions for Chapter 5, this dissertation contains no direct test of

signals, their generation or receiving by the partner states. These results are only

suggestive: states are behaving *as if* they are signaling each other, and only in the post-

1950 era.

The next section, following the outline in the previous chapter, will examine the

limited data from the pre-1950 span to see if the findings in this section will hold.

6.4 Tests from the Pre-1950 Era

Contrary to the results above, identical tests, restricted to weighted indices from

the pre-1950 era, produce almost unqualified support for the interest-based logic of the

liberal peace and no support for the signaling hypothesis. Table 6.5, below, is identical to

Table 6.1, but only reports results using trade date from pre-1950 (hence, uses Oneal and

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[131] Chapter 8 undertakes a re-examination of the liberal peace itself; thus, questions of the validity of the
liberal peace are left for later.

Russett data, not Gleditsch or Barbieri data). Previous results from the pre-1950 era

(Chapter 5 tests of initiation) found no association between unbalanced trade and the

initiation of interstate conflict, so these strong results are somewhat of a surprise. The

caveat to this analysis is that the only data that support this conclusion are the weakest of

the three used in the dissertation: the Oneal and Russett data that only uses "politically

relevant dyads," hence the much smaller sample size.

Overwhelmingly, the data support the interest-based logic of the liberal peace and

show that the greater the unbalanced trade the less likely a state is to use increasingly

hostile levels of interstate conflict against the partner state.

Table 6.5: Effect of Trade-Based Variables on Probability of Conflict Hostility: Results of Ordered Logit Analysis for Trade Based Variables.
(Oneal and Russett Data; Politically Relevant Dyads Only; Pre-1950)

	Trade Concentration Weighted Index	Trade Dependence Weighted Index
Oneal and Russett (1885 – 1949) (Pol. Relevant Dyads)	**-2.86** **(0.00)** **(N=579)**	**-2.97** **(0.00)** **(N=591)**

There seems clear evidence that states have changed behavior around 1950.

Where measures of trade (including measures examining the dependence of one state on

the other) before 1950 are clearly associated with more pacific actions during interstate

conflicts, those same measures after 1950 are associated with the use of higher hostility

during interstate conflicts. Figure 6.2, below, graphically illustrates the effect of trade

dependence (using the weighted index of trade dependence) on types of conflict hostility,

drawing on the Oneal and Russett data.

Figure 6.2: Probabilities for Highest Level of Hostility for Different Values of Weighted Index of Trade Dependence. (Oneal and Russett Data, 1885 to 1949)
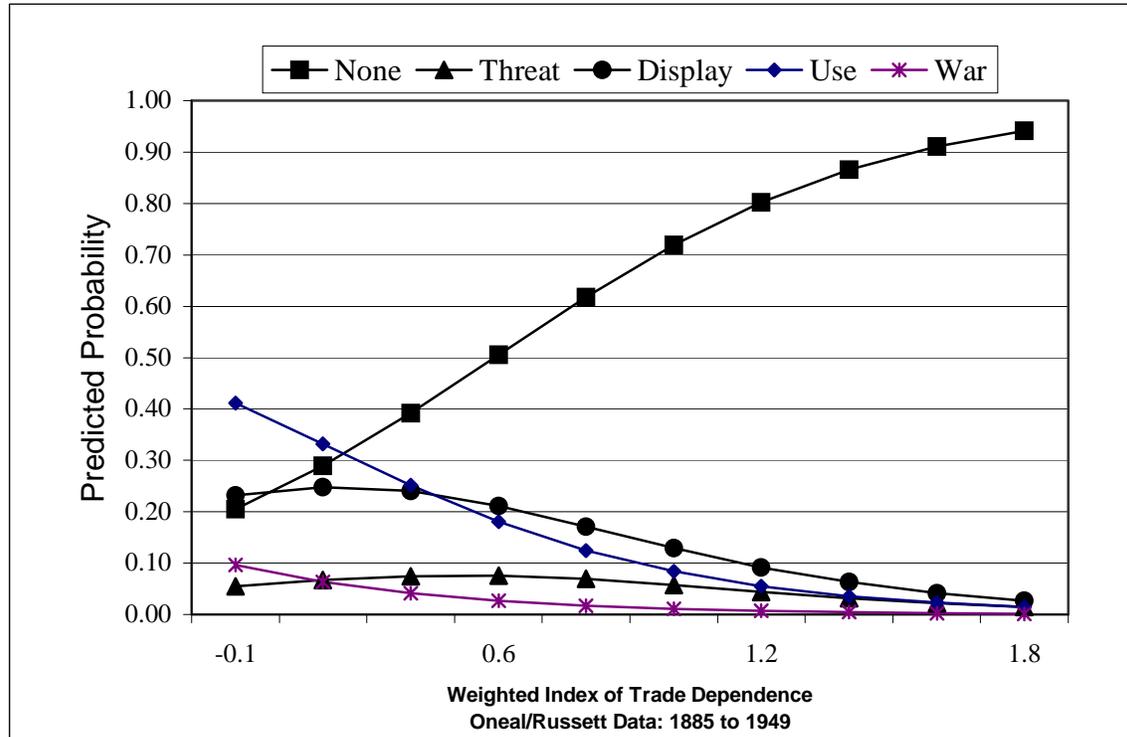
At very low levels of the weighted index, use of force is approximately 40%

likely, the most probable form of hostility used by the primary state against the partner.

Display of force and no action are equally likely (about a 20% chance for either), and war

and threat are both about 10% likely. As the value of the weighted index climbs, the

probability of all forms of hostility fall, and the odds of the primary state taking no action

climb significantly. At the very highest levels of the weighted index (many times the

standard deviation away from the mean value – in other words, highly unlikely, but still

possible), the chance of the primary state taking any action besides none drops to almost

zero. These results are a textbook example of what the interest-based logic of the liberal

peace argues: as the primary state becomes more and more dependent on the partner

state, it is more and more constrained in the actions it can take for fear of harming the economic relationship. While there is no direct proof that this is the logic driving the primary state's behavior (this study does not attempt to measure or judge the actual decision-making that states do), it is entirely consistent with the broader interest-based theory of the liberal peace.[132]

Given that there was radical change in the nature of the weighted indices pre- and post-1950, the next logical test is to replicate the temporal span analysis from Chapter 5 by breaking the data into 5-year spans to examine if the relationships that are found over the 50 year post-1950 span and 65 year pre-1950 span hold for smaller slices of time. However, there is one complication: the data used in this analysis have many fewer cases than the test of conflict initiation (the Ns for this chapter are never higher than about 2500, and can be as low as just under 600 for the Oneal and Russett data). In addition, while the test of initiation allowed only two categories of responses (initiate or not), there are five categories for the response variable in these tests. In order to get valid ordered logistic regression results, there must be sufficient cases that all five response categories have valid results. With significantly reduced cases, and more data needed to fill the response categories, the results below are more prone to error than the corresponding results in Chapter 5. Nonetheless, in order to get some determination relating to whether the signs of the weighted indices shift at 1950, or at other points within the temporal span, the analysis is necessary. Table 6.6, below, details the Z-score and significance of the respective weighted indices for all the data sets.

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[132] Results for the other (non-trade) independent variables are not discussed here. The coefficients for the independent variables in the pre-1950 analyses differ from those found in the post-1950 analyses. However, the focus of this dissertation is the measures of trade dependence and concentration. An extended discussion of the changes of the effects of the non-trade based independent variables would divert attention from the primary focus of the dissertation.

Table 6.6: Effect of Trade Concentration Index and Trade Dependence Index on Probability of Conflict Hostility, by Five-Year Temporal Spans, Multiple Data Sources, for Conflict Directed-Dyads (Z-Scores and Probability of Significance Reported)

	KSG Conc. Index	KSG Dep. Index	Barb. Conc. Index	Barb. Dep. Index	Russett Conc. Index	Russett Dep. Index
1870-1875	(n/a)	(n/a)	-1.63 (0.10) (N=31)	(n/a)	(n/a)	(n/a)
1876-1880	(n/a)	(n/a)	-0.69 (0.49) (N=39)	(n/a)	(n/a)	(n/a)
1881-1885	(n/a)	(n/a)	-1.55 (0.12)* (N=39)	(n/a)	(n/a)	(n/a)
1886-1890	(n/a)	(n/a)	-1.01 (0.31) (N=42)	(n/a)	-1.43 (0.15)* (N=28)	-0.73 (0.46)* (N=28)
1891-1895	(n/a)	(n/a)	-0.68 (0.50)* (N=42)	(n/a)	-1.33 (0.18)* (N=34)	-1.56 (0.12)* (N=34)
1896-1900	(n/a)	(n/a)	-1.51 (0.13) (N=64)	(n/a)	-1.61 (0.11) (N=50)	-1.49 (0.14) (N=50)
1901-1905	(n/a)	(n/a)	-0.54 (0.59) (N=38)	(n/a)	-0.73 (0.47) (N=42)	-0.23 (0.82) (N=42)
1906-1910	(n/a)	(n/a)	0.96 (0.34) (N=61)	(n/a)	-0.26 (0.80) (N=39)	-0.57 (0.57) (N=39)
1911-1915	(n/a)	(n/a)	-1.77 (0.08) (N=96)	(n/a)	1.05 (0.29) (N=64)	0.34 (0.73) (N=64)
1916-1920	(n/a)	(n/a)	(Insufficient Observations)	(n/a)	(Insufficient Observations)	(Insufficient Observations)
1921-1925	(n/a)	(n/a)	0.62 (0.53)* (N=72)	(n/a)	-1.39 (0.16)* (N=67)	-0.69 (0.49)* (N=67)
1926-1930	(n/a)	(n/a)	-0.88 (0.38) (N=32)	(n/a)	-1.34 (0.18)* (N=44)	-1.39 (0.16) (N=48)
1931-1935	(n/a)	(n/a)	-0.90 (0.37)* (N=56)	(n/a)	-1.11 (0.27)* (N=49)	-0.96 (0.34) (N=57)
1936-1940	(n/a)	(n/a)	**-3.18 (0.00) (N=137)**	(n/a)	**-2.93 (0.00) (N=137)**	-1.31 (0.19) (N=137)
1941-1945	(n/a)	(n/a)	(Insufficient Observations)	(n/a)	(Insufficient Observations)	(Insufficient Observations)
1946-1950	(n/a)	(n/a)	(Insufficient Observations)	(n/a)	(Insufficient Observations)	(Insufficient Observations)
1951-1955	1.88 (0.06) (N=214)	0.71 (0.48) (N=241)	**2.03 (0.04) (N=82)**	0.88 (0.38) (N=80)	0.32 (0.75) (N=52)	0.83 (0.41) (N=96)
1956-1960	1.59 (0.11) (N=304)	0.77 (0.44) (N=304)	1.89 (0.06)* (N=175)	0.99 (0.32)* (N=159)	**2.07 (0.04)* (N=127)**	0.15 (0.88)* (N=194)
1961-1965	0.71 (0.48) (N=295)	1.03 (0.30) (N=295)	**2.08 (0.04) (N=187)**	**2.50 (0.01) (N=166)**	0.58 (0.56) (N=187)	0.70 (0.48) (N=231)
1966-1970	-1.55 (0.12) (N=236)	-1.25 (0.21) (N=236)	1.69 (0.09) (N=125)	1.46 (0.14) (N=121)	0.36 (0.72) (N=150)	0.57 (0.57) (N=161)
1971-1975	1.44 (0.15) (N=241)	0.05 (0.96) (N=241)	1.73 (0.08) (N=156)	1.23 (0.22) (N=156)	-0.51 (0.61) (N=134)	0.09 (0.93) (N=170)
1976-1980	**3.11 (0.00) (N=305)**	0.38 (0.71) (N=305)	**2.87 (0.00) (N=205)**	1.90 (0.06) (N=207)	1.81 (0.07) (N=174)	**2.15 (0.03) (N=201)**
1981-1985	0.65 (0.52) (N=307)	1.07 (0.29) (N=307)	0.10 (0.92) (N=243)	-0.65 (0.52) (N=241)	0.24 (0.81) (N=197)	0.17 (0.86) (N=221)
1986-1990	-0.87 (0.39) (N=268)	0.01 (0.99) (N=268)	-0.02 (0.99) (N=210)	-0.54 (0.59) (N=204)	-1.13 (0.26)* (N=156)	-0.79 (0.43) (N=170)
1991-1995	1.86 (0.06) (N=354)	-0.70 (0.48) (N=354)	1.56 (0.12) (N=96; 1991,1992)	1.27 (0.20) (N=96; 1991, 1992)	(Insufficient Observations)	(Insufficient Observations)
1996-2000	0.17 (0.87) (N=336)	0.79 (0.43) (N=336)	(n/a)	(n/a)	(n/a)	(n/a)

(NOTE: Cells with an (*) indicate results were obtained from the logistic regression, but there were problems with the equation: collinearity, dropped variables due to perfect predictions, or missing data that prevented a complete result. Treat these results with caution.)

While few of the pre-1950 cells, with either the early Barbieri or Oneal and Russett data, contain statistically significant coefficients, there is an almost uniform negative association between the weighted indices of trade and increased interstate conflict hostility. In other words, the results of the tests over the 65-year span (Table 6.5) that argued for a general result seem to be accurate: there is no "clump" of cases, in the pre-1950 era, that drives the general results. Of the 16 five-year observation periods observed with the Barbieri data pre-1950, only two report positive associations (though not significantly). Of the twenty five-year observation periods observed with both weighted indices of the Oneal and Russett data, 18 are negative and only two positive (again, not significantly).

The same generalizable result in the post-1950 era exists. The empirical results reported in Tables 6.1 and 6.3 are consistent with the argument that (in general) there is a strong and positive correlation between greater trade imbalance and higher hostility post-1950. In Table 6.6, looking at the results based on the Gleditsch data, only one of 20 5-year test periods produced a statistically significant result (1976 – 1980, positive for the weighted index of trade concentration), but of the 20 test periods, 16 were positive and only four negative (none significantly). Of the eighteen five-year periods post-1950 containing measures constructed with the Barbieri data, sixteen are positive (four significantly) while only two are negative (not significantly). Of the 16 post-1950 Oneal and Russett five-year periods, only three produce negative Z-scores, and thirteen positive (two significantly).

These results, unlike those in Chapter 5 that seemed to have "clumps" of positive and negative associations over time, suggest that 1950 may represent a "break" in the

relationship between unbalanced trade and increased hostility in interstate conflict. While this is a very tentative conclusion, as few of the coefficients reported in Table 6 are statistically significant, what evidence exists does point in this direction. As was mentioned before, 1950 was the date at which large volumes of GDP series data began to be recorded, so few researchers have pushed back before that date because of data availability issues. By happenstance, however, 1950 seems to be a time when the relationship between these variables did change.

Why that should be the case is beyond the scope of this dissertation, though one can speculate. Clearly, around 1950 there was a fairly seismic shift in the nature of the international system: the Euro-centric multi-polar world was giving way to a more global bi-polar world. In addition, with the introduction of the United Nations and (perhaps more importantly) the International Monetary Fund and World Bank, trade itself became both more important to states, and more important as a political issue between states. Given a focus on trade as a significant political issue in this newly politicized bi-polar world, perhaps states (using the signaling-logic that the post-1950 evidence indicates was operating) began to see (and use) trade relations as ways to communicate – to signal to each other. Hence, trade began to be part of conflict issues as it was not before.

This, however, is all speculation. The preceding analysis seems to show that, unlike the test of initiation, there is no "clump" of years that drives either the pre- or post-1950 general results. Through coincidence, 1950 seems to be a date when both the data availability as well as the relationship between unbalanced trade and interstate conflict changed.

6.5 "Cutting" The Data in Additional Ways

Following the pattern established in the previous chapter, it is worth examining the results here more closely, and trying to determine if there are additional interactions between other independent variables and the weighted indices. The goal with this further analysis is to use the same ordered logistic regression on smaller groups of the conflict sample, to see if different types of states behave differently than all the states in the conflict sample together.

Following the progression from the previous chapter, the first test is one of whether the power status (major/minor) of the primary state has an effect on the relationship between unbalanced trade and conflict hostility. Next is region, followed by democracy, alliance and wealth.

It is worth noting two caveats to the procedure, however. One, as discussed above, the conflict sample examined here is significantly smaller than the overall global populations tested in Chapter 5. Some analyses involve very small sub-groups, and a complete ordered logistic regression is not possible. Where this occurs, I will clearly note the statistical problems. Second, given the previous section's results that indicate a bias in the Gleditsch data for the conflict sample, both the Gleditsch and Barbieri data will be tested to see if there are discrepancies. We should have more confidence in results from the Barbieri data, but the data set is smaller in both temporal span (it stops in 1992 whereas Gleditsch's data run through 2000) and number of cases.

As was discussed in Chapter 5, there are no theoretical reasons to expect that either power status or region should have an effect on the general relationship between unbalanced trade and interstate conflict. Nothing in either theory of the liberal peace

argues that either variable should influence the fundamental interest-based or signaling-based logics by which states connect trade to conflict. Nor have other liberal peace researchers made theoretical arguments about any effects. Thus, no preliminary hypothesis can be created for the empirical tests.

Table 6.7: Summary of Effects of Weighted Indices on Interstate Conflict Hostility by Region and Power Status for Conflict Directed-Dyads.
(Post-1950: Gleditsch and Barbieri Data)

	Gleditsch Data	Gleditsch Data	Barbieri Data	Barbieri Data
	Trade Concentration Index	Trade Dependence Index	Trade Concentration Index	Trade Dependence Index
Major Powers	-0.90 (0.37) (N=601)	**-2.02 (0.04)** **(N=601)**	-1.02 (0.31) (N=286)	**-2.32 (0.02)** **(N=281)**
Minor Powers	**2.81 (0.01)** **(N=2286)**	1.33 (0.19) (N=2286)	**4.41 (0.00)** **(N=1205)**	**3.10 (0.00)** **(N=1161)**
America	**3.33 (0.00)** **(N=368)**	1.12 (0.26) (N=368)	**3.78 (0.00)** **(N=299)**	**2.79 (0.01)** **(N=297)**
Europe	-0.23 (0.82) (N=522)	-1.74 (0.08) (N=522)	0.61 (0.54) (N=324)	1.06 (0.29) (N=322)
Africa	-1.60 (0.11) (N=405)	-0.61 (0.54) (N=405)	-0.70 (0.49) (N=282)	-0.85 (0.39) (N=281)
Middle East	**2.52 (0.01)** **(N=467)**	1.73 (0.08) (N=467)	**2.10 (0.04)** **(N=292)**	0.70 (0.49) (N=280)
Asia	-0.57 (0.57) (N=591)	0.51 (0.61) (N=591)	**1.97 (0.05)** **(N=294)**	1.54 (0.12) (N=262)

Analysis in the previous section (and Appendix 1) determined that there was a "clump" of poorer states that use high levels of hostility against the partner state. Thus, it is no surprise when the major/minor power empirical tests reveal that minor powers are significantly more likely to use higher levels of hostility in ongoing interstate conflicts (regardless of the weighted index employed[133]) and major powers are likely to use lower levels of hostility within international conflicts (at least by the weighted index of trade dependence; the weighted index of trade concentration was negative as well, but not

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[133] The weighted index of trade dependence created with the Gleditsch index does not produce a statistically significant coefficient, but as the last section demonstrated there is a bias in this variable that artificially suppresses the values for smaller states. Since this test examines minor versus major states, the variable can be somewhat discounted for this test.

statistically significant). While the result that minor powers use greater levels of hostility

is not a surprise (given the previous sections results, and Appendix 1), the finding that

major powers use lower levels of hostility is new, though the result is only true for the

weighted index of trade dependence (not trade concentration).

With respect to regional differences, the Americas are clearly a region in which

trade dependence and trade concentration beget greater hostility: by both weighted

indices,[134] states in North and South America are significantly more likely to use higher

levels of hostility than other regions. There is no known theory or historical explanation

for this result, but the empirical results are clear. Other regions have mixed results; both

Barbieri and Gleditsch's weighted index of trade concentration are positive and

significant in the Middle East (but not the weighted index of trade dependence), and the

Barbieri-based index of trade concentration produces some association in Asia. Nowhere

in these regional groupings do we find a statistically negative association between the

indices of trade dependence/concentration and the level of hostility. Unlike the test of

initiation (where for Africa and the Middle East the indices of trade dependence and

concentration were positive and significantly related to higher levels of hostility, trade

dependence and concentration does not generate higher levels of hostility in Africa (the

coefficients are negative, though not significant). The association in the Middle East

remains positive. Given the lack of theory guiding these tests, it is difficult to form

conclusions both within these regional tests, or comparing these results to the regional

tests in the initiation chapter.

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[134] Again, we can discount the Gleditsch weighted index of trade dependence since the variable is biased
against poor states. The coefficient for index is positive, but only significant at a 0.25 level.

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Unlike for the previous two tests, initial hypothesis, drawn from previous empirical work on the liberal peace, exists for the next set of tests on democracy, alliance, and wealth. Other liberal peace studies have argued that each of these variables should influence the relationship of trade to interstate conflict.

Rich/Poor: Wealthy states should be less likely than poor states to use higher levels of hostility in conflict with trade partners.
Alliance: States tied together through security alliances should be less likely than states that do not have security ties to use higher levels of hostility in conflict with trade partners.
Democracy: Democracies should be less likely than other forms of government to use higher levels of hostility in conflict against trade partners.

Given these initial hypotheses, there are expectations about what the results of the empirical tests should be. Table 6.8 summarizes in a single table the effects of the indices when the conflict sample is broken down by wealth, alliance, and democracy:

Table 6.8: Effect of Trade-based Weighted Indices on Probability of Interstate Conflict Hostility by Wealth, Alliance and Democratic Status of Primary State for Conflict Directed-Dyads.
(1950+; Gleditsch and Barbieri data, Z-Scores and significant reported)

		Gleditsch Data		Barbieri Data	
		Trade Concentration Index	Trade Dependence Index	Trade Concentration Index	Trade Dependence Index
Wealth	Poor States	-0.38 (0.71) (N=583)	-0.70 (0.49) (N=583)	1.49 (0.14) (N=351)	1.65 (0.10) (N=340)
	Middle-Income States	**3.59 (0.00) (N=1297)**	**2.99 (0.00) (N=1297)**	**3.65 (0.00) (N=842)**	**2.47 (0.01) (N=838)**
	Rich States	0.37 (0.71) (N=473)	0.43 (0.67) (N=473)	1.78 (0.08) (N=298)	0.31 (0.76) (N=264)
Alliances	Security Alliance	**2.21 (0.02) (N=556)**	-0.24 (0.81) (N=556)	**3.04 (0.00) (N=390)**	**2.03 (0.04) (N=380)**
	No Security Alliance	-0.29 (0.77) (N=2331)	1.14 (0.26) (N=2331)	**2.17 (0.03) (N=1101)**	1.13 (0.26) (N=1062)
Democracy	Democracy	1.02 (0.31) (N=835)	0.69 (0.49) (N=835)	**2.06 (0.04) (N=473)**	1.31 (0.19) (N=468)
	Anocracy	**3.65 (0.00) (N=807)**	1.43 (0.15) (N=807)	**4.45 (0.00) (N=389)**	**3.55 (0.00) (N=365)**
	Autocracy	0.01 (0.99) (N=1245)	1.10 (0.27) (N=1245)	**2.07 (0.04) (N=629)**	1.43 (0.15) (N=609)

The empirical results indicate that not a single sub-group was *less* likely to use greater hostility in interstate conflicts as a result of higher imbalances in trade with the trading partner, though there are several groups of states that are *more* likely to use greater hostility when trade is more imbalanced. With respect to wealth, rich states are not less likely than other economic types to use higher levels of hostility in interstate conflicts – middle-income states are more likely to use higher levels of hostility in interstate conflicts compared to poor and wealthy states. This result is somewhat surprising given the previous sections analysis that uncovered the "clump" of smaller states with high levels of hostility; the expectation here would be that poor states would be more hostile. A possible explanation for this is that the middle-income category contains a large number of states (almost 60% of all cases), and thus has a wide range of income within the large grouping (i.e., there could be very hostile, relatively poor states that are classified as middle-income, not poor), though why this should hold for tests of hostility, and not initiation, is unclear. It is somewhat more surprising to find rich states have no negative relationship between unbalanced trade and conflict hostility (not even statistically insignificant results).

Alliances, however, behave exactly opposite as the hypothesized predictions. States with tight security alliances and highly unbalanced trade are at higher risk of any subsequent interstate conflict being highly hostile (true in three of four tests; the Gleditsch trade dependence index is the exception). States with no alliance and highly unbalanced trade with the partner have no reduction in hostility (in fact, the Barbieri trade concentration index finds it also increases hostility, but is the only test of four to do so). It seems that only states with tighter security ties seem to create the possibilities of more

violent conflict. This result, while contrary to the hypothesis, does seem in some way to be consistent with the signaling logic: much like trading relationships, political ties create the ability for states to make costly, credible statements by initiating or escalating interstate conflicts to "prove" their increased capabilities or resolve about the issues surrounding the conflict. While further examination of this result is beyond the scope of the dissertation, it is worth noting the potential link between this and the general results of the dissertation thus far in terms of the signaling logic.

The results of the tests for democracy are also not what were hypothesized, though not to the extent that the alliance results found. Anocracies (neither democracies nor autocracies) with unbalanced trade are significantly more likely to use higher levels of hostility than either democracies or autocracies with unbalanced trade. Three of the four tests were positive and significant (and the exception was the biased Gleditsch index of trade dependence, which was positive but not significant). While Barbieri's weighted index of trade concentration found positive and significant results with both democracies and autocracies, none of the other variables were significant. Thus, while democracy does not reduce hostility, being an anocracy increases hostility. Interestingly, autocracies are no more likely than democracies to either use more or less hostility. While the democracy tests do not fully endorse the initial hypothesis, much like the test of wealth, the results are not inconsistent with the initial hypothesis.

6.6 Conclusions

The results from this chapter, combined with the previous chapter, can be discussed in three ways: first, what these results mean for the core of this dissertation: interests versus signaling; second, what these results mean for the "boundary conditions"

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of the liberal peace; and third, what light they shed on the Gleditsch versus Barbieri data controversy.

With respect to the primary goal of this dissertation, in the post-1950 era there is support for the signaling-based explanations of the liberal peace and no support for the interest-based explanations. The empirical results for the pre-1950 temporal span are more in favor of interests over signaling, though that result is wholly because of the tests of hostility (the results of this chapter): no evidence was found for any association between trade and interstate conflict initiation in the pre-1950 era.

Post-1950, looking only at the results for tests of conflict initiation, there is no compelling argument in favor of signaling as the explanation for the liberal peace, though the preponderance of evidence points in that direction. While seven of eight tests from the previous chapter had a positive and significant association between the measures of unbalanced trade and interstate conflict initiation, subsequent analysis (Section 5.5, and some of Section 5.6) mitigated much of those results.

Tables 6.1 and 6.3 of this chapter provide almost as compelling a result: three of the four tests using both measures of the imbalance in trade found a significant and positive association between unbalanced trade and higher levels of interstate conflict hostility. Moreover, this result rises to three of three tests if one accepts that the Gleditsch GDP data (taken from the Penn World Tables) has a bias among the conflict sample that systematically under represents the GDP of the smaller/poorer states compared to their trade volumes. This bias suppresses the trade dependency values and the weighted index of trade dependence, resulting in empirical tests that find no significance (where the Barbieri tests, using unbiased data, find significance). Thus, I

have argued, you can largely ignore the Gleditsch results using the weighted index of trade dependence in favor of the Barbieri results, even though Barbieri's number of cases is less than half the number that Gleditsch has. In other words, there is much stronger evidence in favor of signaling over interests in the tests of this chapter that look to interstate conflict hostility rather than tests of initiation.

Given these results, one can confidently argue that there is reasonable evidence that states behave *as if* they are using the signaling logic, not the interest-based logic of the liberal peace. The "*as if*" part of the conclusion is critical, as none of these tests looks for specific evidence of signaling on the part of any state. These conclusions rest solely on the indirect evidence of states acting as signaling predicts they should. Nonetheless, the empirical results in the post-1950 era are strongly suggestive of signaling, just as the empirical results in the pre-1950 era are at least suggestive (not as strongly, as the tests of initiation showed no association) of interests.

The overall results have a clear temporal determinant: positive correlations between unbalanced trade and increasing hostility in interstate conflicts (evidence in favor of signaling) are only found in the post-1950 era, while negative correlations (evidence in favor of interests) are only found in the pre-1950 era. There are no exceptions to that rule, making this the strongest finding of these two chapters. This clear bifurcation of results makes a general conclusion more complicated. Of course, the two temporal spans mark two vastly different periods of world history: the earlier marked by multipolarity, colonialism, great power conflicts and global wars; the later by relative international stability in the form of bipolarity, a sharp increase in newly independent states, no direct wars between great powers, though many proxy wars. Which (if any) of

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these global changes is responsible for the radical change in correlation between unbalanced trade and the level of conflict hostility (which is not reflected in the tests of initiation) is unclear, and beyond the scope of this dissertation. The findings for the post-1950 era are inherently more important than the historical facts of a previous time, for policy reasons if not academic ones (the behavior of states today is of more policy relevance than the historical behavior of states). The conclusion from the empirical analyses undertaken in this chapter is that the that the preponderance of evidence favors signaling rather than interests as an explanation for the level of hostility in interstate conflict, even if there is some evidence not fully supportive of this conclusion.

As noted, there are additional, secondary, goals in the dissertation. If one rejects the logic and framework of the tests of interests versus signaling, still this dissertation casts important light on the limits of trade to bring peace, the "boundary conditions" where the liberal peace operates differently from the universally pacific mode in which most assume. The results of this chapter are somewhat troubling for the liberal peace. While these results are not directly comparable to previous liberal peace studies (which have almost uniformly found evidence in favor of the liberal peace; see Chapter 3 for specific discussion), they put boundaries on the idea that trade is a universal good. Unbalanced trade is more likely, these results indicate, to result in higher levels of hostility against the partner in conflicts that break out compared to states that have more balanced trading portfolios. Interstate conflict between trading partners may be (as other liberal peace studies have found) less likely than for states that do not trade, but if a conflict does arise, it is the state that has a more unbalanced trade with the partner that is more likely to be the one who uses higher levels of violence during the conflict. This

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finding is surprising given the assumptions that other researchers have made about the

inherent benefits of international trade. Note, however, in a historical sense that the

opposite is true of the pre-1950 era: unbalanced trade contributed to lower levels of

hostility by the state in an ongoing interstate conflict.

Beyond that finding, the results of Section 6.5 of this chapter add detail to the

picture of what unbalanced trade does to the hostility level that states use. As noted, in a

regional variation (Table 6.7) that is perplexing, states from North and South America

that are in unbalanced economic relationships are significantly more likely to use higher

levels of force than are states from other regions (might this result be driven by the

presence of the US in these regions?). Findings reported in Table 6.8 suggest that

middle-income states, states with security/defense ties to the partner state, and anocracies

(in all cases where the economic relationships are unbalanced) are more likely to use

higher levels of force than are low or high income states, states without security ties to

the partner state, or democracies/autocracies. Moreover, the results from Chapter 5

argued that autocracies were more likely to initiate interstate conflicts than anocracies or

democracies – here, only anocracies have elevated levels of hostility. Again, comparing

to Chapter 5, a lack of security ties raised the probability of unbalanced trade leading to

an interstate conflict: here security ties and unbalanced trade raises the level of hostility

in an ongoing conflict. These "boundary conditions" continue to argue that trade is not

universally pacific.

Clearly the conclusion is not to cease international trade, but it should be more

closely examined for further "boundary conditions" (Mansfield and Pollins, 2002) that

put limits on the pacific effects of trade. As the states of the world move more and more

towards increased economic ties it is important for policy analysts and politicians to understand what those ties entail. The results of this chapter indicate that increasing trade has the potential to increase the level of hostility in interstate disputes. Trade may well, as other research has shown, impede the initiation of interstate conflict, but it also may increase the level of violence that dependent states might employ against states they are dependent upon. This is not a trivial finding.

Finally, and in terms of academic liberal peace studies perhaps the most significant finding, the issues of the differences between the Gleditsch and Barbieri data proved to be of enormous significance. As was noted above, the Gleditsch index of trade dependence was the only measure that failed to find a positive and significant relationship to increased hostility. Diagnostic statistics in this chapter (and Appendix 1) showed clearly that the built-in bias of the "adjusted" GDP of the Penn World Tables that Gleditsch used combined with a "clump" of poor states that used higher levels of hostility led to that result – which the "unbiased" Barbieri measure disagreed with. The tests of initiation found no significant difference between the Gleditsch and the Barbieri data, but this was clearly not the case with the tests in this chapter. Other liberal peace researchers (or other conflict researchers who use this data as a "control" for other tests) need to be careful in the use of these data, and ensure that the biases are clearly understood and not influencing their results. The bias in the Gleditsch data creates a dilemma: the Gleditsch data are biased, but significantly more complete than the Barbieri data (Gleditsch's use of interpolation/extrapolation does not seem to create any bias or validity problems), while the Barbieri data are unbiased, but not nearly as comprehensive.

The next step is to test the results of these last two chapters against some alternative theories of the liberal peace (not the logics of the liberal peace, but actual alternatives to the relationship between trade and interstate conflict that the liberal peace argues for). In this way, we can gain a fuller understanding of the liberal peace, and the limits to its operation.

7.0 Chapter 7: Additional Empirical Analysis - Related Tests That Seek to Determine the Validity of Chapter 5 and 6 Tests

7.1 Introduction

The purpose of this chapter is to describe additional tests that were done to determine if the empirical findings in Chapter 5 and 6 are robust: to see if the effects of the measures of unbalanced trade on dispute initiation and hostility are unaltered when exposed to contending explanations. The contending explanations discussed in this chapter are general theories of the relationship between trade and conflict, not always motivated by the liberal peace. I will concentrate on arguments offered by Copeland and Rosecrance. While Copeland's work (1996) is directly associated with liberal peace studies, his theories have not generally been empirically tested in other studies of the liberal peace. Rosecrance's (1986) "trading states" theory pre-dates most of the empirical work on the liberal peace, and has only recently been incorporated into liberal peace studies. Below, I bring these two perspectives on trade and conflict into the discussion of unbalanced trade.

This chapter proceeds by examining, first, the Copeland (1996) "trends" idea, and testing trends in unbalanced trade first on conflict initiation and then conflict hostility. Both the Gleditsch and Barbieri data are used for the post-1950 span, then the Oneal and Russett data for the pre-1950 span. The next Section of the chapter introduces Rosecrance's "trading states" theory, and tests that against unbalanced trade. Again, Gleditsch and Barbieri data are used post-1950, and Oneal and Russett data pre-1950. The final section of the chapter examines the adjacent levels of hostility (rather than in a

single analysis, as was done in Chapter 6) in an effort to determine if both signaling and interests are taking place within the larger set of all conflict hostility.

7.2 Effect of Trend in Trade on Militarized Interstate Disputes

Copeland (1996) argues that there is an effect of trade on interstate violence, but it is not the effect that the peace research community normally explores. He posits that decision-makers for states do not operate in a vacuum. They are aware not only of immediate circumstances, but of general trends. Specifically, decision-makers pay less attention to the current volume of trade between economic partners, than to the general trend of economic relations between the states. He notes that empirical tests of the liberal peace generally operationalize trade between partners by examining the volume of trade in a single year (as this dissertation does), and argues that a single snapshot is insufficient to indicate either the normal trading relationship between states or the trend in the relationship. A small volume of trade between economic partners in one year is normally interpreted in liberal peace studies as indicating little reason for the state to be pacific.[135] Copeland argues that a small volume of trade in a given year may be irrelevant if the general trend in trade between the partners is growing. If trade is small, but growing strongly, decision-makers would be more strongly influenced by the potential future benefits of the improving economic partnership than by the immediate volume of trade.

Of course, Copeland's argument works in the opposite direction as well. A moderate, or even high, volume of trade may mask a general downward trend in

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[135] This would be true for either the interest-based or signaling-based logics of the liberal peace. The interest-based logic would argue that a small volume of trade could not hurt the overall economy, and hence would create no interest-based incentive to avoid ending the trade. The signaling-based logic would argue that the presence of a small volume of trade would not be sufficient to generate a credible signal of resolve or capability as the volume of trade would simply be too small.

economic ties between states. Leaders deciding questions of interstate conflict may give more credence to the diminishing value of the economic relationship and, thus, be less restrained *vis-à-vis* avoiding violence as a means to resolve disputes. Copeland implicitly criticizes all liberal peace studies for being insufficiently "contextual" -- for not looking beyond a given year's statistics.

Copeland is clearly of the interest-based logic school of the liberal peace, but the argument he lays out is relevant to both interest-based and signaling explanations. The results from Chapters 5 and 6 are dependent on measures of the degree of unbalanced trade between states in directed-dyads based on single-year observations – just Copeland's criticism. It is possible that the results from those chapters are accurate and the conclusions based on them (a preponderance of evidence in favor of signaling over interests) are sound, but if the trend in trade for the partners was examined, the results could radically change (a positive, favorable trend in trade balances could lead to decreased likelihood of initiation and/or hostility, evidence in favor of interests).

While Copeland's logic is couched in terms of simple trade dependence (not the weighted indices looking at levels of imbalance that have been used in Chapters 5 and 6), the same logic would hold in the case of trends in unbalanced trade. For the interest-based logic, a decreasing imbalance (a trend in the imbalance that is moving towards equal levels of trade for the states in the dyad) means that the relative importance of trade for the state is declining, and the relative decrease in "dependence" means the state has less interest in maintaining the relationship; the state is, thus, less restrained with respect to interstate conflict. By the same token, an increasing imbalance (a trend in the trade

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imbalance moving to make the primary state more dependent on the partner in the future) provides just the opposite incentive.

With respect to the signaling-logic of the liberal peace, positive trends will enhance the ability of the primary state to signal (an increasing trend means the state is risking a potentially valuable present – or future – trading relation, which makes statements more credible), while negative trends will reduce the credibility of the signal (a decline trading relationship is not "worth" as much, as the trend over time makes it less valuable; thus, statements risking the end of trade are less "credible" because of the declining importance of the trading relationship). The interest-based logic and signaling-based logic continue to predict opposite empirical outcomes for the case of trends, as they did for the case of unbalanced trading relations themselves.

In order to test the validity of the results from the earlier analysis, tests of the trend in trade concentration/dependence are undertaken. These tests will be identical to the ones in Chapter 5 and 6, except the weighted indices (constructed from a single year's data) will be replaced with variables that measure the trend in the weighted index (using several years worth of data). In other words, these tests will be the same as the ones in Chapters 5 and 6, but will use an independent variable that looks at the trend in unbalanced trade.

A basic issue to address is the number of years to consider when constructing a trend line for interstate trade. The lengthier the interval, the less volatile the trend line. On the other hand, the lengthier the interval, the fewer observations we have with which to work. For example, Gleditsch's data on GDP begin in 1950. If I were to adopt an interval of ten years to construct a trend line, the first disputes available to analyze would

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be those starting in 1961 (ten years of data to create a trend means the first available year to test would be the eleventh year of data). A decade of dispute data would have to be ignored. Thus, the volatility (and validity) of the trend indicator must be weighed against the loss of observations.

For our analysis, a five-year interval will be employed. Not only does it address concerns about loss of data in what is already a brief post-1950 data series, it also seems to be a reasonable conceptualization of what decision-makers might consider when making policy choices. In most democratic countries, the chief executive must go before the public about every three to five years for election/re-election. Additionally, politics beyond a few years ago seems to fall over into "ancient history". Thus, five years worth of trade trends is all any chief executive is likely to be concerned with. Additionally, the five-year temporal span is the same period used for testing the effects of temporal spans in both Chapters 5 and 6; using a similar temporal span here is nicely symmetric.

As discussed in Chapter 4, the economic trade data are available on a yearly basis for states, so previous years' trade volumes can be compared to "current" year trade volumes in order to try and quantify the trend in trade. To create the measure, I subtracted the value of the economic trade variable at (t-4) from the value of the variable at time (t), and divided the result by the value of the variable at time (t-4).[136] This creates a measure of the change in the value of the economic trade variable compared to five years earlier. I repeat this procedure for years (t-3), (t-2), and (t-1). This provides me with four separate measures of the change in the trade variable compared to five, four, three, two and one year earlier. If I average these four measures, the resulting value is a

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[136] Time (t) is the base year at which all independent variables were measured for the results in Chapters 5 and 6. Dependent variables are measured at year (t+1) – the subsequent year to when the values for the independent variables are measured in order to assure causality.

reasonable measure of the net change in the economic trade variable over a short-term

trend. By averaging the change over four past years (rather than just using a single value,

for example the value of the change compared to five years earlier) the measure provides

a more valid approximation of the trend in trade (single year observations, as Copeland

argues, are vulnerable to temporary shifts). The end result is a unitless average

percentage change in the trade-based economic variable (trade concentration, trade

dependence, and weighted index for both). Identical tests to Chapters 5 and 6 (tests of

initiation and tests of hostility at different temporal spans) are undertaken. While the

trends in the trade concentration and trade dependence are created, as with the tests in

Chapters 5 and 6, those variables are not directly used here (trade concentration and trade

dependence are necessary to create the weighted indices).

The results of the tests of initiation and hostility show no connection between any

weighted index of trade concentration or weighted index of trade dependence and the

state's probability of initiating a militarized interstate dispute or using any higher or

lower level of hostility in an ongoing militarized conflict.[137] This lack of results provides

no evidence for or against the interest-based or signaling-based explanations of the liberal

peace theories, but is troubling for Copeland's "trend" thesis. Overall, the trend in trade

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[137] The derived trend weighted indices were never significant over 12 tests of initiation: four tests with the
Gleditsch (1950 – 2000) data (a test with each of the weighted indices for both the "restricted" and "broad"
set of initiations), four tests with the Barbieri (1950 – 1992) data (a test with each of the weighted indices
for both "restricted" and "broad" initiations), and four tests with the Oneal and Russett (1885 – 1949) data
(a test with each of the weighted indices for both the "restricted" and "broad" initiations, both pre-1950).
Six tests of hostility were done (each weighted index for Gleditsch 1950 to 2000; for Barbieri 1950 – 1992;
for Oneal and Russett 1885 – 1949): no statistically significant association was found with the Copeland
trend variables for any test. All tests of initiation and hostility were then re-tested, now including the
single-year weighted index measures (i.e., the tests were re-run with both the trend weighted indices and
the single-year weighted indices that were used in Chapters 5 and 6), and the trend variable again proved
insignificant in any test. Finally, the tests were run again, this time incorporating the trend in trade
dependence and trend in trade concentration variables instead of the trend in weighted index variables.
Again, no significant association was found in any test. Overall, approximately 50 separate tests were done
with various forms of trend-based variables. In not a single test was a trend-based variable significant.

is not a significant predictor of the probability of conflict hostility or initiation, and hence offers no support for or against either logic of the liberal peace.

7.3 Effect of Overall Trade on Militarized Interstate Disputes (Trading States Theory)

Another alternative theory to the traditional interest-based liberal peace is Rosecrance's (1986) idea of trading states. The theory argues that states that are more concerned with economic growth (versus military expansion) will commit themselves to policies and practices that encourage other states (not only their trading partners) to trade with them, and avoid policies and practices that discourage that trade. Hence, states caught up in disputes with other states must consider how their actions will be viewed not just by the opposing state, but also by a host of other states around the world that interact with them (or might trade/interact with them in the future). It is not simply a matter of determining whether the opposing state is an economic partner, but how the state's actions in the dispute affect the economic relations the state has with the entire world. States that are perceived by the world as more likely to use force and violence in disputes are not ones with which other trading states – states that prize predictability and peacefulness in order to achieve higher levels of economic growth – would want to do business.

Thus, primary states that are "trading states" are less likely to initiate militarized interstate disputes or use higher levels of hostility within a militarized interstate dispute no matter what the specific balance or imbalance of the economic relationship with the partner state. In order to avoid being shunned or ostracized by the global trading community, a state would want to avoid using violence because of the signal it sends to

the world. A state that uses force is seen as unreliable or risky to trading partners and potential trading partners, and not a good trading risk.

This theory does not directly contradict the liberal peace. The liberal peace holds that trade between two specific states has a direct effect on the probability of conflict between those states; Rosecrance's "trading states" theory argues that the total trade (with all partners and future partners) by a state affects that state's probability of conflict with all other states. The liberal peace is a dyadic argument (a theory about the relationship between two specific states), while the "trading states" theory is a monadic argument (a theory about a specific characteristic of a single state in relation to many other states). It is possible for both theories to be correct. There could be a specific dyadic effect of trade with a partner that dampens conflict with that partner (the liberal peace), while trade in general dampens a state's propensity to engage in any interstate conflict (the "trading states" theory). However, it is also possible that the effects observed in this dissertation with respect to dyadic trade are conflated with the effects of general trade: states that trade in high volumes with a partner also engage in high volumes of trade with many other states. Given both theories focus on trade and economic interdependence, and the possible correlative issues, an examination of the effect of "trading states" on conflict is warranted. Previous research (Domke, 1988; Hegre, 2000) has generally found a correlation between "trading states" variables and lesser instances of interstate conflict, though those studies have only been performed at the dyadic level and the intent of the researchers was not to directly test the liberal peace against the "trading states" argument.

A recent disagreement[138] between prominent liberal peace theorists has renewed debate over this issue. Gartzke and Li (2003a) examined the question as to why different

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[138] This is first mentioned in Chapter 4, footnote 95.

choices in interdependence measures lead to contradictory results with respect to the liberal peace. While the Oneal and Russett (2001, 2003) research program finds consistently that trade and interstate conflict are inversely related (hence, that there is a liberal peace), Barbieri's (1995, 2002) results have been mixed at best. Gartzke and Li investigate the question of whether construction of the variables that measure interdependence in the two research programs might be decisive in determining why Barbieri's research finds different empirical results than that of Oneal and Russett (and the other researchers who use their data) when both purport to measure the same thing (interdependence). Given the use of both measures (trade concentration and trade dependence) here, the results are worth discussing.

The key, according to Gartzke and Li, is that the two different measures of interdependence are related mathematically through a measure of general overall trade by a state (what has come to be called *openness*: $trade_i / GDP_i$).[139] For our purposes, trade openness is a good operationalization of the concepts that Rosecrance (1986) employs: states that have a high degree of *openness* are states where trade makes up a large degree of their overall economic activity, and are much more likely to be dominated by the kind of political and economic concerns that Rosecrance ascribes to "trading states". Hence, the arguments between Gartzke and Li and Barbieri bear directly on the questions in this paper.

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[139] I would disagree with calling this variable "openness", as one could have a high volume of total trade/GDP and still be a "closed" economy in the sense that one has high barriers to trade (for example, Japan is clearly a state that trades a great deal, but has long suffered complaints from trading partners that Japan is not open to foreign traders). This is the operationalization of Rosecrance's "trading states" that I will use, as I feel this measure captures how great a state's depends on other states for economic progress – precisely what Rosecrance's theory describes. Thus, I see this measure as more of an indicator of trading state potential than as a measure of how open an economy is to foreign trade.

Gartzke and Li point out that that there is a mathematical relationship among

trade dependence, *trade concentration* and *openness*:

$$\textit{trade dependence} = \textit{trade concentration} \text{ times } \textit{openness}$$

$$\frac{trade_{ij}}{GDP_i} = \frac{trade_{ij}}{trade_i} \times \frac{trade_i}{GDP_i}$$

Gartzke and Li argue that the reason Barbieri finds results that contradict the

Russett/Oneal program is that measures of interdependence based on trade concentration

are not effective measures of interdependence. They show that as the number of trading

partners increases, the value of trade concentration goes down. While they show this

mathematically, it is also intuitive. Trade concentration measures how much the primary

state's overall trade is tied up with the partner state. If a state has but one or two trading

partners, a measure of trade concentration can be very high (maximum of 100% for a

single trading partner, two equal trading partners would make the measure fall to 50%,

and so on). As a state has more and more trading partners, it is more and more unlikely

that the measure of trade concentration with any one of them is particularly high –

possible, but unlikely. Hence, as a state has more and more trading partners, its measure

of trade concentration with any particular trading partner should fall. The exact opposite

effect occurs with respect to measures of what they call "openness". Gartzke and Li have

mathematical and empirical proof, but intuition again suffices. As states add more and

more trading partners, unless the trade is trivial, then more and more of a states economy

is tied up in trade – and values of openness increase. Hence, trade concentration (what

Gartzke and Li call "trade share") is negatively correlated with openness (Gartzke and Li,

2003a, 559 – 560). Thus, in empirical tests that use the measures, if openness is inversely

correlated with interstate conflict, trade concentration will be positively correlated –

precisely what Barbieri finds.[140]

Since trade dependence is constructed by multiplying trade concentration times

openness (which are negatively correlated), trade dependence does not always provide

robust results in empirical tests. To the degree that openness is positively correlated with

decreased conflict and trade concentration is negatively correlated, trade dependence is

pulled in opposite directions by its constituent components.

Gartzke and Li go on to incorporate the openness measure into empirical tests

originally done by Barbieri and Oneal and Russett. Their intention is to show the effects

of the different measures of interdependence, highlighting the importance of variable

construction. For my purposes, it is enough to note that when a measure of openness is

included in their empirical tests, it is always statistically significant. Gartzke and Li do

not have as the purpose of their article a discussion of the importance of openness in the

liberal peace, but their results (confirmed by Barbieri in her rebuttal, see Barbieri, 2003)

highlight that openness, representing the theoretical arguments of Rosecrance, has a place

in the liberal peace debate. However, the Gartzke and Li study still allows for further

examination of the issue. Gartzke and Li use dyads as the cases for their tests (replicating

the Barbieri and Oneal and Russett studies they are commenting on), and their focus is on

understanding why trade concentration and trade dependence have not found similar

results in other studies. The issue in this dissertation is what effect that openness, taken

as a measure of a trading state, has on the overall results found from the previous

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[140] This explanation differs significantly from the discussion (in Chapters 5, 6 and Appendix 1) of the difference between the Barbieri and Gleditsch data. Gartzke and Li are discussing the difference between trade dependence and trade concentration as operationalizations of the concept "interdependence". My discussion (again, Chapters 5, 6 and Appendix 1) related to the differences between Barbieri's data and Gleditsch's data with regard to measures of trade dependence.

chapters when it is included as a measure. In addition, do the dyadic effects of openness (as Gartzke and Li found) continue when tested on directed-dyads and including a measure of the degree of unbalanced trade (the weighted indices)?

Operationalizing the concept of "trading states" is relatively simple. As was noted above, what the liberal peace research community has standardized on is a measure called "openness" which records for each state how much of their total economic activity is involved with trade with all of that state's trading partners. It is created by taking the total value of all trade by that state (the denominator of the trade concentration variable) and dividing it by the GDP of the state (the denominator of the trade dependence variable). This measure is unitless and, like the weighted indices of trade concentration and trade dependence, comparable across time and across countries. The higher the value of the measure, the more of the state's economy is tied into trade with other states. It is a good operationalization of the theoretical concept of a "trading state." The more of a state's overall economic activity is related to other states, the more care that state must take in its political relations with both those trading partners (for fear of having falling trade that harms the economy) and other states (who are potential trading partners, but more importantly the state does not want to cause any current trading partner to withdraw and must moderate all political actions).

This variable can be created from all three sets of data (Gleditsch: 1950 – 2000; Barbieri: 1950 – 1992; and Oneal and Russett: 1885 - 1949), with the same limitations that existed in earlier chapters: the Oneal and Russett data only include "politically relevant" dyads and are used only for the historical (pre-1950) analysis, and the aforementioned problems between the Gleditsch GDP data and the Barbieri GDP data

(Chapters 5, 6 and Appendix 1).[141] The empirical tests are similar to the previous

chapters (a set of tests of initiation and a set of tests of hostility, broken by temporal

span), with the addition of the openness variable.

Table 7.1, below, presents the results from the logistic regression tests of

interstate conflict initiation for the population of directed-dyads. It is identical in form to

Table 5.1 from Chapter 5, presenting results for both the "restrictive" and "broad" sets of

interstate conflict initiations, and uses only the Gleditsch data. The only difference

between this Table 7.1 and Chapter 5's Table 5.1 is that this table also presents the effect

of the trading state variable (openness). In this table, the top number in each cell

represents, again through the use of Z-scores, the effect of the independent trade variable

(corresponding to the column) on the odds of interstate conflict initiation and the

probability of the variable being significant. The bottom number represents the Z-score

of the trading state variable and probability of its being significant.

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[141] Analysis in previous chapters (Chapters 5, 6, and Appendix 1) discovered a "bias" in the Gleditsch measure of trade dependence (and associated weighted index of trade dependence). The use of "adjusted" GDPs (from the Penn World Tables) in construction of the trade dependence measure makes the measure relatively smaller ("adjusted" GDPs are larger than their unadjusted equivalents) compared to "unadjusted" GDPs. This is especially true for poorer states (with smaller GDPs). In a coincidence (see Appendix 1, Section III), there are a significant number of poorer states that engage in militarized conflicts. Thus, the measures of trade dependence and the weighted index of trade dependence for those critical cases (states participating in MIDs) are correspondingly smaller. This bias was seen in some of the dissimilar results when similar tests were performed with the Gleditsch versus the Barbieri data.

Table 7.2: Effect of "Openness" (Rosecrance's Trading States Theory) on Interstate Conflict Initiation for All Directed-Dyads: Gleditsch Data (1950 – 2000).

(N=808227)		Weighted Trade Concentration Index (N=245826)	Weighted Trade Dependence Index (N=245823)
Restrictive set of initiations	Directed-Dyad Trade Variable (Listed at top of Column)	**2.61** **(0.01)**	**2.21** **(0.03)**
	Trading State Variable (Openness)	**2.84** **(0.00)**	**2.28** **(0.02)**
Broad set of initiations	Directed-Dyad Trade Variable (Listed at top of Column)	0.64 (0.53)	**2.60** **(0.01)**
	Trading State Variable (Openness)	**3.54** **(0.00)**	**2.94** **(0.00)**

Comparing this table to Table 5.1 in Chapter 5 reveals little change in the trade-based directed-dyad variables. The corresponding cells from Chapter 5, Table 5.1 remain significant. Overall, the addition of the trading states variable has little effect on the magnitude and significance of the weighted indices. In other words, the addition of the trading states variable does not change the basic conclusions for this part of the analysis.

Examining the bottom half of each cell clearly shows that there is a strong significant effect for the trading state variable. In every single cell, the coefficient for the trading state variable is significant and positive, indicating that the more that trade is an important component of a state's overall economy, the more likely that state is to initiate interstate conflicts. This is the opposite of the predictions that Rosecrance makes in his hypothesis. The overall significance of trade has a strong effect on the propensity of interstate conflict, but it serves to increase, not decrease, the odds of a conflict beginning. These results are in marked contrast to the effect of the trading state variable when tested with the Barbieri data, as Table 7.2 (below) indicates.

Table 7.2: Effect of "Openness" (Rosecrance's Trading States Theory) on Interstate Conflict Initiation for All Directed-Dyads: Barbieri Data (1950 – 1992).

		Weighted Trade Concentration Index (N=256514)	Weighted Trade Dependence Index (N=254616)
Restrictive set of initiations	Directed-Dyad Trade Variable (Listed at top of Column)	**5.42** **(0.00)**	**4.89** **(0.00)**
	Trading State Variable (Openness)	**-4.56** **(0.00)**	**-5.55** **(0.00)**
Broad set of initiations	Directed-Dyad Trade Variable (Listed at top of Column)	**3.72** **(0.00)**	**3.62** **(0.00)**
	Trading State Variable (Openness)	**-3.95** **(0.00)**	**-4.62** **(0.00)**

Table 7.2 is comparable to Table 5.3 in Chapter 5, with this table including the effects of the trading state variable. The addition of the trading state variable has made starker the fact that results are dependent on the choice of data. As was discussed in Chapter 5, the Gleditsch data are the most complete (with the caveat that some of the data are interpolated, and the GDP figures used to create the data are adjusted, while the trade data are not) in terms of providing the most number of cases.

The effects of the trading state variable, however, are entirely different when tested with the Barbieri data compared to the Gleditsch. Using the Gleditsch data (Table 7.1, above) we find that the more a state is dependent on overall trade, the more likely it is to initiate interstate conflict. The results in Table 7.2 (based on the Barbieri data) suggest precisely the opposite: greater overall trade dependence (i.e., the value of trade divided by GDP) makes a state significantly less likely to initiate interstate conflict.

The starkly different results deriving from the data sets can be explained in one of two ways: the different cases in the two data sets (Gleditsch's 800,000+ cases to

Barbieri's 240,000+) generate very different distributions, and/or the difference between

the "adjusted" and "unadjusted" GDP data (which is critical in creating the measure of

"openness") causes the Gleditsch data to be fundamentally different from the Barbieri

data. There is no way of determining which of these scenarios (or both) causes the

different results without further analysis.

As an initial test, the Gleditsch tests with openness (Table 7.1) are re-tested with

only the cases Barbieri (Table 7.2) used (limiting the test to only those cases with valid,

or non-missing, Barbieri data). If the difference in results is only due to case selection,

then using the Gleditsch data in substitute for the Barbieri data should result in the same

results as the Barbieri tests (Table 7.2). It does not. While no table is presented, the

results of the test are that in all four cells (both weighted indices and restricted versus

broad initiations) the Z-score of the openness measure drops close to zero, with no

significance. In other words, openness becomes neither positively nor negatively

associated with interstate conflict using the Gleditsch data and the (valid) Barbieri cases.

This means that the difference in cases between Gleditsch and Barbieri partially accounts

for some of the change between Table 7.1 and Table 7.2, but also that difference in the

data themselves account for some of the change.

Chapter 5 and 6 (and Appendix 1) demonstrated that the values of the "adjusted"

GDP data are larger than the "unadjusted" GDP data, in particular for small-GDP states.

The "openness" measure is created by dividing the total trade (in a single year) by the

GDP of that year: if the "adjusted" GDP is larger than the "unadjusted" GDP, then the

value of the Gleditsch "openness" measure should be smaller (total trade divided by a

larger GDP) than the Barbieri "openness" measure (the same total trade figure – both are

derived from identical IMF data – divided by a smaller GDP). If the Barbieri "openness" variable is significantly larger than the Gleditsch "openness" variable for those states that initiate interstate conflict, then there is an additional explanation for why Barbieri finds significance and Gleditsch does not (in the same sample). Table 7.3, below, lists the mean value of the openness measure for similar Gleditsch and Barbieri samples for those states that do not initiate an interstate conflict and then for the group that does:

Table 7.3: Comparison of Mean "Openness" Score for Identical Samples of Gleditsch and Barbieri Data (all Directed-Dyads)
(Gleditsch and Barbieri Data; 1950 – 1992)[142]

	Gleditsch Data (N=277193)	Barbieri Data (N=277193)
No Initiation in Subsequent Year	0.221 (N=276520)	0.445 (N=276520)
Initiation in Subsequent Year	0.171 (N=673)	0.311 (N=673)

Recognize that Gleditsch GDP, being larger for smaller states, results in an "openness" measure that is smaller in both categories. However, the significant distinction is the size of the difference between the categories for both data sets. The Gleditsch data has the mean "openness" score for initiating states only 0.05 less than the mean for non-initiating states (a T-test shows this is still a significant difference) while the Barbieri data has the mean "openness" score for initiating states 0.14 less than the mean for non-initiating states (again, a significant difference). When these two variables are used in otherwise identical logistic regressions (Table 7.2 for the Barbieri data; the analysis with the Gleditsch data was discussed but not displayed a page or so ago), the Barbieri-based "openness" measure is negatively and statistically significantly related to conflict initiation, while the Gleditsch-based measure is negative (close to zero for a Z-score), but not statistically significantly related. Table 7.3 demonstrates that the raw

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[142] The table uses only the "restricted" set of initiations. A similar comparison for the "broad" set of initiations was performed with no significant differences. It is omitted for brevity.

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difference between the data results in a much larger divergence for the Barbieri-based measure than the Gleditsch-based measure. Thus, it is possible (likely, I argue) that the logistic regression produces a significant association between more "openness" and less conflict using the Barbieri data, and no association between "openness" and conflict using the Gleditsch data because of this difference in the data.

This digression again highlights the fact that the two data sets used in liberal peace studies contain significant differences in cases and values, and the complexity of the variation between the data sets complicates the ability of researchers to accurately determine the relationships between relevant and theoretically interesting independent variables. In this case, given that the Barbieri data are more accurate (because they are "unadjusted", like the trade data; not because "adjustment" is inherently wrong), the more likely conclusion is that "openness" is negatively associated with interstate conflict initiation (as other researchers have found). However, this conclusion cannot be definitive without a more accurate and expansive set of "unadjusted" GDP data that narrows the gap in cases between the Barbieri data set and the much larger Gleditsch data set.

Results of tests of the trading state variables on data before 1950 are not displayed. There were no significant results for either of the weighted indices or the trading state variables (results similar to Chapter 5).

Overall, at least in the era of the Cold War, it is clear that there is a "trading states" effect on the probability of interstate conflict initiation. What that effect is, however, is less clear. While the trading state variable is mostly significant (in the post

1950 temporal span)[143], it changes sign depending on the source of the data used for the testing. While there is a ready explanation for that result (Gleditsch's use of "adjusted" GDP), the lack of coherence between the cases and data from the two sources does reduce the credibility of the tests.

Perhaps more importantly the addition of the "openness" measure did nothing to change the significance or sign of the measure of unbalanced trade: the results of Chapter 5, tentative as they are, remain consistent when "openness" is added to the overall equation. The tests of initiation, while not definitive, continue to produce some evidence that states act like they are signaling, rather than driven by economic interests.[144]

Results of the tests of hostility are more uniform with respect to the choice of data. Table 7.4, below, presents the results of the ordered logistic regressions using both data sources in the post-1950 era (this table parallels Table 6.1 – for the Gleditsch data – and Table 6.3 – for the Barbieri data - in Chapter 6; it adds to those tables the results for the trading state variable).

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[143] "Openness" was insignificant only when the Gleditsch data were limited to valid Barbieri cases, and those results, I argued, were due to the differences in the actual data between the Gleditsch data set and the Barbieri data set.

[144] I have neglected to discuss the other independent variables. This is deliberate. Adding the trading state variable to the analysis did not significantly alter either the magnitude or statistical significance of the coefficients associated with the other independent variables from what was previously reported (Table 5.2, Chapter 5). For the purposes of this dissertation, and its focus on trade-based measures of interdependence and interstate conflict, this is sufficient.

Table 7.4: Effect of "Openness" (Rosecrance's Trading States Theory) and Unbalanced Trade on Probability of Interstate Conflict Hostility for All Conflict Directed-Dyads.
(Gleditsch and Barbieri Data; 1950+)

		Weighted Trade Concentration Index	Weighted Trade Dependence Index
Gleditsch: 1950 – 2000 (N=2871)	Directed-Dyad Trade Variable (Listed at top of Column)	**2.49** **(0.01)**	**1.99** **(0.05)**
	Trading State Variable (Openness)	**-1.98** **(0.05)**	**-2.60** **(0.01)**
Barbeiri: 1950 - 1992 (N=1455)	Directed-Dyad Trade Variable (Listed at top of Column)	**4.37** **(0.00)**	**3.56** **(0.00)**
	Trading State Variable (Openness)	-1.19 (0.23)	**-1.93** **(0.05)**

The results here are similar, but not identical, to the results for the analysis without the trading state variable (Tables 6.1 and 6.3 in Chapter 6). The only change in sign or significance is that the Gleditsch-based weighted index of trade dependence (insignificant, though positive, in Table 6.1, Chapter 6) is now positive and statistically significant. The Gleditsch-based weighted index of trade concentration, and both Barbieri-based weighted indices, were previously positive and significant, and the addition of the trading states variable does not change that. There is still substantial, but not overwhelming, support for the signaling-based logic of the liberal peace and no support for the interest-based logic.

The coefficient for the trading state variable is statistically significant in three of four tests reported in Table 7.4, with uniform results.[145] The greater the "openness", the lower the probability of the state using high levels of hostility in an ongoing interstate dispute. This is true across both sources of data: Gleditsch and Barbieri. The greater a

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[145] Even in the case of the result that was not significant, the sign of the coefficients is negative.

state trades with all other partners, the greater the probability that in any dispute that does break out (with the trading partners or other states) the state will limit itself to lower levels of hostility.

Rosecrance based his trading state theory on an interest-based model: states will have an incentive to act in more pacific/less conflictual ways because their overall economic performance with all other partners depends on them being perceived as stable and peaceful. Thus, to paraphrase Rosecrance, states have an interest in behaving pacifically as it facilitates their goals of economic growth and prosperity. While little evidence in favor of the interest-based logic for the dyadic liberal peace has been found in the post-1950 era, the results of the tests of hostility (and to the degree that one accepts the Barbieri data over the Gleditsch data, the results of the tests of initiation support this argument as well) find significant evidence in favor of overall state's interests driving their actions with respect to interstate conflicts. There is support for the interest-based logic of the liberal peace, but not at the dyadic level. Trading states, regardless of the volume and significance of trade with the partner state, will act more pacifically. The results here and the results from Chapters 5 and 6 are not necessarily contradictory: "trading states" arguably have (because of their high dependence on trade with many sources) a different view of the relationship between trade and conflict than states paired with single trading partners. Signaling (as shown in the Chapter 5 and 6 results) may allow credible communication to facilitate conflict resolution, but those credible acts (initiating a conflict or using higher levels of hostility) may damage overall trading relations (for "trading states") such that signaling becomes counter-productive (when compared to the potential damage to all other economic relations) for the dyadic

relationship. This, however, is conjecture. Further research in this area would need to examine specific cases to determine how trading states pursue foreign policy in general, and how they manage their potential or actual conflicts specifically. For the purposes of this dissertation, it is sufficient only to note that the addition of the "trading state" variable does not change the overall conclusions of the previous chapters.

The results above for "trading states," however, are only true for the post-1950 era. Table 7.5, below, reports the finding from tests of hostility and the trading state variable for the pre-1950 period. The Oneal and Russett data are used to compute the "openness" measure for the pre-1950 period: neither the Gleditsch nor the Barbieri data set contains the economic data for this earlier time period.

Table 7.5: Effect of "Openness" (Rosecrance's Trading States Theory) and Unbalanced Trade on Probability of Interstate Conflict Hostility.
(Oneal and Russett data; 1885 - 1949)

		Weighted Trade Concentration Index	Weighted Trade Dependence Index
Oneal and Russett: 1885 - 1949 (N=585)	Directed-Dyad Trade Variable (Listed at top of Column)	**-2.92** **(0.00)**	**-2.72** **(0.01)**
	Trading State Variable (Openness)	-1.12 (0.26)	0.08 (0.94)

The results for the weighted indices of trade concentration and trade dependence are the same as the tests that did not include the trading state variable (Table 6.5, Chapter 6) – clear support for the interest-based logic of the liberal peace in that higher levels of unbalanced trade correlate with lower levels of hostility used in interstate conflicts. However, the measure of openness is not statistically significant. This is consistent with the results from the tests of initiation reported previously in this chapter: whatever the

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effects of the trading state variable, they appear to be a relatively recent phenomena, manifesting themselves only in the post-1950 era.

Overall, the Rosecrance's trading states theory has significantly more empirical support than Copeland's trend theory. While the results are only statistically significant for the post-1950 era, it is clear that greater overall trade (as measured as a percentage of the overall economy) has a significant influence on a state's actions with respect to interstate conflict. With respect to the level of conflict hostility, the results are clear and uniform: greater overall trade dependence reduces the likelihood of subsequent interstate conflicts rising to more hostile levels. With respect to initiation, the results are contradictory. Using the Gleditsch data, greater overall trade dependence makes a state more likely to initiate interstate disputes; using the Barbieri data produces precisely the opposite conclusion. To the degree that one accepts the Barbieri data as more accurate because both the trade and GDP data used to generate the measures tested in this dissertation are both "unadjusted" (though the Gleditsch data is more complete), the overall conclusion is that being a trading state makes one less likely to initiate interstate conflicts and less likely to use high levels of hostility in those conflicts. This conclusion finds general support for the Rosecrance theory (being a trading state limits both interstate conflict hostility and conflict initiation).

For the purposes of this dissertations main focus, examining the interest-based and signaling-based logics, the results here do not change the earlier conclusions. The addition of the trading state variable, while significant and interesting in its own right, does not substantially change the results of the previous chapter. There is still support for the operation of the signaling logic in the post-1950 period (and little to no evidence of

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interests operating in that same time) and support for interests operating before that (with no support for signaling in the earlier temporal period) - whether the trading state variable is included in the analysis or not.

7.4 Test of Adjacent Levels of Hostility

One advantage of ordered logistic regression (used in the analyses of the effect of trade-based dependence on different levels of hostility in Chapter 6 and this chapter) is that it permits us to fit a single, nonlinear function across all categories of a limited (i.e., categorical) dependent variable. This allows us to employ in a single analysis all observations on the dependent variable. The technique achieves this by assuming that the effects of the independent variables are consistent across the categories of the dependent variable. The disadvantage of this approach is that we ignore the possibility that the effects may be different from category to category. It is to this concern that I now turn.

The Militarized Interstate Dispute dataset, which serves as the source for the values of the dependent variables examined in this dissertation, assigns action/non-action in militarized disputes as falling into one of five categories (no action, threat, display of force, use of force, and war). Some of the finding reported earlier in this dissertation suggest that greater imbalance in trade is associated with increased probability of higher levels of hostility being used in militarized interstate dispute, results that support the signaling-based logic of the liberal peace. An obvious question to ask is whether this effect is consistent across levels of hostility, or do the effects of the associated weighted indices differ depending on the level of hostility being considered?

To answer this question, I conducted another test of the effects of trade imbalance on the primary state's level of hostility used in the militarized interstate dispute. Instead

of the ordered logistic regression techniques used in Chapter 6 that tested all the observations across all the levels of hostility at the same time, the approach here is to use ordinary logistic regression to test the effects of the trade-based measures of dependence on adjacent levels of hostility. In other words, for the first test I eliminate the three highest levels of hostility (display of force, use of force, and war) and use regular (two-category) logistic regression to run an otherwise identical set of tests (same independent variables) as was done in Chapter 6, but only on the lowest two categories of force (no action and threat of force). The next test is identical, but would eliminate the lowest and two highest levels of hostility (no action, use of force, and war) and find the effects of the independent variables on states choosing between those next adjacent levels (threat of force and display of force). The results of these limited tests indicate what effects (if any) the trade-based measures of dependence have with respect to moving from one level of hostility to the next highest level. Interest-based explanations of the liberal peace would argue that for each separate comparison of adjacent levels, the higher the value of the measures of trade-based dependence, the less likely a state would be to use the higher level of hostility. The signaling-based logic would argue the opposite: that the more dependent a state, the more it would need to choose higher levels of hostility in order to send a stronger, more credible signal. Previous tests (Chapter 6) using ordered logistic regression found that trade-based measures of dependence were correlated with higher levels of hostility when all observations were tested. What if, however, interests dominated at higher levels of hostility (fewer states are willing to risk war even for the chance of sending credible signals – an interest-based logic), but signaling dominated at lower levels (states do see a need to credibly send messages at relatively low cost).

Ordered logistic regression has no provision for examining the effects of the independent variables between categories, just across all the categories at once.

The tests here mimic the tests from Chapter 6 as much as possible: the primary focus is on the Gleditsch and Barbieri data, with the Oneal and Russett data only used for pre-1950 analysis. Given the large number of tests, the tables are of a different format than used in previous sections. Each row represents a different test of adjacent levels (described in the left most column). The only reported results are the effects of the state's two weighted indices on the probability of escalation between the adjacent hostility levels. The other independent variables were included in the test, but their results are not reported in the tables. Tables 7.6, 7.7, and 7.8 report the Z-scores (and probabilities) for measures of unbalanced trade for each adjacent level of hostility in the MID data, with each table representing a different source of data (Table 7.6 uses only Gleditsch data, Table 7.7 only Barbieri, and Table 7.8 only Oneal and Russett in the pre-1950 era).

Table 7.6: Effect of Unbalanced Trade on Probability of Escalation to Adjacent Levels of Hostility for Conflict Directed-Dyads.
(Gleditsch Data; 1950 – 2000)

	Weighted Index of Trade Concentration	Weighted Index of Trade Dependence
"No Action" versus "Threat of Force" (N=792)	0.65 (0.52)	1.04 (0.30)
"Threat of Force" versus "Display of Force" (N=751)	-0.78 (0.43)	-1.43 (0.15)
"Display of Force" versus "Use of Force" (N=1954)	**3.26 (0.00)**	**1.98 (0.05)**
"Use of Force" versus "War" (N=1426)	-1.27 (0.20)	-1.72 (0.09)

Only a single set of comparisons is statistically significant: comparing the "display of force" to "use of force" categories. Given the lack of significance in all other comparisons, it is difficult to make conclusions about whether interests or signaling (or both) are operating at different levels. What is clear is that there is no consistent effect of unbalanced trade across all levels of hostility. Table 7.6 argues that most of the results from Chapter 6 (the general tests of hostility) might be driven only by the differences between two categories (use of force and display of force). Table 7.7, below, replicates Table 7.6, but uses the Barbieri data.

Table 7.7: Effect of Unbalanced Trade on Probability of Escalation to Adjacent Levels of Hostility for Conflict Directed-Dyads.
(Barbieri Data; 1950 – 1992)

	Weighted Index of Trade Concentration	Weighted Index of Trade Dependence
"No Action" versus "Threat of Force" (N=403)	-0.25 (0.80)	0.08 (0.94)
"Threat of Force" versus "Display of Force" (N=365)	0.47 (0.64)	-0.56 (0.58)
"Display of Force" versus "Use of Force" (N=995)	**2.92 (0.00)**	**3.08 (0.00)**
"Use of Force" versus "War" (N=723)	-1.28 (0.20)	-1.20 (0.23)

The Gleditsch and Barbieri (Tables 7.6 and 7.7) results are similar: unbalanced trade is irrelevant at influencing the probabilities of higher levels of conflict except for the third row (the "Display of Force" versus "Use of Force categories – MID numbers 3 and 4) where both weighted indices are significant and positive (the more trade dependent the primary state is or the more concentrated its trade with the partner state, the greater the probability it will actually use force rather than simply display force).

Overall, the results of these tests comparing adjacent levels of hostility, while potentially calling into question the use of ordered logistic regression in Chapter 6, continue to provide evidence in favor of the signaling-based logic of the liberal peace, and no evidence in favor of the interest-based logic, at least in the post-1950 period (pre-1950 results are discussed below). The results here using the Gleditsch and Barbieri data suggest that the Chapter 6 results (which show that unbalanced trade is positively associated with increased hostility) are concentrated in only some of the interstate conflicts: more dependent states are more likely to use force, where less dependent states only display force. Unbalanced or balanced trade has no effect on states at other levels of hostility.

What is clear is that there is no evidence for the interest-based logic. It was theoretically possible for a negative correlation (evidence in favor of the interest-based logic) for one test of adjacent levels to exist at the same time a positive correlation (evidence in favor of signaling, as was found) exists at other levels. If these results had been found (and they were not), it would have opened up the possibility that the general results (Chapter 6) masked a more complicated picture were both signaling and interests operated at different points in the conflict process. This is clearly not the case.

However, the results here do open up the possibility that the previous chapter's results are being driven not by a general correlation between unbalanced trade and higher levels of hostility across all conflicts, but a specific correlation between two adjacent levels. In other words, unbalanced trade and hostility are generally unrelated except for these two adjacent levels of hostility (use of force and display of force). This is still evidence in favor of signaling, but does potentially argue against a general conclusion

that unbalanced trade and conflict hostility are positively associated. This conclusion, however, cannot be positively asserted.

The limitations of these tests are reflected in the smaller size of the samples tested. The results from the Chapter 6 tests of hostility used all the conflict data to look across all the levels of hostility for the effects of trade. These results use smaller subsets of the data to only look at adjacent levels. As such, the Chapter 6 results are a better test of the general effects of unbalanced trade on overall conflict hostility. The results here can provide more evidence for or against the previous conclusions, but are not superior to (or take the place of) the previous tests. These results eliminate the possibility that some aspect of the interest-based logic was "hiding" in the conflict data, but do not conclusively demonstrate that the previous general correlation between trade and conflict hostility are only the result of the interaction of trade and specific levels of hostility.

Table 7.8 reflects the results of the data from the pre-1950 span. Chapter 6 argued that there was general evidence in favor of interests, rather than signaling, in this period, and these results minimally support that conclusion. Like the tests of the post-1950 span, only a single comparison of adjacent levels produces statistically significant results. Consistent with the Chapter 6 results, this single significant comparison shows a negative correlation between both weighted indices and higher levels of adjacent hostility (evidence in favor of the interest-based logic). These results do, as in the previous set, show that there is no evidence of signaling "hiding" in the conflict data: not a single test of adjacent levels generates any statistically significant positive correlation between a trade-based measure of dependence and conflict hostility.

Table 7.8: Effect of Unbalanced Trade on Probability of Escalation to Adjacent Levels of Hostility for Conflict Directed-Dyads.
(Oneal and Russett Data; 1885 – 1949)

	Weighted Index of Trade Concentration	Weighted Index of Trade Dependence
"No Action" versus "Threat of Force" (N=318)	**-2.30** **(0.02)**	**-1.93** **(0.05)**
"Threat of Force" versus "Display of Force" (N=185)	-0.70 (0.49)	-0.91 (0.36)
"Display of Force" versus "Use of Force" (N=300)	1.08 (0.28)	0.45 (0.65)
"Use of Force" versus "War" (N=154)	-1.68 (0.09)	-0.12 (0.90)

Overall, the results for both temporal spans provide no countervailing evidence against, and some additional supporting evidence for, the conclusions from Chapter 6: there is evidence in favor of the signaling-based logic operating since 1950 and the interest-based logic operating before that.

7.5 Conclusions

The results in this chapter both helped and hurt the general conclusions of Chapters 5 and 6. Three alternative theories for relationships between trade and interstate conflict were presented and tested here: Copeland's (1996) argument that states respond to the trend in trade (up or down), not the short-term, single-year measures of trade most researchers use (trade measured the year before the variable that measures conflict); Rosecrance's (1986) conception of "trading states" where states with significant trade ties will seek to avoid conflict with all other states (trading partners or not) in order to avoid harming their trading interests; and an argument that the results from Chapter 6 (tests of

hostility) derive from only certain levels of hostility, not a general trend that relates greater trade imbalance to higher levels of hostility at all levels.

Most simply, the Copeland (1996) derived tests showed no relationship at all between a measure of trend in trade imbalance and greater (or lower) levels of interstate conflict initiation or conflict hostility. Bluntly, there is no empirical evidence that states respond to the trend in trade imbalance in decisions relating to any aspect of interstate conflict.

The tests of the Rosecrance (1986) "trading states" argument were more complicated. Once again, differences between the Gleditsch and Barbieri data sets drove the results: when a similar sample of cases was tested using Gleditsch data, "trading states" (defined as states with higher levels of trade as a percentage of GDP) were statistically unrelated to interstate conflicts initiation against trading partners; when using Barbieri data, "trading states" were significantly less likely to initiate interstate conflicts against trading partners. Further tests of the data found that the use of "adjusted" GDP measures in the Gleditsch data compressed the differences between those states that did initiate interstate conflicts and those states that did not. In other words, the use of "adjusted" GDP data minimized the difference in the measure of "trading state"-ness between the cases of conflict initiation and the cases of no conflict initiation, such that when tested in the logistic regression the Gleditsch data produced no association and the Barbieri data (with "unadjusted" GDP data) produced a negative association (because the measure of "trading state"-ness was further apart between no initiation cases and initiation cases). While complicated, the end result of this is to argue, once again, that the differences between the Gleditsch and Barbieri data sets are not trivial, and researchers

cannot blindly use either data set without recognizing their respective limitations. Given the wild swings in sign and significance of the "trading states" variable, it is impossible to say with any degree of certainty what the effect of "trading states" is on conflict initiation. What can be said, with certainty, is that the results of Chapter 5 (tests of trade imbalance on conflict initiation) were unaffected by the presence of the "trading state" variable. Those results showed some degree of evidence in favor of the signaling logic (and none for interests), and those results remain unchanged when the "trading state" variable is included in the equation.

In contrast, testing the effects of "trading states" on interstate conflict hostility proved simple. With both sets of data (Gleditsch and Barbieri), the "trading state" variable was significant and negative (arguing that trading states are significantly less likely to use higher levels of hostility in interstate conflicts than non-trading states) and the results from Chapter 6 (tests of trade imbalance on conflict hostility) were unchanged. The addition of the "trading states" measure has no effect on the conclusions in that chapter: there is substantial indirect support for signaling over interests.

With respect to the idea of "boundary conditions" (Mansfield and Pollins, 2001) of the liberal peace, the test of "trading states" is complicated (the test of trade trends (Copeland, 1996), was irrelevant). Given the contradictory findings of the different data sets, there are no firm conclusions that can be drawn about conflict initiation. However, it seems clear that the more a state trades in general (as a percentage of overall GDP) the more this will lessen the levels of hostility used in any interstate conflict that state gets in (with any other state – trading partner or not). These results argue that the "boundary conditions" for this empirical result are wide: here, it seems, is an almost universal

"good" for trade – greater trade (whether it results in decreased incidents of initiation or not) does reduce the hostility level of states who trade. To my knowledge, this result is new for the liberal peace.

In a historical note, the "trading state" theory found no empirical support in any of the pre-1950 tests of initiation or hostility.

As was already discussed, the tests of adjacent levels of hostility were mixed. In the post-1950 era the results suggest that only one particular comparison – the "display of force" compared to the "use of force" levels (MID categories 3 and 4) – is positive and statistically significant. One can read this result in either a pessimistic or optimistic manner. It is pessimistic in the sense that it argues the Chapter 6 results are not generalizable across all levels of hostility: that those results are driven by only one category. In this way, the conclusion that there is reasonable evidence in favor of signaling can be significantly limited: in general, trade imbalance has no effect on most categories of hostility. On the other hand, in an optimistic assessment, the results in Section 7.4 of this chapter demonstrate that there are no "hidden" interest-based actions within the general results. In other words, the comparison of adjacent levels shows that states do not act as the signaling logic argues at lower levels of hostility and as interests argue at higher levels of hostility (or vice versa). Moreover, the tests of adjacent levels is a (deliberately) limited test; one can plausibly argue that the ordinal logistic regression tests (Chapter 6) are a more accurate test of the effects of trade imbalance on hostility than the limited tests of adjacent levels which by their nature ignore large amounts of data (the other levels not being tested). I am inclined towards the optimistic assessment of these tests (in particular, the argument that the ordinal logistic regression with all the

data is better than the five regular logistic regressions with limited data), but recognize that the results of Section 7.4 do dampen the more sweeping conclusions from Chapter 6. Realize, however, that there were no "hidden" aspects of the interest-based logic in the empirical actions of states hostility patterns in the post-1950 era: even if one rejects the arguments that states act as if the signaling logic were present, it is becoming more and more difficult to argue that states follow the interest-based logic.

With respect to the "boundary conditions" ideas, the tests of adjacent levels suggest that unbalanced trade is mostly irrelevant in affecting states already in interstate conflicts, except for the important case of "display" versus "use" of force. This is not a trivial result: a significant line is crossed when states go from threatening or displaying force (the lower levels of the MID hostility scale) to using force or war (the upper levels of the MID scale). The results here argue that unbalanced trade is associated with states moving to that upper tier of violence, rather than the lower. Again, this argues against the universal pacific effects of trade.

The empirical evidence from all sections of this chapter generally supports the previous conclusions. The evidence is not conclusive, but – as I will argue in the concluding chapter – persuasive. The concluding chapter (Chapter 9) will detail the conclusions that can be made from this body of empirical tests, indicate what additional tests would be useful for confirming or disproving the results presented here, and offer suggestions about the state of study of the liberal peace.

However, before those conclusions, there is an obvious set of tests that has been neglected: tests of trade dependence and trade concentration themselves. Trade dependence and trade concentration are the central variables used in the study of the

liberal peace itself (e.g., Russett and Oneal, 2001), though not – so far – in this dissertation. This dissertation has not sought to directly examine the general liberal peace findings, and has used only measures that reflect the balance/imbalance of trade to explore the contradictory competing logics that "explain" the liberal peace. Given the generally pessimistic findings in this chapter, and the previous two, it is clear that there are conditions under which trade leads to increased conflict initiation and hostility. Thus, it is logical to return to the question of the liberal peace itself.

8.0 Chapter 8: Is There A Liberal Peace?

8.1 Introduction

While the dissertation has only considered questions of the effect of trade imbalance on interstate conflict for already unbalanced trading partners, the significant number of tests that find imbalance leading to greater interstate conflict (either initiation or hostility) seems somehow inconsistent with the idea of the liberal peace. This chapter takes a step back, and uses the trade concentration and trade dependence measures (not the weighted indices created from them) and directed-dyads as cases to examine the liberal peace itself.

8.2 Theory

The focus of this dissertation is explicitly not on the existence of the liberal peace itself. While the literature review (Chapters 2 and 3) did discuss how the theory and empirical research on the liberal peace has grown over the last century (particularly in the last two decades), the research design and empirical focus thus far has been on describing and testing the differences between the two logics that compete for explanation of the liberal peace through a test of unbalanced economic relations. The existence of the liberal peace itself has been assumed.

However, given the theoretical and empirical findings in the last several chapters there are specific questions related to the liberal peace itself that have come into sharper focus. In particular, many empirical tests in the last three chapters have found a significant and positive association between unbalanced trade among partnered states and increased international conflict initiation and hostility. This evidence has been

interpreted as favoring the signaling-based logic that supports the liberal peace. However, the underlying meaning of the tests is clear: some manner of trade is associated with increased international conflict. While this may be consistent with signaling, it seems somehow inconsistent with the idea behind the liberal peace: that trade brings pacific relations.

Given the general findings thus far in the dissertation, a next logical step is to use the measures of trade dependence and trade concentration themselves (rather than the weighted indices) in tests with all directed-dyads to examine the effect of trade dependence and trade concentration on the probability of interstate conflict initiation and hostility.

The set of tests in this chapter is not a definitive test of the liberal peace. That is beyond the scope of this dissertation. The central point of this chapter is to add nuance to the overall liberal peace debate by including a set of tests that is implied by previous chapters. It is important to realize that these tests (directed-dyads) are conceptually different from the mainstream form of tests in liberal peace studies (dyads), and that whatever empirical results and conclusions follow from this chapter, this is not a directly comparable set of tests to other work in the liberal peace (e.g., Gleditsch, 2002; Barbieri, 2002; Oneal and Russett, 1999a, 1999c, 2001). Fundamentally, where most other tests use the dyad as the unit of analysis, these tests use the directed-dyad. Because of this difference, the results are not directly comparable. Where dyads are used as the unit of analysis, conclusions must focus on characteristics of the dyad: "When the lowest member of the dyad trades at high levels, this reduces potential for interstate conflict within the dyad." (To paraphrase Oneal and Russett, 2001, with their "least common

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denominator" approach). Where directed-dyads are used as the unit of analysis, conclusions are slightly different: "When the primary state trades at a high level with the partner, this reduces the likelihood of the primary state initiating conflict with the partner state." (Not true; statement used for comparison purposes, not as a conclusion of this chapter). Notice the small, but significant, differences between those statements.

What this means is that because the tests in this chapter are not identical to traditional liberal peace studies, it is not possible to directly discuss previous conclusions. In other words, while previous research has generally found that the more trade in the dyad, the lower the likelihood of interstate conflict beginning, the research here cannot directly dispute that result. It is possible that while trade at the dyad level reduces probabilities of interstate conflict, trade at the directed-dyad level increases interstate conflict.[146] The only way that these results could reject the liberal peace findings is if the directed-dyad approach can be shown to be a better theoretical test of the liberal peace than the dyadic tests others have done (I argue this, below). The empirical results alone are not sufficient to challenge the liberal peace.

As was discussed in Chapter 4, using directed-dyads to test the liberal peace is not new. Bennett and Stam (2000) explicitly compare directed-dyads to dyads (using Oneal and Russett data) in a test of the liberal peace. Their results highlight the importance of

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[146] One scenario that would account for both the dyadic findings (trade reduces conflict) and the directed-dyadic findings (trade increases conflict) is that the research design for the dyadic tests (Oneal and Russett, 1999a, Russett and Oneal, 2001) uses the "least common denominator"' approach (see Chapter 4 for a discussion). This approach uses the lowest value of trade for the two states in the dyad as the measure of trade for the entire dyad. The approach taken in this chapter uses only the trade by the primary state as a the measure of trade (ignoring the level of trade from the partner to the primary state). The dyadic finding argues that trade must be high for both states in order for there to be a pacific effect (the "lowest common denominator" means that the trade not included in the analysis is higher than the trade that is included; thus, where trade brings peace, it is high volumes of trade in both directions). The directed-dyadic finding could be from states where one state (the primary state) is trading a great deal with the partner, but the partner has little to no trade with the primary state. This is a logical possibility of the research design, and the directed-dyadic findings would not directly refute the dyadic findings. In fact, these are the general results of this chapter.

research design choices on the results of the empirical tests: using the same empirical

tools used in this dissertation (logistic regression with cubic splines to account for the

lack of temporal independence; see Chapter 4 for a discussion) both dyads and directed-

dyads have a negative relationship between trade and conflict initiation (conflict hostility

was not tested), though only the dyadic tests produced statistically significant findings.

Moreover, the directed-dyad tests became significant (with the same negative

association) when tested using alternative statistical tests (a logistic regression that uses a

fixed effect term for each dyad and general estimating equations – a form of regression

that uses time-correcting parameters to account for auto-correlation; again, see Chapter 4

for a discussion). While not discussed in the Bennett and Stam (2000a) article, the failure

of trade dependence (Oneal and Russett do not use, and have argued against, trade

concentration as a valid measure) to be significant for pacific relations for the directed-

dyad sample is troubling for the liberal peace. Bennett and Stam (2000a) argue that the

significant results using the alternative statistical tests are sufficient to make the case for

the liberal peace, but given the results in this dissertation I argue the question should be

revisited. It is worth noting, however, that Bennett and Stam (2000a) find no positive

(significant or not) association between trade dependence and conflict initiation in any

test by any statistical method (which I have found in both Chapters 5 and 6).

While this dissertation will not use the battery of statistical methods and

variations on the dependent variable that Bennett and Stam (2000a) do, testing again the

basic liberal peace questions is important in many ways. First, Bennett and Stam's

(2000a) tests use Oneal and Russett data, before the advent of the Gleditsch data. Thus,

while Bennett and Stam (2000a) test from 1885 to 1992 (following Oneal and Russett),

they are limited to only about 120,000 cases. Using either the Gleditsch or Barbieri data will significantly increase the number of cases tested, though only in the post-1950 era. Second, given that this dissertation has found that the nature of the relationship between unbalanced trade and interstate conflict has shifted somewhere around 1950 (or, at a minimum, there is a different relationship when the two temporal spans are considered separately), testing the post- and pre-1950 temporal spans separately will be more consistent with recent liberal peace studies and a better test of the difference between dyads and directed-dyads. Third, while most researchers have ignored the trade concentration variable, it does provide insight about the number of trading partners and the concentration of trade on single partners, and the effects this can have on interstate conflict. Trade dependence cannot provide these insights, and no test has looked at trade concentration using directed-dyads. Fourth, given the analysis this dissertation has done about the effects of "adjusted" versus "unadjusted" GDP, it is important to re-examine the liberal peace with the knowledge that "adjusted" GDP values can bias the results. Oneal and Russett used the same Penn World Tables to get "adjusted" GDP values as Gleditsch used later (2002) when assembling his much larger set of data. Thus, the Bennett and Stam (2000a) article also used "adjusted" GDP. Fifth, no one has tested the effect of trade dependence or trade concentration on conflict hostility using directed-dyads: Bennett and Stam (2000a) only test conflict initiation. In that respect, this chapter can add new tests that have never been done.

Sixth, and finally, and perhaps most importantly, the directed-dyad is the most appropriate and theoretically sound unit to test the liberal peace. While previous research has focused on the dyad as the unit of analysis, the directed-dyad is better for the

theoretical underpinnings of the liberal peace, and more useful in terms of the policy prescriptions of the liberal peace.

Dyads do not make decisions. While this may seem obvious and irrelevant, it is important to recognize that the basic unit of analysis in other liberal peace studies focuses on a (non-directional) pair of states. From a policy perspective, this is not very useful. Whatever significant results one finds (whether they show the liberal peace or trade encouraging conflict, or nothing), it is difficult to translate those results into concrete policy options. After all, the "dyad" does not make decisions, does not have policies, and cannot "do" anything. The dyad consists of individual states (which can and do make policies and decisions), but the analysis only talks about the dyad in general.

Of course, if greater trade for the dyad is pacific (in a Russett and Oneal, 2001, "least common denominator" approach), then one can approach either of the states in the dyad and argue for greater trade. However, this is problematic. After all, in order to achieve pacific relations, the state with the lower volume of trade (determined by percentage of GDP) with the partner is the state that conditions the probability of conflict in the dyad (according to the "lowest common denominator" approach). In other words, convincing the state that already has a high trade to GDP ratio to trade more is worthless in terms of reducing the probability of conflict in the dyad: the low GDP-to-trade partner state would have to raise its level of trade in order to manifestly reduce the likelihood of interstate conflict for the dyad. From a policy perspective, the focus on the dyad (as opposed to the state, or the directed-dyad – which is a state paired with a particular other partner state) is strange. If the liberal peace existed at the state (monadic) level, this

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would be significantly more powerful, as then the policy goals become convincing a single actor to trade more rather than the pair together to trade more.

The focus on the state, as opposed to the dyad, is also seen in the original theorizing on the liberal peace. Kant is straightforward about the connection between states and pacific relations: "The spirit of trade cannot coexist with war, and sooner or later this spirit dominates every people" (Humphrey, 1983, 125). When the people of a state (singular) have a "spirit of trade" then that state will eschew war: states that trade are less warlike than states that do not. No mention of dyads. Clearly, when two such states interact, the chances for war decline even further, but a single such state trading with another state that does not trade so much (as a percentage of GDP) still reduces the likelihood of conflict.

While this may seem a secondary issue, the focus of these original theorists on trade's effects on states underlies the policy-based criticism above. The liberal peace may be dyadic: as pairs of states trade at higher and higher joint levels, the odds of interstate conflict between them falls. But the primary focus of the empirical tests should originally have been states (monads or directed-dyads), and the lack of significant empirical testing on levels of analysis below the dyad (states themselves, directed-dyads, or actors within the "black box" of the state) speaks poorly of the connection between theory and research on the liberal peace thus far.

It is not the purpose of this dissertation or this chapter to re-write the liberal peace, or exhaustively test the possible existence of a liberal peace at the directed-dyad level. Given the theoretical discussions in the early chapters, and the empirical results that generally showed an imbalance of trade being associated with greater conflict and

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hostility, a concluding analysis that examined the role of trade itself (with no reference to balance or imbalance with a partner) seems warranted.

This chapter will proceed similarly to the previous empirical chapters: first testing the effect of trade concentration and trade dependence on the probability of interstate conflict initiation. Both the Gleditsch and Barbieri data will be used post-1950, with the Oneal and Russett data only for the pre-1950 era. Following the tests of initiation, I will test the effects of the two variables on the level of interstate conflict hostility. Given the brevity of this initial analysis, I will not examine issues relating to democracy, power status or any of those other "sub-group" analyses that were done at the ends of Chapters 5 or 6. Again, the purpose of this chapter is not to be an all-encompassing test of the liberal peace, but to provide some context for the previous empirical chapters by testing variables that almost all other liberal peace studies have used in the same manner as was done for entirely different variables (trade imbalance) in earlier chapters (5 and 6). This will allow the overall analysis of all the empirical tests to be better understood before the conclusion.

8.3 Test of Initiation

To be more precise, the tests in this section are exactly the same as the tests in Chapter 5 (logistic regressions, where the cases are directed-dyads, using the same "basket" of independent variables to predict conflict initiation), except for one important changes: the weighted indices are removed and are replaced by the trade dependence and trade concentration variables (respectively). The reason for substituting the direct measures of trade dependence and concentration (instead of the weighted indices or ratio measures) was explained above: this is a test of the liberal peace itself (not the underlying

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logics), and (following the logic and research designs used in other tests of the liberal

peace – Russett and Oneal, 2001) the critical measure is the degree of dependence (or

concentration) of the primary state on the partner state, with no reference to balanced or

unbalanced trade – thus, does greater trade (as a percentage of total trade or percentage of

GDP) with a partner state bring more pacific relations by that state towards the partner

state.

Table 8.1, below, is equivalent to Table 5.1 in Chapter 5, except that in this table

the Gleditsch-based measures of trade concentration and trade dependence are used

(instead of the weighted indices). Thus, the interpretation of this table changes.

Examining the weighted indices was asking questions about the effect of the level of

unbalanced trade on the initiation of interstate conflict; positive and significant

coefficients meant that as trade became both more significant and more unbalanced (the

weighted index was a combination of significance and level of balance; see Chapter 4 for

an explanation) there was greater probability of interstate conflict between the states.

This table, on the other hand, only examines the question of the overall significance of

trade with the partner state for the primary state and the effects that has on the probability

of the primary state initiating an interstate conflict with the partner. As per previous

tests, this first test uses only the Gleditsch data (largest data set, most dominant in terms

of use by liberal peace theorists) even though there are problems with the trade

dependence measure (see Chapter 5, Chapter 6, and Appendix 1), and examines both the

"restrictive" and "broad" set of initiations. Given the temporal domain for the Gleditsch

data, this table only covers the 1950 to 2000 period.

Table 8.1: Effect of Trade Dependence and Trade Concentration on Probability of Conflict Initiation for Population of Directed-Dyads.
(Gleditsch Data, 1950 – 2000; Z-score and significance reported)

	Trade Concentration (N=813368)	Trade Dependence (N=808227)
"Restrictive" set of interstate conflicts (2270 initiations)	-1.84 (0.07)	0.17 (0.87)
"Broad" set of interstate conflicts (3400 initiations)	**-3.76 (0.00)**	0.39 (0.70)

These results mostly do not support a liberal peace, or at least a liberal peace as other researchers have argued. Most other liberal peace researchers have used the trade dependence measure as the only (or primary) measure of dependence, neglecting or actively arguing against the trade concentration measure (Russett and Oneal, 2001). This test, however, indicates that trade dependence has no effect on probabilities of interstate conflict regardless of whether the "restrictive" or "broad" set of initiations is used. By contrast, the trade concentration measures seem to provide evidence in favor of the liberal peace; the "broad" set of initiations is clearly strongly significant and negative, while the "restrictive" set of initiations falls just short of the conventionally accepted 0.05 level of statistical significance. Thus, the higher the concentration of trade (regardless of the significance of the trade) with a partner, the less likely a state is to initiate an interstate conflict with that partner.

These results, as was discussed above, do not directly reject previous liberal peace research. These results argue, according to measures of trade dependence, there is no connection between a state's trade with a partner and the probability of that state initiating a conflict with the partner. The dyadic results (by other researchers) argue that

if the lowest level of trade between a pair of states is high, then there is less probability of

a conflict occurring between those states. While these results seem contradictory, in fact

they can be complimentary: if a pair of states tend to avoid conflict due to high levels of

trade dependence (the dyadic results), but high levels of trade dependence are not

significantly associated with the primary state initiating interstate conflict (Table 8.1's

results), then this implies that the dyadic result comes from high levels of trade

dependence by the partner state (on the primary state) being associated with lower levels

of conflict initiation. In other words, given that the primary state's trade dependence is

not associated with the primary state's avoidance of initiation, then it must be the partner

state's trade dependence that is associated with the primary state's lack of initiation.

Table 8.2 demonstrates that this is precisely the case:

Table 8.2: Effect of Partner State's Trade Dependence and Trade Concentration on Primary State's Probability of Conflict Initiation for Population of Directed-Dyads.
(Gleditsch Data, 1950 – 2000; Z-score and significance reported)

	Partner State's Trade Concentration (N=813368)	Partner State's Trade Dependence (N=808227)
"Restrictive" set of interstate conflicts (2270 initiations)	**-4.79** **(0.00)**	**-2.75** **(0.01)**
"Broad" set of interstate conflicts (3400 initiations)	**-5.88** **(0.00)**	**-2.89** **(0.00)**

What Table 8.2 describes is conceptually odd, but clear from the statistics: the

greater the dependence (whether measured through trade dependence or trade

concentration) of the partner states on the primary state, the significantly less likely the

primary state is to initiate an interstate conflict against the partner state. In other words,

states do not initiate conflict against states that are highly dependent upon them, or highly

concentrated upon them. These results say nothing about the imbalance of trade (the primary state that fails to initiate a conflict could be independent of the partner state, or dependent on the partner state, or both states might be interdependent; however, the Table 8.1 results argue that the dependence of the primary state on the partner is irrelevant), but only look to the dependence or concentration of the partner on the primary. The dyadic result (Russett and Oneal, 2001) can be explained: if a pair of states both have high levels of significant trade (concentration or dependence) then in both cases the primary state is less likely to initiate because the partner is dependent. The trade dependence of the state itself has no effect on interstate conflict, but (somehow) a partner state's trade dependence is causally associated with the primary state being less likely to initiate an interstate conflict.

While this result may seem odd, there is a theoretical explanation. Hirschman (1945, 1980) has argued that unequal trade creates a condition of economic dominance, which allows the non-dependent state a degree of influence, if not control, over the dependent state. Hirschman's empirical examples were the systematic Nazi German policies before and during World War II that sought to make much of Eastern Europe economically dependent on Germany, and the subsequent use of that economic domination to force policy change for the benefit of Germany. The empirical evidence above is consistent with this sort of logic: the primary state has no need to initiate interstate conflict against the partner state, as the economic dependence creates an unbalanced political environment so that any issues in disagreement are resolved (in favor of the primary state) before rising to the level of conflict initiation. The larger the degree of dependence by the partner state on the primary, the less the primary state would

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need to initiate an interstate conflict as the economic domination makes the partner state so subservient to the primary state that conflict becomes unnecessary. Thus, trade brings peace, but not a peace that any liberal peace theorist would recognize. The evidence above is not proof of Hirschman's theory (one would need to examine specific cases and delve into whether the dominance of the partner state by the primary increased demands by the primary and/or compliance by the partner, among other things), but the empirical evidence is consistent with Hirschman's theory.

This empirical result is clearly inconsistent with the interest-based theory (which argues precisely the opposite position – that the state with the smaller trade dependence should be more likely to be the initiator), but is consistent with the logic of the signaling-based explanation of the liberal peace. I argued, in Chapter 2, that signaling operates in part by allowing dependent states to credibly signal resolve and/or hidden capabilities through the obvious economic dependence to the partner state. In this case, an explanation for the Table 8.2 results is that the more dependent a partner state is (the higher their measures of trade concentration and/or trade dependence), the more credibly the partner state is able to signal resolve/capability, and the less likely the primary state is to initiate an interstate conflict. The results of Table 8.2 allow the partner state to be the initiating state (Table 8.2 indicates the primary state is unlikely to initiate when the partner state is dependent), which is consistent with the signaling logic. While there is no other empirical test using these data that would prove this line of logic, it is consistent with both the results here, and the results from previous chapters. Again, while the empirical tests throughout this dissertation are not proof of the signaling-based logic, the

results are arguably consistent with signaling, and there remains little to no empirical

evidence that interests motivate states with respect to the liberal peace.

Table 8.3, below, replicates Table 8.1 (above), but uses the Barbieri data instead

of the Gleditsch data. As Chapters 5 and 6 demonstrated, there was a bias in the

Gleditsch supplied GDP data, and hence a bias in the Gleditsch measures of trade

dependence (and trade dependence weighted index). However, these results are generally

consistent with the Gleditsch (Table 8.1) results above:

Table 8.3: Effect of Trade Concentration and Trade Dependence on Probability of Conflict Initiation for Population of Directed-Dyads.
(Barbieri Data, 1950 – 1992; Z-score and significance reported)

	Trade Concentration Weighted Index (N=258889)	Trade Dependence Weighted Index (N=254671)
"Restrictive" Initiations (2270)	1.41 (0.16)	0.73 (0.46)
"Broad" Initiations (3400)	-0.64 (0.52)	0.15 (0.88)

Overall, again, there is little direct evidence in favor of a traditional liberal peace

relationship. Trade concentration and trade dependence are not significantly related to

increased (or decreased) initiation of interstate conflict. Even trade concentration,

significant in one of the Gleditsch tests, is not significant here. Given the difference

between the Gleditsch data and Barbieri data ("adjusted" versus "unadjusted" GDP data),

it is not clear whether there is any significant difference between the data sets for this

test. However, given the general insignificance of results, it is not worth the space or

time for a more detailed test that substitutes the Gleditsch data into the Barbieri cases and

further explores the differences in data between the two sets.

However, given the odd results in the Gleditsch data that argued for the partner state's dependence conditioning the primary state's probability of initiating conflict, a similar test of the partner state's trade is warranted:

Table 8.4: Effect of Partner State's Trade Dependence and Trade Concentration on Primary State's Probability of Conflict Initiation for Population of Directed-Dyads.
(Barbieri Data, 1950 – 1992; Z-score and significance reported)

	Partner State's Trade Concentration (N=258889)	Partner State's Trade Dependence (N=256907)
"Restrictive" set of interstate conflicts (2270 initiations)	**-3.30 (0.00)**	**-2.45 (0.01)**
"Broad" set of interstate conflicts (3400 initiations)	**-4.43 (0.00)**	**-2.37 (0.02)**

These results are quite similar to the Table 8.2 results, above. Again, while the Barbieri data may differ from the Gleditsch data, and both sets do not contain the same cases, the results reported in Table 8.4 indicate that the partner state's trade concentration and trade dependence are significantly and negatively associated with the primary state's probability of initiating interstate conflict. Trading dyads may see less conflict, as many liberal peace researchers have found, but the reason for this is that high levels of trade dependence or trade concentration for the partner state are associated with the primary state in the dyad avoiding conflict, not because the trade causes the partner state itself to avoid interstate conflict.

The final table in this section uses the Oneal and Russett data to see if there is any evidence of a relationship between trade concentration or trade dependence and interstate conflict initiation in the time period before 1950.

Table 8.5: Effect of Trade Concentration and Trade Dependence on Probability of Conflict Initiation for Politically Relevant Set of Directed-Dyads.
(Oneal and Russett Data, 1870 – 1949; Z-Scores and Probability of Significance Reported)

	Trade Concentration (N=22791)	Trade Dependence (N=23015)
Oneal and Russett: "Restrictive" set of interstate conflicts (2270 initiations) (1885 – 1949)	0.75 (0.46)	0.36 (0.72)
Oneal and Russett: "Broad" set of interstate conflicts (3400 initiations) (1885 – 1949)	0.99 (0.32)	-0.71 (0.48)

As with the other tests, there is no evidence of trade dependence or trade concentration having any significant effect on interstate conflict initiation. These results are, again, troubling for the liberal peace.

Table 8.6, below, examines the possibility that (as with the post-1950 results) the partner state's level of trade dependence or concentration is correlated with the primary state's actions.

Table 8.6: Effect of Partner State's Trade Dependence and Trade Concentration on Primary State's Probability of Conflict Initiation.
(Oneal and Russett Data, 1885 – 1949; Z-score and significance reported)

	Partner State's Trade Concentration (N=22791)	Partner State's Trade Dependence (N=23015)
"Restrictive" set of interstate conflicts (2270 initiations)	**3.45** **(0.00)**	**2.10** **(0.04)**
"Broad" set of interstate conflicts (3400 initiations)	**3.57** **(0.00)**	1.11 (0.27)

While this set of tests does not produce entirely consistent results, the findings in Table 8.6 are generally opposite those for the post-1950 era. Here, there is evidence against the liberal peace: higher levels of trade dependence and trade concentration for the partner state correlate with the primary state being more likely to initiate an interstate conflict. This is not the expected result: again, the partner state's trade has an effect on the primary state's actions. Most theories of the liberal peace argue that trade has a pacific effect on the state itself, not the state or states with which it is interacting. Once again, however, the relationship between trade and interstate conflict changes across these two time spans.

The logic of the liberal peace, signaling and interests, has generally only been tested with respect to the initiation of interstate conflict. However, both logics extend to questions of hostility. Thus, the final section of this chapter uses trade dependence and trade concentration to examine the effects of trade on states already engaged in an ongoing interstate conflict.

8.4 Test of Hostility

Parallel to the tests of initiation, this section will examine the relationship between trade concentration and trade dependence as independent variables and interstate conflict hostility. While Chapter 6 focused on the weighted indices as measures of unbalanced trade in order to examine the two underlying theories of the liberal peace, this section only uses the most commonly defined measures of interdependence in the study of the liberal peace (trade dependence and trade concentration, as was done in the previous section of this chapter) in the examination of the effect of trade on interstate

conflict. Moreover, an empirical test that looks to the levels of hostility that states use within interstate conflicts is not a common test for the liberal peace. In this way, this section adds to the general knowledge of the study.

Following the outlines in previous tests, the first test will use only the Gleditsch data. While Chapters 5 and 6 (and Appendix 1) discuss the empirical problems with the Gleditsch data, I continue to use these data for comparative purposes as almost all other liberal peace studies rely on them. Following this test, the Barbieri data will be used, then the Oneal and Russett data for the pre-1950 era.

Table 8.7, below, presents the results using the Gleditsch data. Since the focus here is on conflict hostility, there is no "restrictive" or "broad" set of initiations, only the set of states engaged in conflict (regardless of who initiated) and the action they took in the conflict.

Table 8.7: Effect of Trade Dependence and Trade Concentration on Probability of Conflict Hostility for Population of Conflict Directed-Dyads.
(Gleditsch Data, 1950 – 2000; Z-score and significance reported)

	Trade Concentration (N=2922)	Trade Dependence (N=2871)
Gleditsch, 1950 - 2000	**2.01** **(0.05)**	0.94 (0.35)

The results here indicate that the more widely accepted measure of dependence (trade dependence) is unrelated to the level of conflict hostility, while the trade concentration measure (which measures how concentrated the trade with the partner is compared with total trade) is significantly and positively associated.

This evidence does not support the liberal peace. While most theoretical arguments for the liberal peace have restricted themselves to questions of conflict

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initiation (not questions of what effect pre-conflict trade relations have on the actions of the states once the conflict begins), these results indicate that trade, in some circumstances, increases the violence between states already in conflict. If the liberal peace is narrowly construed to discuss only conflict initiation, these results do not directly reject that relationship (though the results cannot be seen as supporting it). If the liberal peace is more broadly defined to include issues of trade and ongoing conflicts, then these results can be seen as troubling for the general findings. Following Mansfield and Pollins (2001), perhaps the best way to characterize these results is that they provide some "boundary conditions" (limits) to the extent of the liberal peace. As in the tests of the same variables and conflict initiation, these results cannot directly speak to previous liberal peace research, as the cases used here are different from the vast majority of other empirical studies (directed-dyads versus dyads). In the end, however, the results do not indicate that trade (dependence or concentration) reduces the violence that a state would use against a partner in the event of an interstate conflict.

Given the results from earlier in the chapter – namely, a correlation between the extent of a partner state's trade dependence (or trade concentration) and the probability that the primary state would initiate an interstate conflict - it is worth a brief examination of the same variables for the tests of conflict hostility. Table 8.8, below, summarizes those results.

Table 8.8: Effect of Partner State's Trade Dependence and Trade Concentration on Probability of Conflict Hostility by Primary State for Population of Conflict Directed-Dyads.
(Gleditsch Data, 1950 – 2000; Z-score and significance reported)

	Trade Concentration (N=2922)	Trade Dependence (N=2871)
Gleditsch, 1950 - 2000	**-2.50** **(0.01)**	-1.24 (0.21)

Similar to the tests of initiation, the signs on both the trade concentration and trade dependence measure are negative: the greater the concentration and dependence (not significant, but the sign is consistent) of the partner state on the primary state, the lower the levels of hostility the primary state is likely to use in the ongoing interstate conflict. Again, to the degree this result means anything, it seems to support the signaling-based logic of the liberal peace more than the interest-based logic. A dependent partner state is more clearly able to signal (according to the interpretation of signaling logic presented in Chapter 3 and 4) to a primary state than a less-dependent partner. The empirical results show this, though not as strongly as the test of initiation. An alternative explanation, again drawing on Hirschman (1945, 1980), is that the primary state has no need to use higher levels of hostility as the partner state is so dependent that it caves and gives the primary state whatever it wants; the greater the dependence, the less hostility needed – which is what Table 8.8 indicates.

This, however, is a digression on a digression. The primary purpose of this section is to test the effects of trade for the primary state on levels of hostility in ongoing interstate conflicts. Table 8.7, above, indicates clearly that there is an effect for trade concentration, but not for trade dependence. However, as was discussed before, there are some significant flaws in the Gleditsch data that lead, especially in tests of conflict

hostility (see Chapter 6, and Appendix 1), to inaccurate results. Thus, comparing the

Gleditsch results to the Barbieri results is critical.

Table 8.9, below, replicates Table 8.7, but uses the Barbieri data instead of the

Gleditsch data. Once again, there is a significant difference between the Barbieri results

and the Gleditsch results.

Table 8.9: Effect of Trade Dependence and Trade Concentration on Probability of Conflict Escalation for Population of Conflict Directed-Dyads.
(Barbieri Data, 1950 – 1992; Z-score and significance reported)

	Trade Concentration (N=1488)	Trade Dependence (N=1459)
Barbieri, 1950 - 1992	**4.47** **(0.00)**	**3.07** **(0.00)**

As with the test using the Gleditsch data, the analysis employing the Barbieri data

finds a positive and significant relationship between the trade concentration measure and

the level of hostility employed by the primary state. However, only the test using the

Barbieri data finds a positive and statistically significant relationship between the trade

dependence (the widely accepted) measure and the level of hostility employed by the

primary state. Given the previous analysis (see Chapters 5, 6 and Appendix 1), the initial

hypothesis on the difference is that once again the "adjusted" versus "unadjusted" GDP

issue has arisen. As was done in previous chapters, this can be investigated. The first

step is to substitute the Gleditsch data into the cases for which Barbieri has data, to

eliminate that difference between Tables 8.7 and 8.9. Table 8.10, below, provides a

direct comparison of the differences arising from the two data sets.

Table 8.10: Effect of Trade Dependence and Trade Concentration on Probability of Conflict Hostility: Results of Ordered Logit Analysis for Trade Based Variables for Common Directed-Dyads.
(Gleditsch and Barbieri data, 1950+; Z-Scores, significance, and Ns reported)

	Trade Concentration	Trade Dependence
Row 1: Gleditsch (1950 – 2000) (From Table 7)	**2.01** **(0.05)** **(N=2922)**	0.94 (0.35) (N=2971)
Row 2: Gleditsch (1950 – 1992)	**2.18** **(0.03)** **(N=2345)**	0.71 (0.48) (N=2345)
Row 3: Barbieri (1950 – 1992) (From Table 9)	**4.47** **(0.00)** **(N=1488)**	**3.07** **(0.00)** **(N=1489)**
Row 4: Gleditsch data substituted into valid Barbieri cases.	**4.46** **(0.00)** **(N=1455)**	0.97 (0.33) (N=1455)

Once again, as in Chapter 6, limiting the Gleditsch data to the Barbieri cases does

not rectify the differences between the data sets. Limiting the Gleditsch data to the

Barbieri cases produces results for the trade concentration measure that are almost

identical to those produced with the Barbieri data, but this is not the case for the tests

using the trade dependence measure. Clearly, the Barbieri and Gleditsch data themselves

are different. Moreover, this is a significant difference: using the Gleditsch data shows

no association between trade dependence and the level of conflict hostility – a result that

is essentially neutral with regard to the liberal peace. Using the Barbieri data, there is a

statistically significant positive association between trade (both dependence and

concentration) and the level of conflict hostility, which is a troubling finding for the

liberal peace.

A direct comparison of the measure of trade dependence using the Gleditsch and

Barbieri data once again, as in the case for the weighted index of trade dependence,

produces very different scores for certain states. Table 8.11, below, compares the

differences between the two measures.

Table 8.11: Comparison of Barbieri and Gleditsch values of Trade Dependence by Hostility Category.
(Barbieri and Gleditsch Data; 1950 – 1992)

	Column 1: Number of cases where Barbieri's trade dependence score is less than Gleditsch's.	Column 2: Number of Cases where Barbieri's trade dependence score equal's Gleditsch's	Column 3: Number of cases where Barbieri's score exceeds Gleditsch's	Column 4: Average ratio of Barbieri's TrdDep to Gleditsch's
Primary State takes no action.	63	17	274	2.5
Primary State Threatens Partner	11	1	46	3.0
Primary State displays force to Partner	64	10	247	2.3
Primary State uses force against Partner.	133	23	571	3.25
Primary State goes to War against Partner	13	0	23	1.6
Totals	280	51	1161	2.8 (Average for all cases)

As was discussed in Chapter 4, dividing trade volume by GDP creates the trade

dependence measure (which is then used to create the weighted index). As Appendix 1

demonstrates, the "adjusted" GDP data from the Penn World Tables are larger than the

"unadjusted" GDP data from the World Bank. A larger GDP means that the trade

dependence measures created with the Gleditsch data are smaller (trade as a percent of

GDP is small when the GDP is large) when compared to the trade dependence measures

created with the Barbieri data. Table 11, above, compares the number of times, for each

level of hostility, the Gleditsch measures of trade dependence are larger than the Barbieri

measures, the number of times they are identical, and the number of times the Barbieri

measures exceed the Gleditsch measures (Columns 1 through 3). For every level of

hostility, the number of cases where the Barbieri measure of trade dependence exceeds

the Gleditsch is far larger than the number of cases where the Gleditsch exceeds the

Barbieri. On average, the Barbieri trade dependence measure is greater than the

Gleditsch trade dependence measure four out of five times across all the hostility

categories.

The Barbieri trade dependence measure is about 2.8 times, overall, as large as the

Gleditsch measure (the breakdown of the ratio by hostility category is the right hand

column; column 4). Statistically, it is irrelevant for the coefficients produced in

regression (including logistic and ordered logistic regression) if a variable is multiplied

by a fixed constant. In other words, if a variable has a statistically significant relationship

to the dependent variable, multiplying the variable by a fixed amount will not change the

overall relationship (the coefficient resulting from the logistic regression will change,

though the statistically significance of the coefficient will not) between the independent

and dependent variables. However, that is not what is happening here. With these

results, the Barbieri data are not simply a straight-forward multiple of the Gleditsch data.

The ordered logistic regression results (Table 8.10) show that high values of Barbieri-

measured trade dependence are associated, in a statistically significant manner, with a

greater likelihood that a state will employ higher levels of hostility, whereas for those

same cases, using measures of trade dependence created with Gleditsch data, the

relationship is not statistically significant.

Table 8.11 suggests a plausible explanation. While the average ratio of Barbieri-

to-Gleditsch trade dependence is 2.8 to 1 (on average, the Barbieri measure of trade

dependence data is 2.8 times greater than the Gleditsch data for this sample of cases), the table indicates what the ratio is for each category of hostility, as well as the number of cases in each category. The largest number of cases (571) and highest ratio (3.25) occur in the use of force category. When testing these data using ordered logistic regression, this "clump" of both a larger than average ratio in the size of the Barbieri-based versus Gleditsch-based measure of trade dependence and a large number of highly hostile conflicts contributes to the discovered result: that higher levels of trade dependence are associated with greater levels of hostility used in the interstate conflict. This is the same "clump" of cases discussed in Chapter 6 and Appendix 1.

The analysis in Chapter 6 resulted in a hypothesis that the "clump" of cases involved states that are generally poor, and further research confirmed this hypothesis (see Appendix 1 for details). This "clump" of poor states where the Barbieri-derived GDP values are much smaller than the Gleditsch-derived GDP values (hence, the Barbieri trade dependence measure is much larger than the Gleditsch trade dependence measure) remains unexplained by liberal peace theory. Why these states should be more likely to use force, rather than threaten or display force is explained by the signaling-based theory: states with smaller GDPs are more likely to be dependent (the larger the GDP, the harder it is for that economy to be dependent on other economies), and (according to signaling theory), dependent states are more likely to use higher levels of hostility to make the non-dependent partner state pay attention and resolve the conflict. Hence, the Barbieri-based GDP figures seem to more accurately convey the correct economic relationship between the primary and partner state, and the use of adjusted-for-PPP GDP figures can obscure the relationship. Much like the results from Chapter 6, the

evidence here indicates that the more accurate relationship between trade and conflict hostility is the one derived from the Barbieri-based measures, not the Gleditsch-based ones.

These results do not reject the liberal peace (again, the focus on directed-dyads and hostility not initiation separates this test from the bulk of work done by other liberal peace researchers), but do add additional evidence that the Gleditsch data are fundamentally differences from the Barbieri data. These are not fundamental flaws, per se, but the differences between "adjusted" and "unadjusted" GDPs create measurable and significant variations in empirical results.

Testing the effect of the partner state's trade dependence and trade concentration with the Barbieri data (as Table 8.8 did for the Gleditsch data; the results are not reproduced here) reveals that both measures are negative (as the Gleditsch variables were), but insignificant.

The final test for this chapter uses the Oneal and Russett data to examine the effects of trade dependence and trade concentration in the pre-1950 era. The analytic results reported in Table 8.5 suggest there is no significant association between trade dependence and trade concentration and conflict initiation during the period. However, the effect on level of hostility (presented in Table 8.12) appears quite different.

Table 8.12: Effect of Trade Dependence and Trade Concentration on Probability of Conflict Escalation for Sample of Politically Relevant Directed-Dyads in Conflict. (Oneal and Russett, 1885 – 1949; Z-score and significance reported)

	Trade Concentration (N=591)	Trade Dependence (N=591)
Oneal and Russett, 1885 - 1949	**-2.87** **(0.00)**	**-3.03** **(0.00)**

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Here, finally, is clear evidence in favor of both the general liberal peace and the interest-based logic of the liberal peace. Both measures of trade have a statistically significant negative association: the greater the concentration and the greater the dependence of the primary state on the partner state the greater the likelihood that the primary state will employ a low rather than high level of hostility in the conflict with the partner state. Moreover, this is the relationship predicted by the interest-based logic of the liberal peace: states with an interest in trade will act to avoid harming that interest, and will dampen interstate conflicts with trading partners in hopes of minimizing the damage to their economic interests. Interesting, when tested (results not shown), the partner's trade dependence and trade concentration are insignificant with respect to influencing the primary state's level of hostility.

This result seems to indicate that the effect of trade on interstate conflict changed substantially at some point close to the beginning of the cold war period. There is some evidence (both from this chapter and before) that the interest-based logic operated for the liberal peace in the earlier time span, but that signaling (and perhaps even a diminution of the liberal peace itself) has operated since.

8.5 Conclusions

Overall, the results in this chapter are troubling for a universal liberal peace, but do not directly reject the findings of most other researchers. Unexpectedly, additional support was found for the signaling-based logic of the liberal peace.

The empirical tests in this chapter do not support, by and large, the general liberal peace. These tests used traditional measures of trade (trade/GDP and trade/total trade) and directed-dyads as cases and found little correlation with lower interstate conflict

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initiation or lower conflict hostility in the post-1950 era. Of the eight tests of conflict initiation using both Gleditsch and Barbieri trade data, only one provides evidence in favor of the liberal peace (greater trade leads to less conflict). The other seven tests produce no significant association between trade and conflict initiation. As I argued above, this is not a direct refutation of other liberal peace studies, but the results are not what one would expect, given the findings of other researchers. More damaging to the liberal peace are the tests of hostility in the post-1950 era: three of four tests produce positive correlations between measures of trade and conflict hostility (the fourth test, using the Gleditsch-based trade dependence measure, is arguably wrong due to biases in the Gleditsch data, so the overall result is likely even stronger against the liberal peace). These tests indicate that higher levels of trade dependence and concentration are associated with more violence, not less, and are clearly not consistent with the ideas behind the liberal peace. However, an underlying assumption of the "least common denominator" school (dominant in the dyadic studies that make up the majority of liberal peace research) is that whichever state in the dyad has the lowest trade dependence or trade concentration value (and hence determines the value for the dyad) is the one that is most likely to initiate an interstate conflict. The results here show that trade dependence and trade concentration have no direct effect on that state initiating an interstate conflict (the primary state's trade with a partner state does not affect the odds of it initiating a conflict against the partner state), but high levels of trade concentration or trade dependence by the partner state are significantly associated with the primary state being less likely to initiate an interstate conflict. These results do not argue against the dyadic results, but do complicate the story that must be told to explain the dyadic results; as

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noted, the traditional liberal peace interest-based logic will not work, though the signaling-based logic will (proof of the signaling logic has not been shown here, just that these results are consistent with the logic) and the results are also consistent with Hirschman's (1945, 1980) arguments about economic domination (which, in turn, is a form of an interest-based argument, though not one associated with the liberal peace). The bottom line is that these results complicate a simple understanding of the liberal peace (higher amounts of trade cause the trading state to refrain from conflict), and argue for a more detailed explanation of the complex links between trade and interstate conflict.

With all the tests run, and results discussed at least preliminarily, the final chapter of the dissertation (Chapter 9) will synthesize the findings presented in the empirical chapters, and argue for what conclusions can be drawn from this body of work.

9.0 Chapter 9: Conclusions

9.1 A Summary of the Results

The dissertation tested the effect of unbalanced trade on both the probability of interstate conflict beginning between two states, and the probability of an ongoing interstate conflict resulting in high levels of hostility used by the state. Unbalanced trade, I argued in Chapter 4, is a circumstance where the interest-based logic of the liberal peace and the signaling-based logic of the liberal peace predict that states should behave in different ways.

While the results were not entirely consistent with either logic, there was support for the signaling-based logic of the liberal peace over the interest-based logic. Most importantly, there was no evidence at all in favor of the interest-based logic. This statement holds true for both the tests of conflict initiation and conflict hostility.

With respect to conflict initiation (Chapter 5), eight separate tests are run for the post-1950 period, using various combinations of the Gleditsch and Barbieri data[147], "restrictive" and "broad" definitions of initiation[148], and two separate measures of the imbalance of trade[149]. Seven of those eight tests (Tables 5.1 and 5.3 in Chapter 5)

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[147] The Gleditsch data set (Gleditsch, 2002) is the most widely used source of international trade data for testing the liberal peace. It is the largest, and most complete, data. However, Gleditsch achieved this completeness by extrapolating, interpolating, and using multiple sources to replace the missing data. In addition, the GDP data he uses are adjusted for purchasing power parity (cost of living), while the trade data are not. These two actions (replacing missing data and the use of adjusted GDP data) have caused some researchers to question the validity of the data. The Barbieri data set (1995, 2002) employs neither of the two controversial elements of the Gleditsch data set, but as a result is much less complete. See Chapter 4 for a full discussion of the two data sets.

[148] The "restrictive" definition of conflict initiation incorporates only those states that were the original participants in a militarized conflict. The "broad" definition incorporates, in addition, states that join the conflict after it has begun. See Chapter 4 for an explanation.

[149] The two variables that measure the imbalance of trade between the primary and partner state are the weighted index of trade dependence and the weighted index of trade concentration. The weighted index of trade dependence is created from the commonly used trade dependence (dyadic trade/GDP) measure, and uses a ratio of the primary state's trade dependence to the partner state's trade dependence. The weighted

produce statistically significant, positive associations between higher levels of trade imbalance and greater likelihood of initiating interstate conflict. As I argued, this is evidence in favor of signaling: the more dependent state, in order to make the less dependent partner pay attention, uses the initiation of the interstate conflict as a "costly" signal (see Chapter 2 and Chapter 4) to help resolve whatever the dispute is about. By the interest-based logic, the more dependent state would have no incentive to take action that might threaten the economic relationship on which it depends, and hence should not initiate an interstate conflict.

These results are not a direct confirmation of the signaling logic. No attempt was made at any point in this dissertation to measure the sending or receiving of signals themselves by states or decision-makers within states. The evidence shows that state's actions were generally consistent with the actions a state would have taken if its decision-making were guided by signaling logic.

There is, however, a significant caveat to this conclusion. The final section of Chapter 5 detailed a set of additional tests that "cut" the data by several state-level factors. While a full discussion of these results is presented below, two tests in particular cast some doubt on the conclusions of the initiation tests: tests of the post-1950 era that excluded the decade of the 1950s (thus, 1960 to 2000 for the Gleditsch data, 1960 to 1992 for the Barbieri data) reduced three of four tests to insignificance (only "restrictive" cases of initiation were used, not "broad," for simplicities sake). Thus, the conclusion that there is a broad-based, statistically significant, and positive correlation between

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index of trade concentration uses the much less commonly used trade concentration measure (dyadic trade/state's total trade), and creates a similar ratio of the primary state to the partner state. The indices are weighted by multiplying the ratio by either the trade dependence or the trade concentration of the primary state. See Chapter 4 for a complete explanation.

unbalanced trade and increased conflict initiation is tenuous. There is a positive

relationship (Tables 5.1 and 5.3 demonstrate it), but it is fragile. The additional analyses

demonstrate that removing a decade of data (the 1950s) reduces the statistical

significance of the association below the commonly accepted 0.05-level. These results

argue for a qualification of the conclusions, not a rejection.

However, the results were clearer for the tests of hostility (Chapter 6). Of the four

tests[150], three found a statistically significant, positive relationship between unbalanced

trade and increased hostility in ongoing interstate conflicts, and I argued in Chapter 6

(and Appendix 1) that the one test that did not should be discounted because of biases in

the data (the argument is summarized below). In this way, I argue, all the tests of

hostility produce results that are consistent with the signaling logic. However one

interprets the single test that was not consistent with signaling, there was no empirical

result that was consistent with the interest-based logic.

Where temporal-span analysis in the tests of initiation qualified the conclusions of

that chapter (demonstrating that removing the 1950s from the post-1950 analysis

removed much of the significance of the results), the same analysis of the tests of

hostility showed no such limitations. The correlation of imbalanced trade to higher levels

of hostility was relatively equal across all five-year intervals in the post-1950 era.

As a check on the preceding results, the final section of Chapter 7 investigated the

possibility that both signaling and interests operate at different stages in the conflict

process (in other words, the possibility that while the ordered logistic regression results

for conflicts as a whole in Chapter 6 indicated a positive relationship between unbalanced

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[150] There are half as many tests for hostility as for initiation because there is no need to consider "broad"
versus "restrictive" initiations of interstate conflicts: all interstate conflicts (however they were initiated)
are tested. Removing that aspect of the conflicts halves the number of necessary tests.

trade and greater hostility, in fact the association between trade and conflict might change

at different levels of hostility). It was possible, I noted, that greater dependence could

drive states to avoid wars and uses of force (high levels of force; evidence in favor of

interests) while states are willing to threaten and display force in order to signal at lower

levels of interstate conflict (evidence in favor of signaling). To examine this, I tested only

adjacent levels of conflict hostility to see what effect the measures of unbalanced trade

would have on states' actions on a smaller scale. There was no evidence of a differing

effect: while the effects of signaling (a positive association between the measures of

dependence and higher levels of hostility) were concentrated only in a few hostility levels

(and not spread evenly over all the levels of hostility), there was again no evidence that

interests were operating at any point.

Overall, the tests across these three chapters provide significant evidence that

states act as if they are signaling, not as if they are guided by economic interests.

The results of the post-1950 era differed significantly from the pre-1950 era. This

could be because of a change in the nature of how states act, or it could be the result of

differing sources of data used in the analysis. The availability (and quality) of trade data

prior to 1950 is quite limited. Analysts agree that the International Monetary Fund

generates the most valid trade data for the international system, but its data series begins

in 1950. Trade data pre-1950 are a hodge-podge of official government statistics,

information published by some intergovernmental organizations (mostly the League of

Nations), and private (mostly academic) research. Unlike the post-1950 era, there is no

universally agreed upon source of data. Researchers have taken two approaches to this.

First, use the best available data. This is the approach taken by Russett and Oneal (2001);

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they survey the potential sources, and then create a single data set. The problem with this method is that with alternatives available, there is always room for argument about the "best" sources of data from this time. By far the largest numbers of researchers have employed a second approach: ignore data before the IMF, and only examine cases from 1950 and on. There is nothing wrong with this alternative, but it does ignore many years of history, trade, and conflict.

For this dissertation, I have used pre-1950 data. However, given the clear difference in quality (reliability and validity) between the IMF data and non-IMF (pre-1950) data, I examined the two periods separately. Complicating this situation further, the only source of pre-1950 data (data sets made publicly available by Oneal and Russett, based on their 1999 articles) uses a different method of choosing cases (politically relevant cases rather than the universe of possible cases), and thus the nature of the cases examined was different in the two periods analyzed.

Given those limitations, the pre-1950 results are somewhat suspect. Tests from Chapters 5 and 6 clearly indicate no effect of unbalanced trade on conflict initiation (Table 5.5, Chapter 5) during the pre-1950 period, but a strong and significant pacific effect of unbalanced trade on conflict hostility (Table 6.11, Chapter 6). These results clearly differ from the post-1950 results. Given that the source of the data changed between the post-1950 and pre-1950 analysis, it is unclear whether states changed behavior or whether case selection (politically relevant cases versus the universe of all cases) and different sources of data account for the change in observed results. The pacific effects of unbalanced trade on conflict hostility for the years 1885 – 1949 were the only results that supported the interest-based logic of the liberal peace. However,

given that the pre-1950 data are more suspect than the post-1950 data, it is difficult to know what to make of those results.

Finally, Chapter 8 involved analysis of variables not used in Chapters 5, 6, and 7. Chapter 8 uses the "normal" liberal peace variables[151] to test the effects of trade on conflict initiation and conflict hostility, though it uses directed-dyads rather than the nondirected-dyads used in almost all other liberal peace tests. In this sense, Chapter 8 is the closest I come in this dissertation to directly testing the liberal peace. However, the tests in Chapter 8 are not directly comparable to previous empirical studies: the use of directed-dyads rather than nondirected-dyads makes the two types of tests different.

The degree to which Chapter 8 represents a true test of the liberal peace depends on the degree to which the reader accepts directed-dyads (rather than nondirected-dyads) as the appropriate unit of analysis for testing the liberal peace. I argued in Chapter 8 that a close reading of the original theorists (Montesquieu, Kant) leads to the conclusion that they were arguing that economic ties encouraged more pacific actions towards states where some form of dependent economic relationship exists (thus, directed-dyads are the appropriate cases) rather than that economic ties create peace between trading partners irrespective of how important the trade is to a given partner (in which case, nondirected-dyads would be the appropriate units of analysis). This is a crucial point in considering the implications of the empirical results of Chapter 8.

The results of Chapter 8 generally argued that there was no liberal peace in the post-1950 period: using both the Gleditsch and Barbieri data (with the flaws of both), few tests produced any significant association between trade and conflict: of the eight tests of

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[151] Trade dependence operationalized as dyadic trade divided by the primary state's GDP, and trade concentration operationalized as dyadic trade divided by the primary state's total trade – both variables have been used in numerous other liberal peace studies; see Oneal and Russett, 2001, and Barbieri, 2002

conflict initiation (restrictive versus broad, trade dependence versus trade concentration, and Gleditsch versus Barbieri data created eight possible permutations) only a single test (broad, concentration, Gleditsch) produced a significant and negative association between trade and conflict. While this is evidence in favor of the liberal peace (higher percentages of overall trade with a partner lead to less probability of interstate conflict initiation), the fact that the other seven tests found no results is troubling for the liberal peace. Four tests (Gleditsch versus Barbieri data, trade concentration versus trade dependence) of the relationship between trade and the level of conflict hostility produced a very different result. Three of the four tests uncovered a positive and statistically significant association between trade and states using greater force in ongoing interstate conflicts: more trade was statistically associated with greater hostility in interstate conflicts. (The relationship in the fourth test was insignificant.) The absence of correlation in the tests of initiation, and the statistically significant, positive relationship in the test of level of hostility are troubling for the traditional interpretation of the liberal peace. There appears to be no evidence that trade dependence or trade concentration in the post-1950 period, when viewed from the perspective of directed-dyads, has any dampening effect on interstate conflict.

I argued in Chapter 8 that these results do not necessarily refute the more widely known "liberal peace" (Russett and Oneal, 2001) results. The differences in cases (nondirected dyads versus directed-dyads) mean that the tests are not directly comparable. Moreover, it is logically possible for a dyad to experience reduced conflict even if the primary state's (of the directed-dyad) trade is not the cause of the pacifism: it could be the partner state's dependence on the primary state that causes the primary state

to act more pacifically. In fact, this is what the tests showed. The more the partner state

of the directed-dyad was dependent (using both trade concentration and trade dependence

as measures) on the primary state, the less likely the primary state was to initiate an

interstate conflict, or (to a lesser degree) use higher levels of hostility in an ongoing

conflict. This, as noted in Chapter 8, is consistent with the logic of the signaling-based

explanation for the liberal peace, but not at all with the interest-based explanation. These

results are also consistent with a more malevolent form of interest-based logic, one

suggested by Hirschman (1945, 1980), in which the economic dependence of the partner

state allows the primary state to demand concessions that the partner state is willing to

make (without interstate conflict) because of the economic dependence. In either case

(signaling or domination), the empirical results are not consistent with the idea of the

liberal peace first suggested by Montesquieu and Kant.

9.2 Interests versus Signaling

The results across all the chapters argue that if there is a liberal peace, it does not

exist in the fashion that most peace researchers recognize. The findings in the

dissertation buttress the logic that states signal though their trading relationships, and

these "costly" signals facilitate the resolution of conflicts (even while they further the

odds of conflict between unbalanced economic partners).

The basis for this claim, central to the dissertation, stems from the two different

underlying logics that define the two theoretical bases for liberal peace research. These

two hypotheses are the basis for the liberal peace.

The most commonly cited, and most historical, theory to explain the liberal peace is the interest-based hypothesis that argues that trade creates benefits for all parties who participate: trade allows all parties to gain goods and material benefits that they would not otherwise have. These exchanges create an incentive for all parties to continue trading and also to refrain from any action that would prevent or reduce future trading. States have an interest in avoiding actions that will reduce their benefits from trade, and so will avoid both initiating an interstate conflict and using higher levels of hostility if they find themselves in an interstate conflict.

The empirical results reviewed above argue that this logic, which traces roots back several centuries, is not accurate. While it is clear that economic exchange creates domestic political interest in furthering that trade (Kehr, 1932; Jungblut, 1999), the evidence indicates that those interests are not directly connected to foreign policy outputs (pacific relations) in the fashion the interest-based logic of the liberal peace suggests. In this way, the assumptions that underlie the most cited liberal peace research (Oneal and Russett 1999b; Russett and Oneal, 2001; Barbieri, 1995, 2002; Gleditsch, 2002) are incorrect (though those assumptions have no effect on the empirical results any of the researchers report).

The empirical findings in this dissertation raise interesting questions related to the interest-based logic. If economic relationships create economic interests at the individual, group, and state levels, yet the outcome of those interests is not the liberal peace, why do these interests not restrain state behavior? Why, in other words, does the liberal peace not work the way it has been assumed to work for centuries? What happens within states that prevent the individuals and groups from influencing the state's

decision-making process? While well beyond the scope of this dissertation, these are obvious questions to pursue.

As the empirical results indicate, there is significant support for the alternative "signaling-based" logic of the liberal peace, based on Fearon's (1994) work on signaling and interstate conflict. In its general sense, the signaling theory argues that all states always want to avoid interstate conflict. Interstate conflict occurs primarily because states fear being on the losing end of any negotiations: they fear making bargains that grant the other state more than the share it deserves. States will sometimes choose war, or fall into war, as bargaining fails because of states' inability to negotiate effectively and truthfully and avoid "bad" bargains. In other words, if a powerful and a weak state disagree, both would want to resolve the disagreement without violence (which harms both and has costs even for the victor). The powerful state knows that it should get more out of the bargaining than the weak; it is more powerful and can use force to "get" what it wants (at a cost). However, both sides know that, if the powerful state demands too much, the weak state will prefer fighting to giving up what is demanded. In theory, if the weak state truthfully tells the powerful just what it is (minimally) willing to accept, both sides can quickly conclude a negotiation and be (relatively) happy. But the weak state does not want to give up very much, and has an incentive to lie to the powerful state about what capabilities and resolve it has (raising the expected cost to the more powerful state if it chooses to force a resolution, which in turn raises the amount the powerful state will offer to avoid having to use force). The powerful state, thus, has no reason to listen to the weaker state (everything the weaker state says is lies, reasons the powerful state, in order to get a better bargaining outcome for the weaker state), and thus states enter into

international conflict when they may be able to avoid it. Fearon reasons that if either state was able to make statements that the other side could actually believe (credible statements), then real information about credibility and capabilities could be conveyed, which would improve the probability of a non-violent negotiation being successful. One way of making a statement credible is to have information conveyed in a costly manner: statements that cost a country nothing (some public statements, etc.) mean nothing, while statements that cost a state something to make are seen as being more credible.

Trade, in this context, serves as a way of conveying credible information. States with trading relationships that act in ways that threaten to end the economic relationships are credibly conveying information about intentions and capabilities that states that lack trading relationships cannot. States that take actions (continuing a disagreement, threatening force, severing a relationship, imposing sanctions, et cetera) that increase the probability of an ongoing economic relationship ending are sending, per Fearon, signals intended to convey credible information that allow both states to bargain more effectively and resolve whatever outstanding issues exist in a non-violent manner. States without an economic relationship do not have this means of credibly conveying information, and are thus more likely to fall into interstate conflict.

Gartzke *et al*. (1999) empirically demonstrate that pairs of state that trade at high volumes (and are thus positioned to credibly communicate effectively) are less likely to engage in interstate conflicts than pairs of states that do not have high volumes of trade. Gartzke *et al*. (1999) cite the Fearon (1994) article as the theoretical basis for the signaling argument, and outline the relationship between trade, conflict, and signaling in a manner similar to that outlined above. The problem is that these results (which, for the

most part, replicate other liberal peace studies; Oneal and Russett, 2003, 2001) can be explained equally well by the interest-based logic as well as the signaling-based logic.

This dissertation, using a direct test of the two liberal peace logics, found that the Gartzke *et al*. (1999) logic of the liberal peace has substantially more empirical support than the interest-based logic.

This implies, in the manner of the paragraph from Mansfield and Pollins (2001) cited in the introductory chapter, that further exploration of the causal link between trade and interstate conflict should focus, exclusively, on issues of signaling (signals sent, signals received, signalers and receivers). The evidence presented here is, admittedly, circumstantial; proof of the causal linkage of the liberal peace necessitates a research design that is detailed enough to examine specific signals in specific conflicts and at the same time broad enough to encompass different polities, stages of economic development, power status, region, and temporal era. In point of fact, further research to expand our causal knowledge of the liberal peace requires something akin to case studies, or at least a focused set of cases for statistical comparison. The "black box of the state" needs to be opened in order to fully understand the signaling necessary for the liberal peace to operate.

As a secondary goal, given the results here, previous conclusions may need to be revisited. As noted, most of the findings that establish the liberal peace (Russett and Oneal, 2001; Oneal and Russett, 1999b) assume (rather than directly test) the causal relationship underlying the empirical association. The results of this dissertation do not undermine the general claim that there exists a liberal peace. Rather, it raises questions

about why a liberal peace exists, and suggests the need to re-examine the underlying traditional explanation and consider an alternative logic.

Beyond questions related directly to the liberal peace, the results here open up the possibility (and only that) that signaling is a critical component of interstate conflict, and that the Fearon (1994) model of conflict is an accurate representation of the logic that allows interstate conflict. Thus, investigations of other aspects of interstate conflict (beyond the liberal peace) are possible. While signaling-based explanations have been put forward for the democratic peace (Schultz, 1999), perhaps signaling-based explanations for other factors (power status, enduring rivalries, territory, balance of capabilities, etc.) could be pursued. While that seems a grandiose claim, it is not so far fetched to argue that, at a minimum, peace researchers more vocally examine signaling-based logics for interstate conflict.

That, however, is a very broad perspective. The crucial fact to take away from this section is recognition that, at least with respect to the liberal peace, the empirical evidence clearly showed states act as if they are signaling (in the post-1950 era).

9.3 The Logic of the Test

The foundations for the conclusions in the previous two sections were based on the research design presented in Chapter 4. It is worth reviewing the logic that led to the creation of the tests in Chapters 5 through 8, and the creation of the variables used in those tests.

Given that the two underlying logics of the liberal peace have pointed to the same conclusions (the liberal peace exists) in previous research, what is needed is a different research design, derived from the two logics, that empirically tests their accuracy rather

than an additional test of the liberal peace itself. I argued that by examining the actions of directed-dyads (not nondirected dyads) while measuring the degree of unbalanced trade, the two logics of the liberal peace would point to those states taking different actions with respect to decisions to initiate and use higher levels of hostility in interstate conflicts.

The interest-based logic of the liberal peace argues that states that benefit from trade will have an incentive to avoid actions that will remove or harm those economic benefits. States that are economically dependent on a partner, by this logic, would have the greatest incentive to avoid harming that economic relationship: the cost of disrupting trade, because of the economic dependence, is so high that the state would be forced to avoid any action (like initiating or using higher levels of hostility in an interstate conflict) that has the potential to dampen trade with the partner.

The signaling-based logic argues that dependent states, alternatively, should be more likely to initiate and "push" interstate conflicts to higher levels of hostility. The logic of the signaling argument is that all states want to avoid conflict, and seek ways to credibly communicate with other states to facilitate fair bargains (where states are equally dependent, signaling is equally credible, and a liberal peace results). One way of making credible statements is to make the statements costly: if a dependent state acts in a manner that threatens to end or reduce the economic relationship they are dependent on, then the state is seen as making costly communications that serve to make it more credible. By this logic, dependent states have, because of the dependent relationship, a means to credibly convey their capabilities or resolve that non-dependent states do not. Thus, dependent states are more likely to initiate or employ higher levels of hostility in

interstate conflicts as a signal that enables both states to bargain more fairly and effectively.[152]

For dependent states, the interest-based logic argues for the avoidance of all interstate conflict in the name of avoiding harm, while the signaling-based logic argues for more conflictual actions that allow credible communication and a better resolution of the outstanding disputes. This empirically testable difference between the two logics of the liberal peace forms the basis for the dissertation: what effect does the level of dependence of one state on another have on the probability of the first state initiating or using a higher level of hostility in an interstate conflict against the partnered state?

With that central question in mind, the research design (Chapter 4) describes several statistical tests to determine the correlations between the level of dependence of one state on another and the probability of the state either initiating an interstate conflict or using higher levels of hostility against the other state if an interstate conflict has begun. The key difference between the statistical models used in this dissertation and those used in other liberal peace research (for example, Oneal and Russett, 2003) is in terms of case selection. Previous research has tested the liberal peace by asking whether the measured volume of trade between pairs of states makes those pairs more or less likely to engage in interstate conflict. Testing the interest-based and signaling-based logics requires knowledge about the level of dependence of one state on another, not about the overall level of trade. Hence, while other liberal peace studies use dyad-years, the cases studied here are directed-dyad years (specifically identifying which member of the dyad is identified as the "primary state" and which as the "partner state"). The directed-dyad

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[152] This explanation is a distillation of the complete signaling-based logic presented in Chapter 4. For further details, see that chapter.

records the dyad-level data from the perspective of each state (How dependent is Canada on the US? How dependent is the US on Canada?), where the nondirected-dyad ignores the individual state perspectives.[153] This is not to say that nondirected-dyads are an inferior method of studying the liberal peace, only that they do not allow testing of the signaling-versus-interests hypothesis presented here, while directed-dyads do.

The correct way to measure dependence is debated in the peace research community. The most common measure (*trade dependence*), drawn from Oneal and Russett (1999c) and Russett and Oneal (2001) uses the volume of trade between states as the numerator and the GDP of the state as the denominator. An alternative measure (*trade concentration*), drawn from Barbieri (1995, 1996, 2002) uses the volume of trade between states as the numerator and the volume of total trade for a state (trade with all partners) as the denominator. *Trade dependence* measures the importance of trade with the partner state as a percentage of overall economic activity, while *trade concentration* measures the importance of the partner to overall trade. Unfortunately, measures of *trade dependence* and *trade concentration* alone are not sufficient indicators of unbalanced trade. Thus, using *trade concentration* and *trade dependence*, weighted indices were created that expressed the relative strength and balanced of trade between the primary and partner state in the directed-dyad. It is these weighted indices (the *weighted index of trade dependence* and *weighted index of trade concentration*) that are the variables used in the critical chapters of the dissertation (Chapters 5 through 7).

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[153] The accepted approach when using dyads is to calculate the data from both states' perspectives (as directed-dyads do), compare the two numbers, and use the least favorable (lowest or highest, depending on the variable) for testing the hypothesis. Russett and Oneal (2001) call this the "least common denominator" approach. See Chapter 4 for an extensive discussion.

Additionally, there are numerous sources of trade and GDP data available for use, complicating the question of how to measure dependence of one state on another. The raw data on the volume of trade between states come from the International Monetary Fund (for 1948 and on; those few studies that look back earlier use data generated by the League of Nations). However, these raw data must be translated into usable form for academic research. Several researchers have done this, and there is debate about their different methods. The most widely used data come from Gleditsch (2002). He uses outside sources (World Bank, CIA Fact Book, etc.) to supplement some of the missing IMF data, and uses statistical techniques to fill in other missing data. In addition, the GDP data that Gleditsch provides are adjusted for the differing cost of living around the world (while the IMF based trade data are not). Barbieri (1995) offers a set of IMF-derived trade data that are not supplemented by other sources and with no interpolated or extrapolated data. Additionally, her measures of GDP are (like the IMF data) unadjusted for differing costs of living. I draw on both sets of data to test the comparability of the data sets: if the different data sources all lead to similar statistical results, then the debate over data can be left behind. If the different sets of data produce different results, why? What differences in the two data sets produce the differences in results? Finally, Oneal and Russett (1999) have used a data set similar to Gleditsch's (subsequent to the 1999 publication they use the Gleditsch data), but draw upon League of Nations' data to generate both *trade concentration* and *trade dependence* variables back to 1885, which neither Gleditsch nor Barbeiri do.[154] However, the Oneal and Russett data look only to "politically relevant" dyads (not the entire set of dyads, as do the Barbieri and Gleditsch

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[154] Barbieri has data on *trade concentration* back to 1870, but not *trade dependence*. Her data are used as well in tests before 1950 as a supplement to the Oneal and Russett data.

data), and are thus not directly comparable to those larger data sets. The Oneal and

Russett data are used substantially to test the actions of states before 1950 in this

dissertation. Finally, other independent variables that previous liberal peace research has

shown to be relevant are included to make the analysis more comparable to previous

investigations. The complete discussion of the research design is in Chapter 4.

9.4 Conclusions to "Boundary Conditions" Issues

If one rejects the framework of the dissertation (argued in Chapter 4) for the

testing of interests versus signaling explanations of the liberal peace, then the empirical

results in this dissertation at least provide some limits to the benefits of trade for

interstate conflict. This is what Mansfield and Pollins (2001, 2003) describe as the

"boundary conditions" of the liberal peace: areas where the "normal," simple liberal

peace logic (more trade equals more peace) begins to break down, and a more

complicated relationship between trade and conflict begins. This dissertation is not the

first to look at this subject, though other researchers have not generally used the

"boundary conditions" phrase. For example, Hegre (2000) re-examines the liberal peace,

introducing a measure of development for the states in the dyad. He finds that there is a

liberal peace (trade does restrain interstate conflict initiation), but that the more

developed the dyad, the greater the effect of trade. Thus, a "boundary condition": the

richer the dyad, the more trade can bring peace.

The approach in this dissertation is to look to specific types of trade (balanced

versus unbalanced) as opposed to specific types of states (Hegre's developed versus

underdeveloped states). There is no value judgment attached to this; both are examples

of testing the "boundary conditions" of the liberal peace.

The results from Chapters 5 and 6 suggest that unbalanced trade creates a significant limitation to the general results of liberal peace studies (trade brings peace). With respect to the tests of interstate conflict initiation, the results of Chapter 5 indicate that the more dependent a state is on another (in terms of the imbalance and significance of the trading relationship), the greater is the likelihood that it will initiate a militarized conflict against that other state. Chapter 6 indicates that this unbalanced trade relationship also brings a greater likelihood that the dependent state will employ high levels of hostility against its trading partner if militarized interstate conflict occurs. These results are generally true whether the tests are run with the trade concentration-based measures of unbalanced trade or the trade dependence-based measures of unbalanced trade.

This is a significant limitation for the liberal peace. These empirical results argue that a universal push for greater global trade (as, for example, free-trade zones implicitly suggest) can result in a poorer overall security situation (whatever the economic effects), if trade rises for some states, but not for others. As rising trade results in greater imbalance combined with greater dependence, increased likelihood of conflict and more violent conflict results. More simply, trade is not a universal pacific benefit.

Beyond the "big" results from the chapters, the last section of Chapters 5 and 6 examined smaller sub-groups of states, and what the relationship was between those types of states and unbalanced trade and interstate conflict.

Table 9.3: Results of "Boundary Condition" Tests from Chapters 5 and 6

Boundary Condition: Unbalanced Trade And:	Effect on Conflict Initiation	Effect on Conflict Hostility
Democracy	Autocracies more likely to initiate	Anocracies more likely to use higher levels of hostility
Wealth	Rich states more likely to initiate	Middle Income states more likely to use higher levels of hostility
Alliance	No Security Alliance more likely to initiate	Security Alliance more likely to use higher levels of hostility

The results from both chapters together do not suggest any consistency across the tests of initiation and hostility. No single factor either increased or decreased the odds of initiation *and* higher levels of hostility in interstate conflict. The strongest statements (accurate for both initiation and hostility) which can be made with respect to the results of the tests are ones about the variables not discussed in the table above: being a democracy with unbalanced trade neither increases nor decreases odds of initiation or higher levels of hostility in a resulting conflict (being an anocracy makes you more likely to use higher levels; being an autocracy makes you more likely to initiate); being a poor state neither increases nor decreases your odds of conflict (being rich makes you more likely to initiate; being middle-income makes you more likely to use higher levels of hostility). However, these are not strong statements.

The key to these conclusions is that the results of Table 9.1 are filtered through unbalanced trade. These results do not argue for general conditions (i.e., all democracy brings peace), but for how unbalanced trade affects the odds of initiation and greater hostility in the presence of democracy, security treaties, and wealth.

These results do argue that the relationship between trade and interstate conflict is not a simple one, and is likely more complex than previous liberal peace studies have suggested. For example, Russett and Oneal (2001) have argued that democracy acts with

trade and joint international organization membership to form a "Kantian" peace. The findings in this dissertation indicate that democracy, for states with highly unbalanced trade, has no additional pacific benefit (in some conditions, democracy increases the odds of conflict). While this result does nothing to either confirm or rebut their thesis, the results do add limits to their conception of how trade and democracy affect peace, which is a significant revision of their conclusion.

Hegre (2000) has argued that the liberal peace is primarily a characteristic of relationships among the richer states, and trade does little to impede conflict for poorer states. The findings in this dissertation suggest that richer states, in the presence of unbalanced economic relationships, are more likely to initiate conflicts. This, once again, argues for a limitation of previous conclusions: rich states do have a different trade/conflict relationship than poor states (Hegre, 2000), but unbalanced trade dampens the benefits of trade for the rich states, undermining the pacific benefits of trade for that category of states. It should be noted that, empirically, richer states are far less likely to become economically dependent – given the size of a rich state's GDP, having a significant percentage of the total GDP tied up in trade with a single other partner is difficult. Thus, the caution of unbalanced trade for rich states may be more theoretical than practical.

Finally, Benson (2004) argued that both security and economic ties have pacific effects at the dyadic level (her article also argues that ties to a system leader have effects as well, but that is irrelevant for this discussion). The results from Chapters 5 and 6 do not disagree (though in no case did a security alliance reduce odds of initiation or greater hostility). However, the weighted index of trade concentration was positive and

significant for most tests of those states with security ties, arguing that when trade is highly concentrated and unbalanced on a partner with which the state has a security tie, there is an increased risk of conflict initiation and greater hostility. These results were not true for the tests using the weighted index of trade dependence. Once again, the "boundary conditions" for where trade ceases to bring pacific effects are somewhat narrowed by the results here, though not substantially.

Finally, I tested to see if there were regional variations for the general results. This was a very simple five-region grouping: America (North and South), Africa (includes Egypt), the Middle East (ends at Iran), Europe (includes all former Soviet republics and Soviet satellite/client states in Europe and Russia), and Asia (begins at Pakistan/Afghanistan and all countries to the east). There was no consistency of results for regions: no single region was more or less likely to increase the odds of initiation *and* higher hostility. The closest was the Americas, where three of four tests of hostility produced statistically significant, positive associations, though only a single test of initiation found the same. All other regions had more mixed results. Thus, it is difficult to avoid the conclusion that states with high levels of unbalanced trade in no region stand out for being more (or less) prone to interstate conflict and increased conflict hostility.

I make no claim to have exhaustively explored the extent of the "boundary conditions" for the liberal peace. The issue of the "boundary conditions" was separate from the primary purpose of the dissertation: interests versus signaling. However, I believe that much more can be discovered about the liberal peace by explicitly paying attention to the limitations of the general "trade brings peace" point of view. Finding the "boundary conditions" is not an attack on the liberal peace, but an attempt to describe the

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limitations. This has two purposes. First, by describing and explaining where the liberal peace breaks down, we gain a greater understanding of the liberal peace itself. Given the extensive criticism of the liberal peace (from Chapters 2 and 3), it is clear that even if one accepts the empirical findings, there is no general agreement on the underlying causal connections. Hence, an exploration of the limits to the liberal peace provides ways of exploring how states behave with respect to trade and conflict that will (hopefully) lead to better explanations and understandings in the future. Second, to the degree that the liberal peace is used as a policy initiative, it is important to understand its limits. As the "boundary conditions" at the center of this dissertation show, improving trade between states when that trade is unbalanced can lead to greater likelihood of interstate conflict rather than less. Only balanced trade (small or large) is likely to reduce the odds of interstate conflict (and reduce the severity of conflict if it begins).

9.5 Conclusions to Sources of Data Issues

The original hope in including, for the purposes of empirical analyses, two sources of data (Gleditsch and Barbieri), was that the empirical findings would be consistent regardless of data source, indicating that debates over the greater validity of one source of data compared to the other were irrelevant. The results did not meet that hope. It is clear that the Gleditsch and Barbieri data can produce significantly different results when testing trade-dependence based measures, but substantially the same results when testing trade-concentration based measures.[155]

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[155] The Gleditsch and Barbieri data produced similar results (for both trade-concentration based measures and trade-dependence based measures) in the tests of initiation (Chapter 5), but very different results (for the trade-dependence based measures; the trade-concentration based measures for both data sets remained similar) for the tests of hostility (Chapter 6).

The two data sets differ substantially in two areas: first, Gleditsch uses interpolation, extrapolation, and non-IMF sources of data to significantly expand the number of cases in his data set (using round numbers, there are approximately 900,000 valid Gleditsch cases and only about 250,000 cases for Barbieri)[156]; second, the trade data used by Gleditsch (and Barbieri) are not adjusted for cost-of-living price changes, while the GDP data used by Gleditsch are adjusted for purchasing power (Barbieri's source of GDP data uses unadjusted figures).[157]

Results from Chapters 5 and 6 (Tables 5.4 and 6.4) indicate that when the Gleditsch and Barbieri trade concentration-based weighted indices (which use only trade data and do not mix adjusted and unadjusted data in both data sets) are limited to only cases for which both the Barbieri data set and the Gleditsch data set have valid data, there are no significant differences in results. This is not the case for tests using the indicator of trade dependence. For the latter, even when the cases analyzed are limited to those for which Barbieri has valid data, the empirical results are substantially different depending on whether the weighted index of trade dependence is constructed from Gleditsch or Barbieri data. This clearly shows that the data sets are different.

Further testing in Appendix 1 reveals that the adjusted GDP data used by Gleditsch differ substantially from the unadjusted GDP data used by Barbieri; moreover the difference was most apparent for states with smaller GDPs (at higher values of GDP, the difference between adjusted and unadjusted data was smallest). For small GDPs, adjusting for cost of living raises the overall GDP value. When this is translated into the

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[156] This difference is most significant for the tests of initiation, where all possible cases are used. The difference in the number of cases for tests of hostility is substantially smaller (though the Gleditsch data set is still larger, in terms of the number of cases).

[157] In order to create any of the trade dependence based measures, both trade and GDP data are required. Only trade data alone is necessary for the trade concentration based measures.

measure of trade dependence, it has the effect of making the Gleditsch-based trade dependence value smaller (trade dependence is trade divided by GDP; as GDP gets larger for poorer states, the effect is to make the overall trade dependence value fall) than the equivalent (same case) Barbieri-based value for the same case. Additional analysis showed that this difference between adjusted and unadjusted GDP had little effect on the tests of initiation, but substantial effects on the tests of hostility.[158]

The end result of all this analysis is to leave the statistical study of the liberal peace in worse shape. I argued that the Barbieri data (with unadjusted GDP) were more accurate than the Gleditsch data, but the Gleditsch data were more complete than Barbieri. Both major liberal peace data sets (though the Gleditsch is used far more than the Barbieri) have flaws, and researchers choosing between them must choose, in a sense, which flaws they prefer. Using the Barbieri data allows researchers to construct more valid indicators of trade dependence-derived variables, but limits them to fewer cases. Using the Gleditsch data allows researchers to use data that approximates the universe of cases, but the adjusted GDP data introduce a bias to trade dependence measures that can generate inaccurate results.

The differences between the two sets of data were starkly demonstrated in the test of openness (Rosecrance, 1986) on conflict initiation in Chapter 7. Using the Gleditsch data, openness was significantly and positively associated with conflict initiation (a finding at odds with most existing empirical research), while using the Barbieri data, the

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[158] The difference between the adjusted and unadjusted GDP led, in Chapter 6, directly to the finding that there was not a statistically significant association between the Gleditsch-based measure of trade dependence and the level of hostility in interstate conflict, yet there was a statistically significant, positive relationship when the Barbieri-based measure was employed.

same variable was significantly, but negatively associated with conflict initiation. The choice of data leads directly to radically different conclusions.

While the comparison of data sources was a peripheral part of the dissertation, and has no policy or larger political science relevance, it is the most significant result with respect to liberal peace research. At a minimum, the tests here demonstrate that just choosing one set of data over another can generate substantially different results. I make no value judgments concerning whether the Gleditsch or Barbieri data are "better" (they both have problems that lead to different results). The important point is that results (and conclusions drawn from them) can differ substantially depending on which set of data is chosen. Thus, previous conclusions of the liberal peace that are based on either data set should be seen as suspect until data issues are resolved.

Resolution of the data issue is less complicated than might be imagined. Tests in this dissertation have demonstrated that Gleditsch's interpolation/extrapolation and substitution approach did not change results substantially when compared to Barbieri's data (all comparisons of trade concentration only, so there are no issues of adjusted versus unadjusted data). While I would hope other researchers would undertake their own comparisons, these preliminary results argue that expanding the number of cases through the Gleditsch techniques is a reliable and valid method. Thus, there seems (subject to some confirmatory tests by other researchers) no objection to that aspect of the Gleditsch data. However, the use of adjusted GDP data is clearly problematic. The solution is to use Barbieri's source of GDP data (The World Bank) as opposed to Gleditsch's source of GDP data (the Penn World Tables), and then use the

interpolation/extrapolation and substitution method to expand the GDP data so that they cover as large a selection of cases as does the (uncontroversial) trade data.

That solution is beyond the scope of this dissertation, but is practical in terms of a next step for further research. Given the number of studies that rely on the Gleditsch data, and the problems revealed in this dissertation, resolution of the data issue must be a high priority for liberal peace research, as it has the potential to overturn some previous empirical conclusions.

9.6 The Liberal Peace and Peace Research

Overall, the results discussed here (that indicate a greater imbalance of trade is associated with higher likelihood of conflict initiation and greater conflict hostility, with caveats) are not opposed to the general liberal peace finding that more trade leads to more pacific relations.

The critical difference is in the cases examined: the more numerous results (Oneal and Russett, 2003; Barbieri, 2002) use nondirected-dyads and find that the greater overall trade in the dyad, the less likely any militarized dispute is to break out between the pair, where the results here talk about the specific actions of states with partners (directed-dyads) and find that the more a state engages in unbalanced trade with a partner, the more likely a militarized conflict is and a more hostile conflict if one results. Both statements can be true: pairs of trading states are less likely to engage in militarized interstate conflict and dependent states are more likely to initiate and use higher levels of hostility in ongoing interstate conflict.

This dissertation is directly concerned with the question of the underlying logic of the liberal peace. The preponderance of evidence as outlined above, I argue, indicates

that the presence of an economic channel for states to signal credibly is the more likely explanation for the results seen by other prominent researchers (Oneal and Russett, 2003; Russett and Oneal, 2001; Barbieri 2002) than the more widely cited interest-based logic that forms the basis for most of the previous cited studies. It is not proof of that channel, as I note, since that would require a research design that examined micro-level actions of specific decision-makers, interest groups and branches of government. The results, however, are clearly suggestive of further research in that direction (as opposed to research that focuses solely on the actions of economic interest groups and their connection to decision-makers, as the interest-based logic of the liberal peace would argue).

This has implications for the study of the liberal peace. While the signaling-based logic does not require any detailed re-examination of the empirical results found by other researchers, the results here indicate that the field may need to re-examine the logics for those results. To take a single example, Russett and Oneal (2001) offer the liberal peace as part of the Kantian peace, where trade serves as one leg of a tripod (international organizations and democratic states are the other legs) that leads states towards peace. The rational for Russett and Oneal is explicitly interest-based: they point to the interest-based arguments of Kant as the basis for their research program. If, in fact, states act more with signals in mind than interests, what does that do to the Kantian peace? Empirically, the results remain the same, but conceptually, one leg of the tripod is suspect.

Beyond the questions of interests and signaling, this dissertation also begins to empirically define the "boundary conditions" of the liberal peace. Given the results here,

trade is not universally or monotonically pacific. Unbalanced trade can result in a greater

likelihood of interstate conflict, as can the economic dependence of one state on another.

A state that trades with a single or few partners is more likely to engage in interstate

conflict than a state with a balanced and diverse set of trading partners. While there are

clear economic and political benefits to trade, as well as pacific benefits, trade is not a

universal good – a panacea for trouble in the world. What are the other limiting

conditions for trade? When, and for which states, does trade create increased chances for

interstate conflict? What are the other "boundary conditions" for trade?

The pacific effects of trade are not the same for every state. There is no uniform

and monotonic association between increased trade and greater peace. This dissertation

shows that trade does have potentially negative consequences for international peace –

that states in unbalanced trading relations are more likely to initiate an interstate conflict

and use higher levels of hostility during that conflict with a trading partner. Trade may

bring more pacific relations between states that jointly and evenly trade, but if the trade is

unbalanced the more dependent state will be more likely to begin an interstate conflict.

Thus, we can already begin to qualify the previous liberal peace results: instead of "trade

brings peace," more accurately it is "significant, balanced trade brings peace."

Significant trade with a partner allows effective signaling (both states in the dyad can

make credible statements, making successful negotiations more likely); significant trade

with many other states makes that state more "open" (see Chapter 7), which reduces the

likelihood of conflict; and balanced trade is less likely to lead to one state or the other

needing to initiate interstate conflict in order to credible signal.

This dissertation reflects a work in progress, and the results raise many questions that can be explored in future research. Beyond the general questions, there are specific avenues for research that are suggested by the results here. The significant shift in the correlation of trade to conflict that occurs when the data are separated into pre-1950 and post-1950 periods needs to be understood. Why the change? Is it related to cold-war issues, or another change in the international order?

Other questions for future research include: what effect does dependence have on the resolution and aftermath of conflict? Theoretically, what do the signaling and interest-based logics say about how states should resolve interstate conflict? Who should win, and what should the loser lose? Empirically, what are the effects of dependence on the outcome?

Outside of the scope of the liberal peace, there are further implications for the study of conflict. This dissertation argues for circumstantial evidence in favor of an application of the Fearon (1994) signaling logic: trade brings peace because states use the trading relation to send costly signals. If states signal to avoid interstate conflict in this case, how does that change the general peace research community understanding of other debates on interstate conflict (e.g., democratic peace)? If states use economic channels to signal to avoid conflict, what other channels allow costly (versus costless) communication, beyond trade? I do not make grand claims that this dissertation has proven that signaling beats interests (the results here are equivocal, as I have shown), but they do raise the question of how states do make decisions.

In particular, future research should study the micro-foundations of state decision-making within the liberal peace (and in interstate relations more generally). Why do

states pursue strategies that correlate with signaling logics, and how do domestic economic interest groups fit into an overall explanation of economics and conflict? Given the correlative evidence, the next logical step would be a detailed examination of specific cases, to begin to understand the process of how states take in information, communicate with adversaries, resolve internal debates, and make decisions – all in the context of their economic and trading circumstances. The correlative evidence here is suggestive, but tells us little about the actual process of signaling (or interests) in state actions.

This dissertation set out to begin the process of testing competing theories of how the liberal peace operates. The different logics were examined, and a specific empirical test that looked at the level of dependence of individual states with their trading partners was devised. The key question was how dependence correlated with the initiation of, and level of hostile action in, interstate conflicts. At the end, a tentative conclusion can be made: the empirical results suggest a correlation between dependence and more conflictual actions, which is correlative evidence in favor of the signaling-based logic of the liberal peace.

10.0 Appendix: An Examination of the Effects of "Adjusted" versus "Unadjusted" GDP figures, and Their Effects on the Analysis.

10.1 Introduction

This appendix contains some of the detailed analysis of the effects of "adjusted" versus "unadjusted" GDP measures, and details the effects each has on the data used in this dissertation. As was noted in Chapter 4, the two competing data sets used in studies of the liberal peace (Gleditsch, 2002, and Barbieri, 1995, 2002) differ in two main ways. First, Gleditsch "fills in" some of the missing trade data (data the IMF, the primary source for Gleditsch and the sole source for Barbieri, fail to contain) from outside sources (CIA World Factbook, et cetera) and employs interpolation/extrapolation of already existing data. Second, while both sets of data use GDP figures to generate measures used in analyses, the Barbieri source of GDP data is the World Bank, the Gleditsch source is the Penn World Tables. The Penn World Tables are more complete, but the GDP data are "adjusted" for purchasing power parity. Adjusting for purchasing power parity involves taking into account the differences in cost of living in different states (see Chapter 4). The Gleditsch data have been criticized for this, though no statistical analysis has been done to examine what specific differences exist between the two forms of GDP data, and what effect those differences create (if any) for liberal peace measures created with the two sets of GDP data.

This Appendix presents some of the analysis done for this dissertation on this subject. The first section (Section 10.2) examines the specific effects of "adjusted" versus "unadjusted" GDP when categorizing states into poor, middle and rich for the additional analysis in Chapter 5. The next section (Section 10.3) examines the specific

effects of using "adjusted" versus "unadjusted" GDPs in the creation of the economic

variables of importance in this dissertation (trade dependence and the weighted index of

trade dependence) that come from discussion of the issues in Chapter 6.

Overall, this Appendix will demonstrate that the use of "adjusted" GDP data

should be approached with extreme caution, as it has the potential to significantly bias the

measures used to test the liberal peace, and change the results of the statistical analysis.

10.2 Analysis of "Adjusted" and "Unadjusted" GDP in terms of Classification of States by Wealth (Chapter 5, Section V)

Section 5.5 of Chapter 5 seeks to classify states into three groups by wealth, and

uses GDP per capita as the means to classify states. Most analysts use the World Bank

GDP figures as measures of development.[159] On the other hand, almost all analyses done

with development indicators uses the actual data (GDP per capita) as an independent

variable in the empirical test. In this case, given that we want to look at groups of rich,

middle, and poor states, the GDP per capita figures as an independent measure

additionally does not suffice. The World Bank uses a complex system to determine,

yearly, where the dividing line of GDP per capita is between poor, middle, and rich

states. Unfortunately, they have not published the yearly cut-off points back to 1950

(when the data in this dissertation for GDP begin). The earliest published data (1987)

indicate that to be considered poor, a state had to have GDP per capita below about $400,

and rich states were above $6000. The 2000 survey uses $600 as the poor state cut-off,

and $10000 as the rich state cut-off. Since all the economic data used in this dissertation

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[159] GDP per capita, dividing GDP by population, is the most common approach to defining the relative wealth of states, though the United Nations uses a human development indicator scale that looks at social factors (average mortality, average literacy, et cetera). See Hegre, 2000, for a brief discussion of the different sources of data.

are in "real dollars" – the prices for trade and GDP are not adjusted for inflation, and cannot be directly compared across years – we need to adjust the World Bank cut-off points backward to 1950 to identify the groups. Using the growth of the rich state cut-off from 1987 to 2000 (from $6000 to $10000) gives us a growth of about 3% per year.[160] By this logic, the 1950 cut-offs for poor states is $134, and is $2000 for rich states. This is not a perfect solution, but one that is reasonably valid for a peripheral part of the dissertation.

However, one problem immediately arises. As was discussed before, the Gleditsch data use PPP (purchasing power parity) adjusted GDP figures. While this seemed to make no difference in the analysis early in Chapter 5, the cut-off points used by the World Bank for poor, medium, and rich states are based on measures of GDP that are unadjusted. Thus, using the GDP per capita figures derived from Gleditsch GDP data to categorize states can potentially lead to incorrect groupings: if the unadjusted GDP based cut-off points are used to classify adjusted GDP countries, some countries might be miscategorized. The Barbieri measures of GDP are not adjusted for purchasing power. As can be seen in Table 10.1, below, there is a significant difference if states are classified using the Gleditsch rather than Barbieri data. The comparison is limited to cases from 1950 to 1992, as the Barbieri GDP data stop in that year.

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[160] Estimating the global inflation rate at 3% is potentially an underestimation. If 3% understates global inflation over this time period, the consequence of this is that the "rich" states will be under-represented, while "poor" states will be over-represented. However, the World Bank's change in the "cutoff" line over this time frame represents a 3% inflation rate, and by assumption that should be sufficient for this analysis.

Table 10.1: Comparison of Rich/Poor classifications by Gleditsch GDP and Barbieri GDP Measures for the Population of States.
(Each cell records the percentage of overall directed-dyads, 1950 – 1992, that fall into each economic category.)

	Gleditsch GDP Data	Barbieri GDP Data
Countries classified as "Poor"	3.56%	25.17%
Countries Classified as "Middle Income"	77.53%	57.18%
Countries Classified as "Rich"	18.91%	17.66%

Classifying states based on adjusted GDP (the Gleditsch data) leads to a far smaller proportion of states being categorized as "poor." The proportion of states categorized as "rich" using the Gleditsch and Barbieri data is relatively equal. What are we to infer? At the most rudimentary level, we can conclude that the Gleditsch data and the Barbieri data generate different distributions. Based on what was said above, it is probably fair to say that the distribution presented in the right hand column of Table 10.1 (using the Barbieri data) more closely approximates the World Bank's conceptualization of poor, middle income, and rich than does the distribution based on the Gleditsch data. Again, the cut-points used to delineate the poor, middle income, and rich categories in the table are extrapolated from the World Bank's unadjusted GDP figures. The GDP data Barbieri uses are unadjusted and, therefore, the cut points are unproblematic. The Gleditsch data are adjusted to reflect purchasing power, and the adjustment appears to have had substantial effects on the classification of states. To understand the specific effects, we need to look a bit more deeply into the relationship between adjusted and unadjusted GDP.

Adjusted GDP reflects economic activity (barter trade, black market transactions, subsistence farming, and so forth) that is not included in official calculations of GDP. It

is normal and expected that GDP adjusted for Purchasing Power Parity will be higher for poor countries than for wealthy countries. The result is that some states that would be categorized as "poor" using the unadjusted GDP figures contained in the Barbieri data set "score" above the threshold and, therefore, are categorized as "middle income" in the Gleditsch data set. Because, for wealthy countries, official calculations of GDP are likely to accurately reflect the value of domestic economic activity, adjusted GDP is not likely to differ greatly from unadjusted GDP. Hence, whether one uses the Gleditsch or Barbieri data set should not substantially affect the proportion of states that will be classified as "wealthy."

Figure 10.1, below, graphically illustrates these points. To create this figure, the adjusted and unadjusted GDPs of each state were averaged over the available temporal span (1950 to 1992), creating a single value for the adjusted GDP and a single value for the unadjusted GDP of each state for the entire temporal span. Subtracting the average unadjusted GDP from the average adjusted GDP of each state, and dividing by the unadjusted GDP, gives the average percent that the adjusted GDP exceeds the unadjusted GDP.

Figure 10.1: Average Amount Adjusted GDP (Gleditsch) is above/below Unadjusted GDP (Barbieri) by COW State Number.

(Adjusted and Unadjusted GDPs averaged for each state from 1950 to 1992[161])
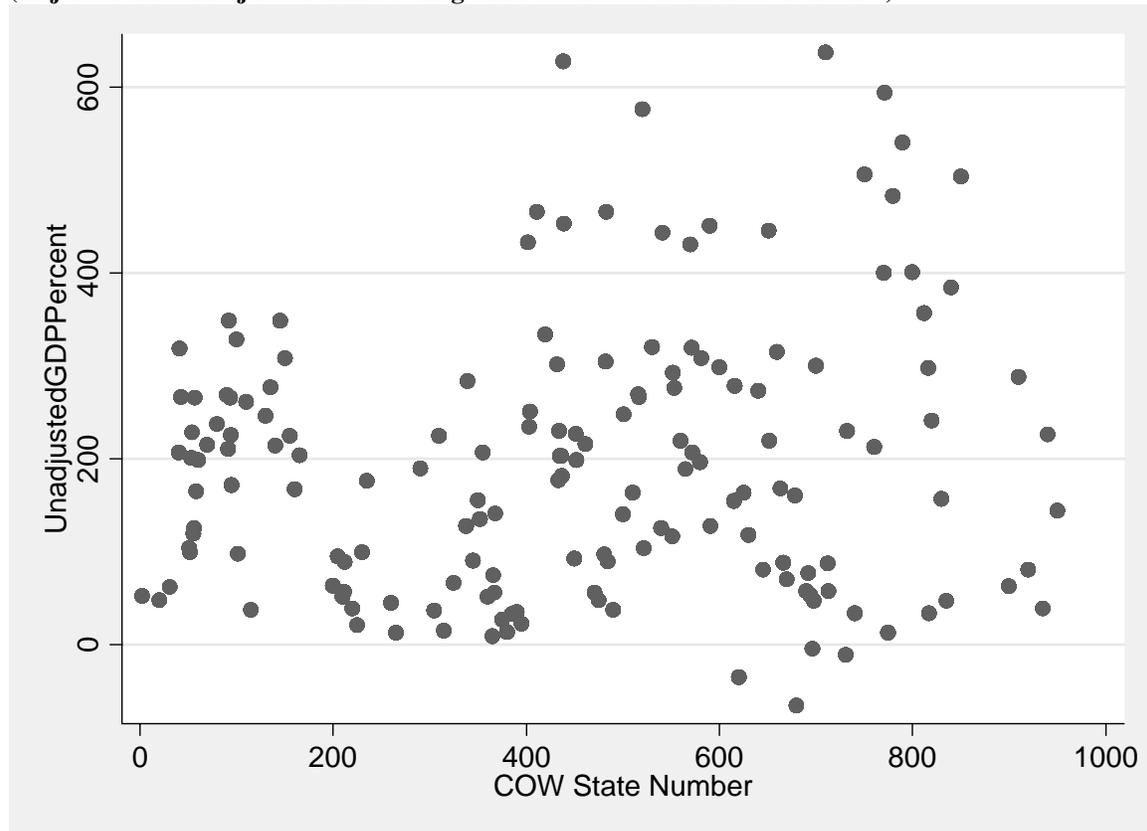
While the scatterplot contains "noise", a few conclusions are abundantly clear.

First, the adjusted GDP is almost always higher than the unadjusted GDP.[162] Second,

there seems to be a regional bias to the percent by which the adjusted GDP exceeds the

unadjusted GDP. Correlates of War (COW) country codes are grouped by region.

Numbers 1 through 199 cover all the countries in North and South America. Europe

(broadly defined, including Russia and the former Soviet republics) is in the 200 to 400

range. Africa numbers from 400 to 620, the Middle East from 625 to 700, and Asia

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[161] COW State numbers 781 (Maldives) and 811 (Cambodia) have been dropped from the figure. For both states, the average percent that adjusted GDP exceeded unadjusted GDP was greater than 1000. Presenting these two data points would compress the vertical scale in Figure 10.1 such that other detail would be lost.
[162] There are only four exceptions to this: Libya, People's Republic of Yemen, United Arab Emirates, and North Korea. All other states had percentage increases above 0, meaning the adjusted GDPs were, on average, higher than the unadjusted GDPs.

above 700. The key point, with respect to Figure 10.1, is that where the world contains larger numbers of "richer" states (Europe) we see small differences between adjusted and unadjusted GDP (the data points are closer to zero). Where regions of the world contain large numbers of poor states (Africa, Latin America), we see larger differences (the data points are much greater than zero). The gap between adjusted and unadjusted GDP is largest for "poor" states, and adjusted GDP is (almost) always larger than unadjusted GDP.

This "eyeball validity" test is supported by two simple ANOVA tests. First, ANOVA results indicate that the regional differences described in the preceding paragraph are statistically significant (the regions have statistically different average differences, beyond 0.05 significance).[163] Second, ANOVA results indicate that the average difference between adjusted and unadjusted GDP per capita for rich states is only 2.6%, while the same difference for middle income states is 107%, and 308% for poor states (and that these differences are statistically significant at the 0.05 level).[164] In short, knowing whether the state is "poor", "middle", or "rich" is a good predictor of how close the adjusted GDP is to the unadjusted GDP, and that the adjusted GDP is significantly different from the unadjusted GDP for the poorer states.

What are the implications of this for testing the liberal peace? This digression gives us pause about using certain Gleditsch-based trade data, specifically measures of trade dependence (and the weighted index derived from it). Gleditsch's trade dependence

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[163] The only exception is that the Americas were not significantly different from Africa. All other two-way comparisons showed a significant variation in the average difference of GDP. The raw ANOVA outputs are not reproduced in the dissertation.

[164] In other words, the adjusted GDP per capita for rich states exceeded the unadjusted GDP per capita for rich states by an average of only 2.6% (almost no difference). The adjusted GDP per capita exceeded the unadjusted GDP per capita for middle income states by an average of 107%, and by an average of 308% for poor states. These raw ANOVA results are not included in the dissertation.

measures divided unadjusted trade date (from the IMF) by GDP data adjusted for

purchasing power parity (from the Penn World Tables). Had Gleditsch adjusted the IMF-

supplied trade data, or used unadjusted World Bank GDP data, there would be no

problem. The difficulty is that by mixing the adjusted with the unadjusted data, Gleditsch

appears to have introduced an unanticipated bias. While this does not appear to have

affected the analyses reported in Tables 5.1 and 5.3 of Chapter 5, it is something that

surfaced in other tests and to which we need to be attentive.

10.3 Analysis of "Adjusted" versus "Unadjusted" GDP and the Effects on Measures of Trade Dependence and Interstate Conflict Hostility.

The empirical results in Table 6.4, Section 6.3, of Chapter 6 demonstrate that,

like the tests of conflict initiation, substitution of the Gleditsch data into the (valid)

Barbieri cases does not rectify the differences between the two sets of data. Even taking

into account the differences in cases (as Table 6.4, Chapter 6 demonstrates), the Barbieri

data set and the Gleditsch data set continue to produce different empirical results.

Given the results of Section 10.2 of this Appendix, the prime suspect for why the

two sets of data produce different results even when tested with substantially the same

cases is the difference between the "adjusted" GDP data used in the Gleditsch data set

and the "unadjusted" GDP data used in the Barbieri data set. Moreover, given that the

results produced with Barbieri data (Table 6.4, Chapter 6) produce a statistically

significant positive association between trade dependence and increased conflict hostility

for cases of ongoing interstate conflict, while results produced using the Gleditsch data

find no statistically positive association, the weighted index of trade dependence

produced with the Barbieri data must not only be larger than the corresponding (same

cases) weighted index of trade dependence produced with the Gleditsch data, but those weighted indices (high values in the case of the weighted indices produced using Barbieri data, low values when produced using the Gleditsch data) must correlate with highly hostile interstate conflicts.

This is testable. It is possible to directly compare the Barbieri value to the Gleditsch value of the weighted index of trade dependence for the same case. Table 10.2, below, breaks down by hostility category the differences between the two measures of the weighted index of trade dependence. Each of the first three columns represents a comparison between the Barbieri and Gleditsch measures of the weighted index of trade dependence: Column 1 records the number of times (per category of hostility) that the value of the Gleditsch index exceeds the value of the Barbieri index; Column 2 records the number of times the two indices are identical, and Column 3 records the number of times the Barbieri index exceeds the Gleditsch index. Column 4 records, for each hostility category, the ratio of the Barbieri index to the Gleditsch index.[165]

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[165] A ratio greater than one (1) means the average Barbieri index is greater than the Gleditsch index. A ratio that is a fraction indicates that the Gleditsch index is greater than the Barbieri index. It is possible for the average to be negative (see Row 4, below); this indicates that some of the ratios being averaged were negative (the ratio is the Barbieri weighted index of trade dependence divided by the Gleditsch index of trade dependence; if either one or the other of those measures is negative, the ratio will have a negative number) and large. These outliers are removed to facilitate the analysis. See Footnote 166, below.

Table 10.2: Comparison of Barbieri and Gleditsch Weighted Indices of Trade Dependence for the Population of all Directed-Dyads in an Ongoing Conflict.

(Columns 1 through 3 describe the number of cases in each category of MID hostility where the Gleditsch index exceeds the Barbieri index, where they are identical, and where the Barbieri index exceeds the Gleditsch index. Column 4 records the average ratio of Barbieri's index to Gleditsch's.)

	Column 1: Number of cases where Barbieri's trade dependence score is less than Gleditsch's.	Column 2: Number of Cases where Barbieri's trade dependence score equals Gleditsch's	Column 3: Number of cases where Barbieri's score exceeds Gleditsch's	Column 4: Average ratio of Barbieri's index to Gleditsch's index
Primary State takes no action.	160	17	175	4.69
Primary State Threatens Partner	42	1	15	0.23
Primary State displays force to Partner	172	9	130	1.79
Primary State uses force against Partner.	362	23	328	-4.81/3.78[166]
Primary State goes to War against Partner	21	0	15	1.39
Totals	757	50	663	0.55/3.42[167]

The results are clear. The first three columns demonstrate that Barbieri's index measurements are not, in terms of their raw numbers, greater than Gleditsch's. In only one category does the number of "Barbieri greater than Gleditsch" cases exceed the number of "Gleditsch greater than Barbieri" cases (the "no response" category). This means there is no "clump" of cases where (relatively) higher Barbieri values of the index correlate to higher levels of hostility, which would produce the results in Table 6.3, above. In a very rough sense, the numbers are about equal: for each category of hostility,

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[166] Two numbers are presented here. The first is the average of all the ratios of Barbieri's weighted index divided by Gleditsch's weighted index. The second is the same average with significant outliers removed. As you can see, the average ratio hovers very close to 3, while some of the outliers were less than –1000 or greater than 1000 (there were only four cases with ratios that high or low). Given that this is an average of all the ratios, the outliers have tremendous effect on the mean. In this sense, the second "corrected" number is a more accurate average than the first "uncorrected" number; it is much closer to the median ratio (1.70) than the "uncorrected" ratio.

[167] As Footnote 166 indicates, I removed some outliers from the "Use of Force" row in order to facilitate the comparison. The first number includes the outliers, while the second does not.

the Gleditsch measure exceeds the Barbieri measure about the same number of times the

Barbieri measure exceeds the Gleditsch.

However, Column 4 shows that, on average, the Barbieri index exceeds the

Gleditsch index significantly.[168] Moreover, the ratio by which the Barbieri measure

exceeds the Gleditsch measure varies by category of hostility. Other than the lowest

level of hostility (category 1, "no response", where the ratio of the Barbieri measure to

the Gleditsch measure was the highest, at 4.69 to 1), the ratio of Barbieri to Gleditsch

climbs generally as you move up the hostility ladder: 0.23 to 1 ("threat") -> 1.79 to 1

("display") -> 3.78 to 1 ("use"; outliers removed from the calculation of this ratio) and

then back down to 1.39 to 1 for the "war" category. In other words, climbing up the

hostility ladder is correlated with Barbieri's measure of the weighted index of trade

dependence getting (relatively) much larger than Gleditsch's measure of the same

variable. This seems a reasonable explanation for why the Barbieri data produce a

positive and significant result with the trade dependence index, while the Gleditsch data

does not: for the conflict sample, the Barbieri weighted index is highly correlated with

more hostile actions (thus, the Table 6.3, Chapter 6 results) while the Gleditsch weighted

index does not have that same degree of correlation between the measure and cases of

hostility (thus, the Table 6.1, Chapter 6 results).

The next obvious question is where these results come from. What the analysis

immediately above describes is a set of states where the measurement of their weighted

index of trade dependence with the Barbieri data is higher than the Gleditsch

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[168] While this would seem to be contradictory to the results in the first three columns, it is not. It is possible (and, given these results, is clearly the case) that while the Gleditsch measure exceeds the Barbieri measure for most categories of hostility, when the Barbieri measure exceeds the Gleditsch measure it does so significantly. Thus, while there are more cases where the Gleditsch measure exceeds the Barbieri, on average, the Barbieri measure is many times higher than the Gleditsch measure.

measurement of the same variable. While it is not immediately clear for which set of states this would be true, an initial working hypothesis is that it has something to do with the "adjusted" versus "unadjusted" GDP data issues that were discussed in Chapter 4, and initially investigated in Chapter 5. The Barbieri and Gleditsch data share the same IMF-supplied trade data, but differ in terms of the source of the GDP data used to create the trade dependence variables (and, hence, the weighted index of trade dependence). Previous analysis (Section 10.2 of this appendix) indicated that the "adjusted" GDP was different from the "unadjusted" GDP, but that at very low levels of GDP the difference was fairly pronounced, with the "unadjusted" GDP being larger than the "adjusted".

This leads to a working hypothesis to explain the results of Table 6.4, Chapter 6: that there are a larger-than-average number of poor (in the sense of low GDPs) states using higher levels of hostility. If this were true, it would explain the results from the table above (Table2), as well as the results in Chapter 6 (Table 6.1 and Table 6.3; summarized in Table 6.4) that find very different empirical findings when using the different data sets. The above analysis shows that the Barbieri weighted index is greater than the equivalent Gleditsch weighted index at higher levels of hostility. Two necessary conditions must be fulfilled for us to confirm that the "higher" Barbieri weighted index of trade dependence is associated with the "adjusted" versus "unadjusted" GDP debate: first, that the Barbieri weighted index of trade dependence is greater than the Gleditsch for poor states more than for rich states (if this is true, it will tie the values of the different weighted indices to the debate over "adjusted' versus "unadjusted" GDP); second, that a larger portion of poorer states are using higher levels of hostility than would be expected from the global population (if this is true, this will demonstrate that "adjusted" versus

"unadjusted" GDP had a direct influence on the Table 6.4, Chapter 6 results that showed

a clear disjuncture between the Gleditsch and Barbieri data sets). Put more simply, if

poorer states have higher Barbieri-supplied weighted indices of trade dependence, and if

poorer states use higher levels of hostility in interstate conflicts, then this explains why

Barbieri's data are the only data that find a significant association between the weighted

index and increased hostility in Table 6.4, Chapter 6.

There are two tests to perform. Working backwards, the first test will examine

whether there is any relationship between the relative wealth of the state and the level of

hostility the state undertakes in the ongoing interstate conflict. Table 10.3, below, uses

data not previously included in this dissertation – Real GDP per capita in 1996 dollars

from Gleditsch via the Penn World Tables. Unlike the GDP data used in the dissertation,

this GDP data is standardized to a single year (1996), making it comparable across years.

In other words, these GDP data take into account inflation. Dividing it by the population

in each state in each year provides a GDP per capita figure that is comparable across time

spans, making this statistic useful for determining whether people are actually getting

richer over time. Using simple ANOVA analysis allows comparison of the average GDP

per capita of the states in each category of hostility. One other note: the data used in this

comparison are the Gleditsch "adjusted" GDP data (though it is "real", meaning adjusted

for inflation, it is still also adjusted for purchasing power parity), and so the GDP per

capita figures for smaller states are inflated more than would be if we had equivalent

Barbieri/World Bank real GDP per capita data. This has no effect on the analysis, though

if "unadjusted" GDP per capita data were available, it would magnify the result found

below.

The results, while not always consistent, generally support the idea that states with lower GDP per capita use higher levels of hostility in interstate conflicts. More specifically, the average GDP per capita of states taking "no action" (MID category 1) and "displaying force" (MID category 3) are significantly greater (according to the ANOVA analysis) than those states "using force" and "war" (MID categories 4 and 5). The "threat of force" (MID category 2) is never significantly different than either more hostile or less hostile categories, and can be considered an outlier or evidence against the hypothesis.

Table 10.3: Comparison of Average Real GDP per Capita by Level of Hostility for All Directed-Dyads Engaged in Interstate Conflict.

(Gleditsch Data, 1950+; The results presented in Columns 2 through 5 represent the probability that the value in Column 1 is significantly different from the GDP per capita value referenced in the top of the column. In other words, looking across rows, what is the probability that the GDP per capita figures are statistically different from each other)

Hostility Category:	Column 1: Average GDP per capita, real 1996 dollars	Column 2: Probability of being significantly different from "Threat of Force" average GDP	Column 3: Probability of being significantly different from "Display of Force" average GDP	Column 4: Probability of being significantly different from "Use of Force" average GDP	Column 5: Probability of being significantly different from "War" average GDP
"No Action" (N=710)	6560.40	(0.71)	(0.10)	**(0.00)**	**(0.00)**
"Threat of Force" (N=112)	5669.19	(N/A)	(0.07)	(0.59)	(0.53)
"Display of Force" (N=672)	7463.38	(0.07)	(N/A)	**(0.00)**	**(0.00)**
"Use of Force" (N=1367)	4685.15	(0.59)	**(0.00)**	(N/A)	(0.98)
"War" (N=133)	4304.99	(0.53)	**(0.00)**	(0.98)	(N/A)

Nonetheless, there is a (general) monotonic progression of the average real GDP per capita as one moves from a low level of hostility to higher levels. Overall, this seems

sufficient evidence to warrant accepting the hypothesis that it is the smaller or poorer

states that are more likely to use the higher levels of hostility in interstate conflicts.

The second hypothesis to investigate focuses on whether those same poor states

have Barbieri-based weighted index of trade dependence scores that are significantly

different from the scores on the same Gleditsch-based variable, and whether the disparity

between the scores for the poor states is greater than for the rich states.

One way to investigate this question is to create a new measure that records the

ratio between the two weighted indices of trade dependence.[169] If the hypothesis is

correct, the ratio should be much larger for poorer states than for richer states, which

would indicate that the Barbieri measure differs from the Gleditsch measure more for

poorer states than for richer ones. There are several ways of separating the richer states

from the poorer ones. One way is to examine how the average ratio varies by region:

historically, some regions (Africa, for example) contain a larger proportion of poor states

than other regions (Europe, for example). Table 10.4, below, indicates the average ratio

between the two weighted indices in each region for the entire population (not just the

unbalanced directed-dyads), and for the cases where there is ongoing interstate conflict.

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[169] More precisely, the created ratio measure divides the Barbieri weighted index of trade dependence by
the Gleditsch weighted index of trade dependence for each case where both variables exist (i.e., neither
measure is missing). Given that the expectation is that the Barbieri weighted index exceeds the Gleditsch
weighted index, the ratio provides the number of Gleditsch index values needed to create a Barbieri index.

Table 10.4: Comparison of Average Ratio between Barbieri and Gleditsch Weighted Index of Trade Dependence by Region for the Population of Directed-Dyads.[170]
(1950+ Data Only, Outliers excluded – see above for explanation)

Region	Global Population (N=259085)	Ongoing Interstate Conflict (N = 2031)
America	3.24	2.99
Europe	1.82	5.07
Africa	3.32	3.08
Mid East	2.58	1.43
Asia	3.60	16.40

What this table indicates is that the samples of cases that experience interstate conflict seem to be different from the global population of cases, though statistically there is no difference (the average ratios were not statistically different from each other). Each cell in the table represents the average ratio by which the Barbieri weighted index of trade dependence exceeds the same Gleditsch measure (in all cases the average ratio is greater than one, meaning the Barbieri weighted index is always greater than the Gleditsch weighted index). For example, in the Americas the average Barbieri weighted index is 3.24 times greater than the Gleditsch weighted index in the global population of cases; in the interstate conflict sample, the Barbieri weighted index is 2.99 times (on average) greater than the Gleditsch.

What jumps out immediately is how much greater the Barbieri weighted index is than the Gleditsch in Asia in the conflict sample: the Barbieri weighted index is 16.4 times the Gleditsch in the conflict sample, where it is only 3.6 times as great in the global population column. Other than that, this analysis reveals little: some historically poor regions had their average ratio shrink in the conflict sample (Africa), while some historically rich regions had their average ratio increase greatly in the conflict sample

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[170] An ANOVA test was preformed to see if the average ratios in each region were significantly different from the average ratios in the other regions. No region was statistically different from any other.

(Europe) – both evidence that the Bariberi weighted index is not more distorted for poorer versus richer states than the Gleditsch data. This analysis, however, is limited: regions are not a perfect proxy for any economic or size based measure, and no region (either in the global population or conflict sample) was significantly different from any other region when these numbers were tested in an ANOVA test.[171]

Pursuing the theory that the difference between the Barbieri and Gleditsch weighted index is traceable to the bias introduced by Gleditsch's use of "adjusted" GDP data in poorer economies, the next test looks to compare the same statistic (ratio of the Barbieri to Gleditsch weighted index) by a different comparison variable: major versus minor powers. Again, while this is not a perfect substitution for rich versus poor, in general major powers are going to be the states with the largest and most robust economies, while minor powers will have smaller economies. If the "adjusted" GDP data is artificially inflating the trade dependence for poorer states, then the weighted index created from the biased trade dependence measure is likely to be artificially depressed[172], which would explain why Barbieri's weighted index (created from "unadjusted" GDP data) is always larger. Table 10.5, below, replicates Table 10.4, but divides the world into directed-dyads where the primary state is either a major or minor power instead of regions.

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[171] The standard deviations for these means are huge (sometimes above 5000), which reduces the ability of the ANOVA tests to find significance).

[172] The weighted index is created by taking the trade dependence measure for the primary state and dividing it by the trade dependence measure of the partner state, then taking the natural logarithm of that, and finally multiplying by the trade dependence of the primary state (see Chapter 4). For a given dyad the trade dependence score based on the Gleditsch data will be smaller than the score based on the Barbieri data, because the Gleditsch trade dependence measure is created with the larger "adjusted" GDP data. In other words, if the "adjusted" GDP data result in larger GDPs, then inflating the GDP will result in lower trade dependence for the primary state – and, consequently, a lower weighted index of trade dependence as well.

Table 10.5: Comparison of Average Ratio of Barbieri to Gleditsch Weighted Index of Trade Dependence by Major and Minor Powers for the Population of all Directed-Dyads

(1950+ Data Only, Outliers excluded – see above for explanation)

Power Status	Global Population (N=259085)	Ongoing Interstate Conflict (N = 2031)
Minor Powers	2.85	6.37
Major Powers	2.08[173]	2.08

Here is unequivocal evidence in favor of the hypothesis. Table 10.5 clearly indicates that the difference between the Barbieri and Gleditsch weighted indices is related to the economic size (rich/poor) of the country – significantly so in the case of the global population. For directed-dyads in which the primary state is a major power, the difference between the Barbieri and Gleditsch weighted index is much less than for dyads in which the primary state is a minor power. To the degree that "adjusted" GDP data introduces a bias into the measure of trade dependence and the weighted index of trade dependence, there is a greater bias in the minor powers than the major powers.

There is a potential flaw in the above logic: while it is clear that the "adjusted" GDP biases the trade dependence for smaller states, the weighted index is constructed through dividing the trade dependence of one state by the trade dependence of another. If both states are biased by the use of "adjusted" GDP data (in other words, if the directed-dyad is made up of two minor powers) then the overall problem might be minimized (not eliminated, as the bias affects the size of the GDP – even among smaller economies the amount the "adjusted" GDP will vary is not constant). In other words, if the partner state is more often a minor power then the bias will be reduced than if the partner state is a

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[173] The mean percentage difference between the Barbieri and Gleditsch weighted indices for the global population was statistically significant at the 0.04 level in a simple ANOVA test. The mean percentage difference for the conflict sample was not statistically significantly different; the smaller sample size combined with huge standard deviations explains that.

major power. Table 10.6, below, displays the percentage of cases for which the *partner*

state is a major power and minor power for the entire sample, for the conflict cases, and

for each level of hostility within the conflict cases.

Table 10.6: Comparison of Partner State Power Status - How often is the Partner a Major Power versus a Minor Power for the Population of all Directed-Dyads?
(Data from Gleditsch, 1950+)

	Percentage of Cases where Partner is Minor Power	Percentage of Cases where Partner is a Major Power
Global Sample (N=774098)	96.32	3.68
Conflict Sample (N=2424)	78.92	21.08
"No Action" Sample (N=543)	75.87	24.13
"Threat of Force" Sample (N=102)	83.33	16.67
"Display of Force" Sample (N=484)	79.13	20.87
"Use of Force" Sample (N=1181)	80.27	19.73
"War" Sample (N=114)	74.56	25.44

This table indicates that, compared to the global population of cases, the conflict

sample has a much higher percentage of cases where the partner state is a major power.

Major powers have larger GDPs, which should be less affected by the bias in the

"adjusted" GDP measures. Thus, in a sample that contains more major powers as partner

states than the overall population (which is what Table 10.6 demonstrates both for the

entire conflict sample, and for every level of hostility within), there should be less

difference between the Barbieri and Gleditsch weighted indices. The data (in Table 10.5)

indicate that the conflict sample is generally even more distorted than the global

population. This means that even though the conflict sample should be less distorted than

the global population, the fact that the difference between the Barbieri and Gleditsch

weighted indices is even greater means that there is terrific bias in the Gleditsch

"adjusted" GDP trade dependence and weighted index of trade dependence for the

sample of conflict cases tested here (and in Chapter 6).

Finally, it is possible to directly test whether there is bias in the Gleditsch

weighted index for poorer states by using the estimated categories from the previous

chapter. The World Bank, using "unadjusted" GDP data, categorizes the world into

"poor," "middle income," and "rich" states in every year going back into the 1950s.

While Barbieri's GDP data are the World Bank GDP data, there is no public information

concerning what GDP per capita cut-off points the World Bank used each year to create

the categories. As was described in Chapter 5 (and Section II of this Appendix, above), I

used what values were public (including a data point from the 1980s and one from 2000)

to extrapolate back to create an estimated cut-off point for every year from 1950 on, then

used the Barbieri GDP data (and population data, to create GDP per capita) to categorize

the states. This is not fully accurate, but a reasonable approximation of what the World

Bank has historically done in describing the relative wealth of the states in the world.

Table 10.7, below, uses those categories to generate the average ratio of the Barbieri

weighted index to the Gleditsch weighted index for the global population of cases by

wealth category, and compares those figures to the average ratio for the conflict sample.

Table 10.7: Comparison of Average Ratio of Barbieri to Gleditsch Weighted Index of Trade Dependence by World Bank Estimated Poor/Middle/Rich Income Countries for Population of Directed-Dyads
(1950+ Data Only, Outliers excluded – see above for explanation)

Wealth	Global Population (N=259085)	Ongoing Interstate Conflict (N = 2031)
Poor	4.79[174]	17.40[175]
Middle Income	2.71	2.57
Rich	1.46	1.49

These results clearly indicate that Barbieri's weighted index of trade dependence differs (significantly) from Gleditsch's weighted index as wealth of the state changes. Directed-dyads where the primary state is poor show the greatest difference, while directed-dyads where the primary state is rich show the smallest difference between the two measures. The average ratio within the population for directed-dyads where the primary state is poor is 4.79 – statistically different from the average for directed-dyads where the primary state is middle-income (2.71) or rich (1.46). However, the average ratio for directed-dyads where the primary state is poor within the conflict sample is an astoundingly high 17.4 – again, statistically different from the average middle-income ratio (2.57) or rich ratio (1.49).

The difference between the Barbieri weighted index of trade dependence and the Gleditsch weighted index of trade dependence is the GDP data (the trade data, as noted, is taken from the IMF for both). It is clear that adjusting for purchasing power parity creates GDP data that are higher than unadjusted GDP scores, especially for poorer states – that is the point of adjusting for purchasing power parity. It is clear, from the research

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[174] Per ANOVA test results, "poor" is statistically different from both the "middle" and "rich" categories at the 0.01 level. In addition, the "middle" is statistically different from "rich" at the 0.05 level.
[175] The ANOVA results here indicate that the "poor" category is statistically different from both the "middle" and "rich" categories at the 0.05 level. The "middle" category is not statistically different from the "rich" category, however.

above, that the inflated "adjusted" GDP creates trade dependence weighted indices (Gleditsch) that are substantially lower than those same weighted trade dependence indices (Barbieri) that are created from "unadjusted" GDP data. Moreover, due to a coincidence in the militarized interstate dispute (MID) conflict data, a greater number of poorer states use higher levels of hostility than expected.

The conclusion here is to justify the Table 6.4, Chapter 6 results that showcased the significant differences between the Barbieri-based measures and the Gleditsch-based measures. The difference between the Barbieri data and the Gleditsch data is not simply one of size (the Gleditsch data set contains many more cases than the Barbieri). There are, additionally and significantly, important differences between the GDP measures that both data sets provide. When these data are used in testing the liberal peace, significant differences in conclusions can result (as we have seen in Chapter 6, and again in the tests of "openness" in Chapter 7).

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