H-**Diplo** | **ISSF**

Roundtable, Volume VIII, No. 11 (2016)

A production of H-Diplo with the journals *Security Studies*, *International Security*, *Journal of Strategic Studies*, and the International Studies Association's Security Studies Section (ISSS).

http://issforum.org

H-Diplo/ISSF Editors: **James McAllister and Diane Labrosse**
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Introduction by James McAllister

Dale C. Copeland. ***Economic Interdependence and War.*** Princeton University Press, 2015.
ISBN: 9780691161594 (paperback, $32.95/£22.95).

Published by H-Diplo/ISSF on **29 February 2016**

Shortlink: tiny.cc/ISSF-Roundtable-8-11
Permalink: http://issforum.org/roundtables/8-11-economic-interdependence
PDF URL: http://issforum.org/ISSF/PDF/ISSF-Roundtable-8-11.pdf

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Introduction by James McAllister, Williams College

Dale Copeland's *Economic Interdependence and War* is an ambitious book that should receive close attention from both international-relations theorists and diplomatic historians. The author's main objective is to offer an alternative explanation of the relationship between commerce and international conflict, one that challenges both liberal and realist theories. In his view, liberals are correct to believe that increasing trade and investment flows can enhance the prospects for peace, but realists also have solid grounds for believing that increased economic interdependence can lead to conflict and war. It is because both theories are plausible, Copeland argues, that it is necessary to consider an additional variable that he defines as "a state's expectations of the future trade and investment environment" (2). When states have positive expectations about the future trade and investment environment, they are unlikely to resort to war. But if a state has negative expectations about the future, it is going to be more willing to consider war as an attractive policy option. It is this argument that Copeland tests against the historical evidence of great-power conflict from 1790-1991 and also applies to the future of U.S.-Chinese relations in the twenty-first century.

All the reviewers have some degree of praise for *Economic Interdependence and War*. Richard Maass argues that the book "is a landmark example of how diplomatic history and statistical findings can be productively married in the pursuit of a persuasive answer to an important question." Benjamin Fordham suggests that Copeland "advances the sensible argument that state leaders worry about future access to vital markets, and that their efforts to insure this access can lead them into conflict with other states." Patrick Shea emphasizes the methodological contributions the book offers to the field; in his view Copeland "provides a methodological bridge between those who favor a statistical approach and those who use case-driven approaches…his forty case study, medium- N approach is a methodological achievement."

To be sure, it is also true that all the reviewers have some important criticisms and concerns about various aspects of Copeland's argument. Shea is concerned that Copeland does not really answer the all-important question of how far states look into the future when making decisions related to war and peace. Maass identifies a variety of issues that he has with Copeland's theoretical chapters, which he suggests "overemphasize several factors that ultimately leave them less satisfying than they should be." Fordham, by far the most critical of the reviewers, argues that Copeland's "ambitious theoretical and methodological claims make for arresting reading, but they are not persuasive." In his detailed response, Copeland responds to these and other criticisms while also noting important areas of agreement among the contributors to the roundtable.

H-Diplo/ISSF thanks Professor Copeland and all the reviewers for contributing to a roundtable that will be of great interest to both international relations theorists and historians.

Participants

Dale C. Copeland is Associate Professor in the Department of Politics at the University of Virginia. He is the author of *The Origins of Major War* (Cornell University Press, 2000) and numerous articles on international relations theory and security affairs. He is currently completing a book manuscript entitled *Commerce, War, and American Foreign Policy, 1790 to the Present Era*.

Benjamin O. Fordham is Professor of Political Science at Binghamton University (SUNY). He is the author of *Building the Cold War Consensus* (University of Michigan Press, 1998), as well as articles on foreign policy and international political economy in journals such as *International Organization*, *International Studies Quarterly*, the *Journal of Conflict Resolution*, and the *Journal of Politics.*

Richard W. Maass is Assistant Professor of Political Science at the University of Evansville, where he is working on his first book manuscript, "Annexation: Domestic Constraints on International Ambition and the U.S. Rise to Power." He earned his Ph.D. from the University of Notre Dame in 2013, and has published research in *Diplomatic History*, *International Security*, *Terrorism and Political Violence*, and *Historical Methods*.

Patrick Shea is Assistant Professor of Political Science at the University of Houston. His research focuses on the political economy of conflict processes. His previous work has appeared in the *Journal of Conflict Resolution* and *International Studies Quarterly*.

Review by Benjamin O. Fordham, Binghamton University

International trade and investment have always been important issues in international politics. The end of Cold War ideological conflict and the rise of contemporary globalization have arguably increased scholarly interest in untangling the complicated relationship between economic interaction and political-military conflict. Dale Copeland's ambitious new book contributes to this effort. *Economic Interdependence and War* advances the sensible argument that state leaders worry about future access to vital markets, and that their efforts to insure this access can lead them into conflict with other states. It presents a range of case studies that illustrate how this process works. I share the author's intuition that economic issues are central to world politics, as well as his interest in international history, but I have some concerns about both the book's theoretical argument and its empirical research design.

The book's central argument is that major foreign policy decisions rest on state leaders' expectations about future commercial opportunities in their relations with other states. In the author's account, state leaders care about these economic relationships mainly because of their contribution to national power rather than the wealth that commerce provides to their societies. When they expect increasing benefits from a particular relationship, they will pursue cooperative policies toward their international partner. On the other hand, when they expect the benefits of commercial interaction to decline, they may adopt aggressive policies to avoid the costs associated with this outcome. Concern about the future reliability of a trading partner can produce a self-reinforcing "trade-security spiral" in which states attempt to reduce their dependence on one another. Once the spiral begins, states may use the leverage that trade itself provides, as well as political and military means, to reduce their dependence or to maintain economically important relationships. The book sets out six factors that might change states' trade expectations and produce such a spiral (43-6). These range from concern about third parties, including small states as well as other major powers, to internal processes within the trading states that may make them unreliable partners in the future.

Historical case studies intended to test this argument comprise most of the book. It introduces "a new approach to qualitative historical analysis for rare events research--one that minimizes the problems of selection bias and generalizability by covering the essential universe of cases for a chosen historical period" (13). The book summarizes 40 cases of major power crises and wars between 1790 and 1991 in Table 2.7 (80-90). The author writes that a close reading of the archival record in each case is critical because "[i]n-depth documentary work provides us with a window into the thinking of key decision makers as they make estimates of future realities, grapple with trade-offs associated with feedback loops and escalatory spirals, and adjust their behavior to alter the factors that will serve their ends" (75). The book uses this evidence to support not only its theoretical argument about trade expectations, but also broader generalizations about the relative importance of international trade and domestic political processes in shaping major-power conflict. It concludes that trade is critically important but that domestic political processes "hardly ever" matter in decisions about war and peace (14).

The book's ambitious theoretical and methodological claims make for arresting reading, but they are not persuasive. The difficulties fall into two categories. The first concerns the evidence the book presents. The case studies are engagingly written but they are yoked to a research design that does not support the book's broader conclusions. The second concerns the logic of the theoretical argument. Here the book's critique of liberal arguments about trade and conflict, along with its concomitant effort to link its argument to the realist tradition, undercuts its otherwise reasonable claim about the importance of expected future trade.

The book's claims about research methods are as ambitious as its substantive claims about world politics. The book's "new approach to the qualitative study of rare events in international relations" (71) seeks to analyze documentary evidence in "pretty well every important case period since 1790 involving two or more great powers" (431). This plan is bold but unrealistic. In light of the number of cases and the volume of archival evidence available about each one, as well as the considerable linguistic and contextual knowledge required to interpret all this material, it is probably beyond the reach of an entire scholarly career, let alone a single book. In fact, while the book covers some cases in great depth, it sets others aside in a few sentences. For instance, on the strength of two citations to the work of other political scientists, it states that European conflict between 1792 and 1801 "was a war to reestablish the system's ideological homogeneity, pure and simple" (321). Accuracy aside, the trouble with such summary judgments is that they do not provide enough evidence to support the claims in Table 2.7, which lists the principal stakes in each case period, the primary and secondary causal factors leading to conflict, and the overall importance of economic interdependence (80-90). To be fair, specific coding rules for making these broad judgments might be impractical. They might instead have to rest on the varied and often unique pieces of historical evidence available in each case, knit together by a thoughtful analyst with a lot of knowledge about the historical context. Nevertheless, if this is so, brief accounts of each case like those the book often provides are bound to be inadequate.

The book has difficulty in consistently defining the cases it hopes to cover. It provides no explicit definition of a 'great power,' a necessary prerequisite for identifying the relevant wars and crises. The cases it actually examines do not clarify matters. The status of China is especially puzzling, since it counts as a great power for purposes of the Opium War of 1839-42, as well as for its conflicts with Japan in 1880-95 and 1931-37, but not for the Korean War of 1950-53, which is listed only as a confrontation between the United States and the Soviet Union. Similarly, the book provides no rules for delimiting each 'case period.' For example, why divide the Cold War into 11 case periods, while the Napoleonic Wars and the Wars of the French Revolution each constitute only one case? These definitional issues are important because some of the author's conclusions are based on the aggregate characteristics of the cases, particularly his claim that trade expectations theory finds support in 86.7 percent of the cases (93, 431).

The book acknowledges the logical problem of "sampling on the dependent variable," the practice of examining only cases where the class of event one wishes to explain took place, and no cases where it did not. Unfortunately, the book's proposed solution to this problem creates other difficulties. "In addition to examining the immediate outbreak of a crisis or war, we can look at the periods leading up to the crises and wars of interest, to see if planning for conflict, levels of tension, and probabilities of war changed as the core independent variables changed" (77). The trouble with this approach is that it defines a biased sample of state interaction: one consisting exclusively of crises/wars, and periods that ended in crises/wars. It omits cases involving major powers that had little or no political-military conflict, such as relations between the United States and Britain after the Venezuelan Boundary Crisis of 1895-6. If the rising opportunity cost of conflict due to trade helps account for these instances of peaceful relations, the book's approach will underestimate the importance of this process.

When the book gets into the documentary evidence in particular cases, the results can be illuminating. There is no denying that the in-depth qualitative study of particular historical cases is a valuable endeavor, revealing important nuances that are not always considered in more general theoretical arguments. Yet even in these instances, the evidence does not always support the book's broader claims about matters such as the unimportance of domestic politics for decisions about war and peace. This particular claim appears to rest on the paucity of evidence that policymakers explicitly discuss domestic political pressures. The inference reveals a limited notion of how domestic political pressures might influence policy choice. These processes affect not only the range of considerations policy makers actually discuss but also the selection of the decision-makers themselves. If selection pressures are strong, producing a homogeneous group with common assumptions and priorities, their actual deliberations may be less important. For instance, Japanese decision-makers may not have discussed domestic pressures in making the decision to attack Pearl Harbor, but many less-than-subtle selection processes, including assassinations, were also at work. These processes helped create an uncompromising group of decision-makers who considered Japan's status as a great power so important that they were willing to embark on a war that many thought they would probably lose in order to maintain it (230). Perhaps another group of policymakers would have made a different decision. I leave judgments about the importance of the selection process in this case to those more deeply immersed in the evidence. The point here is that it merits discussion before dismissing the causal importance of domestic political pressures.

The book's central theoretical claim, that scholars should pay more attention to policymakers' expectations about future trade, makes sense, but the broader argument in which it is embedded is problematic. Many of its difficulties stem from an effort to associate the book with the realist tradition and to distance it from similar research in the liberal tradition.

The author sharply distinguishes his contribution from liberal accounts of the conflict-reducing effects of trade, writing instead that his argument is essentially realist. "While it sometimes aligns with the liberal prediction that commerce can give states an incentive for peace, the deductive reasoning behind this prediction differs significantly from liberalism" (6). On closer examination, the book might better be characterized as a variant of recent research in the liberal tradition. The most important liberal claim about trade and war concerns the impact of trade on the opportunity cost of conflict.[1] Military conflict usually destroys or curtails commercial relations among the belligerents. As the economic relationships become more valuable, military conflict becomes more costly, and state leaders have a greater incentive to avoid it. Although scholars frequently use the current value of trade to measure this opportunity cost, this measure proxies the expected value of future commercial interaction. To consider the expected future value of trade and investment is actually to specify the opportunity cost correctly, and is an improvement over simply assuming current interactions are an adequate guide to those expectations. This expected value could change for a variety of reasons even if the current value of trade and investment remains steady. The book thus advances the opportunity cost argument, but shares its premises.

Denial of the argument's close relationship to the liberal opportunity cost argument leads to a series of inaccurate claims. The book asserts that liberal theorists treat trade "only as a force for peace and not an explanation for war" (34). Although recent scholars have typically stressed the conflict-reducing effects of trade, this claim ignores a substantial body of research. Hobson's explanation of imperialism is one classic example, but there are also more recent uses of liberal premises to explain why states might behave aggressively.[2] For instance, the same opportunity cost that prevents military conflict among trading partners

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[1] Edward D. Mansfield and Brian M. Pollins," Interdependence and Conflict: An Introduction." In *Economic Interdependence and International Conflict*, edited by Edward D. Mansfield and Brian M. Pollins (Ann Arbor: University of Michigan Press, 2003), 1-28; Solomon W. Polachek, "Conflict and Trade." *Journal of Conflict Resolution* 24(1), 1980: 55-78.

[2] J. A. Hobson, *Imperialism: A Study* (Ann Arbor: University of Michigan Press, 1965 [1902]).

might encourage them to intervene against third states or domestic actors within their trading partner who threaten a valuable commercial relationship.[3] Another potentially important nuance in the opportunity-cost argument stems from the uneven distribution of the benefits of trade within the trading states. Some domestic factions could actually benefit from curtailing the commercial relationship, and might even promote conflictual political relations in order to achieve this end.[4] Both of these processes, like Copeland's claim about trade expectations, are straightforward extensions of the opportunity-cost argument. Self-interest forces me to agree that these arguments are not as widely discussed as they ought to be, but the logic of the liberal opportunity-cost argument certainly does not preclude treating trade as a cause of conflict.

Another way the book seeks to establish its realist credentials is by claiming that domestic interests and processes "hardly ever" matter in decisions about war and peace (14). In view of the book's theoretical premises, this is a puzzling assertion. The logic of trade expectations, like other arguments built on the cost of lost trade, depends on unit-level features of the trading states. The benefits of future economic interaction, and thus the opportunity cost of losing it, accrue mainly to the private economic actors who are actually doing the trading and investing rather than to their home states. For the opportunity-cost argument to work, there must be some unit-level mechanism for transmitting the benefits of trade to state policymakers. This mechanism might work through trade's overall impact on the national economy, its role in producing military capabilities, its usefulness as a tool of international influence, or the domestic political clout of its beneficiaries. The author associates only the last of these mechanisms with the liberal argument, and contends that one advantage of his theory is that it does not have to use unit-level processes to explain war. This position is unsustainable. Trade's contribution to overall national power depends on a range of unit-level considerations, including military technology and the nature of the goods being traded, just as an argument about interest group influence would. Many other characteristics of state leaders, such as their discount rate for future gains and perhaps their degree of risk acceptance, are also certain to influence the expected value of trade. Exploring these considerations would be a natural extension of the trade-expectations argument yet the author regards such domestic-level arguments as a "simple trick" (12) to which liberal theorists resort when they run out of better ideas. The book's essential theoretical argument offers no basis for this outlook. Nor does the evidence it presents. Indeed, domestic considerations come up in some of the case studies and form the basis for many of the quantitative findings cited to bolster the book's argument.

The author's insistence on minimizing the role of unit-level considerations lies behind other questionable positions. Perhaps the most puzzling of these is the claim that domestic political constraints might matter, but only in states other than those initiating international conflict. "[I]f unit-level variables play any role in the outbreak of crisis and war, it is usually the unit-level variables of the *other*--not the unit-level characteristics of the initiating state--that best explains why the latter might change its policy and initiate conflict" (435). In the author's view, this "other" state, whose domestic politics affect its reliability as a trading partner, is generally the less dependent in the trading relationship (35, 46-7): "It is [the more dependent state's] behavior that

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[3] Benjamin O. Fordham, "Revisionism Reconsidered: Exports and American Intervention in World War I." *International Organization* 61(2), 2007: 277-310; Jeffry Frieden, "International Investment and Colonial Control: A New Interpretation." *International Organization* 48(4), 1994: 559-93.

[4] Patrick J. McDonald, *The Invisible Hand of Peace* (New York: Cambridge University Press, 2009); and Katja B. Kleinberg and Benjamin O. Fordham, "The Domestic Politics of Trade and Conflict," *International Studies Quarterly* 57(3), 2013: 605-19.

shapes the probability of war within the dyad" (47). Domestic politics thus matters only indirectly, by shaping the relatively more dependent state's expectations about whether the less dependent state will continue to be a reliable trading partner. The trouble with this line of argument is that the onset of a bilateral war involves the choices of both states, not just those of the more trade-dependent state. For instance, as James Morrow has pointed out, the less dependent state could use its leverage in the trading relationship to press the more dependent state for concessions, a process that might lead to military conflict.[5] Indeed, this is essentially what happened between the United States and Japan before Pearl Harbor, the case the book treats in the greatest detail.

Overall, the author's effort to associate his argument with the realist tradition creates unnecessary complications without strengthening his case. It leads him to a dubious critique of the opportunity-cost argument on which he is actually building his own theory. It also motivates an unsustainable rejection of unit-level considerations that are actually important for his argument. Perhaps the most general lesson to be drawn from this book is that organizing and presenting empirical research around grand theoretical traditions like 'realism' and 'liberalism' risks diverting attention from the work's substantive contributions. There is much to be said for considering trade expectations more closely. Explaining why these expectations vary, and how this variation could affect international conflict, is potentially interesting and important for understanding world politics. Figuring out whether research along these lines is 'liberal' or 'realist' is a far less useful endeavor.

[5] James D. Morrow, "How Could Trade Affect Conflict?" *Journal of Peace Research* 36(4), 1999: 481-9.

Review by Richard W. Maass, University of Evansville

In this book, Dale Copeland sets out to resolve the liberal/realist debate over the relationship between *Economic Interdependence and War*—whether interdependence increases the profits of peace and hence the opportunity costs of war (making war less likely), or whether interdependence increases the costs of adjustment following a trade cutoff and hence the vulnerability of states to economic coercion (making war more likely).[1] Building on his 1996 *International Security* article, Copeland argues that both sides of this debate are sometimes correct, with their relevance depending on a state's trade expectations.[2] In short, states which expect significant gains from future trade should see peace as more profitable than war, while states which expect trade to be cut off and see major adjustment costs on the horizon should consider war to avert those costs.

Copeland supports this argument with an impressive array of qualitative case studies spanning two centuries of great power politics, as well as a useful distillation of the quantitative literature on the subject. The result is a landmark example of how diplomatic history and statistical findings can be productively married in the pursuit of a persuasive answer to an important question.

The book's primary contribution lies in reviving the notion that economic trends underlie great power politics, and here it succeeds admirably. Copeland self-consciously builds on the work of Robert Gilpin, Paul Kennedy, and others to emphasize how states' assessments of their own relative power depend crucially on their ability to sustain economic growth into the future.[3] When current events imperil their access to the resources and markets on which their future economic growth depends, states look to restore that access and are often willing to fight to do so. Copeland's case studies are particularly strong in demonstrating how much great powers value their relative power and the ability to chart their own destinies that it affords, and how their leaders can view major war as less undesirable than falling from the great-power ranks due to long-term economic decline.

The case studies and the summary of quantitative research on interdependence and war, which combined make up nearly 85% of the book, should be required reading for those interested in great power politics and the intersection of international security and political economy. The Japanese case receives special attention—the majority of three separate chapters—as Copeland provides a long-term perspective on Pearl Harbor by tracing the long-standing (1890-1941) Japanese obsession with Manchuria (the key to its future economic growth and ability to command its own destiny) and the resulting security competition with Russia, China, and the United States. The Russo-Japanese War (1904-1905) and Sino-Japanese Wars (1894-1895 and 1937-

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[1] Dale C. Copeland, *Economic Interdependence and War* (Princeton, NJ: Princeton University Press, 2015).

[2] Dale C. Copeland, "Economic Interdependence and War: A Theory of Trade Expectations," *International Security*, Vol. 20, No. 4 (Spring 1996), pp. 5-41.

[3] Robert Gilpin, *War and Change in World Politics* (New York: Cambridge University Press, 1981); Paul Kennedy, *The Rise and Fall of the Great Powers: Economic Change and Military Conflict from 1500 to 2000* (New York: Vintage Books, 1987). This focus has been maintained in, e.g., William C. Wohlforth, "The Stability of a Unipolar World," *International Security*, Vol. 24, No. 1 (Summer 1999), 5-41.

1945) often receive scant attention from scholars of great-power politics, and Copeland's examination of the connections between these cases and World War II in the Pacific is especially welcome.

Unfortunately, the theoretical discussions in the book's first two chapters prove to be as obscuring as they are illuminating. Copeland is at his best here when emphasizing the clear merit of big ideas, for example that future economic expectations affect relative power calculations and hence current foreign policy, or that trade involves trade-offs between relative gains and vulnerability. His core argument remains sound—economic trends impact security concerns and can drive leaders' calculations of whether peace or war is more profitable. Yet his theoretical discussions overemphasize several factors that ultimately leave them less satisfying than they should be, including: (1) his own middle-ground approach, (2) the relative importance of economic vs. military factors, (3) great powers' defensive motivations, (4) the value of economic leverage, and (5) bilateral interdependence.

First, Copeland claims that his "trade expectations theory" is a middle-ground approach between liberalism and "economic realism," but the latter is a straw man that caricatures offensive realism by predicting that states reject the very notion of a trade-off, start wars the moment they detect any vulnerability, and reject any possibility of trade (7-8, 21-22). No realist would accept these claims; even John Mearsheimer assumes that states are rational actors who think in cost/benefit terms and "pay attention to the long term as well as the immediate consequences of their actions."[4] Moreover, Copeland backs himself into a confusing paradigmatic corner when his own theory "fuses defensive realist insights with an essentially offensive realist baseline" (43). Why not then simply call his own theory "economic realism"? The book's love/hate relationship with realism is distracting, especially since Copeland's own theory "is fundamentally realist rather than liberal" (27).

Copeland's true theoretical adversaries are static perspectives (which do not account for future expectations) and liberalism (which sees trade as a force for peace). One wishes he had drawn a theoretical 2x2 table with the axes defined by (1) whether a theory views trade as primarily a force for peace or war, and (2) whether a theory focuses primarily on present trade or expectations of future trade. By describing static versions of realism and liberalism as well as a dynamic version of liberalism, and juxtaposing them to his preferred dynamic realism, Copeland could have provided his readers with a more intuitive and authentic theoretical landscape than his middle-ground portrayal. Doing so would also have allowed Copeland to more cleanly adopt the mantle of realism that pervades his case studies, rather than rendering his theory as realism-by-another-name while portraying "economic realism" as a competing theory.

Second, a quick read-through of Copeland's first two chapters may give readers a distorted view of the relative causal importance of economic vs. military factors. Granted, the object of the book is to explore the role of economic factors in great power politics, but the *reason* economic factors matter for Copeland (building on Gilpin, Kennedy, *et al.*) is ultimately because they affect states' assessments of their own security. In other words, economic trends matter for great power politics because of their military implications. Copeland's case studies are saturated by this perspective: in one example, he recounts how the United States adopted increasingly hard-line policies towards the Soviet Union during the summer of 1945, motivated in a proximate sense by "the emerging threat to U.S. access to resource and markets" but more profoundly by the

[4] John J. Mearsheimer, *The Tragedy of Great Power Politics* (New York: W. W. Norton & Company, 2001), 31.

"need to maintain the United States' ongoing power preponderance given Russia's vast potential for future growth" (251).

This perspective is also clearly depicted in Copeland's theory arrow-diagram by the fact that economic dependence and trade expectations do not drive state behavior directly, but rather affect a state's "evaluation of its security situation" which in turn drives its behavior (49, Figure 1.1). Unfortunately, the overemphasis of economic factors in the surrounding discussion and the virtual absence of analysis regarding military factors influencing a state's evaluation of its security situation (not to mention identity or other factors) leave it unclear how much trade expectations matter in that evaluation relative to other factors. Elsewhere, Copeland highlights the "trade-security dilemma" (in which one state's efforts to reinforce its access to economic resources threaten another) as distinct from the "military-security dilemma" (in which one state's efforts to reinforce its military security threaten another). Yet what actually threatens others in the "trade-security dilemma" is when one state's economic pursuits involve "projecting power around the system" (10), making the distinction one of proximate motivations rather than methods (not to mention long-term motivations), which remain military.

Third, in his effort to distance himself from offensive realism, Copeland overemphasizes states' defensive motivations. His claim that "states are security maximizers worried about decline" (318) rings somewhat hollow when worries about decline prove indistinguishable from a desire to rise, a.k.a. power maximization. For example, in the book's most prominent case study, U.S.-Japanese negotiations prior to Pearl Harbor failed because of "Japan's unwillingness to commit to not attacking Russia" (185). Why were Japanese leaders so determined to attack Russia? For half a century before 1941, they had consistently seen Japan's future great-power status as dependent on its economic domination of Manchuria, and Russia was its most powerful competitor in that region. Yet Japan's economic domination could only be achieved by defeating Russian and Chinese resistance and imposing a sphere of influence to match the European colonial empires and U.S. domination of the Western Hemisphere, a.k.a. by establishing regional hegemony. Copeland's underlying concern with relative power finds strong support throughout his case studies, but it is less clear that his focus on preventing decline is more warranted than a focus on continuing to grow (if the two can be theoretically separated at all).

Fourth, Copeland depicts international trade as a "trade-off between relative gains and economic leverage" (11), but he overstates the value of economic leverage. In Copeland's theory, the more-dependent State Y receives relative gains through trade while the less-dependent State X receives a bargaining chip (the threat of trade restrictions) that its leaders can use to coerce "nicer behavior by their dependent adversaries" (38). But why would a rational state ever trade relative gains for economic leverage? Copeland portrays these as equally valuable and implies that trade is therefore possible because it is security-neutral, but there is a major logical problem here. As Y's relative gains make it more powerful over time compared to X, X faces "an incentive to cut Y off later after Y becomes overly dependent or at least to use its economic leverage to coerce Y into concessions Y has no interest in making" (42). In other words, trade is only security-neutral if X periodically coerces Y into giving back its relative gains. This raises the question of why a rational State X would bother with trade in the first place, essentially mortgaging its own security on the gamble that Y will willingly return its relative gains at a later date (when, by the way, it is better able to resist X's demands due to those relative gains). Moreover, this implies that a major source of interdependence-related conflict will come from X's need to eventually recoup Y's relative gains (possibly through a preventive war), which begs the question as to why Copeland focuses only on Y's behavior as determining the probability of war (as it stands, perhaps the title *Economic Dependence and War* would be more fitting).

Since Copeland's theory assumes that the primary goal of states is to ensure their long-term security (27), the only way it might explain why a state would willingly suffer long-term relative losses through trade would be if trade produced a long-term security benefit worth more than relative power. Copeland holds that pursuing security under anarchy requires states to "worry about the future power and intentions of others" (43), and that consequently "the primary roadblock to peace" is states' inability to commit to good behavior in the future (12-13, 40-42), and he suggests that trade helps states build trust over time by establishing reputations for moderate behavior (42-43). This suggestion feels half-hearted, however, on the heels of his much stronger discussion of the "problem of the future": the prospect of Y turning on X once the relative gains of trade have made it more powerful, whether due to a change of heart, a change of leadership, or the depletion of its internal resources (41-42). If Y's future behavior is uncertain, as Copeland argues, and may turn against X for a variety of foreseeable political and natural reasons, why would X trade with Y? Copeland loudly rejects the worst-case assumptions of offensive realism and praises the probabilistic thinking of defensive realism, yet the states in his theory—*which is founded on expectations of the future*—accept at face value signals that conflict is unlikely in the short-term and completely ignore the probability of conflict in the more distant future when the balance of power has turned against them.

Fifth, Copeland's theory is handicapped by his overemphasis of bilateral economic dependence, manifested in the omnipresent scenario of State X and State Y. Whereas the theory focuses on the relative dependence of X and Y on trade between them, most of Copeland's case studies are driven not by bilateral trade but rather by great powers competing for political and economic domination of peripheral areas (e.g., Japan vs. Russia/China in Korea and Manchuria, Japan vs. the U.S. in China and Southeast Asia, the U.S. vs. the U.S.S.R. in Europe and the Middle East, Russia vs. France vs. Britain in Turkey, European imperialism in Africa and Asia). Most often, conflict in the cases occurs not because State X restricts its own trade with State Y, but rather because two states compete for domination of a region that each sees as crucial to its economic growth. Extending the bilateral logic of the theory to the multilateral cases is largely left to the reader. It is also worth noting that multilateralizing the theory would have helped resolve Copeland's dilemma regarding why trade occurs in the first place (despite one state having to accept relative losses in return for economic leverage): states may concede relative losses to one state in order to achieve relative gains compared to another that poses a greater security threat, they may see their peers engaging in trade and fear being left behind, or they may see the relative gains of trade as fluid rather than fixed and anticipate the balance turning in their favor.

It is worth noting that all of these issues ultimately lie at the level of mid-range theory-building. As noted above, Copeland's underlying insight that future economic expectations influence leaders' evaluations of their states' long-term security is powerful and well-supported by the case studies. Only in the details do problems arise—the relative importance of bilateral trade vs. multilateral great-power competition, defensive vs. offensive motivations, relative gains vs. economic leverage. This book will not end debate over these issues, but it makes a strong case that economic forecasts influence relative power calculations and hence patterns of international conflict, and that international economic connections spark conflict when great powers face "looming threats to their future access to raw materials, investments, and markets" (69).[5]

[5] Copeland's thesis finds further support in the economic motives behind the U.S. declaring war on Britain in 1812; see Richard W. Maass, "'Difficult to Relinquish Territory Which Had Been Conquered': Expansionism and the War of 1812," *Diplomatic History*, Vol. 39, No. 1 (January 2015), 70-97.

Overall, Copeland has performed a great service to the field in composing this impressive investigation into an area of major importance to international relations. Students and scholars alike would be hard-pressed to find a better launch pad for their investigations of the intersection of international security and international political economy, and this book is sure to find a prominent place on syllabi and bookshelves across the discipline.

Review by Patrick Shea, University of Houston

Dale C. Copeland's book, *Economic Interdependence and War*, is an ambitious endeavor that attempts to fill a theoretical and empirical void in the interdependence and conflict literature. Copeland focuses on a gap between liberal theories that assert that interdependence leads to peace and realist theories that assert that interdependence leads to vulnerability and conflict. Despite the abundance of research on interdependence and war, no clear consensus has emerged regarding the nature of this relationship. As a result, Copeland sees an opportunity to push the discussion forward, and he largely delivers. In addition, Copeland provides a methodological bridge between those who favor a statistical approach and those who use case-driven approaches. In sum, Copeland provides at least three major contributions. First, he highlights the importance of examining interdependence and conflict in a dynamic framework. Second, his analysis reveals that despite the proliferation of research related to interdependence and conflict, there are (too) many outstanding theoretical and empirical questions left unanswered. Third, his forty case study, medium-N approach is a methodological achievement.

With that said, this book left me with questions. I identify two areas that caused confusion: (1) the ambiguous role of time in trade expectations and (2) the disconnections between the theory and empirical analysis. I discuss each of these areas in turn.

Trade expectations theory asserts that if a state expects its trade benefits to decline in the future it may be more likely to go to war. Copeland argues that this focus on future expectation departs from existing static theories. However, although time is supposedly foundational to Copeland's theory, it plays a passive role. For example, there are no temporal boundaries to states' expectations, which begs the question of how far do states look into the future. As John Maynard Keynes noted, "The long run is a misleading guide to current affairs. In the long run we are all dead."[1] Without any guidance whether states are more motivated by declining trade expectations in one year versus one hundred years, it is difficult to explain and predict the role of interdependence.

One way to provide theoretical guidance about how states value the future is to theorize about states' discount factors. Copeland assumes that states value future benefits as much as present benefits. In other words, states have no discount factors. This assumption is troubling for two reasons. First, it ignores that time horizons are associated with trade policies and war is associated with time horizons.[2] A truly dynamic theory of trade and war needs to fully consider time horizons.

The second problem with assuming that discount factors are zero is that it makes it difficult to make costs benefit comparisons. Copeland gives the example of state Y, which has 100 units of GNP (35). If state Y trades, it can raise its GNP to 110. However, if trade is severed, GNP would only be 85 because of the costs of having specialized economic activity. Copeland then argues that this expected decline to 85 can motivate

[1] John Maynard Keynes, *A Tract on Monetary Reform*, McMillian: London (1924), 80

[2] Daniel Yuichi Kono and Gabriella R. Montinola, "Foreign Aid, Time Horizons, and Trade Policy," *Comparative Political Studies* forthcoming (2015); Douglass C. North and Barry R. Weingast, "Constitutions and Commitment: The Evolution of Institutional Governing Public Choice in Seventeenth-Century England," *The Journal of Economic History* 49:4 (1989): 803–832.

states to go to war. Why does the analysis stop here? Over time, states can de-specialize their economies and return to the autarkic level of 100 units of GNP. Thus, the value of trade expectations is not negative as Copeland argues, but instead should eventually return to the first level of GNP.

The ambiguous role of time also leads to several empirical limitations. To begin, chapter 2's quantitative studies focus mostly on current levels of interdependence and conflict. Copeland argues that trade expectations theory can help reconcile contradictions between these quantitative analyses. However, Copeland's theory about expectations of future trade does not explain why states would fight in periods of decline in the present. Statistical analysis of Copeland's theory requires a measure of expected trade that is absent from the statistical models presented in this book.

There are similar concerns for the case analysis. If states are concerned with future trade, we should expect that Japan attacked the U.S. before the U.S. shut off the oil supply to Japan before the Pacific War. Once the U.S. shut off the oil supply to Japan, it is not clear what role trade expectations has in the calculations to go to war. Once there is no trade, and a state expects this to continue, trade expectations theory becomes a constant. However, perhaps theories that connect low or no trade between states and war would be better at explaining the onset of war in this case.

Besides the problems of time, there are other limitations to the empirical strategy. First, it is difficult to interpret the statistical models from the tables Copeland presents in Chapter Two given that many of them contain interaction variables. Magnitude, direction, and statistical significance can all be misleading when interactions are present in empirical models with limited dependent variables.[3] To interpret the potential empirical effects in these models, marginal effects graphs are a reader's needed crutch in order to make any statistical inference.

Copeland also makes suggestions that his theory, coupled with some of the statistical results, explains more about war and peace than the democratic peace. Democracy and trade are, however, concepts that are highly interrelated. Thus, it is difficult to favor one over the other in the same statistical model because of multicollinearity and since the plausibility that trade leads to democracy or democracy leads to trade. No inferences for or against the democratic peace should be made from Chapter 2.

The chapter on statistical analysis has limited utility to say anything in support of trade expectations theory. The brunt of the potential evidence lays in the qualitative analysis. This analysis is incredibly deep and weaves a convincing narrative in support of Copeland's theory. However, as with any analysis, there are some reasons to question the main conclusions. While my questions relate to most of Copeland's cases, I will limit my focus to the onset of the Pacific War.

First, there are questions related to the timing of war. The theory predicts that states should go to war when future trade expectations are negative, though some of the cases involve war after trade declined. As noted above, given trade expectations theory, we should expect that Japan attacked the U.S. before the oil embargo or before the global trade market tightened in the early 1930s. However, in this case, we do not observe war until after trade declined substantially. In addition, from Copeland's analysis it appears that war in the Pacific

[3] Thomas Brambor, William Roberts Clark, and Matt Golder, "Understanding Interaction Models: Improving Empirical Analyses," *Political Analysis* 14:1 (2006): 63– 82.

occurs whether the U.S. cut off trade or not. If a deal were struck between the U.S. and Japan, Japan would likely attack the Soviet Union. In this scenario, the U.S. would probably mobilize against Japan. If no deal were struck, Japan would attack the U.S., as we observed. In what way does trade expectations theory affect the probability of war in these scenarios?

There are also questions as to how to evaluate the competing theories. Copeland concludes that historical, traditional realist, and liberal explanations do not help us understand Japan before the Pacific War. However, Copeland's evidence shows that when the U.S. cut off oil to the Japan, Japan pacified its behavior towards Russia and that this motivated Japan to reenter negotiations with the U.S. The U.S. embargo signaled to Japan that the U.S. was resolved to defend the Soviet Union against a two-front war. Copeland notes that Tokyo "[c]learly …got the message" (214). This analysis is consistent with liberal trade theories that assert that trade provides states signaling options to demonstrate resolve and intentions.

The questions and criticisms I have presented do not undercut the totality of Copeland's work. As a result of Copeland's research, the literature on interdependence and war moves forward. I expect that future empirical progress will be made by focusing on the role of time in the relationship between war and interdependence, along with issues related to exit costs and the interdependence between networks of states.[4]

[4] Mark J. C. Crescenzi, "Economic Exit, Interdependence, and Conflict," *The Journal of Politics* 65 (3) (2003), 809–832; Yonatan Lupu and Vincent A. Traag, "Trading Communities, the Networked Structure of International Relations, and the Kantian Peace," *Journal of Conflict Resolution* 57:6 (2013): 1011–1042.

Author's Response by Dale C. Copeland, University of Virginia

Trade Expectations and the Reality of Great Power Politics: A Response

I greatly appreciate the opportunity to respond to three provocative critiques of my recent book, *Economic Interdependence and War.*[1] Grappling with the reviewers' comments has forced me to reexamine the foundations of my argument, my methodology, and my use of historical evidence to support the book's claims. In what follows, I will try not only to respond to the main points the three individuals raise, but also to clarify what I see as the future research agenda that comes out of the critics' concerns. In short, I hope that this response essay, even as it defends the book's main premises and conclusions, will offer a positive contribution to the reformulation of competing theories that link interdependence to peace and military conflict. Such a reformulation will allow liberals and other domestic-level theorists to incorporate the future more self-consciously into their theoretical agendas, thereby providing a better test of their arguments against the more realist-based arguments that I and others are proposing.

Since the reviewers have provided nice summaries of my main theoretical arguments, I will just plunge directly into their core concerns and see where that leads. Before I do, however, I want to mention briefly the dogs that didn't bark, that is, the critical points that were not made, and what that suggests. None of the three scholars went after the deductive logic of trade expectations theory per se, but only after its assumptions and implications. Moreover, there were no direct challenges in the three essays to my fundamental interpretations of some very controversial cases, including the Russo-Japanese War, the Crimean War, World War I, and the origins of the Cold War. As we will see, my views on the Pacific War did raise a few questions. Yet this was not because my evidence or interpretation was seen as off-track, but rather because there was some feeling that a result other than a Japanese-American war in December 1941 might have come about had conditions been different. I find the apparent acceptance of my core causal reasoning and historical analysis quite satisfying. It indicates that the trade expectations approach not only provides a potentially fruitful way to bridge the ongoing divides between liberals and realists and between offensive realists and defensive realists, but that the theory does work to explain real-world events across time and space.

Let me now turn to the reviews. Patrick Shea begins by arguing that I should have spent more time bringing in states' discount factors. He correctly notes that I assume leaders of great powers are rational actors who value the future as much as they value the present. That is, they have long time horizons and do not discount benefits, costs, and risks simply because those are expected to arise some time into the future. Truly dynamic theories of trade and war, he argues, must incorporate varying time horizons, since such theories are inherently about the role of the future.[2] Shea's point helps us clarify the difference between a theory's deductive structure and its real-world applications and testing. Theories of international relations (IR) must make assumptions to get their deductive logics going. What this means in practice is that a theorist will set

[1] Dale C. Copeland, *Economic Interdependence and War* (Princeton: Princeton University Press, 2015). I thank Mike Poznansky for helpful comments on an earlier version of this essay.

[2] A similar point was made by David Edelstein in a roundtable held at the Cato Institute on May 12, 2015 (www.cato.org/events/economic-interdependence-war).

many potential variables such as discount factors, actor ends, and level of rationality at constant and seemingly "unrealistic" values in order to isolate the logical effects of the independent variables of interest.

This procedure, however, is just a first not a final step. As I emphasize in the book, if theorists do not make explicit their assumptions, we cannot know to what extent their causal claims differ from competing arguments. Most importantly, once we turn to historical evidence, we would not know what we are really testing, or what sort of evidence would disconfirm one theory as it supports another (28). I make a number of strong assumptions in the book: that leaders do not discount the future; that they are rational in their calculating of the best means to their ends, given the information available; that they are cost and risk neutral; that they operate in a realm of essential domestic autonomy; and so forth (27-28, 27 n.22, 36 n.38). The most important assumption of all is that leaders in the anarchic world of great power politics are concerned solely with maximizing the security of their nation-states. They do not care about welfare-maximization for the society as a whole or for particular regions or groups, nor do they worry about their personal survival or status in office.[3] It is this latter assumption, as we will see, that most differentiates the realist-based logic of the book from the type of liberal arguments put forward by scholars such as Benjamin Fordham.[4]

Once we have set up a theory with clear assumptions and causal variables, we can go to the evidence to see how often the theory works to explain the basic patterns of historical decision-making relative to its theoretical competitors. And when a theory does not explain every facet of a case, we can relax the assumptions to see what elements of the deductive structure were perhaps unrealistic for that particular situation. Thus, as I note, time horizons can be used in case studies to explain how myopia can lead certain officials to respond to short-term opportunities and threats in a way that is inconsistent with my predictions (36, n.38). Note what we gain by such a procedure. We are able to parse what causal roles different elements are playing in specific cases. We might see, for example, leaders choosing war because of negative trade expectations and the fear of the rise of another great power, but jumping too quickly into preventive war because of their short-term time horizons. This would allow us to reject liberal explanations rooted in the domestic politics of the initiating state, even as we acknowledge that in this case, leaders were not perfectly rational in their means-end calculations.

Shea's second major point focuses on my discussion of how interdependence should be conceptualized. In the book, I argue that if two autarchic great powers, X and Y, are considering trading, state Y should not see its true level of future dependence on X as a function simply of the "gains from trade" it might receive from open trade (the liberal view), but also of any "costs of adjustment" to the economy that might be incurred should X later cut Y off from access to valuable raw materials, investments, and markets (the realist concern). If states X and Y start off at 100 units of GDP, for example, trade might raise Y's GDP to 110, but if trade is

[3] In other words, while these things might be means to the end of national security -- an ill President Franklin D. Roosevelt seeking reelection in 1944, for example, because he felt a duty to see the war to its conclusion -- they are never ends in and of themselves.

[4] See in particular Katja B. Kleinberg and Benjamin O. Fordham, "The Domestic Politics of Trade and Conflict," *International Studies Quarterly* 57 (3), 2013: 605-19; Benjamin O. Fordham, "Revisionism Reconsidered: Exports and American Intervention in World War I," *International Organization* 61 (2), 2007: 277-310.

then severed, GDP might fall to 85 units. The 25-unit gap (110 minus 85) is Y's true dependence level.[5] Thus we see, I argue, the limitations of current conceptualizations in the field. Liberals consider only the +10 gain from trade as the "opportunity cost" of a trade cut-off, while realists focus on the -15 cost of adjustment as the increased vulnerability state Y incurs by trading with another great power (8-9, 35-36). (My argument considers both elements simultaneously, against the backdrop of positive or negative expectations of future trade.)

Shea offers the insight that over time dependent states that specialize and are then cut off can "de-specialize" to eliminate the costs of adjustment and get back to the original 100 units of GDP. Thus the impact of a state having negative expectations is not as dire as I argue. Yet what Shea overlooks is the fact that my argument is all about the impact of trade on the long-term *relative* positions of states in the rough-and-tumble world of great power politics. State Y, the more dependent state in the relationship according to my deductive set-up, may indeed be able to inch its way back to 100 absolute units of GDP by de-specialization after a cut-off. In the meantime, however, its economy may have suffered a huge blow to its relative position versus X and perhaps neighboring states W and Z. If national survival is key, getting back to 100 over five years means little if X, W, and Z have grown during this time to, say, 120, 130, and 115 respectively. Consider Japan 1941. The Japanese leaders knew that in absolute terms they might be able to recover from a complete oil cut-off. But in relative terms, the oil-rich states of United States and Russia would have continued to grow economically, allowing them to overtake the Japanese in regional military power. The relative decline caused by falling expectations of trade in 1941 thus propelled Japanese leaders into a preventive war with the United States that they would otherwise have wished to have avoided.

Shea makes two points against my evaluation of the large-N quantitative work on interdependence and war. He suggests that any statistical analysis of my theory requires a measure of expected trade that is absent in the statistical models I summarize in the book. He also claims that we cannot test arguments for the democratic peace against arguments for an economic peace using these models, simply because democracy and trade are too causally interrelated. Mutual democracy tends to lead to high levels of trade while high trade in turn tends to foster greater democratization.

Regarding the first point, it is important to note, as I did in the book, that it is impossible to get a truly accurate measure of leader expectations of future trade, simply because we have no general surveys of leader opinions about their future expectations (13, 74-75). International relations is not like the fields of economics or American politics, where individuals can be regularly interviewed regarding their optimism or pessimism about the future. Most of our key actors are dead or would refuse to grant interviews, at least while in office. As a result, I argue that the best way to reveal how expectations drive policy is to go to the historical documents and see what leaders were thinking at the time. Yet the measures employed by the cutting-edge large-N scholars I survey do capture elements of the future, or at least allow us to infer how leaders looking at these measures might themselves have used them as bases for estimating future developments. Patrick McDonald, for example, measures the levels of tariffs imposed by one power on another (66-69).[6] High

[5] If state X receives a gain of +7, but would fall to a GDP of 95 after a severing of trade, it is the relatively less dependent state in the relationship, with a total of 12 units of dependence.

[6] Patrick J. McDonald, *The Invisible Hand of Peace: Capitalism, the War Machine, and International Relations Theory* (Cambridge: Cambridge University Press, 2009).

tariffs are a static measure of a current situation, to be sure. But we can imagine that a state that has had high tariffs imposed on it, all things being equal, would be more likely to have negative expectations about the future level of trade with its trading partner (consider Japan after the United States imposed the Smoot-Hawley tariff in 1930). Michael Mousseau's measure of "contract-intensive economies" (CIEs) likewise allows us to take a static snapshot variable and infer how actors in strong CIEs might be able to signal their willingness and ability to keep trade flowing over the long term (58-60).[7] So yes, such proxies for expectations are less-than-perfect, meaning we must ultimately go to the documents to test any expectational approach. But these proxies are still useful as a first-cut test of the impact of leader expectations on state behavior, and this is how they are treated in the book.

Shea's claim that the multicollinearity between democracy and trade invalidates any test of an economic-peace argument against the established arguments of the democratic peace is interesting, but again far too definitive. First of all, the claim itself is based solely on an intuition that between democracies, trade will always be high. A glance at historical periods of "great depression" such as from 1875 to 1895 and the 1930s quickly dispels this notion. More significantly, however, the models developed by McDonald, Mousseau, Christopher Gelpi and Joseph Grieco, Erik Garztke, and others are designed to highlight the relative causal significance of the democracy and trade variables, both in connection with levels of militarized conflict and with each other.[8] Were multicollinearity as high as Shea claims, the interactive models that these scholars employ would produce messy and inconclusive results. Yet in practice, depending on their specifications, they show that democracy sometimes falls out as a significant variable once interacted with commercial flows (55-69). This at least makes us question the traditional normative and institutional explanations for the empirical finding that democracies don't seem to fight each other. It also leads us to consider the possibility that this finding, while valid, is more the result of the ability of democratic states to signal their future willingness to trade with each other, even if in the present they have been imposing high trade restrictions (consider again the 1873-95 and 1929-41 periods).

Finally, let me deal briefly with Shea's arguments against my historical evidence. Shea claims that for cases such as Japan and the United States in the interwar period, we should predict by the nature of my argument that Japan would have attacked the United States before the oil embargo of 1941 or even before the decline of global trade after 1929. Moreover, he argues that by my own reasoning, had Japan and United States struck a deal in 1941 to get trade reinstated, Japan would still have gone to war with the Soviet Union. So what difference did negative trade expectations make to the probability of war in the Pacific?

Both of these empirical critiques are off the mark. I have a hard time understanding why Shea thinks my argument would expect to see Japan attack the United States in the 1920s, given that trade from 1919 to 1928 was rebounding after the disaster of the First World War. His argument implies that Japan's economy

[7] Michael Mousseau, "The Social Market Roots of the Democratic Peace," *International Security* 33 (4), 2009: 52-86.

[8] McDonald, *Invisible Hand of Peace*; Mousseau, "Social Market Roots of the Democratic Peace;" Christopher Gelpi and Joseph M. Grieco, "Economic Interdependence, the Democratic State, and the Liberal Peace," in *Economic Interdependence and International Conflict*, edited by Edward D. Mansfield and Brian M. Pollins (Ann Arbor: University of Michigan Press, 2003); Erik Gartzke, "The Capitalist Peace," *American Journal of Political Science* 51 (1), 2007: 166-91.

was stronger in relative terms prior to the Great Depression, making preventive war optimal. But this is retrospective thinking based on what we now know about the 1930s. No one anticipated the precipitous decline of world trade after 1929, and thus prior to this date, Japanese expectations of the future were positive not negative. Moreover, my argument indicates that great powers are quite reluctant to get into "trade-security spirals" with each other, and thus will use diplomacy to try to re-right trade after it has fallen. Thus even in 1941 the Japanese government tried three rounds of diplomacy to try to get Washington to relax its sanctions. It was only in late November, when it was clear that Roosevelt would not relent, that Japan moved definitively toward war (chap. 5).

Shea's point that war in the Pacific was essentially inevitable because Japan was determined to attack either north or south is correct. But it misses the thrust of my empirical effort: to explain why the "Pacific War" of 1941 was *not* a war between Japan and Russia but one between Japan and the United States (149-50, 245-46). After all, the whole debate in historiography and political science is centered around one question, namely, why the Japanese government attacked the United States when it could have avoided a devastating war with this economic powerhouse (144-45, 184). My answer to the question is controversial but clear: it is precisely because Japan would have used any deal with Washington that restored oil flows to attack Russia that Roosevelt had to retain the sanctions to knowingly draw Japan south. A third-party effect -- a likely Japanese attack on Russia that would have split Soviet forces just when they were needed to stop Adolf Hitler from dominating Eurasia -- led to a US-Japanese war. And it is this war that must be explained.

The reviews by Richard Maass and Benjamin Fordham ultimately pose bigger challenges for my overall approach. For one thing, the two essays, placed side by side, seem to be in an odd tension with each other. Maass sees my argument as a clear realist argument that effectively shows the limitations of the commercial liberal world-view. Yet he wishes I had been more explicit about my debt to realism. Instead of positioning my logic against the "straw man" of economic realism, I should have shown that my argument is really an updated version *of* economic realism. Fordham, on the other hand, sees trade expectations theory as a liberal argument in disguise. If only I had been willing to acknowledge both that I draw from liberal opportunity-cost theory and that domestic factors are necessarily embedded in the way commerce affects policy, I would have developed a much better theory with more real-world relevance.

This divide suggests that the book can sometimes act like a Rorschach test, with scholars labeling its arguments one way or another depending on their preexisting mind-sets and what they see as the significance of its evidence for the long-standing liberal-realist debate. In the end, however, the labels are unimportant, as I tried to stress at the start of chapter 1. We cannot make progress in IR theory unless we figure out what the competing causal arguments are all about -- where they are similar, and where they are different. The terms "liberalism" and "realism" are useful only because they help us quickly identify the presuppositions that different groups of individual theories share. These presuppositions include assumptions about what actors want from their policies (their ends), which individuals or domestic groups are most influential in driving policy (who matters), and the overall functional role that trade and investment ties play in the onset of peace or war (18).

The trade expectations argument is clearly grounded in well-established realist assumptions, not liberal ones. It assumes, as noted, that leaders of great powers are concerned only with national security, not with the welfare of social groups or with reelection and personal status. Moreover, because leaders operate in an uncertain anarchic world, they will focus on any development that affects their states' long-term levels of economic and military power, given power's critical role as a means to national survival (6-7, 27-28, 428-29).

From this base, I can then show how defensive realists concerns for spiraling can be incorporated into an improved realist logic for how trade might lead to war or to peace (10-12, 39-40, 45-49, 429-30).

As we will see, Fordham's effort to redefine my theory as liberal and to thus push it toward the unit level is ultimately unconvincing. But let me deal first with some of the main points of Maass's thoughtful critique. Maass contends that my theory is even more realist than I claim, and that I set up John Mearsheimer's argument about trade and war -- what I call "economic realism" -- as a straw man that I can easily knock down. I find this latter charge particularly puzzling. First of all, I find that economic realism often beats my own argument for important cases such as the Sino-Japanese War of 1894-95 and Japan's treatment of China and its entry into Siberia from 1914 to 1918. Indeed, the economic realist logic plays an important or decisive role in more than a quarter of the forty case periods. I do find that my own argument more frequently explains the bulk of the cases than economic realism. But given my acknowledgement of the latter's empirical successes, it is hard to sustain the idea that my summary of economic realism is deliberately distorted to help my case.

Maass's main concern seems to be this: while my trade expectations approach is founded on states that grapple with multiple trade-offs, he thinks that I don't acknowledge that John Mearsheimer's offensive realism, the basis for economic realism, also recognizes trade-offs. Yet the only constraining trade-off Maass identifies is one that I explicitly tie to offensive/economic realism, namely, that when states believe expansion will have costs greater than benefits, they will be moderate in their policies (22 n.11). Maass's discussion misses a key element of Mearsheimer's deductive set-up, namely, the assertion that states are forced by anarchy to assume worst-case about the future. Because of this, dependent actors will be constantly watching for opportunities to grab territory or increase their power in order to ensure access to raw materials and markets. My points against Mearsheimer's economic realism were straight-forward. First, by assuming worst-case, economic realism ignores the fact that hard-line behavior can cause states to see one another as threats. The potential for spiraling levels of mistrust and hostility, a defensive realist point I build into my theory, is eliminated by fiat, simply because each state's mistrust is already at the highest possible level. States in my argument are assumed to be aware of the trade-security dilemma, and thus for any given cost of military action, they have an incentive to be more moderate in their policies than Mearsheimer would otherwise allow (7-12, 42-49, 429-30).

Second, all neorealists who deal with the intersection of economic and military power argue that great powers want to avoid both increasing vulnerability and relative losses through trade. Yet these two objectives are almost always at odds with one another. The state that is getting the relative gain from trade is also typically the state that is becoming relatively more vulnerable to a severing of trade ties, as we saw with Japan vis-à-vis the United States from 1905 to 1941. Exposing this trade-off helps us understand why great powers might trade with each other in the first place. One of the actors needs the relative gain, and the other is willing to accept a relative loss in order to gain leverage over the rising actor so as to compel its cooperation (8-9, 28-29).

Maass argues later in his essay that I overstate the value of economic leverage, and that no rational great power would ever allow relative losses in order to increase its leverage. By my own argumentation, he claims, a state X that gives Y a relative gain through trade will only have to cut Y off later to recoup the relative loss. So why, Maass asks, would X ever trade with Y in the first place?[9] Yet Maass's focus is only on the potential risk of trading, namely, X's suffering of a relative loss. What he fails to mention here is the clear downside for X of *not* trading with Y in both the short term and long term. If trading with Y helps X "buy" Y's cooperation, and indeed keeps Y from initiating war, this is a huge benefit to X's security in the short and long terms (think of US trading with China from 1985 to 2010). Moreover, even if Y begins to narrow the gap between X and Y, state X knows that Y's growth is not solely the result of trade and that Y might never overtake X in relative power should Y fall prey to domestic problems and diminishing marginal returns (China again comes to mind, especially after 2010). So while rising state Y must indeed worry that X might cut it off later, it also knows that X has incentives to build a reputation as a good trading partner to avoid a Y-initiated trade-security spiral. Likewise, I argue, Y has an incentive to be a moderate actor in order to keep the trade benefits flowing. In this way, we can explain clear anomalies for offensive realism, such as why the United States and China have kept increasing their trade levels since 1985 and why both states have been relatively cooperative in their relations, avoiding the cold war spiraling of the 1945 to 1985 period (39-43, 438-44).

Maass agrees that the security benefits of maintaining trade ties might be potentially important enough to keep states trading despite relative gains concerns. But he wishes that I had simply admitted that state fears of future problems are always going to overwhelm the desire for peaceful trade in the short term. Thus he argues that my effort to show that states' understanding of the trade-security spiral will often keep them moderate is a "half-hearted" argument relative to my "stronger discussion" about the issues that an uncertain future poses. Yet this is exactly the problem with the offensive realist argument that he ultimately wants me to support. One can't say *a priori* whether the forces pushing states to be moderate will dominate or be dominated by the forces pushing states to be conflictual. One must examine the specific conditions of each situation, watching how these pressures change over time. Only in that way can we predict variations in the dependent variable of the probability of conflict and war.

In the book, I describe a number of such exogenous factors that determine how states might "lean" when making critical trade-offs (43-49). These factors include the role of third parties and the relative growth rate of state Y, the state that is usually getting the relative gain from trade. Maass ignores my discussion of these issues. He contends, for example, that I overemphasize the bilateral relationship of X and Y to the detriment of the broader geopolitical environment. Yet his point that trade may well occur between great powers in multipolarity for fear of being left behind is explicitly part of my discussion of why great powers trade in the first place (30, and 30 fn.26). Moreover, as my discussion on pages 43-45 and my empirical case studies show, third parties often get in the way of positive expectations between actors X and Y. Taking the bargaining model of war seriously, I seek to show that negotiated deals within an X-Y bargaining space may be difficult to achieve when third parties force X or Y to intervene in W or Z to protect their interests. Such actions can hurt Y's confidence in the X's willingness to be reasonable over the long term, thus leading to economic restrictions that will only fuel a spiral of mistrust and hostility.

In the end, if we are going to build better theories that rest on realist assumptions, we must avoid the tendency to assume great powers are forced by anarchy into hard-line trading postures. Yet we must also acknowledge that offensive realists have a point: fear of the future can, under certain circumstances, lead

[9] A small technical point: I do not argue that X will *necessarily* cut off Y later (the discussion below shows why). In the passage from my book (42) that Maass quotes, I was only trying to show what state Y might think state X *might* do later should Y continue to grow in relative power.

actors to be more assertive and expansionistic that they might otherwise want to be given concerns about security spirals. I went to some length to describe why the trade-offs inherent to international relations could go either way, depending on the circumstances (8-11, 42-47, 439-44). Maass may believe that my own arguments for why states shouldn't trade are more powerful than the ones I make for why they will trade. But again, this cannot be established upfront. Rational states will therefore engage in a detailed examination of both their specific circumstances as well as the impact of policy shifts on the level of great-power mistrust before deciding to turn to the hard-line end of the spectrum.

Let me now turn to the Fordham review. It is hard to know where to start with an essay that imposes a narrow and distorted reading on my argument and then characterizes the logic as part of a liberal world-view, rather than a realist one. At its core, this essay can be seen as an effort to make us think that my historical evidence would have been supportive of the liberal paradigm, if only the book had acknowledged its debt to the liberal understanding of opportunity cost and had provided a proper grounding in domestic politics. My argument, he claims, should be seen as merely a "variant of recent research in the liberal tradition" rather than one standing on its own as a competing argument. The best of this recent research -- most notably, Jeffry Frieden's and his own -- has already captured the notion that expectations of the loss of trade can drive conflict. But because this research also realizes that domestic politics must be incorporated into any good theory of international relations, it offers a stronger basis for understanding the true causal link between interdependence and war. My argument, on the other hand, presumes that it can explain shifts in the probability of war without recourse to unit-level variables. This, for Fordham, is an "unsustainable" theoretical position. The trade expectations approach might still be salvageable, but only if we go back to the drawing board to build in a necessary role for domestic politics in the study of war and peace.

At first glance, Fordham's argument might seem to have surface plausibility. But this is only because it is built on a series of half-truths about the argument I actually make and on distorted claims about what liberals are doing in their own research. Moreover, by deploying the technique of argument-by-fiat -- specifically, the notion that all theories worth their salt must necessarily start with domestic politics -- the review can make us think that self-proclaimed systemic theories are ultimately just weak versions of unit-level liberal theories. To see the deep flaws in Fordham's analysis, we will need to dissect the elements of his argument step by step.

Let us begin with Fordham's claim that I deny the "close relationship" between the liberal opportunity cost argument and my own, something that leads me to miss how liberals already understand the potential role that future lost trade might play in a state's decision for peace or war. It is first worth noting that I acknowledge on many occasions the importance of the liberal understanding of opportunity cost to my own argument (2, 35-36, 428). Indeed, the gains from trade that liberals emphasize are a foundational part of my definition of interdependence, as the above discussion reiterates (see also 8-9, 35-36). Yet I argue that the liberal view, while not wrong, is *incomplete*. It does not incorporate the realist sense of the "costs of adjustment" that may, as mentioned above, plunge an economy into a potentially deep relative decline should trade later be cut off (33-36). Moreover, my argument is based not on the welfare benefits a society and its groups receive from trade -- and the associated welfare losses should trade be severed -- but on how any gains from trade as well as any real costs of adjustment might affect a state's overall power position and thus its level of national security (2-3, 6-9, 27-28, 33-34, 47-49).

Fordham does not engage either of these elements of my theoretical set-up. He never mentions that my argument is founded on a security-maximization assumption, nor how such an assumption might lead to a different approach to state decision-making, one couched at the systemic level. Perhaps most surprisingly, he seems unaware of my argument linking negative expectations of future trade and costs of adjustment to a state's anticipation of long-term decline in power. Such concerns, I argue, can lead even moderate leaders who feel *constrained* by domestic politics to still initiate preventive actions and war to protect the long-term security of the state. In short, there may be absolutely no domestic forces pushing for war, and indeed many internal groups pressuring leaders to avoid conflict, and yet leaders may still feel driven by the systemic circumstances into hard-line behavior. This is not only a purely logical possibility within my theory's deductive structure (73). As I show with France and the scramble for Africa, or with Germany and the start of World War I, this can be the situation "on the ground" in real-world cases (391-94, 123-24).[10]

If worries about costs of adjustment and declining power can drive leaders to war despite domestic constraints, it is clearly incorrect for Fordham to argue that all theories of trade and conflict "must [build in] unit-level mechanisms" for transmitting the gains of trade to policy-makers or they are "unsustainable." In history it may be true that domestic politics are causally linked to the outbreak of many or even most wars. But this is an empirical question, not a theoretical question. To make progress in IR theory-making, we must leave behind all such arguments-by-fiat, and turn instead to a clear analysis of the competing claims of the different theories.[11]

Part of the problem with Fordham's set-up comes with his inflated claims regarding the liberal opportunity-cost argument. While he acknowledges that liberals frequently use the current value of trade to measure opportunity cost, he believes that this measure nonetheless "proxies the expected value of future commercial interaction." My argument regarding expectations is thus largely a clarification of a concept already embedded within liberalism as a whole. A careful reading of the book shows the limitations of this analysis. I state right at the start of chapter 1 that commercial liberals do occasionally acknowledge the importance of future trade, but that they do so within a set of theoretical presuppositions that reinforce the importance of current trade. Drawing from neoliberal institutionalism, they assume that trade going on today will continue into the future so long as all sides have an incentive to punish defectors, given that the absolute welfare benefits of continued cooperation will be seen as greater than the value of severed trade (17, 37).

Fordham says nothing about this argument, so it is hard to know how he would refute it. But more to the point, once we bring in realist insights regarding the costs of adjustment and vulnerability, we can see why current trade *cannot* be a good proxy for the expected value of trade. As I stress repeatedly (5, 16, 37-38, 248, 304), expectations for future trade can be negative and thus the expected value for trade low even when current trade is high, because leaders have a good reason to believe others are getting ready to cut them off, or have already started to do so (this was Germany's situation from 1900 to 1914). Conversely, when expectations for future trade are positive, even when current trade is low or non-existent, the expected value of trade can be high, giving states an incentive for peaceful relations (the U.S.-Soviet relationship 1972-74). So

[10] See also Dale C. Copeland, *The Origins of Major War* (Ithaca: Cornell University Press, 2000), chaps. 3-4.

[11] Fordham's claim that all theories must incorporate unit-level mechanisms is based on an erroneous reading of the terms of the discipline. Realists regularly talk about things such as a state's technology, the nature of the goods it exports and imports, the influence it garners by trade, and other factors mentioned by Fordham. But these things are not "unit-level" by definition, as he seems to think. Rather, they automatically become systemic once we look at them in relative comparison to the other great powers in the system. True unit-level explanations for behavior involve something that is only going on within the state whose policies we are trying to explain.

to suggest that liberals have already understood the importance of trade expectations, even if they have not yet fully exploited the notion in their use of current-trade proxies, is clearly off the mark. The vast majority of liberals simply haven't realized what happens to their arguments once costs of adjustment and expectations are fully theorized within a properly specified causal logic. If states that are dependent but have negative expectations can be propelled into hard-line behavior and preventive war even when current trade is high and there are no unit-level forces pushing for war, suddenly trade doesn't always seem like such a great thing.

We have seen so far that Fordham's theoretical critiques are largely empty of content, based as they are on a highly selective reading of the book's argument. But what about his analysis of its historical evidence? There is little question that the book poses a strong empirical challenge to liberalism. Once we go into the documents, we see that across forty case periods since 1790, the liberal opportunity-cost model can provide a partial explanation of the reasons for state action in only seven percent of the cases. The trade expectations argument and economic realism do significantly better, providing partial or decisive explanations for state behavior in 65 percent and 28 percent respectively (431-32).[12] These figures are potentially devastating to the commercial liberal program that has dominated the study of interdependence and conflict for three decades. In his fight to save the liberal program, Fordham chooses three tactics: attack my methodology; challenge my existing evidence; and, perhaps most surprisingly, accept that the book's documentary work is "illuminating" but then imply that it is potentially consistent with two liberal arguments that already incorporate actor fears of the loss of current trade (Jeffry Frieden's and his own).

Regarding methodology, Fordham argues that my effort to cover the essential universe of great power cases from 1790 to 1991 is misplaced. It leads me to go into great depth for some cases, but to cover others in only a few paragraphs. Yet as I discuss (78), two main methodological questions animate the book. First, how often does economic interdependence have anything to do with a great power shift to war/crisis or to the dramatic ending of an enduring rivalry? (Is it ten percent of the time? Twenty? Ninety?) Once this question is answered, we go to a second: Within the cases involving interdependence, how frequently does liberalism, economic realism, neo-Marxism, or the trade expectation approach explain the decision-making of the key leaders? Since this is a book that is first and foremost about the link between commerce and conflict (preface, vii), the ten case periods that are not about interdependence are typically covered in short order. This allows us to get an overall sense of how often systemic versus unit-level factors were at play in the sweep of history without getting bogged down in cases having little to do with trade and commerce (94-95). When the consensus opinion of historians favors unit-level causes (the French Revolutionary Wars, the French and Austrian interventions of the early 1820s, the wars of Italian reunification, etc.), I simply accept this opinion and move on (321, 326-27, 377). In this way, we can give full credit when credit is due to the larger liberal

[12] Because I allow the theories to explain aspects of particular cases at the same time, the numbers can add to over one hundred percent (93 n.40). This is especially so once we focus just on the thirty of the forty case periods where economic interdependence played a critical role in the onset of war/crisis or peace (here, the percentages are 10 percent, 87 percent, and 37 percent for liberalism, trade expectations theory, and economic realism respectively).

world-view, a view which stresses the importance of domestic and psychological pathologies and pressures as causes of war (13-14, 95 n.45, 432 n.2).[13]

Notwithstanding the above clarification, the thrust of Fordham's methodological critique here is evident. He believes that had I gone into more depth for the short cases, the results would not have been as rosy for trade expectations theory. He wrongly claims that I believe my theory explains "86.7 percent of the cases." The real figure, as noted above, is 65 percent of the total of forty case periods and 87 percent of the thirty periods having something to do with interdependence.[14] But by quoting the 87 percent figure out of context just after talking about my poor methodological set-up, Fordham seems to imply that I breeze through the cases with a clear theoretical bias, finding positive support for my own argument, while quickly rejecting any domestic-level arguments that run counter to my interpretations. Later in the same discussion, after briefly reviewing my case study of Japan 1941, he is more explicit. He states that the book needed to give more attention to discussions of the internal nature of states "before dismissing the causal importance of domestic political pressures."[15]

The puzzling claim that I dismiss unit-level arguments in my case studies reinforces the sense that many of the problems with Fordham's critique stem from an incomplete reading of the book. In every controversial case, include Japan up to Pearl Harbor, I self-consciously posed my systemic realist-based argument against both my best systemic challenger, economic realism, and against the domestic-level arguments for war *inherent* in both the commercial liberal and neo-Marxist arguments. For Japan up to the Pacific War, for example, I stress that the primary explanation in the historiography and IR literature is the idea that domestic and ideological pathologies took hold after 1930, driving the Japanese state into an irrational war (144-45, 184). As for other key cases, potentially better domestic-level explanations were always under discussion. Was

[13] I also deliberately added in what some might consider to be marginal cases, such as the French/Austrian interventions of 1821-23 and the Belgium crisis of 1830 -- cases that did not support my argument -- to avoid charges of selection bias (76 n.30, 96).

[14] He also implies that my results would have been different had I realized that China after 1880 was not a great power. If one wants to accept this claim, the Sino-Japanese War of 1894-95 and the Sino-Japanese War of 1937-45 can simply be left out of the data set. But since I give the first case to economic realism and the second to the broader paradigm of liberalism, reducing the number of case periods from 40 to 38 only increases the relative number of cases that trade expectations theory explains. I would still claim that China was a great power in the multipolar region of East Asia, albeit a weak one paralleling Italy's position in the Europe theater. But if Fordham wants me to drop the two Sino-Japanese wars and recalculate the percentages, I am not opposed; trade expectations theory only looks better after the procedure.

[15] I should note in passing that Fordham claims that "the book provides no rules for delimiting each 'case period'." Why cut the Cold War into 11 periods, he asks, but make the Napoleonic Wars and French Revolutionary Wars only one case each? In fact, the book provides two pages of discussion on this issue, laying out three criteria for drawing dividing lines between case periods: dramatic shifts in great power behavior and the probability of war; the geographic focus of the dispute; and the relative degree of independence of the cases. I note, with regard to the latter, that it would make little sense to treat, for example, the series of wars from 1803 to 1815 as separate cases, since they were all shaped by one overarching factor, namely, the hegemonic aspirations of Napoleonic France. Cases like the Napoleonic Wars work well for trade expectations theory. I thus deliberately imposed strict restrictions on such cases, counting them as "one" not "many" in order to avoid giving my own theory more hits than it deserved (79, continued on 91). Fordham may disagree with the use of these three criteria. But not to engage in a discussion of them, and then to imply that a consistently fine cutting-up of cases across the board would necessarily hurt the overall explanatory power of my theory, is clearly a non-starter.

Napoleon really driven by ego and glory? Did the British bring on the Crimean War and later the Boer War for social imperialist reasons? Were French and German leaders scrambling for African colonies in 1882-85 to improve their domestic positions? Were Russian bureaucratic struggles the cause of the Russo-Japanese War? Did Joseph Stalin's ideology and greed lead to the crises of the early Cold War, including the Korean War? Was Mikhail Gorbachev's alternative ideology the main reason for the ending of the Cold War? At every step of the way, I spent a great deal of time grappling with alternative unit-level explanations for the cases. I thus find myself baffled by Fordham's claim that I dismiss domestic-level explanations in my rush to confirm my theory.

One might even ask the opposite question: Why did I spend so much time considering domestic politics when my theory is a realist-based logic pitched at the systemic level? Why not shorten an already-long book by sticking with "plausibility probes" that would at least illustrate the potential value of the argument? The answer has to do with the nature of the competing arguments and the methodological issues outlined above. Considering domestic explanations was critical to my own research agenda, given that I wanted to test how often the liberal hypothesis that domestic forces are unleashed when trade falls actually provides the best explanation for the cases. After doing as objective an analysis of the documents as I could, and after giving the larger liberal paradigm full credit for cases that Fordham would find marginal (the French/Austrian interventions, the Sino-Japanese War of 1937), I found myself genuinely surprised at the paucity of evidence for the more specific claims of commercial liberalism. The fact that I picked a historical epoch, 1790 to 1991, that was generally favorable to commercial liberal theories and hard for realist-based economic arguments (the era after the golden age of mercantilism and the seminal writings of Adam Smith) only reinforced my surprise (76).[16]

While scholars may find the book's empirical results unsettling, it is up to the proponents of commercial liberalism to show that domestic factors tied to interdependence did indeed drive states into war more often than I assert. It is not enough to contend, using Japan 1941 as his example, that domestic selection pressures often produce a homogeneous group of decision-makers driven by common assumptions and priorities, and that in such cases "perhaps [a different] group of decision-makers would have made a different decision." For the Japan case, I devote two chapters on the shift from the peace of the 1920s to the final year of US-Japanese negotiations and the tragic plunge into the Pacific War. The domestic argument of Jack Snyder and others -- that a militaristic domestic culture took over after 1930 and drove Japan into an irrational war -- is a prime and ongoing target of my analysis. I show that Japan's relatively moderate policies of the 1920s had little to do with "Taisho democracy" or reasonable politicians such as Shidehara Kijuro. I also demonstrate that even by 1941 there was a diversity of opinions regarding war with the United States, and that indeed those wanting a deal with America held sway through all three rounds of negotiations during that fateful year. A detailed

[16] My larger methodological agenda included the testing of systemic and unit-level variables across all the cases. Here, I was also surprised by the overall weakness of unit-level arguments. Thus my statement in the introductory chapter that the book shows that domestic forces were "hardly ever" the primary propelling forces pushing states into war (14). Fordham quotes this phrase out of context and then claims that I assert that "domestic political processes 'hardly ever' matter in decisions about war and peace." Reading the full paragraph shows my larger, if still provocative, point. I argue that across the broad sweep of cases, domestic factors did often play subsidiary causal roles "as factors that reinforced or facilitated a leader's desire to get a war going." I then mention a number of instances where unit-level factors clearly did "stand out" as "important contributing causes of conflict," including in Nazi Germany and Russia before 1904. Yet I argue that documentary evidence shows that these factors should really be seen as forces that "existed alongside more geostrategic reasons for action, making it unclear just how necessary [they] were to the eventual outbreak of war" (14).

examination of the Japanese conferences from June 1940 to November 1941 reveals that military hot-heads never "hijacked" the state, and that the Emperor and the civilian and military leaders reluctantly accepted war as the lesser of two evils, despite the low estimates of ultimate victory (chaps. 4-5).

Fordham utilizes one final tactic to restore the plausibility of liberal unit-level arguments in the face of the book's historical evidence. He claims that I ignore a substantial body of research by liberal scholars asserting that trade can indeed lead to conflict as well as to peace. Certain domestic factions might benefit from a curtailing of commercial relations, and thus might even promote conflictual relations toward this end. Third-party concerns might also cause the executives of dependent states to intervene to protect the current trade benefits of domestic constituents. He cites his own work and Frieden's as exemplars here. He then goes on to claim, as we have seen, that all good arguments dealing with commerce and war, including my own, necessarily depend on the unit-level features of trading states. In this way, Fordham's review can leave us wondering whether I am simply reworking arguments already made by liberal scholars, arguments that possess a more "sustainable" theoretical foundation in domestic politics.

There are, however, a number of distortions lurking behind Fordham's position. First, it ignores my summary of the diverse array of liberal, as well as neo-Marxist, arguments found in the literature. I spend a fair amount of time discussing those liberal scholars who emphasize the way different social groups may have separate "vested interests" in open trade or restricted trade. As I note, scholars such as Beth Simmons, Jonathan Kirshner, and Patrick McDonald have shown that the liberal argument that trade constrains actors from going to war should only really apply to those groups that benefit from trade.[17] It is their fear of the opportunity costs of a cut-off of trade that keeps the peace, while other actors not benefitting from trade may have little commitment to peaceful relations (19-20).[18] Yet notice how different such arguments are from my own. The claim of these scholars is that those social groups whose welfare benefits or profits might be destroyed by a trade cut-off are the ones most concerned with protecting current trade and seeing it continue. My argument focuses instead on the long-term security objectives of state leaders. It argues that leaders will

[17] Beth Simmons, "Pax Mercatoria and the Theory of the State," in *Economic Interdependence and International Conflict*, edited by Edward D. Mansfield and Brian M. Pollins (Ann Arbor: University of Michigan Press, 2003); Jonathan Kirshner, *Appeasing Bankers: Financial Caution on the Road to War* (Princeton: Princeton University Press, 2007); McDonald, *Invisible Hand of Peace.*

[18] Those scholars arguing that certain economic actors might push for hard-line policies and military expansion to feather their own nests, such as J. A. Hobson and Kevin Narizny, I generally put in the neo-Marxist camp (22-23, 23 n.14). This is where I would place the 1994 Frieden article cited by Fordham. For Frieden, it is the economic characteristics of particular sectors and the vulnerability of these sectors' overseas profits to host-government actions that determine whether these sectors will pressure the metropole executive branch to intervene abroad, even to the point of setting up a colonial administration. But Frieden theorizes and measures only these sectors' vulnerability to cut-off, rather than examining their expectations of future trade and the role of the diplomatic bargaining between states. All such theories influenced by neo-Marxist thinking could indeed be made more dynamic by the explicit incorporation of expectations into their deductive structures. But this has not yet been done. Unfortunately, even if neo-Marxist approaches were to be made more dynamic, they would still likely have empirical problems. As my historical work shows, there is little historical support for the view that core sectoral interests push reluctant great power executives into war or aggressive behavior (the best case being the Opium Wars of 1839-42).

turn to hard-line behavior and war only when expectations for future trade turn negative, creating a fear of the decline in the state's relative economic and military power position vis-à-vis other great powers.

Fordham's own published work on this subject nicely aligns with the vested-interests and welfare-maximizing assumptions of Simmons, Kirshner, and McDonald. In one of the recent articles he references, he stresses that "concerns about welfare gains" lead politicians in the US Congress to have "differing attitudes toward potentially hostile trading partners depending on the interests of their constituents."[19] Constituents that fail to benefit from trade with China, for example, will pressure their representatives to lobby for trade restrictions on China. This might have the effect, Fordham notes in two brief passages, of causing the Chinese government to take note of increasing US domestic opposition to trade, leading in turn to a deterioration in political-military relations between the two states.[20] Note what this argument is asserting. It is not saying that domestic political pressures within the United States tied to the opportunity costs of trade push the American executive to undertake hostile military policies (the dependent variable of interest in my book). Rather, it is saying that those domestic actors receiving the benefits of trade want peace, and those that lose from trade care little about peace per se, but want the trade to end for self-centered welfare reasons. But the latter group, given that it might lead the United States to adopt trade restrictions, could lead the *other* great power in the relationship, such as a China highly dependent on continued trade with America, to embark on more hostile policies, including in the military realm.

This type of argument is perfectly consistent with what I argue liberal scholarship should be doing in its explorations of unit-level factors. It should be looking at how domestic infighting within a less dependent state might lead more dependent states to fear the future trade relationship and to respond with a hostile military policy that creates a trade-security spiral (445-46).[21] I explore this phenomenon in my case study of U.S. domestic politics in 1972-74. It shows how congressional political struggles undermined Soviet expectations of future trade, leading to a new round of cold-war struggle (304-10). The key point is this: Simply stating that domestic politics and trade can lead to conflict does not mean one has a unit-level theory of war if the actor pushed into militarized behavior is not the state suffering from internal divisions, but rather the more security-driven state that is *observing* developments within the first state and concluding that the trade relationship is now on shaky grounds. The security-maximizing state is responding to something outside itself, as opposed to being pressured into hard-line behavior by its own domestic situation (28 n.23, 46-49, 46 n.51, 95, 435-36, 445-46).[22]

[19] Kleinberg and Fordham, "The Domestic Politics of Trade and Conflict," 605.

[20] Ibid., 606, 615.

[21] Since this is what Fordham's article achieves, it is ironic that on the last page of his critique he finds my recommendation problematic.

[22] This point, drawing from Stephen Walt's influential balance-of-threat theory (95), was first articulated in Dale C. Copeland, "Economic Interdependence and War: A Theory of Trade Expectations," *International Security* 20 (4), 1996: 5-41; Dale C. Copeland, "Trade Expectations and the Outbreak of Peace: Détente 1970-74 and the End of the Cold War 1985-91," *Security Studies* 9 (1-2), 1999-2000: 15-58. See Stephen M. Walt, *The Origins of Alliances* (Ithaca: Cornell University Press, 1987).

Fordham's other referenced article comes closer to explaining hard-line military decisions as a function of the intersection of trade and domestic politics. The article reexamines the revisionist arguments from the interwar period that claim that Wilson entered into World War I for economic reasons.[23] Fordham shows convincingly that those regions that were receiving huge windfall gains from U.S.-British and U.S.-French trade after 1914 were far more keen on protecting these new-found gains than those that did not experience an upsurge in trade with Europe. Notice, however, how this argument really works, and how different it is from the kind of explanation trade expectations theory would put forward.[24] Fordham argues that Wilson and other politicians felt "societal pressure" to protect the burgeoning U.S.-British and U.S.-French trade because of the "opportunity costs of lost trade," and that Wilson in particular feared he would not get reelected in 1916 if he did not respond to this pressure. Fordham takes a snapshot of the increase in trade for particular American states from 1913 to 1916 to show that regions experiencing a large trade jump were more inclined to support Wilson in his efforts to protect this trade, even at the risk of war.[25]

This is a good start to the building of a new liberal theory of trade and war that takes into account potential threats to current trade and not simply the trade benefits that increased commerce brings. But in terms of theoretical set-up, it is closer to the economic realist and neo-Marxist position that increases of trade on their own can lead to more hostile behavior, simply because the actors -- in this case, the domestic constituents benefitting from high trade -- will push for hard-line policies to reduce their growing vulnerability to cut-off (7-8, 21-23). Fordham has no measures for expectations in his article (nor theorization of the concept). Instead, to test his argument, he simply examines changes in current trade, assuming that more dependent regions will be more hard-line because they believe Germany might threaten the now-larger U.S. trade with Britain and France.[26]

What we have here is a notable beginning of an effort to make commercial liberalism a more plausible theory of international relations. Other liberal scholars should follow Fordham's lead and incorporate into their arguments unit-level mechanisms that allow high-trading regions to push executives into hard-line policies due to increasing vulnerability. This would help liberalism build a theory that can compete empirically with

[23] Fordham, "Revisionism Reconsidered."

[24] In my companion volume on economic interdependence and American foreign policy, I have a separate chapter on America's entry into World War I, in which I take on neo-revisionist arguments such as the one Fordham puts forward. Dale C. Copeland, *Commerce, War, and American Foreign Policy, 1790 to the Present Era*, forthcoming, chap. 4.

[25] Fordham, "Revisionism Reconsidered," 280-82, 287-88, 295-304.

[26] It is worth noting that Fordham ends his measures of increased current trade in 1916. After May of that year, U.S.-German relations were on a firmer basis as a result of diplomatic concessions by both sides (U.S.-British relations meanwhile deteriorated). When Wilson campaigned in 1916, he could thus claim that he "kept us out of war." Any support Wilson received from the high trading regions of the United States during the May-December 1916 period was therefore based on the regions' sense of economic vulnerability and worst-case thinking (economic realist notions) rather than on expectations regarding the US-German relationship per se. Indeed, it was only the double-whammy surprise of the Zimmermann telegram and the start of Germany's unrestricted submarine warfare campaign in early 1917 that finally pushed a reluctant Wilson and his national security team to accept armed neutrality and then a declaration of war. See Arthur S. Link, *Wilson: Campaigns for Progressivism and Peace, 1916-17* (Princeton: Princeton University Press, 1965); Copeland, *Commerce, War, and American Foreign Policy*, chap. 4.

economic realism and trade expectations theory. But to create a truly powerful theory, liberal scholars should focus not just on snapshots of increasing regional trade dependence within states, but on each region's expectations of future trade and the degree of confidence actors have in diplomatic solutions to current trade problems. Liberalism would then have a truly dynamic theory with the ability to predict changing foreign policies over time, even when current levels of trade are high and largely static over time.[27]

Such a reformulation of liberalism would allow for a clear set of empirical tests, posing this new more dynamic argument rooted in domestic politics against economic realism and trade expectations theory, with their assumptions of security-maximizing states. The three arguments would each pose different causal mechanisms to war and peace, allowing in-depth documentary work to reveal how often each theory can explain the events of world history. Yet as we have seen, it is an exercise in futility for scholars to try to force realist-based arguments such as trade expectations theory into some sort of expanded (and potentially hegemonic) liberal paradigm. Domestic political factors, even if they are sometimes determinative, are not critical forces of state behavior in every case, as my evidence reveals. Moreover, even when they are involved, they may be operating as facilitating or reinforcing factors, rather than as propelling ones (14, 73). So instead of dismissing evidence showing that leaders are often pushed into war because of external forces and fears of decline, liberal scholars should stay focused on how often one theory works better than another to explain specific cases (77-78). Only in this way can political scientists help leaders understand the conditions under which one theory is more likely to be useful than another in the real world of great power politics.

[27] Neo-Marxist-influenced arguments could also be reformulated along these lines. See footnote 18 above.