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## **THE EMPERORS' NEW TAUTOLOGY:**

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### **REPLY TO KRASNER, LAKE, IKENBERRY — AND ALSO SKOCPOL**

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I am delighted to participate in this debate with Stephen Krasner, David Lake, and John Ikenberry, and I thank them for the comments. However, I should note that the main object of my critique was Theda Skocpol's approach, and Skocpol, unfortunately, has opted not to reply. If she should choose to address my criticisms in the future, I look forward to reading her comments.

My principal argument is that the statist analysis of business interests is based on tautological reasoning, in which *a priori* assumptions guide the research and predetermine the conclusion. I am a bit surprised, therefore, that Krasner, Lake, and Ikenberry pass over this point. Krasner and Lake touch upon the tautology question but they fail to address it in any serious way; Ikenberry does not mention it at all. Instead, they seriously misconstrue my arguments. Ikenberry, for example, says that my "main argument is that this research [statism] needs to pay more attention to 'business interests' in explaining American government policy." Ikenberry is mistaken; my main argument is that the statist approach is a tautology.

Krasner, Lake, and Ikenberry all criticize my essay for not providing an adequate theoretical model of business influence. However, my

essay never claimed to provide a theoretical model; I merely evaluated the statist approach. To be sure, I have advanced an alternative approach, termed "the business conflict model," and have discussed it at length elsewhere.<sup>2</sup> I have not brought it into the present debate (until now) mostly because it is not my subject here. However, as others have speculated about my theoretical perspective, let me pause and describe the model. My business conflict model posits that under certain circumstances, divisions within the business community can interact with state institutions and can significantly influence policy. This model was the basis for my book-length study of foreign intervention in Central Africa and has also influenced major studies by others of the French oil industry, U.S. relations with Latin America, U.S. trade policy, and interwar diplomacy in the Far East.<sup>3</sup>

This is not the place to discuss at length my theoretical perspective, but I would like to clear up some misunderstandings. Lake refers to the "instrumentalist school of which Gibbs appears to be part." Actually, my theoretical approach is sharply different from instrumentalism, especially on the question of conflict within the business community. Krasner points out that "one of the most disappointing elements of Gibbs's discussion is his implicit assumption that business can be treated as some kind of unified whole." Krasner's characterization is off the mark. This is the opposite of my business conflict model, which not only acknowledges that business is divided but insists that such divisions constitute a major feature of politics in market economies. And Ikenberry notes, "Without a more sophisticated theoretical orientation, the focus on business interests quickly becomes an exercise in 'gotcha' journalism: an ad hoc search for tidbits of information." My business conflict model, in fact, provides precisely this theoretical orientation, with which business influence can be examined systematically. I would be happy to argue further the merits of my model, but for reasons of space, I must do so in a separate debate.

In any case, this is a distraction. Whether I have an adequate alternative theory or not, my argument here is that the statist approach is tautological. Is this argument valid or invalid? That is the question on which attention should be focused.

Let me now discuss each of the three replies, beginning with Stephen Krasner's. His reply presents arguments that purport to explain why the president and secretary of state are insulated from business interests. The president, Krasner says, faces a national constituency and the sheer size of that constituency prevents businesses from influencing

key agencies of the executive branch. On reflection, however, this point does not hold up. A national constituency does not imply an autonomous executive; it only implies that *powerful* companies will influence the executive, while the small fry will be left to influence Congress. But Krasner at least attempts to explain executive insulation, and this strengthens his case somewhat. (Krasner also raises nonsubstantial matters, complaining, for example, that I failed to cite his famous article and that I allegedly mischaracterized his book index, but these do not require comment.)

In "Taking the State Back Out," one of my principal criticisms was that Krasner presented one-sided accounts of U.S. interventions in Guatemala, Iran, and Chile. Yet Krasner makes only the weakest effort to defend his case studies. On Guatemala, he simply makes a sarcastic comment about a secretary's ties to the United Fruit company, while he avoids completely the United Fruit connections of the CIA director, the secretary of state, the undersecretary of state, the assistant secretary of state, and the U.N. ambassador. These facts do seem relevant to an analysis of whether United Fruit influenced foreign policy, but they are absent from both Krasner's book and his reply. Krasner stresses that the Justice Department brought an antitrust action against United Fruit and that this supports his contention that the company had little influence — but he concedes that the company had enough clout to delay divestiture for almost two decades after the intervention in Guatemala. On Iran, Krasner notes that the U.S. government helped a variety of oil companies although, so far as I can see, this fact in no way challenges my arguments. And, again, he neglects to mention the Dulles brothers, their ties to Standard Oil of New Jersey, or that Jersey-Standard profited immensely from the Iran intervention of 1953. He makes no effort at all to refute my criticisms of the Chile case study. Krasner is surely right to emphasize that it is difficult to prove causality in international relations, but that does not justify his empirical inadequacies here.

Krasner also recounts numerous additional cases which, he claims, support his views. I lack the expertise to comment on most of these cases. It should be noted, however, that Krasner's claims that the U.S. government failed to support the major oil companies during the 1970s and that business interests did not influence the U.S. decision to go to war in Korea are contradicted by major secondary studies.<sup>4</sup> There is one case that Krasner mentions — U.S. intervention in the Congo/Zaire — on which I do have some expertise, since I have spent nearly a decade researching this

and have had some two thousand pages of government documents declassified under the Freedom of Information Act. I have also visited all the major U.S.-based archives pertaining to this subject. On this case, I find Krasner's account to be misinformed and inaccurate. In fact, business interests were major, often decisive, influences on U.S. policy in the Congo, despite the relatively low level of American investment there.<sup>5</sup> Based on this case, and also Krasner's inadequate responses to my previous criticisms regarding the Guatemala, Iran, and Chile cases, I reaffirm my conclusion: The empirical sections of *Defending the National Interest* are seriously flawed. In his reply, Krasner seems to recognize the problem and succinctly states, "Gibbs also takes me and my colleagues to task for sloppy empirical work."

Finally, Krasner reveals the tautological nature of his approach when he writes, "I defined the state as roles and agencies with a 'high degree of insulation from specific societal pressures. . . .' [T]he presidency and the State Department are the clearest examples." Inadvertently, Krasner confirms my main argument. Key government agencies are considered autonomous *by definition* and it is this definition, presumably, that guided Krasner's research. If this is not a tautology, what is?

David Lake's reply I found very difficult to interpret. First, it is internally contradictory. At one point, Lake implies that his book developed a theory of foreign trade policy and tested it (he notes that statist predictions "could be, and were, subsequently assessed against the historical record" and that "I found some evidence to support my theory"). Later, Lake implies that he never tested anything, and that the empirical discussions in his book were just descriptive or illustrative ("I did not seek to test in any rigorous way the explanatory power of my theory against its possible rivals, although I attempted some general assessments"). There is a basic inconsistency here and such tergiversations mar the reply. Or, perhaps, Lake is just not making himself clear. At another point, he claims to use a novel method for evaluating business influence, and this method makes it unnecessary to consider the activities of specific firms; but he fails to say what this novel method is or, at least, if he does specify his method, I cannot understand it. Lake denies that his theory is tautological ("Gibbs mistakes theory for tautology"), but this is neither explained nor justified, and so I cannot evaluate it.

Lake also tries to defend his theory and, like Krasner, he says that the executive branch is autonomous because of the size of its constituency. This is one of Lake's stronger points but, once again, the reasoning does

not work: A national constituency does not necessarily insulate executive agencies from business, it only insulates them from small, localized businesses. And Lake adds: "Nowhere does he [Gibbs] demonstrate . . . how or why a shift in empirical focus would produce a more powerful explanation." This is correct. I criticized Lake for biased selection of evidence and advocated a more balanced approach. I left open the possibility that a balanced approach might confirm Lake's views on state autonomy — or might disconfirm them. The issue must remain hypothetical, however, since Lake's analysis was not balanced.

In "Taking the State Back Out," I raised the following specific objections to Lake's analysis: His book virtually omitted private interests from discussion; it cited the Ferguson study on the New Deal but failed to mention Ferguson's substantial evidence of business influence on trade policy; and the book contained only four sentences pertaining to business influences on the U.S. executive branch. I argued, in short, that Lake assumed state autonomy and then avoided contrary evidence. In his reply, Lake does not dispute these things. He even concedes a measure of bias, that he and his colleagues "may overstate the importance of the state." This is an understatement.

I will now consider John Ikenberry's reply. It begins by noting that my essay is "remarkably modest — even disappointing." Well, I argued that the statist analysis of business-government relations is "an extended exercise in tautology," and readers can judge whether I am unduly modest.

Ikenberry's reply raises some valid points in defense of his 1988 article and his book (both of which analyzed U.S. oil policy during the 1970s). He is correct to state that the proposed windfall profits tax does weigh somewhat against the argument of business influence (although it should be noted that many oil companies made very substantial profits from price decontrol, despite this tax). Also, Ikenberry does address one of my main points, that many independent oil companies initially supported the Carter presidency and that this support might have influenced Carter's oil policy. Ikenberry, however, focuses purely on electoral considerations and fails to consider the question of oil company financial contributions, which were, by far, the companies' main source of influence. But Ikenberry does address my point.

To consider points that Ikenberry's reply does not dispute: In "Taking the State Back Out," I argued that Ikenberry's 1988 article was inadequately researched, that it included almost no data on oil companies.

and that the entire article contained exactly six sentences that even mentioned specific business interests. I argued, in short, that the article dismissed business influence without ever looking at business in any serious way. These points are left unchallenged.

Ikenberry attempts a very general defense of his thesis, which seems feeble. His reply notes, for example, that "My discussion of the domestic oil industry does make one important distinction: between producers and refiners." The discussion that Ikenberry refers to is so limited that it does not even contain the names of most of these producers and refiners (he mentions exactly two companies). Looking at Ikenberry's footnotes, one can see that his oil research was based almost entirely on secondary sources. Also, it does not include some of the most basic studies of the oil industry, such as those of Robert Engler, Michael Tanzer, or John Blair,<sup>6</sup> nor does it cite any of the major oil publications. Even such easily available sources as the *Petroleum Intelligence Weekly* or the *Petroleum Economist* are absent. At one point in his reply, Ikenberry confidently asserts with regard to oil decontrol that the "interests were many and divided. The most important ones were not even business; they were consumers." It is hard to take this claim seriously since Ikenberry has not even told us which interests were involved, and hence is in no position to evaluate their relative importance.

Thus, Ikenberry dismissed the role of oil companies in policy-making, even though he never really studied oil companies. In his reply, Ikenberry seems to concede this: "Gibbs notes that I mention two specific oil companies, and he argues that this reveals a bias in my analysis. Gibbs needs to show why mentioning ten or even fifty firms, for example, would or should alter the conclusion." It is difficult to know how to respond to a comment of this type, except to point out that detailed information on oil companies is obviously vital for Ikenberry's argument that the oil companies played little or no role in policy-making.

Ikenberry also defends his research on the role of state officials, and here he is on stronger ground. In fact, this section of his 1988 article does indeed provide important insights into policy-making. Ikenberry has undertaken considerable and impressive research on state officials — but he managed to neglect private companies almost completely. There seems to be an asymmetry here. It comes as no surprise that Ikenberry found state officials more important than oil companies, and, given his methodology, it is difficult to see how he could have found anything else. Ikenberry's

argument appears to be based on an *a priori* assumption of state autonomy. An argument like this cannot be falsified.

The autonomy of the state is now a fashionable view in the social sciences. It was not always so, however, as earlier works by such persons as E. E. Schattschneider and Charles Beard suggest. There is also a body of literature on the question of business conflict and politics, which will continue to grow both in size and in empirical and theoretical sophistication. Statism still offers much that is valuable, but its analysis of business interests is tautological and empty.

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#### NOTES

- 1 Comments were provided by Edward Muller, Diana Rix, William Dixon, Thomas Ferguson, John Theis, Cary Nederman, and David Wilkins.
- 2 David N. Gibbs, *The Political Economy of Third World Intervention* (Chicago: University of Chicago Press, 1991), introduction and chap. 1.
- 3 See citations in endnote 46 of "Taking the State Back Out."
- 4 Michael Tanzer, *The Energy Crisis* (New York: Modern Reader, 1974); and Bruce Cumings, *The Origins of the Korean War*, vol. 2 (Princeton: Princeton University Press, 1990), chaps. 1-3. Tanzer, a former economist with Jersey-Standard, discusses U.S. government collaboration with the oil companies throughout the post-World War II period, while Cumings argues at length that conflicts between protectionist and internationalist blocs of business interests significantly influenced U.S. conduct in Korea.
- 5 Gibbs, *The Political Economy*, chaps. 3-6.
- 6 Robert Engler, *The Politics of Oil* (Chicago: University of Chicago Press, 1961); Michael Tanzer, *The Political Economy of International Oil and the Underdeveloped Countries* (Boston: Beacon, 1969); and John Blair, *The Control of Oil* (New York: Pantheon, 1976).