Theory and Method in the Study of International Negotiation: A Rejoinder to Oran Young

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Under what conditions do officials in international organizations wield influence as informal political entrepreneurs? Two conditions, I argue in the article under discussion, must obtain: First, an asymmetry in the availability of information or ideas must impede national governments from negotiating Pareto-efficiently. High transaction costs, relative to gains, must induce a coordination failure. Actors able to provide the three essential entrepreneurial functions—initiatives, mediation, mobilization—must be scarce. Second, international officials must enjoy or be accorded privileged access to the information or ideas necessary to act as entrepreneurs and induce more efficient interstate bargains. Where either of these two conditions is absent, third-party entrepreneurial activities are likely to be redundant (because states, as stakeholders, would have greater means and incentives to act as entrepreneurs) or futile (because a consensual support among states would not be forthcoming anyway).

My empirical analysis reveals that in the European Union (EU) these conditions, and therefore effective influence by entrepreneurs, are exceedingly rare. They arise only where particular domestic, not international, coordination problems exist—such as in the mobilization of bureaucracies and multinational business around the Single European Act in the mid-1980s. Far from being scarce, an abundance of actors tends to be willing to initiate, mediate, or mobilize; hence, entrepreneurship rarely imposes a binding constraint on negotiation. Moreover, when barriers to efficient international negotiation are present, they tend to be domestic, not interstate—a finding with significant implications for our understanding of international cooperation.

Oran Young, who has contributed as much as any modern international relations theorist to a more subtle and sophisticated understanding of entrepreneurship and negotiation, challenges these conclusions. I restrict my response to two underlying areas of theoretical disagreement that are particularly relevant to future scholarship in this area.

First, Young is simply incorrect when he asserts that my framework "misses the essential role that entrepreneurial leaders play," namely, to overcome "rigidities arising from the use of threats, promises, committal tactics, and other stratagems on the

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part of actors seeking to maximize their own payoffs." This type of bargaining failure, the most common found in the bargaining literature, is indeed consistent with my approach; in the article I *do* in fact consider it in detail as one of five basic sources of bargaining failure. Young fails to confront my detailed theoretical and empirical analysis.

Under the rubric "The 'Honest Broker': Impartial Mediation," I consider the possibility that "the strategic incentive to conceal information about preferences constitutes the 'real' bargaining problem." On a theoretical level, I am skeptical because "parties with an incentive to withhold information from one another will have a similar incentive to withhold information from a mediator . . . absent discretionary powers on the part of that mediator"—an insight for which I cite none other than Young himself! On an empirical level, I point out that "shared interests and democratic openness offer many opportunities to discover the relative intensity of preferences and to locate opportunities for mutually beneficial linkage"—a conclusion confirmed by cross-national and cross-issue studies of interstate bargaining and (albeit often unwittingly) by some recent studies of entrepreneurship in the EU.[1]

On an empirical level, my analysis of major EU decisions provides strong evidence that the "honest broker" view indeed lacks explanatory power. If it were correct, we should observe at least four things: (1) governments have asymmetrical, incorrect, or uncertain assessments of one another's true preferences; (2) governments fail to advance critical compromise proposals on their own; (3) governments treat supranational officials as more impartial than one other; and (4) third-party entrepreneurship should be relatively more important in cases of high distributional conflict, such as negotiations over agriculture. Cross-issue comparison and close, primary-source-based analysis confirms none of these four hypotheses—even in cases where informal entrepreneurship appears to have influenced the negotiated outcome. Further debate over entrepreneurship should take place, it seems to me, not at the level of conjecture, but at this more fine-grained level of deductive consistency, testable hypotheses, and empirical data.

Young's second criticism is more general. It concerns the likely location of transaction-cost barriers to efficient international negotiation among developed countries. Young continues to believe that they lie primarily in *interstate* bargaining failures. He therefore remains skeptical of my suggestion that the system-level transaction and adjustment costs of interstate negotiation are relatively low, at least in stable negotiations among developed countries, relative to the gains states can reap—far lower, anyway, than corresponding domestic and transnational costs. As evidence for the existence of interstate transaction-cost barriers to negotiation, Young points to the extreme length of some international negotiations (for example, SALT talks, GATT rounds, and long periods between EU activism) and the failure of others (for example, Climate Change and Law of the Sea). In such negotiations, Young asserts, third-party leadership is likely to play an important role.

Narrowly construed, of course, Young's premise is correct: international negotiations are often complex and difficult. Young is correct, moreover, that states may face

1. See, for example, Sandholtz 1992, 304.

greater difficulty in locating Pareto-improving bargains in negotiations with more actors, less sophisticated states, or looser rules. Yet should we therefore assume, as Young appears to do, that bargaining failures in such cases could have been overcome by more skillful third parties or appropriate institutions?

Not necessarily. If EU negotiations are any indication, what *appear on the surface* to be delays and suboptimal outcomes imposed by interstate coordination failures are most often cases of limited or slowly evolving win-sets imposed by *domestic* structural constraints. The swift conclusion of negotiations—efficient international bargaining on a "spot market," as it were—is often precluded by the slow evolution of societal demands for cooperation (for example, liberalization of nontariff barriers in the EU did not occur until strong economic pressures emerged in the 1980s), the search by governments for optimal electoral timing (for example, the Maastricht agreement was designed to suit Helmut Kohl's continuous electoral calculations), and the need to craft a complex domestic bargain (for example, the constraining role of EU agricultural reform on the Uruguay Round). Failure often reflects the absence of a strong consensus of governments in favor of the imposition of short-term economic costs (for example, climate change). To take another of Young's examples, East–West talks on nuclear arms reduction proceeded slowly when some governments were unenthusiastic about them, yet talks were concluded almost instantaneously once Mikhail Gorbachev fundamentally altered Soviet domestic and global aims.

It is quite misleading to casually attribute these sorts of failed or delayed negotiations to constraints imposed by unskilled, ill-informed, opportunistic, or unlucky negotiators at the international level, as Young and many in the negotiation analysis literature often do. (It is similarly misleading to casually attribute success to the activities of Tommy Koh in the Law of the Sea negotiations or Mustapha Torbal of the UN Environmental Program at global environmental talks.) The binding constraint in each of these examples appears to lie in domestic adjustment and bargaining costs—electoral, interest group, and bureaucratic politics—not in interstate bargaining failure, as Young asserts. Such structural constraints are unlikely to be loosened by informal political entrepreneurship, except in the narrow subset of circumstances I identify.

The deeper and more general lesson here is that empirical research on international negotiation requires rigorous theoretical and methodological tools. It is here where my analysis seeks to make a distinctive contribution. Theoretically, conclusive causal analysis requires narrower theories of bargaining that generate clear, falsifiable hypotheses within explicit empirical domains. For the most part, existing negotiation analysis does no more than describe various potential channels of influence. Only by moving beyond such work can we explain *variation* in the influence of supranational entrepreneurs, as I seek to do in the case of the EU. Is Young's alternative typology able to do this?

Methodologically, future social scientific analysis that aims to determine whether a "disappointing" negotiated outcome reflects an interstate bargaining failure (let alone one that could have been overcome by entrepreneurship) must be designed to control explicitly for the possibility that we are actually observing domestic bargaining failures or structural constraints. The converse claims similarly require controlled study: The existence of one or more active entrepreneurs and a successful outcome is not necessarily evidence of influence. If entrepreneurship is abundantly supplied, the fact that *some* actor acts as an entrepreneur may be a necessary condition, but it is one that is always fulfilled and thus of little causal interest. At best, the identity of entrepreneurs will be redundant, like the existence of oxygen in explaining a particular fire; at worst, third-party entrepreneurship may be futile, with the real work being done by the interested governments themselves. In either case, rigorous analysis into entrepreneurship requires that the analyst collect data on the underlying preferences, information sets, and strategies of governments and major domestic actors—a task that requires linguistic, archival, and interviewing skills rarely deployed by negotiation analysts—and then conduct an analysis controlled for alternative sources of entrepreneurship.

The absence of such controlled, primary-source-based tests of explicit hypotheses constitutes, I submit, the most fundamental methodological weakness in the study of negotiation today. It is this weakness that my analysis, particularly in the book underlying the article cited by Young, explicitly seeks to redress.[2] Young's alternative conjectures, while intriguing, do not engage my claims on this fundamental level.

Although Young's criticisms are important, they should not obscure the broad underlying areas of agreement between us. We agree that there is likely to be theoretically significant variation in the overall importance and causal forms of informal entrepreneurship across issues, institutions, and countries—variation that could serve as the basis for illuminating comparative studies. In this context, my article should be seen not as an assertion that no significant interstate transaction costs exist, but as a demonstration that there are some areas in which they are very low and therefore that sizeable variation exists. Young and I agree, moreover, on the need to specify more precisely the casual mechanisms by which effective entrepreneurship takes place. In particular, numerous actors, from national leaders to leading scientists, might act as entrepreneurs, creating intriguing and heretofore unexploited opportunities for research around the question *Who* is the entrepreneur? Finally, and above all, we agree—and on this point we are all indebted to Young for his constant reminders—that in an international system where nearly every outcome worth studying is negotiated, ever increasingly within highly institutionalized settings, deeper theoretical and empirical analysis of the issues raised by international entrepreneurship and negotiation is vitally important.

References

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2. Moravscik 1998.