Nicolas Guilhot and Ido Oren both contribute useful insights, in the paper Guilhot published in *Behavioral Sciences*, and in Oren’s review of it here. I would like to offer a more personal perspective based on my own experience. As a young History student at Columbia College from 1969 to 1973 I did, nevertheless, hover on the edges of the Political Science Department and its Institute of War and Peace Studies, the hotbed of IR at Columbia. From 1973 to 1981 I was a graduate student in IR at the Institute—and a direct observer of all of this. William T. R. Fox was my mentor, seconded by the nuclear weapons theorist Warner R. Schilling. Fox’s impact on the field has been dimmed by time but merits greater attention—it was he, after all, who coined the term “superpower,” and whose participation in a study group with...
Bernard Brodie led to the first systematic attempt to consider the international implications of the atomic bomb in *The Absolute Weapon*.1

In any case the Guilhot reconstruction, in my view, does not quite correspond to how I remember the evolution of the field. At the time it was a matter of absorbing a constant stream of articles, papers, and books of various stripes. The views of figures mentioned in this study—not only Morgenthau and Waltz, but Kaplan, Singer, Guetzkow, Snyder, and Verba—and others who go unmentioned, such as Graham Allison, Morton Halperin or Karl Deutsch, were actively taught at Columbia, and those and other figures passed through the doors of War and Peace on their pilgrimages to advocate their particular approaches. *We played* Guetzkow’s Inter-Nation Simulation at Columbia (incidentally, its lesson was that investment in economic development was far more productive than even greater amounts devoted to defense procurement), and the “rationality” of decision-making was a central focus of inquiry. This at a supposed center of “realist” thinking.

The reviewer articulates a certain vision Nicolas Guilhot attributes to the school he calls “behavioralist”—that it was not a matter of “empirical, inductive methods of discovering international realities” but “deductive approaches that abstracted away from these realities.” I agree that an approach like that evolved in the 1980s—but in the 60s and 70s, the timeframe considered here, I believe the IR enterprise was much more one of applying a variety of tools to the analysis of issues. There was no bitter debate among contending factions that I recall. That came later. We believed that the decisions and actions of governments could be illuminated by basic national goals, institutional influences identified by understanding bureaucracies, political interests woven into the mix, with additional factors applying to policy outcomes including technological developments, historical biases, and serendipitous factors.

I also remember the propagation of certain methodological techniques differently than presented here. As a designer of conflict simulations myself I was a close observer of both systems theory and game theoretical techniques. Systems theory was readily accepted because it provided a tool to break down the monolith of government into segments more amenable to analysis. Game theory entered the field from the nuclear warfare thinkers and—to a lesser degree—from peace research, which had adopted the technique themselves, not from a face-off between “realists” and “behaviorists.” These methods were being applied at Columbia throughout the time I was there, and their use was not controversial. Quantitative methods, I would argue, gained popularity from the late 70s, and arose not so much from a dispute between schools of thought in the field as from a sense among IR scholars that they were being surpassed in popularity by practitioners of International Political Economics as well as American Politics (studies of political participation), who could point to results based on quantitative methods. There was also pressure from such arcane fields as “deconstructionism” in literature studies, which transmogrified into content analysis in IR.

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Ido Oren is correct, in my view, to argue for closer attention to Kenneth Waltz. Both Waltz and Graham Allison were standard texts at Columbia throughout the period. Robert Jervis’s psychological approach gained increasing traction from the mid-70s. To the extent that Waltz represented a “behaviorist” point of view (and something similar can be said of Jervis), that makes my point that the “controversy” just was not there. Others who went through IR during the 1960s and 70s may have different recollections but this is the way I remember it.

Bill Fox lamented to me on more than one occasion that the university system, whatever its other faults, was deficient—at least in IR—because the training of Ph.D.s took too long. At Yale and Columbia both, Fox pressed for programs to produce doctorates more smoothly. In the 50s I believe that motivated him at least as much as constructing any “realist” theory of IR—which was Morgenthau’s thing anyway. Fox never described himself to me as a “realist”—or indeed with any label. I know he had important differences with Morgenthau, but they did not add up to a “battle.” In any case, by the 1970s, the universities were already past the threshold of the explosion in tuition costs, which occurred just as I sat down to write my dissertation, and that has continued into this day. This mushrooming has had the perverse effect of actually lengthening the time required to obtain a degree, since students were obliged to study on the side while holding down jobs to pay for their academic programs. Fox wanted universities to graduate IR students in as little as three years. By the 70s the average at Columbia was already seven.

In the 1980s and after there is a better case for a battle between schools of thought, as practitioners of quantitative IR struggled against more traditionally-oriented theorists for faculty slots and grant dollars. In many ways that conflict is similar to the one between Cultural History and Diplomatic and other Histories. What is most regrettable—to adopt the idiom of game theory—is that the emergence of new frames of reference should be perceived in zero-sum terms as a Hobbesian conflict rather than the development of new techniques for the tool box.

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