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Nicolas Guilhot. "Cyborg Pantocrator: International Relations Theory From Decisionism to Rational Choice." *Journal of the History of the Behavioral Sciences* 47:3 (Summer 2011): 279–301. DOI: 10.1002/jhbs.20511.

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Nicolas Guilhot has established himself as arguably the leading disciplinary historian of post-World War II international relations (IR). In an earlier, “must-read” essay Guilhot painstakingly documented how, in the 1950s, the Rockefeller Foundation bankrolled a network of realist scholars and practitioners who set out to build a theory of IR.¹ Although they could not agree on the meaning of “theory,” members of the group, including Hans Morgenthau of the University of Chicago and

¹ Nicolas Guilhot, “The Realist Gambit: Postwar American Political Science and the Birth of IR Theory.” *International Political Sociology* 2:4 (2008): 281–304. See also Nicolas Guilhot (ed.), *The Invention of International Relations Theory: Realism, the Rockefeller Foundation, and the 1954 Conference on Theory* (New York: Columbia University Press, 2011). “Must-read” is from Robert Vitalis, in H-Diplo/ISSF Roundtable Review of Nicolas Guilhot (ed.), *The Invention of International Relations Theory*, <http://www.h-net.org/~diplo/ISSF/PDF/ISSF-Roundtable-3-5.pdf>.

William Fox of Columbia University, were nearly all opposed to the creed that Morgenthau famously attributed to “scientific man”: the “conception of the social and physical world as being intelligible through the same rational processes” and the attendant view that “the social world is susceptible to rational control conceived after the model of the natural sciences.”² Guilhot cogently interpreted the group’s effort to “theorize” IR as a defensive reaction to the surge of the behavioral movement in the social sciences, a movement that appeared to embrace the creed of “scientific man” and that was generously funded by the Ford Foundation. For Morgenthau and his associates, creating “IR theory” was a way of delineating “an independent disciplinary territory” for the field, rendering it “immune to the cues of behavioralism.”³

In the current article, Guilhot broadens his lens to include the behavioral science that the realist theorists battled against. The focal point of his analysis is a heated controversy—known in the lore of the discipline as the “second debate”—which, in the 1960s, pitted largely-realist “traditionalists” against advocates of a behavioral “scientific” approach to IR. Guilhot ambitiously sets out to dispel two layers of “misunderstanding” (281) surrounding this debate: (1) a misunderstanding of the nature of the debate by current IR scholars, and (2) a misunderstanding intrinsic to the debate itself, that is, a misreading by the “traditionalists” of the agenda of their behavioralist critics. I argue that Guilhot successfully dispels the first misunderstanding, but that he may have overstated the degree to which the controversy itself was a product of a misunderstanding.

In contemporary IR, “science” is typically associated with quantitative data analysis. Practicing “scientific” IR is commonly understood to involve hypothesizing a law-like relationship between variables, operationalizing these variables, gathering relevant data, and analyzing the data statistically in order to test the hypothesis. Guilhot implies that contemporary IR scholars erroneously project this physics-like conception of science upon the second debate. He shows that although efforts to turn IR into a quantitative science were already underway at the time of the second debate, pioneers of quantitative IR such as J. David Singer of the University of Michigan were “still steeped in a tradition of peace research and interdisciplinarity that located them on the outer margins of IR.” (284). Thus, Singer and his fellow empiricists were not protagonists in the second debate.

Guilhot demonstrates effectively that the attack on “traditionalism” came not from the then-marginal peace research community so much as from scholars who “had a legitimate realist pedigree or were [realist] fellow travelers” (288). For example, Morton Kaplan, the most vocal proponent of the scientific approach to IR, studied with William Fox and participated in the inter-university seminar on IR theory coordinated by Fox at Columbia University in 1956–1957 (288). According to Guilhot, Kaplan and fellow exponents of “science” such as Harold Guetzkow and Richard Snyder continued to find

² Hans J. Morgenthau, *Scientific Man vs. Power Politics* (Chicago: University of Chicago Press, 1946), 2–3.

³ Guilhot, “The Realist Gambit,” 284.

significant value in the substantive insights of realism but they had little patience with classical realism's German-inflected metaphysical foundations: Nietzsche's "will to power" and Carl Schmitt's "decisionism" (281-282). For alternative foundations they turned not to physics but to the theory of cybernetics and to game theory. Guilhot makes an important contribution to postwar social science history by highlighting the significance of the Center for Advanced Study in the Behavioral Sciences at Palo Alto, lavishly funded by the Ford Foundation—it was there that Kaplan, Guetzkow, and like-minded scholars "bathed in the intellectual placenta of cybernetics" and game theory (286). From these intellectual approaches they borrowed a new vocabulary of "systems," "rational choice," and "decision-making," a vocabulary that allowed them (to their own satisfaction, at least) to ground power politics in a rational "decision science," dispensing with the non-rational, Schmittian "decisionism" in which classical realism was anchored.

In sum, Guilhot makes a compelling case that, contrary to the current (mis)understanding of the second debate, the behavioralist side in the debate advocated not empirical, inductive methods of discovering international realities so much as deductive approaches that abstracted away from, or simulated, these realities. The behavioral project did not aim to kill realism but rather to rearticulate its principles in a language borrowed from cybernetics and game theory.

If the proponents of behavioral science eschewed "social physics" (284), and if their intellectual project essentially amounted to a restatement of realist principles, what then was all the fuss about? If "scientific" IR was basically, as Guilhot puts it, a "blessing in disguise for realism" (289), why did the classical realists not welcome it? Guilhot's answer is that the second debate "was in fact a huge misunderstanding" partly fueled by generational differences (289). The realist old-guard badly misread the agenda of the younger advocates of a "science" of IR.

I wonder, however, whether Guilhot may have overstated the extent to which the realists misunderstood their critics. The behavioralists indeed distanced themselves from "social physics," but was that a sufficient reason for the old guard to cease viewing the behavioralist agenda as an embodiment of "scientific man"? Morgenthau's "scientific man," as noted above, believed that "the social and physical world" were "intelligible through the same rational processes." Unless we interpret this belief unduly narrowly as referring to the idea of patterning social science strictly after physics, it is not clear why it would have been a "huge misunderstanding" for the old guard to associate cybernetics with the creed of "scientific man." Cybernetics may have been rooted in biology and engineering rather than in physics, yet to say that cybernetic science applied "the same rational process" to the analysis of social and physical systems was hardly a misreading of the cybernetic agenda.

Similarly, I wonder if it was a misunderstanding on the part of the old guard to associate the behavioralists with the view, held by "scientific man," that "the social world is

susceptible to rational control conceived after the model of the natural sciences.”⁴ As Guilhot’s article makes clear, the attraction of the new behavioral IR was precisely its purported ability to contribute to “rational control” of U.S. foreign policy—the behavioralists promised, in the words of behavioralist Sidney Verba, to “introduce a higher level of means-ends rationality into foreign policy” (295, quoted by Guilhot). This promise was the reason why behavioral projects such as Guetzkow’s Inter-Nation Simulation won generous funding from the national security agencies of the U.S. government (286). Given their deep-seated skepticism of the ability of social science to offer rational solutions to social problems, why should the old guard have shown more “understanding” toward a social scientific project whose aim was precisely to devise rational solutions to the problems of U.S. foreign policy?

A propos U.S. foreign policy, I want to note that Guilhot did not consider the possibility that the second debate, though it was ostensibly intellectual in nature, in part reflected deep *political* differences between the opposing camps. Could it be that some of the heat surrounding the debate was actually generated by the era’s burning foreign policy controversy—Vietnam? Whereas key figures of the realist old guard, most notably Hans Morgenthau, were among the most outspoken critics of the Vietnam War (as Guilhot acknowledges in his 2008 essay), I believe that the younger behavioralists tended to take more hawkish, or at least less critical, positions toward the war. And even if some of the behavioralists were personally critical of the war, they were implicated in it inasmuch as they basically functioned as defense intellectuals and inasmuch as their major research projects depended on government funding. That Guilhot did not explore the political dimension of the debate is perfectly understandable given the space constraints of a journal article. I hope that he will address this issue in the book that, according to the biographical note, he is currently working on.

Another omission that Guilhot may want to redress in his book is the place of Kenneth Waltz, who Guilhot only mentions in passing, relative to the scholars and the intellectual developments discussed in the article. Waltz, after all, remains a towering figure in the discipline whereas the reputation of scholars like Kaplan and Guetzkow has faded over time. Sociologically and intellectually, Waltz had much in common with the proponents of behavioral IR. Like Kaplan, Waltz studied with William Fox and participated in the inter-university seminar on IR theory at Columbia University. Moreover, Waltz clearly shared the behavioralists’ impatience with the metaphysical aspects of realist theory—in his classic *Man, the State, and War* Waltz blasted Morgenthau and Niebuhr for portraying human nature as the root cause of international conflict.⁵ Yet, in his *Theory of International Politics*, even as he described key works on cybernetics as “useful,” Waltz harshly criticized Kaplan’s systems theory and he characterized the application of the

⁴ Morgenthau, *Scientific Man*, 2-3.

⁵ Kenneth N. Waltz, *Man, the State, and War: A Theoretical Analysis* (New York: Columbia University Press, 1959).

“general systems model” to IR as a “misfortune.”⁶ I would be very interested in Guilhot’s interpretation of Waltz’s position relative to the second debate: how and to what degree did Waltz’s theoretical agenda converge with, or diverge from, the behavioralist agenda? And, to the extent that Waltz did not quite warm up to cybernetics and/or game theory, where did his own conception of “science” come from?

Regardless of whether he will choose to address my questions, I look forward to reading Nicolas Guilhot’s next book. If the present article is any indication, the book promises to be well-researched, well-argued, and thought-provoking.

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⁶ Kenneth N. Waltz, *Theory of International Politics* (New York: Random House, 1979), 40, 59.