

The Progressive Power of Realism

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John Vasquez's assessment of realism suffers from three serious flaws. First, his reliance on Imre Lakatos's (1970) model of scientific progress is problematic, because the Lakatosian model has been largely rejected by contemporary historians and philosophers of science. Second, Vasquez understates the range and diversity of the realist research program and mistakenly sees disagreements among realists as evidence of theoretical degeneration. Finally, he overlooks the progressive character of contemporary realist theory, largely because he does not consider all the relevant literature. Disagreements within and across competing research programs are essential to progress and should be welcomed, but Vasquez's effort suggests that criticism will be most helpful when it seeks to do more than merely delegitimize a particular research tradition.

John Vasquez's evaluation of the realist research program is a misstep on the road to better international relations theory. By portraying realism as "degenerating," Vasquez hopes to influence both "individual decisions about where scholars are willing to place their research bets, as well as collective decisions as to which research programs deserve continued funding, publication, and so forth" (p. 900, emphasis added). In other words, his main goal is to discredit realism as a legitimate approach to the study of world politics, discourage scholars from pursuing a realist research agenda, and make it less likely that scholars working in the realist tradition will receive research funds or access to prominent journals.

What evidence does Vasquez present to justify this extreme position? Vasquez's (1997) criticism of realism is based on one philosopher of science (Imre Lakatos) and a handful of contemporary realist writings. Specifically, to support his charge of "degeneration," he has selected two "realist" books (Waltz 1979, Walt 1987), two articles (Schweller 1994, Christensen and Snyder 1990), and an eleven-page letter to the editor (Elman and Elman 1995). Does such a cursory survey justify discarding a respected intellectual tradition? Would we consider abandoning rational choice theory, the cognitive approach to decision making, organization theory, liberal theory, or the quantitative analysis of voting behavior on the basis of a similar sample? Even if every one of Vasquez's criticisms were valid (which is not the case), his evaluation would say little about the value of the realist perspective.¹

The foundation of Vasquez's critique is Lakatos's (1970) well-known essay on scientific research programs. Lakatos argued that a research program consists of (1) a "hard core" of basic propositions accepted by all members of the research community, (2) a "negative heuristic" that deflects criticism away from

this hard core, and (3) a "positive heuristic" that identifies legitimate puzzles and sets the research agenda. According to Lakatos, a research program is "progressive" if new theoretical refinements lead to "excess empirical content" (i.e., to newly confirmed predictions) when compared with the earlier theory. By contrast, a research program is said to be "degenerating" if each new theory is merely an ad hoc or semantic adjustment that explains an anomaly but does not anticipate some "novel fact."

Influenced by the Lakatosian model, Vasquez portrays realism as a narrow, tightly unified research program that is exhibiting clear signs of degeneration. After describing what he takes to be realism's hard core, he argues that recent theoretical refinements are merely ad hoc adjustments designed to rescue the entire paradigm from its purported empirical failings. He does this by showing that a handful of realists have advanced different theories about alliance formation. Vasquez sees these disagreements as a symptom of degeneration, because the presence of several competing realist theories increases "the probability that the realist paradigm will pass some test" (p. 906).

There are three main problems with Vasquez's criticism. First, his reliance on Lakatos is problematic, both because that model of scientific progress is flawed and because Vasquez's interpretation of it would justify abandoning most (if not all) of social science theory. Second, Vasquez's characterization of the realist tradition is misleading and understates its range and diversity. Third, Vasquez overlooks the progressive character of contemporary realist theorizing, in large part because he did not consider all the relevant literature. In particular, his treatment of my own work is both inconsistent and demonstrably inaccurate. Taken together, these errors explain why his article leaves realism largely unscathed and sheds little light on how it might be improved.

HOW NOT TO JUDGE SOCIAL SCIENCE THEORIES

Vasquez relies on Lakatos's model of scientific progress because he believes that it "has attracted the most consensus" as a source of criteria for judging theoretical merit, at least among mainstream scholars

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¹ As discussed below, Vasquez's sample is not even a comprehensive survey of the relevant works on the (relatively) narrow subject of balancing behavior.

of international relations (pp. 899–900). Even if this assertion were correct, however, it would not be a persuasive justification. Lakatos's now-dated analysis has been largely rejected by contemporary historians and philosophers of science (Diesing 1991, Laudan 1977, Suppe 1977, Toulmin 1972). Why should social scientists embrace a model of scientific progress that has been widely discredited by experts in that field?

Lakatos's analysis has been discarded mainly because it does not square with what we now know about scientific discovery. For example, Lakatos argues that scientists in a particular research program share a "hard core" of common assumptions, but the historical record shows that scientists working in such a program often disagree about its central elements.² Similarly, Lakatos's model implies that progress takes place only at the research frontier, while the hard core of a research program remains unchallenged. In fact, the hard core is often the object of debate, and it evolves in response to new empirical discoveries and conceptual innovations. Thus, Lakatos's model of scientific progress is doubtful on purely historical grounds.

Second, although Lakatos emphasizes that the key criterion in choosing between rival theories is "excess empirical content," he never explains how to perform this sort of comparison. For Lakatos, theory T_2 is progressive if it explains everything that the old theory T_1 did, while simultaneously accounting for some unanticipated "new fact." Yet, comparing the empirical content of rival theories turns out to be especially difficult in practice, which helps explain why "neither Lakatos nor his followers have been able to identify *any* historical case to which the Lakatosian definition of progress can be shown strictly to apply" (Laudan 1977, 77; see also Grunbaum 1976a; McCloskey 1994, chapter 7). A measure of progress that is difficult to operationalize is not a useful guide.

Third, Lakatos's rejection of ad hoc adjustments is inconsistent with actual scientific practice. An ad hoc adjustment that resolves an existing anomaly but does not lead to any other new facts is still an advance in our understanding; after all, it does answer a puzzle. Such an adjustment is problematic only if it simultaneously creates additional conceptual or empirical difficulties. Similarly, a refinement that limits the domain of a theory (i.e., by showing that it only operates under circumscribed conditions) is still an improvement over the prior but incorrect claim that the theory possessed a broader explanatory range. Thus, working scientists routinely embrace ad hoc adjustments and correctly regard them as part of normal scientific procedure.³

These points suggest that Lakatos's model is not a

sound basis for judging realism or any other research program. Indeed, taken to its logical conclusion, Vasquez's application of Lakatos would justify abandoning virtually all social science theory. Vasquez sees a research program as degenerating if (1) new theories offer no new empirical content and (2) if the program "goes through a number of theoryshifts . . . the end result [of which] is that collectively the family of theories fields a set of contradictory hypotheses which greatly increase the probability of at least one passing an empirical test" (p. 901). As we shall see, his claim that certain realist theories have not produced "new facts" is simply wrong. Moreover, his claim that the emergence of contradictory hypotheses is a sign of degeneration in effect means that when scholars in a particular research program advance different theories or reach different conclusions, then that research program is ipso facto beginning to degenerate. This conclusion ignores the possibility that one scholar is correct and another is incorrect, as well as the possibility that a particular disagreement can be reconciled by empirical testing or by a more precise specification of intervening variables or boundary conditions (Schweller 1997). Adopting this standard would force us to reject virtually every research tradition in the social sciences.

WHAT IS REALISM?

Vasquez appears to regard realism as a single, tightly unified research program, centered around the ideas of Kenneth Waltz. This view leads him to see any major disagreement among realists—and especially any departure from Waltz—as a sign of degeneration, which in turn leads him to portray other realists as trying to salvage the larger paradigm through a series of ad hoc amendments. This perspective also allows him to count the discrediting of any particular realist theory as a blow against the entire paradigm.

Vasquez's view rests on an inaccurate picture of contemporary realist thought. In fact, realism is a broad research program that contains a host of competing theories. Realists begin with some general assumptions (such as states are the key actors, the international system is anarchic, power is central to political life).⁴ As with all successful research programs, however, realists also disagree about a host of fundamental ideas. For example, Hans Morgenthau assumes that competition between states arises from the human lust for power (which he termed the *animus dominandi*), while Kenneth Waltz ignores human nature and assumes that states merely aim to survive (Morgenthau 1946, Waltz 1979). "Offensive" realists, such as Mearsheimer (1994–95), argue that great powers seek to maximize security by maximizing their relative power, while "defensive" realists, such as Jack Snyder (1991) or Charles Glaser (1994–95), argue that

² To note one example, both Charles Darwin and Alfred Wallace worked within the Darwinian "research program," insofar as each believed in the core principle of natural selection. Yet, they disagreed on several basic issues, such as the inheritability of acquired characteristics (Richards 1987, chapters 2 and 4).

³ As Laudan (1977, 115) puts it: "If some theory T_2 has solved more empirical problems than its predecessor—even just one more—then T_2 is clearly preferable to T_1 and, *ceteris paribus*, represents cognitive progress with respect to T_1 Ad hoc modifications, by their very definition, are empirically progressive." See also Grunbaum 1976b.

⁴ Some realists might add the assumption that states are more or less rational actors, although several prominent realists (including the present author) also examine how domestic politics can affect the "rational" assessment of strategic interests. For a sample of recent attempts to identify the core features of the realist paradigm, see Mearsheimer 1994–95; Van Evera, n.d., chapter 1; Walt 1992b, 473.

great powers are generally more secure when they refrain from power maximization and seek to defend the status quo. Realists also disagree about the relative importance of domestic versus systems-level causes, the relative stability of bipolar versus multipolar worlds, and the importance of intentions in shaping the calculations of national leaders (to name but a few possibilities). Thus, far from being a narrow intellectual monolith, realism is a large and diverse body of thought whose proponents share a few important ideas but disagree about many others.⁵

Two implications follow. First, it is hardly evidence of degeneration when realists advance contradictory arguments or reach different conclusions, just as it is not a major issue whenever neo-Keynesian economists, Skinnerian psychologists, Darwinian sociobiologists, or quantum physicists are at loggerheads. There are a host of competing theories within the realist paradigm, and not all of them are going to be equally valid or useful. Second, the failure of a particular realist theory does not discredit the entire paradigm, especially since realism deals with a very wide variety of international phenomena. Vasquez focuses on a handful of authors in his attempt to discredit the entire approach, but this step mischaracterizes the broader research tradition and the many different theories it contains.⁶

The real question to ask is whether realism—with all its limitations—has advanced or impeded our understanding of international relations. On this issue, even well-known critics of realism concede that it has been an influential tradition precisely because it sheds considerable, if only partial, light on a number of important international phenomena (Keohane 1984, 1986; Ruggie, 1983; Wendt n.d.).⁷

POWER, THREAT, AND EMPIRICAL CONTENT

The problems with Vasquez's analysis are evident in his discussion of my own work. To begin with, he cannot make up his mind about the theoretical status of balance-of-threat theory. He begins by portraying my

⁵ Recent examples and discussions of the broad body of realist thought include Brooks 1997; Brown, Lynn-Jones, and Miller 1995; Desch 1996; Deudney 1993; Elman 1996; Frankel 1996a, 1996b; Gilpin 1986; Grieco 1990; Van Evera n.d., chapter 1; and Zakaria 1992.

⁶ The most egregious example is Vasquez's claim that Colin and Miriam Elman's 1995 letter to the editor of *International Security* illustrates the response to Schroeder of scholars "sympathetic to realism" (p. 908). Such an assertion would be valid only if realism were in fact a single theory and if all so-called realists agreed with the Elmans' position. Vasquez offers no evidence that this is the case, which it is surely not.

⁷ For example, realism provides cogent explanations for (1) the failure of all modern efforts to gain hegemony over the state system; (2) the nearly universal tendency for great powers to be extremely sensitive to shifts in the balance of power; (3) the constancy of security competition among great powers; (4) the difficulty of sustaining effective international cooperation; (5) the tendency for great powers to acquire either formal empires or informal spheres of influence; and (6) the tendency for great powers to imitate one another over time. Realism does not provide the *only* explanation for these (and other) phenomena, but it contains a set of explanations that one would not want to dismiss out of hand.

theory as a direct refutation of Waltz's neorealist balance-of-power theory, based on my assertion that states tend to balance against *threats* rather than against power alone. In his view, this challenge to Waltz has "devastating" consequences for the realist paradigm. As Vasquez puts it, "if . . . power and threat are independent, as they are perceived to be by the states in Walt's sample, then something may be awry in the realist world" (p. 904).

Yet, it is hardly clear why refuting Waltz would lead us to abandon the realist paradigm in toto. Vasquez clearly regards my work as part of the realist paradigm, so if I have correctly refuted Waltz, then realism is a progressive program after all. To avoid this obvious challenge to his argument, Vasquez reverses course and argues that balance of threat theory is merely a "felicitous phrase" that "makes states' behavior appear much more consistent with the larger paradigm than it actually is." In particular, he claims my theory "does not point to any novel facts other than the discrepant evidence [and] . . . does not have any excess empirical content compared to the original theory, except that it now takes the discrepant evidence and says it supports a new variant of realism" (pp. 904–5). In short, Vasquez begins by calling balance-of-threat theory a "devastating" challenge to Waltz, based on the claim that power and threat are wholly independent concepts. But he quickly backtracks to argue that balance-of-threat theory is merely a semantic repackaging of Waltz's theory that does not point to any novel facts. He cannot have it both ways.

As it turns out, both assertions are incorrect. With respect to the first, I do not see power and threat as independent. Balance-of-threat theory openly incorporates power, subsuming it (along with geography, offensive capabilities, and intentions) within the more general concept of *threat*.⁸ Balance-of-power theory predicts that states will ally against the *strongest* state in the system, but balance-of-threat theory predicts they will tend to ally against the most *threatening*. Thus, the latter can explain not only why a state may align against the strongest power (if its power makes it the most dangerous) but also why one state may balance against another state which is not necessarily the strongest but which is seen as more threatening on account of its proximity, aggressive intentions, or acquisition of especially potent means of conquest. The two theories are not the same, although they share certain elements.

With respect to the second assertion that balance-of-threat theory is merely a semantic repackaging, I point to several of my works that offer novel facts. Balance-of-threat theory was originally laid out in my 1987 book, which examined alliance behavior in the Middle East. The final chapter showed, however, that it can also explain the anomalous distribution of power between the Soviet and American alliance systems

⁸ As I wrote in *The Origins of Alliances*: "Balancing and bandwagoning are usually framed solely in terms of capabilities. . . . This conception should be revised, however, to account for the other factors that statesmen consider when deciding with whom to ally. *Although power is an important part of the equation, it is not the only one*" (Walt 1987, 21, emphasis added; see also 263–4).

during the Cold War. In a subsequent article (not cited by Vasquez), the theory was used to explain the alliance behavior of four different states in Southwest Asia (Walt 1988). Another article (also not mentioned by Vasquez) shows how balance-of-threat theory explains alliance dynamics in Europe during the 1930s, a period that poses an especially demanding test for the theory. In particular, the theory explains why (1) the East European states failed to balance effectively against both Nazi Germany and the Soviet Union, (2) the United States was the last great power to mobilize for World War II, and (3) Great Britain and France balanced more slowly than hindsight might dictate. Parenthetically, this article also shows that Britain and France did not fail to balance the rising threat from Nazi Germany, as Vasquez, Schroeder, and others imply (Walt 1992b). Other scholars have successfully used balance-of-threat theory to explain the formation of the Gulf War coalition in 1990–91 and to analyze the grand strategy of the United States in the post-Cold War period (Garnham 1991, Mastanduno 1997). Finally, Vasquez does not refer to my recent efforts to apply the theory to a new realm—the international consequences of domestic revolutions (Walt 1992a, 1996). Thus, Vasquez's central claim—that balance-of-threat theory does not have “excess empirical content”—is false. And with this error exposed, his argument collapses.

CONCLUSION

Viewed as a whole, Vasquez's essay is a classic illustration of the hazards of small sample size. First, he relies on one modern work on the history and philosophy of science. Second, he relies on five contemporary realist works. Finally, he cursorily surveys the writings he examines, thereby missing the novel facts they uncover. This combination of problems is fatal to his argument, and prevents him from offering a useful criticism of realism in general or the specific body of literature under examination.

This failure is unfortunate, because realism is not without flaws and certainly should be exposed to criticism. The realist perspective offers a simple and powerful way to understand relations among political groups (including states) and offers compelling (albeit imperfect) accounts of a diverse array of international phenomena. But it is hardly the only way to study international relations. In the future, as in the past, scholars will continue to revise and extend the diverse body of realist thought. In doing so, they will inevitably disagree in various ways. At the same time, other scholars will pursue a variety of nonrealist research programs, and the resulting competition among different approaches will help us refine our understanding of international politics. The clash of theories both within and across competing research programs is essential to progress in the social sciences and should be welcomed. Progress will be swifter, however, if criticism seeks to do more than merely delegitimize realism, or any other approach a critic happens to dislike.

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