

From Paradigms to Research Programs: Toward a Post-Kuhnian Political Science



Terence Ball

American Journal of Political Science, Volume 20, Issue 1 (Feb., 1976), 151-177.

Stable URL:

<http://links.jstor.org/sici?sici=0092-5853%28197602%2920%3A1%3C151%3AFPTRPT%3E2.0.CO%3B2-M>

Your use of the JSTOR archive indicates your acceptance of JSTOR's Terms and Conditions of Use, available at <http://www.jstor.org/about/terms.html>. JSTOR's Terms and Conditions of Use provides, in part, that unless you have obtained prior permission, you may not download an entire issue of a journal or multiple copies of articles, and you may use content in the JSTOR archive only for your personal, non-commercial use.

Each copy of any part of a JSTOR transmission must contain the same copyright notice that appears on the screen or printed page of such transmission.

American Journal of Political Science is published by University of Wisconsin Press. Please contact the publisher for further permissions regarding the use of this work. Publisher contact information may be obtained at <http://www.jstor.org/journals/uwisc.html>.

American Journal of Political Science
©1976 University of Wisconsin Press

JSTOR and the JSTOR logo are trademarks of JSTOR, and are Registered in the U.S. Patent and Trademark Office. For more information on JSTOR contact jstor-info@umich.edu.

©2002 JSTOR

<http://www.jstor.org/>
Sat Aug 3 17:53:39 2002

The Workshop

TERENCE BALL
University of Minnesota

*From Paradigms to Research Programs: Toward a Post-Kuhnian Political Science**

This paper traces the reception of T.S. Kuhn's *Structure of Scientific Revolutions* by political scientists through three stages—the first two of uncritical acceptance, the third of rejection. I argue that a critical reading of Kuhn supports none of these positions in toto. I then go on to catalogue the main strengths and weaknesses of Kuhn's theory of scientific choice and change. This examination leads to a consideration of Imre Lakatos' more satisfactory theory of scientific progress. I then go on to suggest what bearing Lakatos' theory has upon the actual practice of political science.

The clash between . . . Kuhn [and his critics] is not about a mere technical point in epistemology. It concerns our central intellectual values, and has implications not only for theoretical physics but also for the underdeveloped social sciences and even for moral and political philosophy.

Imre Lakatos (1970, p. 93)

A decade has passed since political scientists first “discovered” Thomas Kuhn's *Structure of Scientific Revolutions*. The influence of that book upon the thinking of political scientists has been both profound and curious: profound, because political scientists have not only read Kuhn's book but have, as they believe, taken its message to heart; and curious, because few can say with certainty what that message is and what its implications might be for political-scientific inquiry. My aim here is not to provide yet another paean to Kuhn's “relevance” for political science. I want instead to trace the reception of his work by political scientists through three stages—the first two of uncritical acceptance, the third of outright rejection; to suggest that a critical

*Many of the themes and arguments in this essay grew out of conversations with Donald Moon, Stephen Toulmin, and the late Imre Lakatos. It is a pleasure to record my debt to them. I should also like to thank Moon and an anonymous referee for the *American Journal of Political Science* for criticizing an earlier version of this paper.

reading of Kuhn supports none of these positions; to note several respects in which Kuhn's account of scientific change is unsatisfactory; to explicate the more satisfactory alternative to be found in Lakatos' notion of "research programs"; and finally to suggest what bearing Lakatos' perspective has upon the way we practice political science. By these means I hope to outline for political scientists a new methodology (broadly understood) which has lately gained widespread currency among philosophers of science. This methodology purports to meet two criteria. First of all, it supplies a *rational* standard or "demarcation criterion" for distinguishing science from non-science; and in so doing it denies the Kuhnian contention that our demarcation criteria are non-rational and historically mutable. And secondly, this methodology purports to accord with the history of science—and in particular with the way in which scientific theories have been criticized, amended, and even, eventually, rejected. In being historical (*not* historicist), this new methodology has much more commonsense appeal than does the formalistic and ahistorical methodology associated with traditional "positivist" philosophy of science. It thus enables us to steer a middle course between the Scylla of Kuhnian relativism and the Charybdis of positivist formalism.

A Plethora of Paradigms

Kuhn's reception by political scientists has in the past decade gone through three more or less distinct phases. At first Kuhn's work was hailed by proponents of "scientific" or "behavioral" political science as providing (among other things) an explanation for the persistent tendency of political scientists to talk past each other: they have been operating within different "paradigms" of political inquiry; and so long as no authoritative single paradigm emerges, they will continue to do so, and political science will remain in its present "backward" ("underdeveloped," "immature," or "revolutionary") condition. But then, as Kuhn has shown, the natural sciences themselves have at times been similarly unsettled. And this was taken by some political scientists as a sign of hope. Political science had, they believed, undergone its own "revolutionary" phase (lasting two decades, three centuries, or 2,500 years, depending upon how one measures it), but was now about to become a "normal" or "mature" science in Kuhn's sense. This, at least, was the hope expressed in two A.P.S.A. presidential perorations (Truman, 1965; Almond, 1966). One, indeed, went to far as to suggest that "a new more surely scientific [*sic*] paradigm" was emerging in political science (Almond, 1966, p. 869). Others expressed similar hopes. The only trouble was that no one agreed as to whether the emerging paradigm was to be

interest-group analysis, structural functionalism, mathematical game theory, or even "behavioralism" as a whole.¹ Kuhn's own warning to the contrary notwithstanding (1962, p. 15) political scientists—along with other social scientists²—joined in searching for "paradigms" in their respective fields.

If this was the season of hope, it proved to be remarkably short-lived. For the opponents of a "scientific" political science soon found in "their" Kuhn an able ally. And in a kind of pincer movement they pressed a renewed attack upon political "science." These critics focused first upon Kuhn's account of "normal" science and the sociology of settled scientific communities. Following Kuhn, they emphasized the narrowing of focus and the "dogmatism" characteristic of "normal science." If that is what a normal or mature science looks like, then political scientists should want no part of it. Paradigms do not (on their reading of Kuhn) merely dominate, they tyrannize; and so political scientists committed to free inquiry should resist all blandishments to make theirs a "normal" science (Euben, 1969; Wolin, 1968, 1969; Ryan, 1972; Beardsley, 1974). Political science should rather strive, as one commentator put it, to "attain a multiparadigmatic condition" (Beardsley, 1974, p. 60). Or, to put it in Kuhn's terms: political science should remain in its present "immature" state.

The second part of this pincer movement proceeded by drawing out the nonrational and relativistic implications of Kuhn's account of scientific change. In *The Structure of Scientific Revolutions* Kuhn averred that the decision to abandon one paradigm in favor of another is not a rational affair at all, but is rather more closely akin to a conversion experience (1962, pp. 150, 157). This claim, and its implications, did not go unheeded by opponents of "behavioral" political science. For Kuhn had (as they believed) shown the natural sciences to be quite as "subjective" and "normative" as the behavioralists' caricature of "traditional political theory" held that enterprise to be. And so, these new relativists maintained, the acceptance of one paradigm and the rejection of another is not a rational process but a matter of personal "values" and even "existential" choice (Gunnell, 1968, 1969; Miller, 1972). A further and rather more ominous implication (to which I will return in the next section) is that meaningful communication, and indeed truth itself, is necessarily *intraparadigmatic*, i.e. between adherents of different

¹ For a sophisticated analysis of would-be paradigms in political science, see Holt and Richardson (1970). On the role of paradigms in the history of political theory, see Wolin (1968).

² Here I include sociology (Friedrichs, 1971; Smolicz, 1970; Lebowitz, 1971), psychology (Palermo, 1971; Welmes, 1973; Boneau, 1974), and economics (Coats, 1969; Stanfield, 1974).

paradigms no rational communication would be possible; and so a permanent failure to communicate would be the inevitable accompaniment of a "multi-paradigmatic" political science.

The third—and present—phase of Kuhn's reception by political scientists can best be characterized as outright repudiation (Landau, 1972a, ch. 2, 1972b; Stephens, 1973). Frightened by the specter of epistemological relativism, political scientists are nowadays tempted to resort to pre-Kuhnian clichés about objectivity, testability and falsification, and the like. This temptation, although understandable, should be resisted, inasmuch as it represents a retreat to the comfortable—albeit questionable—pre-Kuhnian verities. Fortunately, we need not choose between retreat and surrender. We have another option. For we can—to extend the military metaphor—advance to new and more defensible ground. Lakatos has suggested one way of getting there. In a later section I will explicate his strategy. But first I should outline several persuasive reasons for attempting to move beyond Kuhn to a genuinely *post-Kuhnian* position.

Normal Science or Permanent Revolution?

Although Kuhn's views are too well known to warrant a detailed recounting here, several points should be noted. In *The Structure of Scientific Revolutions* Kuhn disputed the "cumulative" or "text book" conception of scientific change, viz.: that the growth of scientific knowledge is piecemeal, cumulative, and pacific; that such growth results from applying the neutral instrument of "scientific method" to independently existing reality; that this method requires, among other things, that theories be tested against reality and, should they fail, be discarded as "falsified"; and that scientific advance consists in the gradual accumulation of ever-truer hypotheses and theories. This vision of scientific development is plausible, Kuhn argues, only if one ignores the actual (as distinguished from the "textbook") history of science. Close and careful study of the history of science reveals significant conceptual and methodological *discontinuities*, sometimes of "revolutionary" proportions. Revolutions are, however, relatively infrequent. The greater part of the history of science is the story of "normal science," i.e., the enterprise of resolving the "puzzles" which arise, as a matter of course, in attempting to force recalcitrant nature into the conceptual pigeonholes of an exemplary theory or "paradigm." More often than not, natural phenomena can be conceptualized, interpreted—and, if need be, reinterpreted—so as to square with the expectations generated by the dominant paradigm. Sometimes, however, would-be puzzles turn into "anomalies"—that is, phenomena for

which the master theory affords no plausible explanation. The presence, and indeed the proliferation, of anomalies does not, of itself, suffice to falsify the theory. Instead, the members of the scientific community devote their energies and attention to solving the soluble. During such periods, scientific advance conforms to the textbook picture: it is steady, continuous, and cumulative. This idyll may nevertheless be interrupted by the appearance of a rival paradigm which, Athena-like, springs fully formed from the head of a master theoretician and purports to account for the anomalies of the old even as it indicates new directions for research. The appearance of a rival paradigm signals the onset of a "crisis" and even, quite possibly, a thoroughgoing revolution. A revolution, in science as in politics, succeeds when scientists' loyalties and allegiances are transferred from the old to the new paradigm. And this victory, in its turn, inaugurates a new era of normalcy.

My purpose in briefly recounting the original (1962) Kuhnian conception of scientific change is to suggest that—contrary to a widely held misconception—it is *not* the role assigned to "paradigms" that distinguishes Kuhn's account. Indeed, the concept of theoretical "paradigms" is, in the history of philosophy, rather old hat.³ It is, rather, Kuhn's distinction between "normal" and "revolutionary" science that distinguishes, and must carry the weight of, his account of scientific change. That being so, two questions ought now be asked. The first is formal: Is the distinction between normal and revolutionary science a coherent one? I maintain that when the distinction is drawn in one way, Kuhn's account of scientific change is incoherent, i.e., *internally inconsistent*; but, when drawn in another way, it is not. In *The Structure of Scientific Revolutions* Kuhn sometimes appears to draw the distinction in the first way; in subsequent "clarifications" of his position he has drawn it in the second way. Because political scientists are more likely to

³ Although Kuhn has popularized the term "paradigm," he did not coin it. Our word "paradigm" derives from the Greek *paradeigma*, meaning model, pattern, or exemplar. In the *Republic*, for example, Plato speaks of his ideal polity as a "paradigm (*παράδειγμα*) laid up in heaven" (592 b). The first to speak of paradigms in the natural sciences was the eighteenth-century philosopher Georg Christoph Lichtenberg, for whom a paradigm was an accepted standard model or pattern into which we attempt to fit unfamiliar phenomena; and when we have done so we say we have "explained" or "understood" them. It is in this sense also that Wittgenstein (1968) spoke of paradigms and Austin (1970, p. 202) of "thought-models." This earlier use of the term is rather more precise and restricted than Kuhn's original (1962) use of "paradigm" as a portmanteau concept enclosing exemplary scientific achievements, theories, successful experiments, *Gestalten*, and world-views, among other notions (Masterman, 1970). Wanting to control such terminological inflation, Kuhn now (1970a, 1974) prefers to speak instead of exemplary theories, or "exemplars" for short.

know Kuhn's book than his later work, I will direct most of my criticisms against the claims advanced there. Having done that, I will move on to a second, empirical question: Does the history of science support the distinction between normal and revolutionary science?

Whether Kuhn's theory of scientific change is internally consistent or inconsistent depends upon which of two theses he subscribes to. The first I shall call the *thesis of perfect (or strict) incommensurability*; it holds that the phenomena of scientific investigation count as (relevant) facts solely by virtue of their being statable in a theoretical language. All meaningful observation reports are, on this view, "theory-laden." And furthermore, there being no neutral observation language common to any two theories, the observation reports of the one are not translatable into (or recognizable as meaningful) observation reports in the other. The second thesis—the *thesis of imperfect (or partial) commensurability*—holds that scientific theories are, in a way roughly analogous to natural languages, to some degree mutually intertranslatable.⁴ That is, at least some phenomena-reporting sentences of a theory *T* will have roughly corresponding meaning-equivalents in another theory *T'*. Communication between theories in a given domain (e.g., microphysics), however partial and unsatisfactory, is at least possible.

In his book, especially, Kuhn appears to subscribe to the thesis of strict incommensurability (1962, pp. 4, 91, 102, 111, 147); in other essays he denies it explicitly (1970a, 1970b). In any event I shall argue that *if* in fact Kuhn *were* to subscribe to the strict thesis, his theory of scientific change would be internally inconsistent. For consider: According to Kuhn any master theory or "paradigm" permits one not only to explain certain phenomena but also to recognize other phenomena as (presently) inexplicable or anomalous. The solving of "puzzles" and the unearthing of "anomalies" are part of normal science. Now just as any number of compatible facts do not suffice to confirm a theory, neither can any number of recalcitrant phenomena—*anomalies*—serve to disconfirm it. Even so, as Kuhn acknowledges, recalcitrant facts do accumulate and may eventually undermine the scientists' confidence in the paradigm, thus paving the way for the appearance of a rival paradigm which, in a successful revolution, replaces the older anomaly-ridden master theory.

The question immediately arises, however, as to how—given the thesis of strict incommensurability—the second theory (*T'*) could possibly be adjudged *better* than the first (*T*). Surely it is not "better" in that it "fits the facts"

⁴The analogy between paradigms and natural languages is drawn by Popper (1970) and accepted, with amendments, by Kuhn (1970a, pp. 200–204; 1970b, pp. 267–271).

more closely, since (1) the facts that T' explains cannot be (the same as) those that T explains and, further, (2) the phenomena that T fails to explain cannot be the (same) ones that T' does explain (Shapere, 1964, 1966). In other words: if "facts" are recognized as such only in the light of a theory, and if theories are strictly incommensurable, then T and T' necessarily explain quite *different* phenomena. Therefore Kuhn cannot without contradiction subscribe to the strict incommensurability thesis *and* maintain that one theory T' explains phenomena that are anomalous with respect to another theory T . Wholly incommensurable theories cannot—*logically* cannot—even recognize, much less explain, the "same" phenomena. From this it follows that one theory or paradigm cannot, strictly speaking, "rival" another.

In recent "clarifications" of his position Kuhn denies that he ever subscribed to the thesis of strict incommensurability (1970a, 1970b, 1974). He now insists that he accepts the thesis of partial commensurability, and so his theory of scientific change is at least internally consistent.

Internal coherence, however, is not everything. We may still ask whether the history of science supports the distinction between normal and revolutionary science. Is the distinction an accurate and useful one? In his book Kuhn maintained that the history of science is, for the most part, the story of "normal" science, interrupted infrequently by scientific "revolutions." He characterized these revolutions as thoroughgoing "changes of world view" or "gestalt switches" (1962, ch. 10). But, as Kuhn's critics hastened to note, the history of science does not conform to this schema. For example, changes of theory—"paradigm shifts"—are not the sudden all-or-nothing changes that Kuhn pictures them to be, but are more likely to take many years. Scientific change is also rather more piecemeal and continuous than Kuhn suggests. A paradigm does not fall in a great cataclysmic crash; it is more likely to erode over time (Toulmin, 1970). For example, the Newtonian paradigm was not a monolith until Einstein demolished it utterly; it was, rather, a ravaged shell of a theory, cracked in many places, and no longer able to support the ever-increasing weight of the evidence against it. Kuhn's "big bang," or revolutionary, account of scientific change does not fit the facts, even here.

Kuhn now concedes as much. Small-scale minirevolutions, he now maintains (1970b, pp. 249–50), occur frequently in the history of science. The upshot of Kuhn's concession to his critics was, however, to undercut—and render incoherent—his original distinction between "normal" and "revolutionary" science, which after all had been the linchpin of his theory of scientific change. For now all "normal" science is (to some unspecified degree) "revolutionary." Kuhn's account of scientific change is thus trans-

formed into a theory of permanent revolution (as Trotsky might have said) or a "revolution in perpetuity" (as Toulmin [1972, p. 114] did in fact say). But then Kuhn, having rendered otiose the distinction between normal and revolutionary science, has emptied it of much of its supposed heuristic and explanatory value. Ironically, Kuhn's attempted "revolutionizing" of normal science lends support not to a revolutionary theory of scientific change but to an *evolutionary* one.⁵ The boldest and most original aspects of Kuhn's account of scientific change—the ideas of self-contained paradigms, normal and revolutionary science, and the rest—have now been either watered down considerably or abandoned entirely.

In the face of these, and other, apparently decisive objections to Kuhn's account, political scientists are inclined to wring their hands in despair, lamenting that "We are back where we started before the Kuhnian paradigm [*sic*] was adopted by political science" (Stephens, 1973, p. 488). I disagree. Far from going back to Square One, we have made some progress—progress too easily overlooked if we focus only on the errors and omissions of Kuhn's original account. To suggest that we are "back where we started" is to go much farther than Kuhn's critics would have us go.

Even Kuhn's most ardent critics readily acknowledge the value and importance of his contribution to the history and philosophy of science. We must count among Kuhn's achievements the undermining (if not outright destruction) of the "textbook" conception of scientific progress as a steady growth-by-accumulation of ever-truer hypotheses and theories. For all its rhetorical exaggeration, Kuhn's conception of scientific change-through-revolution has had the signal merit of reminding us that the scientific enterprise is above all a dynamic and not wholly "cumulative" one. No one, with the possible exception of Sir Karl Popper, has done more than Kuhn to call this feature to our attention. As a result no political scientist would nowadays maintain that "A science of politics which deserves its name must build from the bottom up An empirical discipline is built by the slow, modest, and piecemeal accumulation of relevant theories and data" (Eulau, 1964, p. 9). Whatever their view of scientific development, Kuhn and his critics are agreed in repudiating this vision of growth-through-accumulation; facts, hypotheses and theories are not building blocks which can be stacked one on top of the other. Disciplines do not grow and develop by simple accretion.

⁵ Kuhn now says that his "view of scientific development is fundamentally evolutionary" (1970b, p. 264); if so, his view has changed dramatically. For two rather different "evolutionary" theories of scientific change, see Popper (1972, ch. 7) and Toulmin (1967, 1972).

Another of Kuhn's important contributions is his recognition of the inseparability of "history" and "philosophy" of science: neither can proceed successfully without the other. Here, too, most of his critics agree with Kuhn. Where they disagree with Kuhn is over his further implication that an historical approach commits one to an *historicist* position in epistemology (Rudner, 1972, p. 827).

Another, even more important, contribution is Kuhn's undermining of naive falsificationism, i.e., the doctrine that *facts* can falsify or confute *theories*. Just as Popper showed that the "test of experience" cannot "prove" or verify the truth of a theory, so Kuhn has shown that the test of experience never suffices, of itself, to refute a theory. Theories are never falsified *simpliciter*, not even by rank anomalies. Therefore falsifiability cannot serve as the "demarcation criterion" for distinguishing science from nonscience (or pseudoscience). On this point Kuhn and his critics are agreed. They disagree, however, with Kuhn's contention that there can be no methodological demarcation criterion, but only, as it were, a *sociological* one; that is, that only the Powers That Be in a given scientific community can decide what is science and what is not, and that their standards of judgment are extrascientific and historically variable—a matter of taste and fashion only, and therefore beyond rational criticism (1962, ch's. 3, 10–12). Kuhn's contention has spurred others—Lakatos foremost among them—to formulate defensible demarcation criteria; that is, criteria more liberal—and therefore more acceptable, from an historical perspective—than the criterion of falsifiability, but which also make the demarcation between science and pseudoscience rationally defensible. With the aid of such a methodological demarcation criterion, we can view the choice between theories or paradigms as a *rational* one made for methodologically sound reasons, rather than—as with Kuhn—an arbitrary and rationally inexplicable "shift" of "allegiance" or "loyalties."

Lakatos' Alternative to Kuhn

Lakatos and Kuhn are agreed on this much: the history of science does not support the view that an elegant theory can be killed (or "falsified") by an ugly fact. Theories are made of sterner stuff, and facts are not so hard and unyielding as classical empiricists had supposed. But if science purports to say anything about the world, then scientists—and philosophers of science—must retain and take seriously the idea of an empirical basis. The mistake of the older empiricists was to assume that facts and theories are wholly separable in all cases, and that the truth content of the latter can be ascertained by

reference to the former. I want now to outline Lakatos' reasons for believing this view to be mistaken.

Lakatos begins by distinguishing three species of falsificationism, which he terms "dogmatic," "methodological," and "sophisticated methodological falsificationism," respectively. *Dogmatic falsificationism* holds that while "facts" never suffice to *prove* theories, they do suffice to *disprove* them. "Falsifiability" thus serves as the demarcation criterion between science and non-science. For in order to qualify as "scientific" a proposition or a theory must be potentially falsifiable: criteria of refutation must be specified and one or more "crucial experiments" conducted; if the theory or proposition fails in its direct encounters or "confrontations" with Nature, it must be given up (Lakatos, 1970, pp. 95-97).

Dogmatic falsificationism rests, however, upon two mistaken assumptions. The first is that there is some clear-cut dividing line between "observational" and "theoretical" propositions.⁶ The second assumption upon which dogmatic falsificationism rests is that if a proposition qualifies as a "basic" or "observational" one, then it is either incorrigibly true or incorrigibly false; that is, "one may say that it was *proved* [or *disproved*] from facts." Lakatos (1970, pp. 97-98) calls this "the doctrine of observational (or experimental) proof."

Both these assumptions are, however, mistaken—the first mistaken in fact, the second in logic. The first is mistaken inasmuch as there is in fact no clear-cut borderline between observational and theoretical propositions. "For there are and can be no sensations unimpregnated by expectations and therefore there is no natural (i.e., psychological) demarcation between observational and theoretical propositions" (Lakatos, 1970, p. 98). By way of example, Lakatos cites the case of Galileo's supposed "refutation" of the Aristotelian theory of flawless celestial spheres:

Galileo claimed that he could "observe" mountains on the moon and spots on the sun and that these "observations" refuted the time-honoured theory that celestial bodies are faultless crystal balls. But his "observations" were not "observational" in the sense of being observed by the—unaided—senses: their reliability depended on the reliability of his telescope—and of the optical theory of the telescope—which was violently questioned by his contemporaries. It was not Galileo's—pure, untheoretical—*observations* that con-

⁶ Popper terms this "the naturalistic doctrine of observation"; Nietzsche called it—rather more colorfully—"the dogma of immaculate perception."

⁷ On this much, at least, Kuhn and Lakatos are agreed. For extended discussions of the "theory-laden" character of observation, see Hanson (1958), Spector (1966), Feyerabend (1970), and Machamer (1971, 1973).

fronted Aristotelian *theory* but rather Galileo's "observations" in the light of his optical theory that confronted the Aristotelians' "observations" in the light of their theory of the heavens. This leaves us with two inconsistent theories, *prima facie* on a par.

Even our most "direct" observations are impregnated with expectations; thus there is no natural dividing line between "basic" or "observational" propositions and "theoretical" ones.⁷

The second assumption upon which dogmatic falsificationism rests—the doctrine of experimental (dis)proof—is also mistaken, but for a different reason. It is mistaken, Lakatos says, because of "a basic point in elementary logic," viz.: "Propositions can only be derived from [and be consistent or inconsistent with] other propositions." Therefore "no factual proposition can ever be proved [or disproved] from an experiment" (1970, p. 99). Dogmatic falsificationism mistakenly posits a world of hard facts that is both independent of and comparable with our systems of propositions, i.e., theories. But it is not independent, inasmuch as no natural line of demarcation separates observation from theory; and it is not comparable, inasmuch as propositions can be compared only with other propositions. Dogmatic falsificationism is therefore untenable.

But dogmatic falsificationism is not the only kind. There is a second and less objectionable kind, which Lakatos calls *methodological* falsificationism. As against dogmatic falsificationism, it holds that the dividing line between observational and theoretical propositions is not natural but conventional; i.e., *where* to draw it is a methodological decision. For example, the sorts of basic or "observational" propositions against which we test the predictions of a theory *T'* are not pure and unsullied "protocol sentences" but are, rather, propositions drawn from another, "touchstone" theory. To return to the Galileo example cited above: What Galileo did in "testing" the Aristotelian theory of celestial bodies was to admit as "basic" or "observational" propositions the *theoretical* propositions of another theory—in this case his (still unrefined) optical theory. For purposes of testing—and falsifying—one theory, we use another. "The methodological falsificationist," says Lakatos, "uses our most successful theories as extensions of our senses . . ." (1970, p. 107). In other words, a proposition which is a "theoretical" one in *T* may be accorded "observational" status in *T'*.⁸

⁸ Lest one think that such "observational" propositions are somehow suspect, if not indeed inferior to direct "eyeball observation," Lakatos reminds us that "calling the reports of our human eye 'observational' only indicates that we 'rely' on some vague physiological theory of human vision" (1970, p. 107).

Methodological falsificationism adopts a demarcation criterion which is, to say the least, more liberal than that proposed by dogmatic falsificationism: it admits as "scientific" any theory which has "potential falsifiers," even if they derive from some other, as yet unfalsified, theory. Consequently, many more theories may now be regarded as scientific ones. Both kinds of falsificationism hold, however, that a theory enjoys "scientific" status by continually "sticking its neck out." The scientist puts his theory's head on the block by "specifying, in advance, an experiment such that if the result contradicts the theory, the theory has to be given up" (Lakatos, 1970, pp. 96, 112). The difference between the dogmatic and methodological falsificationist is that the latter's conception of "observation" and "testing"—and hence "falsification"—is more liberal than the former's.

Even so, Lakatos argues that methodological falsificationism is not liberal enough. Its standards of falsification are too strict. It does not square with, or do justice to, the history of science. For if we apply its standards, many of the most respected theories of yesteryear would be accounted unscientific (or pseudoscientific). Theorists are almost never inclined to bet their theories on a crucial experiment (in which the theory is not "confirmed" once and for all, but merely not-falsified *this time*). Such an all-or-nothing go-for-broke attitude is, Lakatos says, "reckless" and "daredevil" (1970, p. 112). In fact, however, scientists tend to be bold in their conjectures but cautious in their refutations; good theories being hard to come by, they hold fast to what they have already.⁹ And yet: theories *do* get falsified; one theory is rejected in favor of another; the scope and explanatory power of successive theories increases. But how is this sort of progress possible? If we are to answer that question we must first ask: How—by what standards, on what grounds—is one theory to be rejected as inferior to another? Any satisfactory answer to that question, Lakatos avers, cannot be given in isolation from, or in ignorance of, the actual history of science. But then, he says (1970, p. 114), "If we look at the history of science, if we try to see how some of the most celebrated falsifications happened, we have to come to the conclusion that either some of them are plainly irrational, *or* that they rest on rationality principles different from those [espoused by dogmatic and/or methodological falsificationists]." Faced with this choice, Kuhn opts for the former: choosing between rival theories or paradigms is not a wholly rational matter. Lakatos, by contrast, opts for the latter: there *are* principles of scientific rationality, criticism, and falsifiability—only they are *not* the ones advanced

⁹ Up to this point Lakatos agrees with Kuhn. But Kuhn differs from Lakatos in viewing this as a social-psychological fact about the behavior of scientists, rather than as a methodological maxim or principle implicit in scientific practice.

by dogmatic and/or methodological falsificationists. To formulate and defend these principles is the task that Lakatos set himself.

Against Kuhn and earlier falsificationists alike, Lakatos advanced his own "methodology of scientific research programs." The linchpin of his theory of criticism and scientific change is to be found in his doctrine of "sophisticated methodological falsificationism" (or "sophisticated falsificationism" for short). On this account a theory is to be adjudged "scientific" by virtue of its being "falsifiable" in a new, and methodologically more sophisticated, sense:

The sophisticated falsificationist regards a scientific theory T as falsified if and only if another theory T' has been proposed with the following characteristics: (1) T' has excess empirical content over T : that is, it predicts *novel* facts, that is, facts improbable in the light of, or even forbidden by, T ; (2) T' explains the previous success of T , that is, all the unrefuted content of T is contained (within the limits of observational error) in the content of T' ; and (3) some of the excess content of T' is corroborated (Lakatos, 1970, p. 116).

Sophisticated falsificationism has two signal merits. First, it squares with the history of science;¹⁰ and secondly, it avoids the sort of irrationalism implied by Kuhn's earlier (1962) account of scientific choice and change.¹¹

For the sophisticated falsificationist the problem is no longer how to distinguish between science and pseudoscience *simpliciter*, but rather, "how to demarcate between scientific and pseudoscientific *adjustments*, between rational and irrational changes of theory" (Lakatos, 1970, p. 117). What Lakatos proposes, then, is to provide not merely a descriptive account of scientific change, but a *theory of scientific progress*—and progress, moreover, of an eminently critical and rational sort.

Scientific progress, according to Lakatos, can only be gauged by looking at the successes and failures not of single theories but of successive *series of theories*, each sharing common core assumptions. Such a series he calls a "research program." A research program consists of a "hard core" of not-directly-criticizable assumptions.¹² The hardness of this hard core is a

¹⁰Squares, that is, with a "rational reconstruction" of the history of science: see Lakatos (1971).

¹¹Lakatos does not supply us with a wholly rational method for *choosing* a research program, but rather with a means of "keeping score" in the contest between rival research programs (1971, p. 101).

¹²The idea that such assumptions subtend all scientific inquiry is not a new one. Burt (1954) speaks of "metaphysical foundations," Collingwood (1940) of "absolute presuppositions," Popper (1962, ch. 4) of "myths," and Toulmin (1961, ch's. 3–4) of "ideals of natural order," in much the same way that Lakatos talks about the "hard core" of a research program.

methodological hardness; it is assured by the program's *negative heuristic*, i.e., the methodological rule that criticism be directed away from the hard core of the program.¹³ The program's *positive heuristic*, by contrast, prescribes the construction of a "protective belt" of auxiliary assumptions, hypotheses, etc., which serves to protect the program's hard core. "It is this protective belt," Lakatos says, "which has to bear the brunt of tests and get adjusted and re-adjusted, or even completely replaced, to defend the [hard] core. A research program is successful if all this leads to a progressive problem-shift; unsuccessful if it leads to a degenerating problem-shift" (1970, p. 133).¹⁴ We gauge the progressiveness of a research program by looking at the character of the adjustments made in its protective belt:

Let us take a series of theories T_1, T_2, T_3, \dots where each subsequent theory results from adding auxiliary clauses to (or from semantical reinterpretations of) the previous theory in order to accommodate some anomaly, each theory having at least as much content as the unrefuted content of its predecessor. . . . [A] series of theories is *theoretically progressive* (or "constitutes a theoretically progressive problem-shift") if each new theory has some excess empirical content over its predecessor, that is, if it predicts some novel, hitherto unexpected fact. . . . [A] theoretically progressive series of theories is also *empirically progressive* (or "constitutes an empirically progressive problem-shift") if some of this excess empirical content is also corroborated, that is, if each new theory leads us to the actual discovery of some *new* fact. Finally, . . . a problem-shift [is] *progressive* if it is both theoretically and empirically progressive, and *degenerating* if it is not. We "accept" problem-shifts as "scientific" only if they are at least theoretically progressive; if they are not, we "reject" them as "pseudoscientific." Progress is measured by the degree to which a problem-shift is progressive, [i.e.] by the degree to which the series of theories leads us to the discovery of novel facts. We regard a theory in the series "falsified" when it is superseded by a theory with higher corroborated content (Lakatos, 1970, p. 118).

Theories are, then, never falsified absolutely but only relatively, i.e., they are superseded by better theories. As Lakatos puts it: "There is no falsification before the emergence of a better theory" (1970, p. 119).

¹³ Lakatos' point—despite the forbidding jargon in which it is expressed—is essentially a simple and commonsense one. It is that we can never get anywhere if we dwell always upon the "fundamental assumptions" of a theory (or series of theories), instead of its "payoff." The "hands-off" policy prescribed by the negative heuristic allows the scientist to get on with his work without having to constantly defend his core assumptions.

¹⁴ Although never explicitly defined, a problem-shift is a change in directions (or directives) for research. A change is fruitful, promising—"progressive"—if it permits the prediction of new facts even as it accounts for old anomalies, and "degenerating" if it resolves old difficulties by means of verbal and/or ad hoc stratagems which do not point out new directions for research.

By way of illustration Lakatos cites “the most successful research programme ever”—Newton’s gravitational theory. “In Newton’s programme the negative heuristic bids us to divert the *modus tollens* from Newton’s three laws of dynamics and his law of gravitation. This ‘core’ is ‘irrefutable’ by the methodological decision of its protagonists: anomalies must lead to changes only in the protective belt . . .” Newton and the Newtonians did just that. From the beginning Newton’s gravitational theory

was submerged in an ocean of “anomalies” (or, if you wish, “counterexamples”), and opposed by the observational theories supporting these anomalies. But Newtonians turned, with brilliant tenacity and ingenuity, one counterinstance after another into corroborating instances, primarily by overthrowing the original observational theories in the light of which this “contrary evidence” was established. In the process they themselves produced new counterexamples which they again resolved. They “turned [as Laplace said] each new difficulty into a new victory of their programme” (Lakatos, 1970, p. 133).

The lesson to be learned from this, and a myriad of other historical examples, is twofold. It is first of all a lesson in *tenacity*: the task of keeping a theory (or series of theories) afloat in the ocean of anomalies requires dogged persistence and ingenuity on the part of its defenders. The second lesson is one of *tolerance*: critics should be tolerant of attempts to “save” theories from “refutation.” Neither lesson is taught by dogmatic and/or methodological falsificationists; quite the contrary, both are alike in holding that tenacity in defending a theory is very nearly a crime against science. Against this view Lakatos argues that the history of science is the story of bold conjectures boldly—and tenaciously—defended against apparently “decisive” counterevidence.

This does not mean that *theories* cannot be criticized and even, eventually, falsified. It means, rather, that criticism must be directed against successive adjustments in the protective belt surrounding the hard core of that series of theories which constitutes a research program. The critic must ask: Are these adjustments “progressive” or “degenerating” ones *within the context of this particular research program*? That is, are these adjustments content-increasing or content-decreasing ones? Do these adjustments enable us to predict novel facts even as they explain old anomalies?

A good research program is a good swimmer—mainly because its “protective belt” serves as a life belt, keeping the hard core afloat on an ocean of anomalies. So long as this belt can be adjusted in “progressive” (i.e. content-increasing) ways, the research program is in no danger of sinking. But, by the same token, a research program begins to list and take on water when its

protective belt can no longer be adjusted in progressive ways—when, that is, adjustments amount to no more than content-decreasing semantical ones and/or when they fail to anticipate new facts. Only then is the research program itself—hard core and all—in any danger of sinking. But no matter how waterlogged it is, a research program will not sink and have to be abandoned until a better, more buoyant one comes along to replace it. In any case, the decision to abandon one research program in favor of another is not taken lightly (contra earlier falsificationists), nor is it nonrational (contra Kuhn); it is at every step a critical, considered decision in the light of available alternative theories against which the progressiveness (or degeneration) of successive problem-shifts is gauged. Science is, then, both rational and progressive.

But—to ask an old question in a new and different light—is political science such a science? Is it, or can it be, rational and progressive? Are there, or might there one day be, research programs in political science? On these questions Lakatos' methodology of scientific research programs sheds some interesting light.

Research Programs in Political Science

My discussion has so far focused upon a number of interrelated problems in the history and philosophy of science—in particular, problems of commensurability and criticism, falsification and scientific progress. Since the practicing political scientist is likely to wonder what all this has to do with his inquiries, let me enumerate some of the advantages to be gained by viewing political-scientific inquiry through Lakatosian lenses.

The first advantage—alluded to already—is that political science can now dispense with its pseudo-Popperian, or “dogmatic,” conception of falsifiability.¹⁵ That is, we can leave behind us the outmoded and untenable view that a theory can be tested directly against *facts* which are wholly independent of the theory under test, and which, if they do not *correspond*, require us to abandon the theory as *falsified*. We can, with Lakatos' aid, see immediately what is wrong with such claims as these: “Whether [an empirical] proposition is true or false depends on the degree to which the proposition and the real world correspond” (Dahl, 1963, p. 8). “If no evidence about the real world can possibly disprove a proposition, it can hardly be called scientific or empirical in character” (Polsby, 1963, p. 5). These claims exhibit both of the

¹⁵ I say pseudo-Popperian because Popper never was—despite many social scientists' misreading of him—a dogmatic falsificationist; he is, rather, a methodological falsificationist, though not a “sophisticated” one (Lakatos, 1970).

mistakes made by dogmatic falsificationism, viz., the mistake of supposing that there is a "real world" completely *independent of* but *comparable with* our theories; and the *logical* mistake of believing that *facts* can "disprove" *propositions*.

A Lakatosian perspective also throws a kinder light upon attempts to save a research program from criticism by means of various adjustments to its protective belt. Indeed, a scientist operating within the assumptions of a research program will—quite rightly—spend much of his time and effort in strengthening this protective belt, so as to better protect the program's hard core. That is *not* to say that the hard core is not itself "falsifiable," nor that it is to be protected at *all* costs by making just *any* sorts of theoretical adjustments in the protective belt. Not all adjustments are equally acceptable; some represent the "progress," others the "degeneration" of a research program. If an adjustment is a "content-increasing" one, it is progressive. But if an adjustment is merely ad hoc—consisting, e.g., of a redefinition of terms, or the addition of some hypothesis which "explains" an old anomaly but fails to lead to the discovery of new facts—then that adjustment represents a content-decreasing or "degenerating" problem-shift. Lakatos' conception of progressive and degenerating problem-shifts can be made clearer—and brought closer to home—by means of an example drawn from contemporary political science.

There is at least one well-articulated research program in political science, viz., the "rational choice" approach of Downs, Olson, Riker and others (Moon, 1975, p. 195). The hard core of this program consists—as programs in the *social* sciences must—of a certain "model of man," i.e., a "fundamental conceptualization of what it is to be a person, including not only an account of human needs and capacities, but also a view of how a person is related to others" (Moon, 1974, p. 1). The rational choice program's model of man—which may be traced back to Hobbes—views men as rational self-interested calculators. Men are assumed to be rational, and rationality is in turn defined in instrumental terms: to be rational is to choose that course of action (policy, state of the world, etc.) which will be most efficient in satisfying one's own ordered preferences. The objection that these basic assumptions are themselves "unrealistic" or are "refuted" by the "facts" of human behavior in the "real world," carry little weight with contemporary "positive" theorists (Friedman, 1953, ch. 1; Riker and Ordeshook, 1973, ch. 2). To admit as relevant any direct criticism of the "hard core" would be to violate the "negative heuristic," i.e., the methodological rule that criticism be directed away from the hard core and toward the program's protective belt.

The idea of progressive and degenerating problem-shifts in the rational

choice program is especially well illustrated in the various recent attempts to resolve the “paradox of voting.”¹⁶ This paradox arises when one attempts, as Downs did in *An Economic Theory of Democracy*, to explain political participation in terms of a utility-maximizing model of rational choice and behavior. Downs’ theory predicts that a rational agent would not vote, inasmuch as his single vote would not appreciably alter the probability of his getting what he wants (in this case, his preferred candidate’s winning); hence he will not “spend” the time and effort to “buy” what he can get for “free.” The paradox stems not from the fact that people do nevertheless vote—theories are not confuted by facts, *simpliciter*—but from several “internal” or “theoretical” anomalies. For consider: a rational agent will “pay the cost” of voting only if, as a result, *he* stands a better chance of getting what he wants for himself. Now in a two-candidate race in which many votes are cast, one voter’s chance of affecting the outcome is insignificant; therefore it will not “pay” him to vote. But then (each rational calculator will reason) other rational agents will reach the same conclusion, and so no one will vote. In that case he should vote, since the election will end in a 1–0 victory for his candidate. Presumably, however, each rational agent will reach a similar conclusion about the value of his single vote, and so everyone will vote. In that case, however, each voter’s chance of affecting the outcome of the election will be negligible and so—again—no one will vote. This circle, if not vicious, is at least dizzying. It poses, at the very least, a problem for Downs’ theory. But—more than that—if it cannot be resolved by means of one or more theoretical adjustments, the hard core of the rational-choice program itself may be in jeopardy.

Such “adjustments” have been proposed, first by Downs himself and later by Riker and Ordeshook. It may be shown, however, that these adjustments represent content-decreasing or “degenerating” problem-shifts.

Downs attempts to resolve the paradox (or rather anomaly) of voting by means of the following adjustment. A rational citizen, he argues, will pay the cost of voting, even if he thereby gains nothing, because he derives satisfaction from “do[ing] his share in providing long-run benefits” for himself and others, through helping to maintain the democratic system (Downs, 1957, p. 270). But then, as Barry notes (1970, p. 20):

“Doing his share” is a concept foreign to the kind of “economic” rationality with which Downs is working. It requires our citizen to reason that since the benefits he gets depend

¹⁶ The following “Lakatosian” analysis of the paradox of voting was first suggested to me by Donald Moon. I have followed closely—indeed purloined—the account of it given in Moon (1975, pp. 196–204).

on the efforts of others, he should contribute too. This may be good ethics, but it is not consistent with the assumptions of the model, which require the citizen to compute the advantage that accrues to him from his doing x rather than y ; not the advantage that would accrue to him from *himself and others* doing x rather than y , unless, of course his doing it is a necessary and sufficient condition of the others doing it.

Or, to say the same thing in Lakatosian language: Downs' proposed adjustment violates the negative heuristic of the rational choice program, inasmuch as it calls into question the hard-core tenet of instrumental rationality itself. The hard-core assumption is that a rational agent will not "pay" for something if he can get it for nothing. That, after all, is why he will not vote: given that he prefers A to B , and that many people prefer either A or B and will vote accordingly, then the probability that *his* voting for A will result in A 's winning, is very small indeed; and so long as there are *any* "costs" accruing to him from voting, he will not vote. If his candidate wins he can enjoy the fruits of victory without having to pay for them. But then, extending this logic to the long-term rewards accruing to himself and others from maintaining the democratic system, one has to admit—as Downs does—that "he will actually get this reward [too], even if he himself does not vote" (1957, p. 270). Therefore it is *still* irrational for him to vote. To argue otherwise—as Downs insists upon doing—is to reject the hard core of the rational choice program's tenets of self-regardingness and instrumental rationality. Downs' proposed adjustment is therefore impermissible, representing as it does a degenerating problem-shift in the rational choice program.

Another sort of adjustment is proposed by Riker and Ordeshook (1968). Rejecting Downs' introduction of other-regarding (or even altruistic) motives into the calculus of voting, they attempt to resolve the paradox of voting in terms consistent with the program's assumption of self-regardingness. A rational person does stand to gain from voting, they argue, even if those gains be such nonmaterial psychological "satisfactions" as those stemming from "affirming allegiance to the political system," "affirming a partisan preference," and even "compliance with the ethic of voting" (Riker and Ordeshook, 1968, p. 28). This adjustment is, however, sadly ad hoc. As Barry remarks (1970, p. 15):

Now it may well be true that much voting can be accounted for in this way, and one can of course formally fit it into an "economic" framework by saying that people get certain "rewards" from voting. But this *is* purely formal. And it forces us to ask what really is the point and value of the whole "economic" approach. It is no trick to restate all behaviour in terms of "rewards" and "costs"; it may for some purposes be a useful conceptual device, but it does not in itself provide anything more than a set of empty

boxes waiting to be filled. . . . Insofar as it includes voting as a purely expressive act, not undertaken with any expectation of changing the state of the world, it fails to fit the minimum requirements of the means-end model of rational behaviour.

Riker and Ordeshook's adjustment is, then, merely a semantical one: they implicitly redefine the concept of "reward" to include any and all sorts of "satisfactions." But then, broadening the concept of reward in this way, and in this direction, represents a degenerating problem-shift, inasmuch as it resolves a *theoretical* anomaly by means of a *verbal* or terminological adjustment.

If these were the only sorts of adjustments to be made in the protective belt of the rational choice program, we might well wonder whether that program itself—"hard core" and all—is salvageable or even worth saving. However, Ferejohn and Fiorina (1974) have proposed a solution to the paradox of voting which constitutes a genuinely progressive problem-shift in the rational choice program. Their proposed adjustment is easily summarized. Rational choice theorists took a wrong turn, they argue, in "equating the notion of rational behavior with the rule of maximizing expected utility" (1974, p. 535). Their solution is to assume not that voters are expected-utility maximizers but that they are instead maximum-regret minimizers; that is, instead of maximizing gains, they minimize their maximum loss, interpreted as "regret." Their "minimax regretter" votes, not because he expects to increase significantly the probability of his candidate's winning, but because he wishes to avoid the most regretful possible outcome, viz., his candidate's losing by one vote. In contrast with the expected-utility maximizer,

The minimax regret decision maker uses a simpler rule. He imagines himself in each possible future state of the world and looks at how much in error each of his available actions could be, given that state. Then he chooses that action whose maximum error over the states of nature is least. If asked why he voted, a minimax regret decision maker might reply, "My God, what if I didn't vote and my preferred candidate lost by one vote? I'd feel like killing myself." Notice that for the expected-utility maximizer the probability of such an event is very important, whereas for the minimax regret decision maker the mere logical possibility of such an event is enough (Ferejohn and Fiorina, 1974, p. 535).

Assuming, then, that voters are indeed minimax regretters rather than expected-utility maximizers, it is rational for them to vote. Given this assumption, we can now predict higher levels of voter turnout. Thus the paradox of voting is solved, or rather, dissolved.

The "adjustment" proposed by Ferejohn and Fiorina is quite consistent

with the rational choice program's hard core (contra Downs); and it is not merely an ad hoc verbal strategem designed to rid the program of a persistent anomaly (contra Riker and Ordeshook). Their adjustment leaves the hard core untouched, and even strengthened, inasmuch as the newly renovated protective belt now affords even better protection than before. Their adjustment is, moreover, a content-increasing or "progressive" one, in that it leads to the prediction of new and unforeseen facts. For example, it predicts that in a three-candidate race a minimax regretter will not vote for his second choice, even when his most preferred candidate is likely to lose and his least preferred one likely to win. And this, in turn, suggests some promising lines of research into voter choice in multiparty systems.

Of one thing we may be sure: Ferejohn and Fiorina's adjustment in the protective belt of the rational choice program will lead sooner or later to other troubling anomalies which will require further adjustments in the program's protective belt. Just what these anomalies and adjustments will be—and whether these will in their turn represent progressive or degenerating problem-shifts—cannot be predicted, but can only be seen retrospectively with the wisdom of hindsight (Lakatos, 1971). Minerva's owl, as Hegel reminded us, takes flight only at dusk.

How to Be a Good Political Scientist: A Plea for Tolerance in Matters Theoretical¹⁷

In dwelling at some length upon the rational choice program I have not meant to imply that political science has at present only one genuine research program. Quite the contrary: I should rather say that political science has now, or has had, a number of promising research programs. I am thinking particularly of two prominent and late-lamented programs—Marxism and functional analysis. In concluding, I want to suggest—perhaps a bit tendentially—that these programs might have been killed off prematurely. Their protagonists lacked the necessary *tenacity*, their critics the necessary *tolerance*, required to give these programs a fighting chance. Of course their demise may, upon examination, be shown to be no more than just; nevertheless they were rather harshly treated. Never having been adequately protected, they were (in Lakatos' phrase) sitting ducks for the dogmatic falsificationists. These (and perhaps other) downed ducks should be retrieved and their life histories reconstructed and examined with care. We should, in other words, ask ourselves: Were they given a fighting chance? Were their hard cores given adequate protection? Which (if any) of the adjustments in

¹⁷ Title borrowed, with amendment and apologies, from Feyerabend (1963).

their protective belts were content-increasing, and which content-decreasing ones?

Of course critics can, and should, criticize. But how much credence should be given to criticisms of budding research programs? While defending the criticizability of all research programs, Lakatos remarks:

... criticism does not—and must not—kill as fast as Popper [and other falsificationists] imagined. Purely negative, destructive criticism, like “refutation” or demonstration of an inconsistency, does not eliminate a programme. Criticism of a programme is a long and often frustrating process, and one must treat budding research programmes leniently.

Lakatos adds—with a sympathy for the social sciences rare among philosophers of science—a further warning about “the destructive effect of naive falsificationism upon budding research programs [in the social sciences]” (1970, p. 179).

We political scientists have not, I fear, treated our budding research programs leniently. We have, on the contrary, made them into sitting ducks; and in a discipline which includes many accomplished duck hunters, this has often proved fatal.¹⁸ If we are to be good sportsmen we need to take Lakatos’ methodology seriously. This involves a number of moves. It requires, first of all, that we give up our long-held dogmatic falsificationist views; secondly, that we be tenacious in defending and tolerant in criticizing research programs; thirdly, that we distinguish between “hard core” and “protective belt,” and direct our defenses and/or criticisms accordingly; fourthly, that our criticisms be retrospective, and directed against adjustments in the protective belt of the program in question; and finally, that we judge the success-to-date of a research program in terms of the “progressiveness” or “degeneration” of its successive problem-shifts.

Consider, for example, the case for “scientific” Marxism. Is Marxism, as one of its defenders (Ollman, 1973) has recently claimed, the best (if not perhaps the only) basis for a genuinely scientific social science? If we are to answer that question we must begin by demarcating between science and nonscience (or pseudoscience). And this—following Lakatos—requires that we be able “to demarcate between scientific and pseudoscientific *adjustments*,

¹⁸ Among those who have hunted—and presumably killed—structural-functional ducks are Gregor (1968) and Flanigan and Fogelman (1967). And prominent among those who have stalked Marxian ducks are Acton (1957), Plamenatz (1963, vol. II, ch’s. 5, 6), Popper (1963, vol. II, ch’s. 18–21), and Gregor (1965, ch’s. 4, 5). All are superb critics, but poor sportsmen: they went duck hunting with antiaircraft guns. Their sort of falsificationism virtually guarantees success in hunting. Is it then any wonder that we look, usually in vain, for new “paradigms” in our discipline? No duck in its right mind would fly into our sights.

between rational and irrational changes of theory" (1970, p. 117). We need, in other words, to rationally reconstruct the history of Marxism, and then to determine whether successive adjustments have not only taken care of old anomalies but have, at the same time, predicted new facts; that is, we need to know whether successive adjustments in the Marxian research program represent content-increasing problem-shifts, or content-decreasing ones. (For example: Does Lenin's theory of imperialism represent a progressive or degenerating problem-shift within the Marxian research program?) Obviously I cannot undertake that task here. I raise this issue only to indicate a promising line of inquiry and to plead for tolerance in matters theoretical.

Lakatos' theory of scientific progress has wide-ranging implications for the social sciences, and political science in particular. Perhaps one of the most striking of these implications is that the putative gap between "traditional" or "normative" political theory and political science is now narrowed considerably. Indeed, the "normative" theories of yesteryear may now be viewed as methodological prescriptions in Lakatos' sense. For what they "prescribe" or "recommend" is that we view man and society in certain ways and not in others.¹⁹ Thus Hobbes, for example, commended to us the model of man as a rational self-interested calculator; and modern "positive" theorists follow in his footsteps. And Rousseau—arguing against Hobbes—commended to us the view that man is an other-regarding social being, his wants and aspirations being a product of his education and upbringing; and students of "political socialization" and "civic culture" follow in his footsteps. What each of these "normative" theorists did, in other words, was—firstly—to propose a research program consisting of a not-directly-criticizable set of basic assumptions about human nature and society, and—secondly—to construct a crude protective belt of auxiliary hypotheses designed to insulate the hard core from a direct hit. In any event, the relationship between normative political theory and political science is, from a Lakatosian perspective, very nearly symbiotic. The "oughts" of "normative" theory are as much methodological as moral. Thus it appears that the history of political thought is central to the enterprise of political science. For it is only with the wisdom of hindsight that we rationally reconstruct, examine, and criticize our research programs. There is no other way of gauging the progress of this, or any other, scientific discipline.

Manuscript submitted April 30, 1975.

Final manuscript received June 20, 1975.

¹⁹ Of course that is not what they *intended* to recommend, nor is that *all* they did. For lucid and suggestive discussions of these matters, see Wolin (1968) and Moon (1975, pp. 209–216).

REFERENCES

- Acton, H.B. 1957. *The Illusion of the Epoch*. Boston: Beacon Press.
- Almond, Gabriel A. 1966. "Political Theory and Political Science," *American Political Science Review*, December 1966, pp. 869-879.
- Austin, J.L. 1970. *Philosophical Papers*, 2nd ed. Oxford: Clarendon Press.
- Barry, Brian. 1970. *Sociologists, Economists and Democracy*. London: Collier-Macmillan.
- Beardsley, Philip. 1974. "Political Science: The Case of the Missing Paradigm," *Political Theory*, February 1974, pp. 46-61.
- Boneau, C.A. 1974. "Paradigm Regained: Cognitive Behaviorism Restated," *American Psychologist*, May 1974, pp. 297-310.
- Buck, Roger C., and Cohen, Robert S., eds. 1971. *Boston Studies in the Philosophy of Science*, Vol. VIII. Dordrecht: D. Reidel.
- Burt, E.A. 1954. *The Metaphysical Foundations of Modern Science*, 2nd rev. ed. (first publ. 1924). New York: Anchor Books.
- Coats, A.W. 1969. "Is There a 'Structure of Scientific Revolutions' in Economics?," *Kyklos*, Fasc. 2, 1969, pp. 289-296.
- Collingwood, R.G. 1940. *An Essay on Metaphysics*. Oxford: Clarendon Press.
- Dahl, Robert A. 1963. *Modern Political Analysis*. Englewood Cliffs, N.J.: Prentice-Hall.
- Downs, Anthony. 1957. *An Economic Theory of Democracy*. New York: Harper & Row.
- Euben, J. Peter. 1969. "Political Science and Political Silence," in Philip Green and Sanford Levinson, eds., *Power and Community*. New York: Pantheon, pp. 3-58.
- Eulau, Heinz. 1964. *The Behavioral Persuasion in Politics*. New York: Random House.
- Ferejohn, John A., and Fiorina, Morris P. 1974. "The Paradox of Not Voting: A Decision Theoretic Analysis," *American Political Science Review*, June 1974, pp. 525-536.
- Feyerabend, Paul K. 1963. "How to be a Good Empiricist—A Plea for Tolerance in Matters Epistemological," in Bernard Baumrin, ed., *Philosophy of Science: The Delaware Seminar*, Vol. II. New York: Wiley.
- _____. 1970. "Problems of Empiricism II," in Robert G. Colodny, ed., *The Nature and Function of Scientific Theories*. University of Pittsburgh Series in the Philosophy of Science, Vol. IV. Pittsburgh: University of Pittsburgh Press.
- Flanigan, William, and Fogelman, Edwin. 1967. "Functional Analysis," in James C. Charlesworth, ed., *Contemporary Political Analysis*. New York: Free Press, pp. 72-85.

- Friedman, Milton. 1953. *Essays in Positive Economics*. Chicago: University of Chicago Press.
- Friedrichs, Robert. 1971. "Sociological Paradigms: Analogies of Teleology, Apocalypse, and Prophecy," *Sociological Analysis*, Spring 1971, pp. 1-6.
- Gregor, A. James. 1965. *A Survey of Marxism*. New York: Random House.
- _____. 1968. "Political Science and the Uses of Functional Analysis," *American Political Science Review*, June 1968, pp. 425-439.
- Gunnell, John G. 1968. "Social Science and Political Reality: The Problem of Explanation," *Social Research*, Spring 1968, pp. 159-201.
- _____. 1969. "Deduction, Explanation, and Social Scientific Inquiry," *American Political Science Review*, December 1969, pp. 1233-1246.
- Hanson, Norwood Russell. 1958. *Patterns of Discovery*. Cambridge: Cambridge University Press.
- Holt, Robert T., and Richardson, John M., Jr. 1970. "Competing Paradigms in Comparative Politics," in Robert T. Holt and John E. Turner, eds., *The Methodology of Comparative Research*. New York: Free Press, pp. 21-71.
- Kuhn, Thomas S. 1962. *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.
- _____. 1970a. "Postscript" to the 2nd ed. of Kuhn (1962), pp. 174-210.
- _____. 1970b. "Reflections on My Critics," in Lakatos and Musgrave, 1970, pp. 231-278.
- _____. 1974. "Second Thoughts on Paradigms," in Frederick Suppe, ed., *The Structure of Scientific Theories*. Urbana: University of Illinois Press.
- Lakatos, Imre. 1970. "Falsification and the Methodology of Scientific Research Programmes," in Lakatos and Musgrave, 1970, pp. 91-196.
- Lakatos, Imre, and Musgrave, Alan, eds. 1970. *Criticism and the Growth of Knowledge*. Cambridge: Cambridge University Press.
- _____. 1971. "History of Science and Its Rational Reconstructions," in Buck and Cohen, 1971, pp. 91-136.
- Landau, Martin. 1972a. *Political Theory and Political Science*. New York: Macmillan.
- _____. 1972b. "Comment [on Miller, 1972]," *American Political Science Review*, September 1972, pp. 846-856.
- Lebowitz, Barry. 1971. "Paradigms in Sociology: Some Thoughts on an Undebated Issue," a paper read at the 1971 American Sociological Association meetings.
- Machamer, Peter K. 1971. "Observation," in Buck and Cohen, 1971, pp. 187-201.
- _____. 1973. "Feyerabend and Galileo: The Interaction of Theories, and the Reinterpretation of Experience," *Studies in History and Philosophy of Science*, May 1973, pp. 1-46.

- Masterman, Margaret. 1970. "The Nature of a Paradigm," in Lakatos and Musgrave, 1970, pp. 59-90.
- Miller, Eugene F. 1972. "Positivism, Historicism, and Political Inquiry," *American Political Science Review*, September 1972, pp. 796-817.
- Moon, J. Donald. 1974. "Models of Man in Hobbes and Rousseau: On Political Philosophy and the Study of Politics," a paper presented to the Foundations of Political Theory group, at the Seventieth Annual Meeting of the American Political Science Association, Chicago, August 28-September 2, 1974.
- _____. 1975. "The Logic of Political Inquiry: A Synthesis of Opposed Perspectives," in Fred I. Greenstein and Nelson W. Polsby, eds., *Handbook of Political Science*, Vol. I. Reading, Mass.: Addison-Wesley, pp. 131-228.
- Ollman, Bertell. 1973. "Marxism and Political Science: Prolegomenon to a Debate on Marx's Method," *Politics and Society*, Summer 1973, pp. 491-521.
- Palermo, David. 1971. "Is a Scientific Revolution Taking Place in Psychology?," *Science Studies*, 1971, pp. 135-155.
- Polsby, Nelson W. 1963. *Community Power and Political Theory*. New Haven: Yale University Press.
- Popper, Karl R. 1962. *Conjectures and Refutations*. New York: Basic Books.
- _____. 1963. *The Open Society and Its Enemies*, two vols. New York: Harper Torchbooks.
- _____. 1970. "Normal Science and Its Dangers," in Lakatos and Musgrave, 1970, pp. 51-58.
- _____. 1972. *Objective Knowledge*. Oxford: Clarendon Press.
- Riker, William H., and Ordeshook, Peter C. 1968. "A Theory of the Calculus of Voting," *American Political Science Review*, March 1968, pp. 25-42.
- _____. 1973. *An Introduction to Positive Political Theory*. Englewood Cliffs, N.J.: Prentice-Hall.
- Rudner, Richard. 1972. "Comment [on Miller, 1972]," *American Political Science Review*, September 1972, pp. 827-845.
- Ryan, Alan. 1972. "'Normal' Science or Political Ideology?," in Peter Laslett, W.G. Runciman, and Quentin Skinner, eds., *Philosophy, Politics, and Society*, fourth series. Oxford: Blackwell, pp. 86-100.
- Shapere, Dudley. 1964. "The Structure of Scientific Revolutions," *Philosophical Review*, July 1964, pp. 383-394.
- _____. 1966. "Meaning and Scientific Change," in Robert G. Colodny, ed., *Mind and Cosmos: Essays in Contemporary Science and Philosophy*, University of Pittsburgh Series in the Philosophy of Science, Vol. III. Pittsburgh: University of Pittsburgh Press.

- Smolicz, Jerzy. 1970. "Paradigms and Models," *Australian and New Zealand Journal of Sociology*, October 1970, pp. 100-119.
- Spector, Marshall. 1966. "Theory and Observation," *British Journal for the Philosophy of Science*, May-August 1966, pp. 1-20, 89-104.
- Stanfield, J.R. 1974. "Kuhnian Scientific Revolutions and the Keynesian Revolution," *Journal of Economic Issues*, 1974, pp. 97-109.
- Stephens, Jerone. 1973. "The Kuhnian Paradigm and Political Inquiry: An Appraisal," *American Journal of Political Science*, August 1973, pp. 467-488.
- Toulmin, Stephen. 1963. *Foresight and Understanding*. New York: Harper Torchbooks.
- _____. 1967. "The Evolutionary Development of Natural Science," *American Scientist*, December 1967, pp. 456-471.
- _____. 1970. "Does the Distinction Between Normal and Revolutionary Science Hold Water?" in Lakatos and Musgrave, 1970. pp. 39-48.
- _____. 1972. *Human Understanding*, Vol. I. Oxford: Clarendon Press.
- Truman, David B. 1965. "Disillusion and Regeneration: The Quest for a Discipline," *American Political Science Review*, December 1965, pp. 865-873.
- Welmes, W.B. 1973. "Paradigms and Normal Science in Psychology," *Science Studies*, 1973, pp. 211-222.
- Wittgenstein, Ludwig. 1968. "Wittgenstein's Notes for Lectures on Private Experience and 'Sense Data,'" transcribed and annotated by Rush Rhees, *Philosophical Review*, July 1968, pp. 271-320.
- Wolin, Sheldon S. 1968. "Paradigms and Political Theories," in Preston King and B.C. Parekh, eds., *Politics and Experience: Essays Presented to Michael Oakeshott*. Cambridge: Cambridge University Press, pp. 125-152.
- _____. 1969. "Political Theory as a Vocation," *American Political Science Review*, December 1969, pp. 1062-1082.