The Power of Power Politics

From Classical Realism to Neotraditionalism

John A. Vasquez

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1 The role of paradigms in scientific inquiry: a conceptual framework and a set of principles for paradigm evaluation

The work of Thomas Kuhn (1962, 1970a) has attracted much interest from historians and philosophers of science because it offers a way to describe and evaluate scientific inquiry. For this reason it provides a framework for determining whether the realist paradigm has adequately guided inquiry in international relations. Before the framework can be applied, a number of questions that have been raised by critics of Kuhn must be addressed.[1] The three most important are: how to define *paradigm*; whether Kuhn's description of scientific change is correct; and how paradigms can be evaluated. Each of these will be examined in this chapter.

Defining the concept of *paradigm*

Despite its wide use, the paradigm concept remains very difficult to define. The reason for this stems from its original usage by Thomas Kuhn in *The Structure of Scientific Revolutions* (1962). A textual analysis of that work by Margaret Masterman (1970) has shown that the concept of *paradigm* was used by Kuhn in at least twenty-one different ways. In the postscript to the second edition of the book, Kuhn (1970a: 174–191) recognized this criticism and attempted to clarify the definition. He maintains that most of the varying usage is due to stylistic inconsistencies but concedes that even after these inconsistencies are removed, the concept is used in two distinct ways:

> On the one hand, it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given

[1] For a criticism of work in political science that has failed to take note of Kuhn's different definitions see J. Stephens (1973).

> community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science.
>
> (Kuhn 1970a: 175)

The first definition is what Kuhn (1970a: 181) has called ''the constellation of group commitments.'' In this first definition, it is the shared constellation which is the basis of classifying an aggregate of scholars as a community (Kuhn 1970a: 176–178, 182). Kuhn (1970a: 182; 1971: 462–463; 1977: xvi–xxiii) has suggested that this use of the concept *paradigm* may be too broad in scope to support the central thesis of his book. He has therefore chosen to call this notion of paradigm a *disciplinary matrix*, the chief components of which are: (1) symbolic or theoretical generalizations, such as $f = ma$; (2) metaphysical beliefs or beliefs in certain models, such as heat kinetic energy of the constituent parts of bodies; (3) values, such as predictions should be accurate, what constitutes accuracy, what is inconsistency, what is plausibility, what is parsimonious, etc.; (4) an exemplar, which is the element in the disciplinary matrix that by itself forms the second definition of paradigm (Kuhn 1970a: 184–186; 1971; 464).

The second definition is what Kuhn (1970a: 187) has called the *paradigm as exemplar*, or *shared example*. In order to understand what an exemplar is and why it has such force within a scholarly community, it is necessary to examine how future professionals of a discipline are educated. According to Kuhn (1970a: 187–189) scientific education involves primarily ''problem-solving.'' Problem solving is a central component of scientific education in two ways. First, the ability to solve new problems is the primary educational objective of scientific training. Second, the basic means of achieving this objective is to have students solve problems to which the correct answers are already known. The assumption behind this philosophy of education is that if students are capable of arriving at the correct solution to old but difficult problems, they will acquire the ability to solve current and new problems. According to Kuhn (1970a: 189), these sets of problems function to inculcate the student with a fundamental way of viewing the world (see also Kuhn 1971: 472–482). In addition to providing sets of solved problems, the exemplar is used in scientific education to inform the student about the existing unsolved problems or puzzles in the field. The latter bit of information tells the student what is worth knowing. These sets of problems constitute the concrete manifestation

of the exemplar. But the paradigm as exemplar consists not of the problems themselves but of the elements that are used to perceive, define and solve problems.

Unfortunately, this reformation of the concept has not satisfied most of Kuhn's critics (see Shapere 1964, 1971; Toulmin 1967, 1970; Watkins 1970). Their original criticisms can be reduced to two points: that the concept is ambiguous in that it refers to so many aspects of the scientific process that his thesis is almost nonfalsifiable; and that it is so vague that it is difficult to identify (in operational terms, for example) the specific paradigm of a discipline (Shapere 1964: 385–386). The problem of ambiguity is quite severe. At times it seems that the paradigm concept refers to a set of research questions, the publication of a seminal work that changes inquiry in the field (exemplar), a particular theory, an epistemological viewpoint, or a method of investigation (Masterman 1970: 61–65).

Clearly, focusing on one of these elements while ignoring the others will produce a very different description of a discipline. Kuhn's selection of puzzle solutions attempts both to solve this problem and produce an operational indicator. Yet this notion is not adequate. In any science, there are numerous puzzle solutions, and Kuhn does not provide any criteria for distinguishing among or classifying these solutions. Are puzzle solutions to be defined on the basis of their method, their dependent variables, their independent variables, or their connection to an exemplar? Kuhn does not address these questions adequately, and it is not surprising that, of the original critics (compare Shapere 1964 and 1971; Toulmin 1967 and 1970), none is satisfied with his response.

These conceptual problems have led some of the scholars who have applied Kuhn's concept in describing inquiry within political science to produce very different and sometimes contradictory analyses (cf. Stephens 1973). Lijphart (1974) argues that within international relations behavioralism is a paradigm, whereas Beal (1976) argues that Lijphart places too much emphasis on method and ignores the fact that many quantitative scholars have tested traditional propositions. Lijphart and others such as Wolin (1968), who view behavioralism as a paradigm, see it as the attempt to employ the scientific method to study politics and distinguish this approach from traditional and normative methods. Keohane and Nye (1972) are more concerned with the substantive focus and have argued that international relations is dominated by a state-centric paradigm, whereas Handelman

et al. (1973) have argued that a realist paradigm has dominated the field. While Keohane and Nye (1974, 1977) have more recently spoken of the realist rather than the state-centric paradigm, others, for example Ashley (1976), have argued that international relations is in a pre-paradigm stage, and that there are many different conceptual approaches and ''theories'' in the discipline (see also Alker 1971). Such disagreements are primarily a function of emphasizing different aspects of Kuhn's conception of paradigm.

If Kuhn's concept and his subsequent analysis are to be employed, they must be defined more precisely, and procedures must be established for operationalizing them. Since Kuhn has not adequately resolved these problems, this analysis must provide its own stipulative definition. Stipulative definitions are neither correct nor incorrect, since they are not empirical statements (see Ayer 1946; Wilson 1956); rather, they can be evaluated on the basis of their ability to conceptualize a set of phenomena in a way that clarifies rather than obscures relationships. In this sense, the most useful stipulative definition of *paradigm* is one that can utilize most of Kuhn's insights and provide an adequate account of how science proceeds.

To provide such a definition, it is important to stipulate what is not a paradigm. A paradigm is neither a method nor a theory. In the first instance, the scientific method and its various modes of testing (experimentation, simulation, statistical analysis, comparative case studies) cannot constitute a paradigm in any Kuhnian sense, because all the physical sciences share this method and would be dominated by a single paradigm. Clearly, Kuhn is not interested in the shared elements of the physical sciences, but in what makes them individual and coherent disciplines.

The heart of the paradigm concept must be substantive and not methodological, but a paradigm is not necessarily the same thing as a dominant theory. First, there can often be more than one theory in a field or shifts in accepted theories without producing what Kuhn would call a paradigm shift. Second, a paradigm is in some sense prior to theory. It is what gives rise to theories in the first place. Toulmin (1967) in particular is intrigued by the question of what exists in a field when there is no theory (a question certainly relevant to international relations inquiry), and suggests that Collingwood's (1940) notion of absolute presuppositions serves the same function as Kuhn's notion of paradigm.

The concept of *paradigm*, then, could be stipulatively defined as *the*

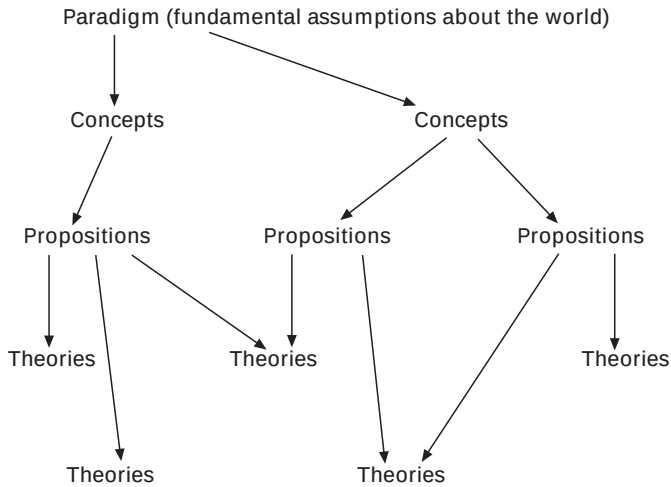


Figure 1.1. The analytical relationship among paradigms, concepts, propositions, and theories

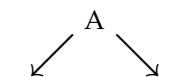
fundamental assumptions scholars make about the world they are studying. These assumptions provide answers to the questions that must be addressed before theorizing even begins. For Kuhn, as Masterman (1970: 62) points out, such questions are: What are the fundamental units of which the world is composed? How do these units interact with each other? What interesting questions may be asked about these units? What kinds of conceptions will provide answers to these inquiries? By responding to these questions, the fundamental assumptions form a picture of the world the scholar is studying and tell the scholar *what is known about the world, what is unknown about it, how one should view the world if one wants to know the unknown, and finally what is worth knowing*.[2]

[2] This stipulative definition differs considerably from the components of a research paradigm that are identified by Alker (1971, reprinted in Ashley 1976: 154). Alker's list is not used here because its requirements are so stringent that only very narrow research efforts, like work on the Richardson arms race model, would be seen as having a paradigm. Ashley (1976: 155) is even more restrictive. Such a position comes close to the notion that the paradigm concept should be employed only to distinguish the narrowest scientific community, the invisible college. At times, in his revisions, Kuhn (1971: 461–462) comes close to saying this, but he recognizes that there are different levels of a scientific community. Each of these in some sense may have its own shared-examples. Clearly, however, classics such as Newton's *Principia* function at the broad disciplinary level and provide an exemplar or paradigm for the

The preceding definition has been stipulated to distinguish a paradigm from a conceptual framework or theory. To clarify this distinction, figure 1.1 specifies the analytical relationships. A paradigm consists of a set of fundamental assumptions of the world. These assumptions focus the attention of the scholar on certain phenomena and interpret those phenomena via concepts. Propositions, in turn, are developed by specifying relationships between concepts. Finally, theories are developed by specifying relationships between propositions.

It can also be seen from figure 1.1 that a pyramid effect is in operation. For example, if A, B, C are concepts, the following propositions, among others, can be logically derived:

1. A ⟶ B
2. B ⟶ C
3. A ⟶ C
4. C ⟶ A
5. A ⟶ B ⟶ C
6. B ⟶ C ⟶ A

7.


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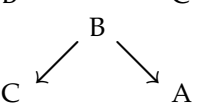

Likewise, as shown in figure 1.1, a given set of propositions can be linked in different ways to give rise to a variety of theories. Therefore it follows that one paradigm can give rise to more than one theory. On the basis of this analysis, it can be stipulated that a paradigm only changes when its fundamental assumptions or view of the world changes.[3] "New" concepts, propositions, or theories that do not change the assumptions of the paradigm do not constitute new paradigms, but only the elaborations, or what Kuhn (1970a: 24, 33–34) calls articulations, of the old one.

disciplinary matrix and not just for the invisible college. As will be seen later, the primary difference between the role of realism in the international relations field and that of other approaches, like decision making or systems, is that some of the fundamental assumptions of realism are shared by most scholars in the discipline, whereas the shared-examples of the other approaches are confined to a narrower group. In this analysis, *paradigm* is defined in a very broad (but not necessarily imprecise) manner. For a recent reconstuction of Kuhn that attempts to delineate how assumptions lead to a picture of the world and then to a research program, see Tornebohm (1976). For an attempt to delineate invisible colleges within international relations, see Russett (1970).

[3] This statement agrees with Kuhn (1970a: ch. 10, "Revolutions as changes of World View").

One of the main advantages of this stipulative definition is that, by reducing the ambiguity of the term, it does not affect most of Kuhn's propositions about scientific inquiry, yet it specifies clearly the conditions under which paradigms change, thereby permitting Kuhn's thesis to be falsified. Throughout the remainder of this analysis, unless otherwise indicated, whenever the concept *paradigm* is employed, including references to Kuhn's use of the term, it should be thought of in terms of the stipulative definition given here.

Describing scientific inquiry

The utility of the paradigm concept can be demonstrated by showing how Kuhn uses the concept to describe scientific inquiry. Kuhn's description is concerned with how paradigms dominate a field and how they are displaced. A dominant paradigm is usually provided by a single work, which is viewed as so unprecedented in its achievement that it becomes an exemplar of scientific analysis in a particular field:

> Aristotle's *Physica*, Ptolemy's *Almagest*, Newton's *Principia* . . . these and many other works served for a time implicitly to define the legitimate problems and method of a research field for succeeding generations of practitioners. They were able to do so because they shared two essential characteristics. Their achievement was sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity. Simultaneously it was sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve. (Kuhn 1970a: 10)

Once a paradigm dominates a field, scholarship enters the stage Kuhn (1970a: 10, 23–25) calls *normal science*. Scholarly behavior in this stage is characterized by extensive articulation of the paradigm by a research program that guides the theory construction, fact gathering, and research of scholars (Kuhn 1970a: 34). Theory construction in normal science is not haphazard, but highly systematic because the paradigm constrains scholars to the elaboration of theories that do not violate the fundamental assumptions of the paradigm (Kuhn 1970a: 24).

In addition to suggesting what are legitimate theories, the paradigm also suggests what, out of the welter of phenomena, are theoretically significant facts (Kuhn 1970a: 25). Much of normal science consists of gathering these facts. Before ''facts'' can be gathered, however,

scientists must create tools that will permit the facts to be measured, just as the thermometer had to be invented in order to observe and measure heat. Finally, having gathered the facts, the theory is tested by matching it with the facts. After the tests, the theory is further elaborated and refined.

Theory construction, fact gathering, and research, then, are systematically linked through a feedback process. This does not mean that there will not be drastic changes in theories. There will be, as theories are tested, but any ''new'' theories will never violate the assumptions of the paradigm (Kuhn 1970a: 33–34). When a truly new theory emerges, it signals the existence of a new paradigm(s) and may under certain conditions result in what Kuhn (1970a: 52–53) calls *scientific crisis and revolution*.

Normal science begins to come to an end when an anomaly – ''the recognition that nature has somehow violated the paradigm-induced expectations'' – is unable to be removed by paradigm articulation (Kuhn 1970a: 52–53). The persistence of the anomaly(ies) results in a crisis in the field. Crisis is met by devising ''numerous articulations and ad hoc modifications of . . . theory in order to eliminate any conflict'' between fact and theory (Kuhn 1970a: 78). However, if the anomaly can be accounted for only by seeing the world in a new and different way (i.e., by the creation of a new paradigm), then the stage is set for a struggle between the adherents of the competing paradigms (Kuhn 1970a: 53, ch. 10). If the struggle results in the displacement of the old paradigm and the dominance of the new paradigm, then this period is viewed with hindsight as a period of scientific discovery and revolution. New textbooks rewrite the history of the field, students are trained to see the world according to the new paradigm, and the process repeats itself.

Some critics (Shapere 1971: 706; Toulmin 1970: 41) have questioned this description of scientific inquiry by challenging the sharp distinction between normal science and revolutionary science (what might be better termed extraordinary science [see Kuhn 1970a: 34]), arguing that the distinction is really a matter of degree and that such discontinuities are not as common as Kuhn implies. This criticism underlines the more general point that within paradigms there can be considerable variations and disagreement, and out of this process there can evolve what Kuhn would call revolutions. For Toulmin, these ''revolutions'' tend to be a product of many earlier changes; he therefore finds the process of change described by Kuhn incomplete

because it does not explain how knowledge evolves through learning (1967: 339–346; 1970: 46). Blachowicz (1971: 182–183, 186–188) goes further, arguing that Kuhn so underestimates the amount of learning and changes that he must see theories as arising from a random process.

Kuhn has in part responded to the criticism by granting that there might be microrevolutions, but he is unwilling to abandon the more fundamental distinction between normal and revolutionary science and insists that normal science can involve considerable conceptual jettisoning without any rejection of the paradigm (see Kuhn 1970b: 249–259, 1970a: 250). He thereby rejects the more evolutionary notion of progress implied by Toulmin, maintaining instead that only certain anomalies and conceptual changes are revolutionary. Paradigm shifts, not variation and microrevolutions, bring about fundamental changes in thought.

These criticisms of Kuhn are primarily empirical and can only be answered by further research. It must be remembered that Kuhn's thesis is based on generalizing from his earlier work on the Copernican revolution (Kuhn 1957) and may not in fact apply to all other cases, as some have readily pointed out in the case of theories of matter (Shapere 1964: 387; Popper 1970: 55; Watkins 1970: 34). Yet one exception is hardly a disconfirmation. Kuhn's thesis needs systematic investigation in the physical sciences and should not be seen as having been ''confirmed'' or refuted by the discussion it has generated (L. P. Williams 1970: 50).

Keeping in mind the various qualifications and caveats that have been introduced, it should be clear that Kuhn provides a theoretically interesting and general conceptual framework for describing scientific inquiry. For international relations inquiry it suggests questions such as: Is the field dominated by a single paradigm? What is that paradigm? How did it displace the old one if there was an old paradigm? How does it guide theory construction, data making, and research? How do conceptual variation and change occur yet still remain within the paradigm? More important, Kuhn's framework provides a way of asking the major questions of this analysis – Is the dominant paradigm adequate? Is it producing knowledge? Before these last two questions can be addressed, a set of criteria for evaluating paradigms must be developed. Here Kuhn provides little aid.

Evaluating scientific inquiry

Evaluation differs from description in that its purpose is to apply a value criterion to a situation or object, whereas the purpose of description is empirical veracity.[4] Therefore, in order to evaluate scientific inquiry, some acceptable value criteria must be employed. Philosophers of science have spent a great deal of time attempting to delineate and justify such criteria. Although there are many disagreements among these philosophers, there is a certain minimal content on which they all agree. Part of this content includes a set of criteria for evaluating theories. Although there is dispute over the logical status of these criteria, there is not a dispute among either philosophers or practicing scientists about what these criteria actually state (see Braybrooke and Rosenberg 1972). It is upon this basis that criteria for evaluating paradigms can be erected.

The main criteria that these scholars accept rest on the assumption that science can produce knowledge. Part of Kuhn's analysis, however, led to a debate in philosophy of science over whether science is a rational enterprise that can claim to be producing knowledge. The part of Kuhn's analysis that caused the debate was his discussion of paradigm comparability and displacement. Kuhn appeared to argue that paradigms were not disproven but discarded on the basis of a struggle for power between the adherents of competing paradigms. Many critics took this argument to mean that Kuhn was maintaining that science was irrational and subjective.[5] In a later work, Kuhn attempted to defend himself by saying that although he maintained that paradigm displacement is a matter of persuasion, he did not mean to suggest "that there are not many good reasons for choosing one theory rather than another . . . These are, furthermore, reasons of exactly the kind standard in philosophy of science: accuracy, scope, simplicity, fruitfulness, and the like" (Kuhn 1970b: 261; see also 1977: 320–339). Kuhn (1970a: 186) maintained that what makes these reasons good is determined by the value component of the disciplinary matrix. This clarification makes it clear that Kuhn is willing to evaluate paradigms by employing the standard criteria

[4] On the differences and similarities of evaluative and empirical analysis see Toulmin (1950); on the relationship between evaluation and value criteria see Urmson (1968: ch. 5) and Frohock (1974: ch. 3).

[5] See Scheffler (1967); Lakatos and Musgrave (1970). Also see Shapere (1964, 1971); Popper (1970); and Shimony (1976).

used in science to determine the adequacy of theories. Therefore, the basic criterion that a paradigm must produce knowledge can be employed to evaluate paradigm adequacy. In order to determine exactly how this basic criterion can be applied and to understand what the debate between Kuhn and his critics has been about, it is necessary to review briefly some of the epistemological arguments that have been made about the confirmation of theories.

The earliest respectable view about confirmation was that theories are proven when there are a sufficient number of facts to support them.[6] The basic fallacy of this position is known as *the riddle of induction*. This debate over induction goes back at least to the time of John Stuart Mill. The debate was replayed in the twentieth century when Rudolph Carnap attempted to derive a logical position asserting that hypotheses could be proven.[7] Carnap, however, was unsuccessful in this effort; the consensus of philosophers of science is that such confirmation is impossible to achieve.

Sir Karl Popper (1935) attempted to place confirmation of theories on a firmer logical foundation by introducing the principle of falsification. According to Popper, a theory is a theory only if it specifies in advance what would be accepted as disproof of the theory. Experimentation in Popper's view never proves a theory but simply fails to falsify it. Popper's principle provides a clear, precise, and logically sound rule for evaluating theories. It was not until Kuhn introduced the concept of paradigm that the principle was seriously challenged.

Despite the fact that Kuhn's claim of paradigm incommensurability has been rejected in part because of the work of Scheffler (1967), the challenge to Popper has carried more weight (see Lakatos 1970). Kuhn (1970a: 146–148) has attempted to show that Popper's rule is simply not followed in the physical sciences. Theories and the paradigms out of which they arise do not stipulate what will count as falsifying evidence. Furthermore, when falsifying evidence is encountered, it does not lead to a rejection of the paradigm. Finally, according to Kuhn no paradigm has ever been "rejected" unless there is a competing paradigm ready to take its place. Popper's (1970: 52–53, 56–58) response is not that this does not occur, but that it need not necessarily occur and will not if scientists are trained properly. What

[6] An excellent history of this debate is Lakatos (1970).

[7] This is obviously a simplification of Carnap's work. The two books that adequately summarize his early work on this question are Carnap (1952, 1962).

most of the debate has been about, then, is how to confirm competing theories that may emerge from competing paradigms and their research programs.

On what basis can one decide to follow one research program rather than another? Lakatos (1970) has attempted to solve the problem by synthesizing Kuhn's work with the standard view of philosophy of science. He has given a major concession to Kuhn in that he admits that confirmation is a matter of decision and not logic.[8] He comes to this conclusion because he maintains that theories and paradigms can produce an infinite number of plausible ad hoc hypotheses to account for falsifying evidence. Nevertheless, he does think that the decision can be based on rules that are clearly stipulated in advance. Among the most important rules are the following: (1) T′ (rival theory) has excess empirical content; that is, it predicts novel experimental outcomes (anomalies) that are improbable or forbidden by T (original theory); (2) T′ explains all the unrefuted content of T; and (3) some of the excess content of T′ is corroborated (Lakatos 1970: 116). Lakatos has thus provided a set of principles that can be used to compare theories. In this scheme, paradigms and their research programs can be evaluated on the basis of the theories they produce.

The philosophical problem over which there is much contention is whether there is some logical foundation for rules that tell scientists when to stop introducing ad hoc explanations or theories, or whether the foundation is merely sociological consensus (see Worrall 1978; Musgrave 1978; Koertge 1978; and Feyerabend 1976). The latter position saves science as a rational enterprise, but whether science can have a more solid logical foundation is a matter of hot debate. At a minimum, the justification of Lakatos' rules could rest on the kind of instrumentalist argument often associated with Toulmin (1953, 1972: 478–503).

This justification rests on the acceptance by philosophers of science and scientists of the following type of argument: (1) the purpose of science is to produce knowledge; (2) knowledge itself is a semantic concept; that is, one can determine whether something is known by stipulatively defining what is meant by knowledge and establishing decision-rules on how to employ the word;[9] and (3) what is meant by knowledge is (at least in part) empirical corroboration of hypotheses.

[8] Some argue that this grants too much to Kuhn; see Musgrave (1976: 482).

[9] For a justification for this position in regard to the word *truth* see Tarski (1949).

A theory or a research program that has the most corroborated hypotheses and the least anomalies is obviously the best or the most promising one to use in order to achieve the purpose of science.

In social science, particularly in international relations inquiry, the problem of evaluating paradigms turns not so much on comparing the corroborated empirical content of rival theories and their research program but on finding any theory with a corroborated content of any significance. Since a paradigm is used to produce theories, it is possible to evaluate the adequacy of a paradigm in terms of the corroborated hypotheses it produces. This is the basic criterion that will be used here to evaluate paradigms. However, as Lakatos suggests, applying this criterion is a matter of decision. How many corroborated hypotheses must there be? How much paradigm-directed research must there be, and for how long must this research continue before a paradigm can be declared inadequate? All of these are unanswered questions in the field of international relations. But it does seem reasonable to assume that if various theories and hypotheses produced by the use of a paradigm fail over time to produce a significant number of findings, the problem may very well be that the picture of the world being used by scholars is simply inadequate. If the science of international relations is to be systematic, it is incumbent upon scholars to examine periodically what paradigm (if any) is dominating the field and to evaluate its usefulness in the terms outlined. In a discipline where there are very few corroborated hypotheses, there will always be disagreements over whether a paradigm and its research program are useful. But attempts at evaluation are important because they provide empirical evidence that scholars can use to come to a rational conclusion. As more research is conducted and more evaluations of it are made, a trend may become clear and the disagreements will probably subside. It is in this spirit that the present evaluation is offered.