The Perils of Paradigm Mentalities: Revisiting Kuhn, Lakatos, and Popper

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A common theme in the Centennial Issue of the American Political Science Review was how subfields have grown more specialized and insulated from one another. In this essay I argue that this trend has been hastened by the inappropriate incorporation of paradigm mentalities, first presented by Thomas Kuhn and later developed by Imre Lakatos. I show how paradigm mentalities help justify rigid opposition to theoretical alternatives and limit critical insight. While paradigm mentalities may be fitting for disciplines that demonstrate Kuhn's *concrete scientific achievements,* they constrain the study of political science and international relations in particular. I begin with a primer that compares Kuhn and Lakatos to Karl Popper. Next, I point to harmful consequences resulting from applying paradigm mentalities to the study of international relations. Among these is the tendency to act as if realism has earned the status of a paradigm and then invoke criteria of incommensurability and "subsumption" to deflect criticism. I conclude by discussing how Popper's model of science provides a better platform for the study of politics by encouraging theoretical and methodological pluralism.

The clash between Popper and Kuhn is not about a mere technical point in epistemology. It concerns our central intellectual values . . .
—Imre Lakatos[1]

The member of a mature scientific community is, like the typical character of Orwell's 1984, the victim of a history rewritten by the powers that be.
—Thomas Kuhn[2]

No political power has ever been unchecked, and as long as men remain human (as long as the 'Brave New World' has not materialized), there can be no absolute and unrestrained political power.
—Karl Popper[3]

While feeding his pet monkeys in Princeton Thomas Kuhn remarked how social scientists regularly misappropriate his idea of paradigms. Robin Fox described Kuhn as "horrified by this particular mangling of his theory of paradigms."[4] In 1962, Kuhn's *The Structure of Scientific Revolutions* transformed the philosophy of science, and intellectual life more generally. Yet Kuhn never intended his ideas for the social sciences. In the preface to *Structure,* Kuhn had indeed emphasized how paradigms set the natural sciences apart from the social sciences. Kuhn characterized the social sciences by their fundamental "disagreements" over the "nature of legitimate scientific problems and methods." The natural sciences, by contrast, failed to "evoke the controversies over fundamentals."[5] As a result, Kuhn grew critical of social scientists seeking to "improve the status of their field by first legislating [paradigms and normal science] . . . They are badly misconstruing my point."[6] Imre Lakatos had a similar reaction to social scientists applying his notion of scientific research programs. He referred to some of these efforts as little more than "phony corroborations" that yield "pseudo-intellectual garbage."[7]

In spite of these caveats, political scientists have frequently looked to Kuhn and Lakatos for metatheoretical guides to inquiry.[8] In his 1965 American Political Science Association (APSA) Presidential Address, David Truman welcomed the application of Kuhn's concept of paradigm as a means to help "regenerate" the discipline.[9] In the following year's APSA Presidential Address, Gabriel Almond also invoked Kuhn—but with some hesitation, admitting that social sciences may be different from the natural sciences: "At any rate, we begin with a dominant paradigm, a formation of the subject matter . . . , specifying variables, parameters, their relations and consequences."[10] This view of paradigms, drawn only loosely from Kuhn's work, is commonly cited in political science.

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Nelson Polsby, however, saw these as "ceremonial footnotes" lacking full appreciation of Kuhn's model of science.[11]

References to Kuhn and Lakatos are especially common in the subfield of International Relations (IR). Several recent works address how Kuhn or Lakatos may apply to IR.[12] Other studies have been critical of importing paradigms, research programs, and their sundry "isms" to the study of IR.[13] Few of these works, however, explore how these various models of science can shape incentives and norms for acceptable scholarly behavior. Fewer still have ventured to explain why Kuhn, and to a lesser degree Lakatos, were so reluctant to lend their ideas to the social sciences.

Reliance on Kuhn and Lakatos becomes more puzzling when we compare it to the less frequent discussions of Karl Popper's philosophy of science. The recent marginalization of Popper is surprising for two reasons. First, Popper was the first philosopher of science to be held in high esteem by the discipline. The American Political Science Association honored Popper with the Benjamin Lippincott Award for *The Open Society* in 1976. Popper joined Hannah Arendt and Louis Hartz as the first three recipients of the award "for publishing highly significant and enduring" works in political science. Second, unlike Kuhn and Lakatos, Popper sought to apply his ideas directly to the social sciences. Yet any cursory view of Ph.D. reading lists or citation indices show that Popper's philosophy of science has become overshadowed by those of Kuhn and Lakatos. Popper may have been marginalized due to a narrow reading of his work. He has frequently been mischaracterized as a logical positivist or a naive "falsificationist."[14] But his model of science encompasses much more. Popper's approach to critical problem solving, theoretical pluralism, conjecture, and refutation can be applied to both the natural and social sciences. His emphasis on indeterminacy and fallibilism—the belief that we cannot achieve absolute certainty but can still falsify wrong conjectures—is particularly fitting for many phenomena studied by political scientists.[15] Students of political science would thus benefit from bringing Popper back into discussions over models of science. While more recent studies in philosophy of science provide particular insights for the discipline, the works of Kuhn, Lakatos, and Popper still pose the most fundamental questions about organizing scientific community, research practices, and the growth of knowledge.[16] When political scientists address philosophy of science, they typically reference Kuhn, Lakatos, and occasionally Popper—or sometimes a curious combination of the three. Yet an appreciation of differences between the three is rarely recognized by students of politics.

In this essay I examine these three models of science and the ways in which they have been applied to the subfield of IR. I show how the subfield's frequent reliance on Kuhn and Lakatos is inappropriate since the field has not achieved the requisite scientific achievements. Relying on Kuhn and Lakatos without these scientific achievements unduly limits its research. The emergence of paradigm mentalities, as depicted by Kuhn and Lakatos, leads to narrow, rigid, highly specialized, and conservative research approaches that suppress alternatives. For Kuhn, evidence that falls outside the dominant framework is considered "incommensurate" and can be "ignored." For Lakatos, such evidence can be "shoved aside" if the research program is held to be progressing. When political scientists are guided by paradigm mentalities they hold tightly to both their theory and their method while seeking to insulate themselves from opposing theory and method. They also engage in hostile, zero-sum turf wars when challenged by alternatives. Paradigm mentalities prompt scholars to break into narrow, highly-specialized, esoteric research communities.

The constricting nature of normal science has been contested by many commentators. Popper's critiques were among the first and remain the most poignant. Popper argued that paradigm mentalities prove detrimental to healthy inquiry by limiting scholarly vision, curiosity, and creativity. In Popper's world, embracing theoretical and methodological diversity while engaging anomalous evidence is essential to the advancement of knowledge. I thus argue that Popper's commitments to theoretical pluralism, hermeneutics, methodological diversity, and fallibilism provide a more appropriate model than paradigm mentalities for both IR and political science more generally.

I divide this essay into four broad sections. In the first, I review the writings of Kuhn, Lakatos, and Popper, emphasizing those aspects often ignored by political scientists. Chief among these are the scientific achievements required for paradigms and the highly conservative nature of research that follows such achievements. I also highlight how, according to Kuhn and Lakatos, rigid adherence to one paradigm can be justified only by an unambiguous growth in knowledge. In the second section, I point to how the paradigm mentality in IR scholarship has impoverished inquiry by marginalizing opposing theoretical frameworks and shunning anomalous evidence. Recent debates in IR regarding the subject of balancing usefully demonstrate how paradigm mentalities may silence criticism. In the third section I examine how Kuhn's multiple definitions of paradigm have contributed to certain confusion in IR over whether a paradigm is a method, a theory, or both. In the conclusion, I reflect on how these various metatheories speak to the evolution of the "Perestroika" movement in political science.[17] I argue that this misappropriation of the models of science of Kuhn and Lakatos, especially their principle of incommensurability, encourages hyper-specialized tribalism within subfields and furthers the Balkanization of political science as a discipline.[18] Conversely, greater awareness of Popper's metatheory may foster the opposite trend, leading to more broad and vibrant research agendas that reach across the deep narrows that often divide political scientists.

The Kuhn Few Read: The Prerequisite of a Concrete Scientific Achievement

Although Kuhn's classic work, *The Structure of Scientific Revolutions,* was a history of the growth of knowledge in the natural sciences, his concepts of paradigm, normal science, incommensurability, and scientific revolution have gained a stylized currency in the social sciences. Political scientists, however, frequently pass over Kuhn's essential points concerning *universally recognized scientific achievements* and the overall productive nature of paradigms. In this brief overview, special emphasis will be paid to prerequisites of a paradigm, the conservative but largely progressive nature of normal science, and reasons why individual scientists frequently choose to work within paradigms.

Kuhn's paradigm might best be understood in terms of its life-cycle. A paradigm is born when a "concrete scientific achievement" resolves debate over foundations, assumptions, and methods in a scientific field of inquiry.[19] The concrete achievement suspends debate over fundamentals and forges a consensus among scientists. This consensus initiates a period of normal science. Kuhn stated unequivocally that paradigms and normal science can exist only when "based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation of their practice."[20] Examples of Kuhn's "universally recognized scientific achievements" include those of Newton, Lavoisier, and Einstein. Kuhnian paradigms cannot be called into being by scholarly consensus alone. Paradigm formation must be anchored by a major scientific achievement that a particular community of scientists finds convincing. From the understanding and consensus wrought by this major scientific accomplishment, Kuhn's paradigm comes to provide worldviews, theories, and methods (rules and standards for scientific practices) to a tightly-bound and highly-invested research community. Social scientists often overlook this requisite springboard for normal science. Instead, as Sanford Schram claimed, social scientists often see paradigms as something that "can be imposed socially . . . "[21] While paradigms can be socially reinforced, uncontested scientific achievements rest at the foundation of all legitimate Kuhnian paradigms.

Once forged, paradigms guide normal science in an exclusive and highly productive manner. Kuhn's normal science can be characterized by the effort to fit small pieces into a large and complex puzzle. The normal scientist's task involves an intense concentration on the small pieces, while ignoring the bigger theoretical picture:

> The areas investigated by normal science are, of course, minuscule; the enterprise now under discussion has drastically restricted vision . . . By focusing attention upon a small range of relatively esoteric problems, the paradigm forces scientists to investigate some part of nature in detail and depth that would otherwise be unimaginable.[22]

By restricting vision and breadth, a paradigm guides scientists narrowly but productively to questions linked to those already agreed-upon core concerns. For Kuhn, periods of normal science must be progressive and based not merely on a promise of answers, but "in the actualization of that promise . . . by increasing the extent of the match between those facts and the paradigm's predictions."[23] Kuhn notes how "measurements undertaken without a paradigm so seldom lead to any conclusions at all."[24] Most periods of growth are characterized by the dominance of one paradigm and the practice of normal science. For Kuhn, this constitutes "the most efficient mode of scientific practice."[25] The normal science that follows scientific achievement is characterized by slow and steady growth in knowledge.

When confronted with major anomalies, Kuhn's normal scientists simply ignore them. Kuhn argues that "no part of the aim of normal science is to call forth new sorts of phenomena; indeed those that will not fit in the box are often not seen at all."[26] By turning a blind eye to anomalies, scientists can devote their exclusive attention to efficiently solving pieces of the paradigmatic puzzle.

Given scientists' strong commitment to the maintenance of their paradigm, they are frequently hostile toward alternatives. In periods of normal science, scientists do not "invent new theories [and] they are often intolerant of those invented by others."[27] Kuhn's normal scientists rigidly adhere to their paradigm even when it is discredited by the evidence. On the strong cognitive commitment that scientists exhibit toward their paradigm, Kuhn quotes Max Planck:

> [A] new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it.[28]

Ignorance and intolerance toward other theoretical frameworks are regrettable features of Kuhn's normal science, but without these constraining features science would be less efficient.

Another important aspect of a Kuhnian paradigm is incommensurability. Since Kuhn describes normal science as "an esoteric, isolated, and largely self-contained discipline . . . its own exclusive audience and judge," anyone outside the paradigm cannot level meaningful criticism.[29] Kuhn acknowledges how the "the proponents of competing paradigms practice their trades in different worlds," resembling "members of different language-culture communities."[30] Meaningful conversations can only occur within a single paradigm. Incommensurability between alternative paradigms is reinforced by the highly specialized training in questions of "theory, methods, and standards" that produce a "strong network of commitments—conceptual, theoretical, instrumental, and methodological."[31]

While far from ideal, the rigidity of normal science is tolerated by scientists because it supposedly provides the best context for the growth of knowledge. There is a clear

tradeoff between multiple theoretical frameworks and efficient science. Since normal scientists waste no effort exploring distant and little understood alternatives, they are able to focus their efforts on immediate questions spawned by the dominant paradigm. Normal science, according to Kuhn, leads to "an immense restriction of the scientist's vision and to a considerable resistance to paradigm change . . . On the other hand, within those areas to which the paradigm directs the attention of the group, normal science leads to a detail of information and to a precision of the observation-theory match that could be achieved in no other way."[32] Normal science is a "highly cumulative enterprise, eminently successful in its aim, the steady extension of the scope and precision of scientific knowledge."[33] In sum, while normal science is narrow, rigid, esoteric, uncritical, and conservative, Kuhn considers it to be the most efficient way to ensure a cumulation of knowledge. The allure of paradigms and normal science rests on the proficiency to produce new knowledge—even if this knowledge is gained in small, incremental, and almost unremarkable installments.

While scientists typically remain loyal to the dominant theoretical framework, paradigms do not endure forever. If a paradigm repeatedly fails to provide stable periods of growth and if anomalies mount, a scientific revolution will begin. Here, Kuhn notes that "scientists begin to behave differently" and are no longer "bound" to pursue questions central to the paradigm. This revolutionary movement is led by scientists "very young or very new to the field."[34] Opponents to the paradigm emerge and come to challenge the orthodoxy of the field. Kuhn is optimistic that in the end the most progressive paradigm—the one best fitting nature—will emerge victorious from the revolution. Kuhn claims that "the judgment leading to that decision [of which paradigm will dominate] involves the comparison of both paradigms with nature and with each other."[35] Revolutions typically end with the emergence of a new paradigm based on concrete scientific achievements that bring forth a new period of conservative but progressive normal science.

This paradigm mentality based on normal science and incommensurability has been widely employed, if not internalized, by political scientists.[36] Political scientists, however, tend to conveniently ignore Kuhn's demanding prerequisites of paradigms—chiefly the concrete scientific achievement and the subsequent growth of knowledge. A similar neglect can be noted in how political scientists read Lakatos. This may be more pertinent since, as Terence Ball has noted, Lakatos may well be a more fitting philosopher-guide for political scientists.[37]

Lakatos: Compatriot of Kuhn

Lakatos has gained a great deal of recent attention in political science and IR. Jon DiCicco and Jack Levy have claimed that for IR scholars "Lakatos is probably now the metatheorist of choice."[38] One recent edited volume draws together many leading scholars in an effort to systematically apply Lakatos' notions of scientific research programs to IR.[39] Although Lakatos is often cited for seeking a middle ground between Popper and Kuhn, his conception of scientific research programs closely resembles Kuhn's notion of paradigms. Lakatos admitted, "Where Kuhn sees *paradigms,* I also see rational *research programmes.*"[40] In this brief review, I will stress the similarities between Kuhn and Lakatos in their commitments to a paradigm mentality. These similarities include the shared faith that science progresses most efficiently when anchored by one exclusive theoretical framework, the comparable treatment of anomalies, and the diminished role of alternative theoretical frameworks. I will begin, however, by dispelling one of the frequently cited disagreements between Lakatos and Kuhn: the charge that Kuhn's paradigms emerge in an irrational manner.

Lakatos unfairly criticized Kuhn for the absence of rational standards to justify a paradigm's emergence. In two poignant passages, Lakatos claimed that for Kuhn "each paradigm contains its own standards . . . is irrational, a matter of mob psychology" and that "scientific change—from one paradigm to another—is a mystical conversion which is not and cannot be governed by rules of reason and which falls totally within the realm of the (social) psychology of discovery. Scientific change is a kind of religious change."[41] In IR, many cite Lakatos' strong commitment to the rationality of science as his most important departure from Kuhn.[42]

Kuhn, however, consistently denied this charge of the "irrationality" of paradigms.[43] Kuhn grounds his commitment to paradigms on "universally recognized scientific achievements" and not on any mystical awakenings or mob pressures. This is frequently overlooked. A few passages from Kuhn will help dispel the misconception:

> Paradigms gain their status because they are more successful than their competitors in solving a few problems that the group of practitioners has come to recognize as acute.[44]
>
> But if a paradigm is ever to triumph it must gain some first supporters, men who will develop it to the point where hard-headed arguments can be produced and multiplied . . . Because scientists are reasonable men, one or another argument will ultimately persuade many of them.[45]
>
> Even when a new candidate for a paradigm has been evoked, scientists will be reluctant to embrace it unless convinced that two all important conditions are being met. First, the new candidate must seem to resolve some outstanding and generally recognized problem that can be met in no other way. Second, the new paradigm must promise to preserve a relatively large part of the concrete problem-solving ability that has accrued to science through its predecessors. . . . Paradigms usually preserve a great deal of the most concrete parts of past achievement and they always permit additional problem-solving solutions besides.[46]

For Kuhn, paradigms appear in the wake of a "universally recognized scientific achievement"[47] and endure as a

result of their ability to successfully fit together pieces of an incomplete puzzle.[48] And they are ultimately replaced only when a competing paradigm "fits the facts better."[49] While Kuhn and Lakatos may differ on how to assess research progress, they basically agree on the rationality of paradigm emergence, even if Lakatos fails to acknowledge this agreement.

Turning to a closer examination of Lakatos' own metatheory, we are struck by other similarities between his scientific research programs and Kuhn's paradigms. Both value scientific inquiry guided by one progressive but conservative and monolithic theoretical framework. Lakatos contends that "the *dogmatism* of *normal science* does not prevent growth."[50] Just as Kuhn's scientists are bound to their paradigm, for Lakatos "the scientist's attention is riveted on building his models following instructions which are laid down in the positive part of his programme."[51] Like Kuhn, Lakatos requires a *concrete, novel,* or *dramatic discovery* that forges a consensus among the scientific élite. Lakatos emphasizes how "great achievements . . . not isolated hypotheses" serve as the bedrock for a scientific research program.[52] For Lakatos, "a theory is scientific (or acceptable) if it has an empirical basis" that has been corroborated.[53] Along with an empirical basis, he stresses the importance of "novel, stunning, or dramatic" discoveries: "My favorite examples . . . [of novel facts] were the return of Halley's comet, the discovery of Neptune, the Einsteinian bending of light rays, the Davisson-Germer experiment."[54] These novel facts help constitute the irrefutable hardcore of the research program. For Lakatos, every research program "has a tenacious *hardcore,* like the three laws of motion and the law of gravitation in Newton's research programme."[55] Again like Kuhn's concrete scientific achievement, Lakatos' hard core must be "generally accepted as true by the scientific élite (professional scientists) after simple controlled experiments."[56] While all research programs cannot claim dramatic novel facts, Lakatos requires research programs to have some important discovery that unifies and guides scientific practices.

Like Kuhn's paradigms, Lakatos' scientific research programs are based not only on stunning discoveries but also on the progress that comes in their wake. To a greater degree than Kuhn, however, Lakatos focuses on evaluating the progressive nature of a scientific community. Scientific research programs, for Lakatos, must also remain theoretically and empirically progressive:

> A research programme is either progressive or degenerating. It is *theoretically progressive* if each modification leads to new unexpected predictions and it is *empirically progressive* if at least some of these novel predictions are corroborated (emphasis in the original).[57]

Kuhn devotes relatively little attention to measuring a paradigm's progress in a rational manner. Lakatos, however, introduces ways of evaluating a research program's progressive nature, and this remains one of his most important contributions. Though difficult to sort out, his discussions of positive heuristics and progressive problemshifts help fill a large void left by Kuhn's limited depiction of progress in normal science.

Lakatos' treatment of anomalies varies only slightly from Kuhn. While Kuhn's scientists will largely ignore anomalies, Lakatos sees anomalies as things to be "listed but shoved aside in the hope that they will turn, in due course, into corroborations of the programme."[58] Lakatos' positive heuristic of a research program depends upon progressive "verifications." Drawing a clear distinction between himself and falsificationists like Popper, Lakatos claimed that "it is the verifications rather than the refutations which provide the contact points with reality. . . . It is the verifications that keep the programme going." A verification is a "corroboration of excess content in the expanding programme."[59] Lakatos later noted that "refutations always abound. What matters is a few dramatic signs of empirical progress."[60] Anomalies and refutations matter very little as long as progress can be clearly discerned.

Faced with the prospects of having hard-earned refutations shoved aside, some scientists will stretch anomalous findings to fit the dominant research program. Lakatos refers to this process as one of "grafting," whereby inconsistent branches are temporarily allowed to grow from the core of the research program. Lakatos claims that "some of the most important research programmes in the history of science were grafted on to older programmes with which they were blatantly inconsistent." This grafting can be progressive in the short run, but such inconsistencies cannot remain for long.

> As the young grafted programme strengthens, the peaceful co-existence comes to an end, the symbiosis becomes competitive and the champions of the new programme try to replace the old programme altogether.[61]

For Lakatos, any period of "peaceful coexistence" between rival research programs will be rare and short-lived. Ultimately one theoretical framework will come to dominate scientific endeavors until it is eliminated by a rival. For both Kuhn and Lakatos, periods of theoretical pluralism will be very brief.

While Colin Elman and Miriam Elman emphasize Lakatos' theoretical tolerance,[62] his work is frustratingly inconsistent on this point. In a few relatively rare passages, Lakatos notes how his methodology allows "the freedom ('anarchy' if Feyerabend prefers the word) in creation and over which programme to work on but the products have to be judged."[63] Lakatos also cautiously advocates a degree of protection for "budding research programmes." These alternatives "should be sheltered for a while from a powerful established rival." Reiterating the importance of an empirical basis for all research programs, Lakatos recommends that scientists should exercise caution during this

"sheltered period" until the budding research program "starts producing 'genuine novel' facts to recognize its truly scientific (or empirical) character." Until it has a clear "empirical or scientific foundation," these emerging alternatives may be thought of as "prescientific (or theoretical)."[64] However, Lakatos later cautions how a "new, promising" research program that "contains no significant corroboration . . . is degenerating."[65] For Lakatos, such *juvenile* research programs can become as "degenerating" as outdated *senile* programs.

There is, however, good reason to doubt Lakatos' commitment to theoretical pluralism. First, he endorsed paradigm mentalities in his "practical advice" to scientists. When asked whether a scientist might devote attention to a new and promising research program, Lakatos remains unequivocal in his conservatism:

> My practical advice is: one should act in any field according to the most 'trustworthy' or most 'reliable' theories in the given field. We construct the body of 'most reliable' knowledge *from the body of scientific knowledge.*[66]

Scientists, as depicted by Lakatos, will rarely stray from the dominant framework. And if they do so, they risk having their findings "shoved aside."

A second reason to doubt Lakatos' tolerance is found in his conception of the relationship between opposing scientific research programs. Like Kuhn, Lakatos leaves little room for theoretical pluralism. Instead, he employs a winner-take-all leitmotif to describe the relationship between rival research programs:

> If a research programme progressively explains more than a rival, it 'supersedes' it, and the rival can be *eliminated*. . . . [No single finding] can *defeat* a research programme *in one blow*. . . . [Scientists] must realise that one's opponent, even if lagging badly behind, may still stage a comeback. . . . There is a hidden *war of attrition* between two research programmes (emphasis added).[67]

Given the heat of such "wars" involving scientific research programs, the reading of Lakatos as a tolerant promoter of theoretical pluralism is difficult to sustain.

The extent of Lakatos' stringent conservatism is most apparent in his criteria of subsumption. The concept of subsumption—popularized by Paul Feyerabend in his lively attacks on the paradigm mentalities made fashionable by Kuhn and Lakatos—conveys how the upstart research program must account for *all* the empirical content of its established rival.[68] In his famous comparison of T to T^1, Lakatos employed the "criteria of subsumption," whereby a rival research program cannot meaningfully arrive until it has explained—or subsumed—*everything* that its predecessor has explained—and more.

> A scientific theory T is falsified if and only if another theory T^1 has been proposed with the following characteristics: (1) T^1 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^1 explains the previous success of T, that is, all the unrefuted content of T is included (within the limits of observational error) in the content of T^1; and (3) some of the excess content of T^1 is corroborated.[69]

By combining this criteria of subsumption with a limited tolerance for alternative theoretical frameworks (i.e., budding research programs), Lakatos helps to ensure that science be guided conservatively by one dominant framework.

For both Kuhn and Lakatos, contentions between opposing theoretical frameworks are not unlike turf wars where the winners dismiss all opposition. The dominant framework defines the turf and determines which questions will be taken seriously. To be taken seriously, challengers must subsume the turf by either a Kuhnian revolution or a Lakatosian triumph of T^1. If a new framework does not subsume an older framework, it is viewed as inferior and unworthy of attention—unless it is in one of those rare periods that Kuhn calls revolutionary or "extraordinary science." Paradigm mentalities discourage intellectual exchange between competing theoretical frameworks.

To conclude, while some important distinctions separate Lakatos from Kuhn, theoretical tolerance is not one of these. Both allude to the inefficiencies and hazards that scientists face in periods of theoretical pluralism. To varying degrees, both stress that the efficient growth of scientific knowledge occurs when one dominant, monopolistic paradigm or research program guides inquiry. Both models of science escalate the stakes and intensity of paradigm wars. The possibility of losing—and having one's work ignored or shoved aside—produces the heated recriminations and unyielding defensiveness to be expected by any group of scholars facing subsumption. In the end, rather than engaging in these "wars of attrition between research programs," researchers may round up their like-minded colleagues and retreat to a table of their own, to borrow Almond's reference to sects in political science.[70]

Popper's Open Society and Theoretical Pluralism

Unlike the stark distinctions that Kuhn and Lakatos draw between natural and social science, Popper emphasizes a "unity of method."[71] His ideas of critical problem-solving by trial and error (falsification) as well as his emphasis on multiple theories and methods are equally pertinent to the social and natural sciences. Popper's aspirations for an open society of thinkers diverge from the restrictions imposed by paradigm mentalities. Key differences rest on the importance Popper places on theoretical and methodological pluralism, vigilant criticism, the embracing rather than shunning of anomalies, and fallibilism—the recognition that even our most corroborated theories may be soon and surprisingly overturned. By acknowledging the tentative nature of our theories and our knowledge base, scientists will not be so invested in defending paradigmatic turf. Instead, they would be encouraged to follow

up anomalies and would strain to falsify existing theories. For Popper, refutations rather than the small increments of normal science are marks of progress. While Kuhn was initially criticizing Popper's model of science, Popper responded with severe criticisms of Kuhn's ideas of normal science and the incommensurability between opposing theories.

Popper's open society calls for competition between various theories, not the hegemonic reign of one paradigm or research program. While Popper acknowledged that scientists may sometimes work within the confines of one dominant theoretical framework, he challenged Kuhn's claim that normal science is the most efficient way to advance knowledge. Popper argued that great science perishes in a paradigm.[72] Popper painted a pitiful portrait of those laboring within the confines of normal science:

> [Kuhn's scientist is one who] accepts the ruling dogma of the day; who does not wish to challenge it; who accepts a new revolutionary theory only if almost everyone else is ready to accept it—if it becomes fashionable. . . . In my view the 'normal' scientist, as Kuhn describes him, is a person one ought to be sorry for.[73]

In an essay addressing the social sciences, Popper lamented how young scientists short on critical thinking and creativity are far too "eager to pick up the latest fashion and the latest jargon. These 'normal' scientists want a framework, a routine, a common and exclusive language of their trade." Popper concluded that "it is the non-normal scientist, the daring scientist, the critical scientist, who breaks through the barrier of normality, who opens the windows and lets in fresh air, who does not think about the impression he makes, but tries to be well understood."[74] Keith Webb was one of the few to emphasize how Popper's ideal scientist is a free-wheeling, independent intellect who will critically explore both anomalies and accepted evidence alike in search of better answers.[75] Agreement and orthodoxy imposed by a paradigm mentality, according to Popper, constitutes "the death of knowledge, since the growth of knowledge depends entirely on the existence of disagreement."[76] Embracing these disagreements over fundamentals presupposes theoretical and methodological pluralism, if not versatility.

Pluralism can be assured through creative speculations that defy convention. In this light, Popper challenges the view that "metaphysics has no value for empirical science."[77] Popper points out that "scientific discovery is impossible without faith in ideas which are of a purely speculative kind, and sometimes even quite hazy . . . a faith which is completely unwarranted from the point of view of science, and which, to that extent, is 'metaphysical'." Since "metaphysical ideas are often the forerunners of scientific ones," Popper regards "intuition and imagination as immensely important."[78] Ian Hacking notes that in "Popper's opinion it is not all that bad to be pre-scientifically metaphysical, for unfalsifiable metaphysics is often the speculative parent of falsifiable science."[79] James Farr draws in part from these aspects of the metaphysical to develop the idea of "Popper's hermeneutics."[80] However, to avoid being misled by these intuitions, we must remain critical, open to alternatives, and creative when devising appropriate tests. For Popper, theoretical speculations are not to be dismissed; just as corroborated theories are not to be uncritically accepted. Both speculative and established theories remain open to critical examination. In Popper's view, no Lakatosian "hardcore" can be protected from scrutiny and criticism and no Kuhnian "concrete scientific achievement" should distract from alternatives.

In the social sciences, theoretical pluralism is essential because select observations and evidence may be derived exclusively from one's preferred theory and then used to further establish the favored theory. In his critique of historicism, Popper argues that the historicist "firmly believes in his favourite trend, and conditions under which it would disappear are to him unthinkable. The poverty of historicism, we might say, is a poverty of imagination."[81] The best way to encourage creativity and imagination is to keep several competing theories on the table. For Popper, historians and social scientists must "keep the flow of ideas running from all tributaries . . . and especially from lay tributaries."[82] Conversely, paradigm mentalities along the lines discussed by Kuhn and Lakatos will reduce the flow of ideas to one narrow but steady stream.

Paradigm mentalities rely on notions of incommensurability to limit theoretical pluralism. Incommensurability is perpetuated by what Popper calls the "myth of the framework," the idea that "a rational and fruitful discussion is impossible unless the participants share a common framework of basic assumptions or, at least, unless they have agreed on such a framework for the purposes of discussion." Popper views this as a "dangerous exaggeration" and counters:

> A discussion between people who share many views is unlikely to be fruitful, even though it may be pleasant; while a discussion between vastly different frameworks can be extremely fruitful, even though it may sometimes be extremely difficult, and perhaps not quite so pleasant (though we may learn to enjoy it).[83]

Contrary to Kuhn and Lakatos, Popper argues that single scientific theories rarely, if ever, dominate without opposition: "There was, ever since antiquity, constant and fruitful discussion between the competing and dominant theories of matter."[84] Disagreement between even the most dissimilar theoretical frameworks is not only possible but essential for important scientific discoveries. This myth of a dominant paradigm and incommensurability can justify turning a blind eye to alternative explanations.

Popper's dim view of paradigm mentalities reflects his two most vital principles for sound inquiry: avoiding narrow specialization and maintaining a highly critical

approach to what is supposedly known. Popper repeatedly argues that the scientist must "shun the danger of narrow specialization . . . [and] help others to understand his field and his work."[85] The esoteric and highly specialized nature of paradigms demands a narrow and rigid training that can prove detrimental to inquiry. Perhaps the most dangerous aspect of normal science lurks in its tendency to suppress, if not castigate, critical thinking. For Popper, "criticism is the engine of the growth of knowledge."[86] Devotion to one framework dampens the critical spirit on which all sound intellectual practices depend.

Finally, theoretical and methodological pluralism are essential for Popper's open society, where a variety of conjectures are critically examined and then tested. The results of these tests are then compared to assess their relative accuracy. Falsification allows scientists to relegate those theories that appear to be inaccurate and elevate those that are better supported by empirical testing. However, Popper's notion of fallibalism reminds us that even the most sound and supported theory risks being overturned. As a result, humility combined with critical awareness of alternatives provides rules for all healthy inquiry. Conversely, incommensurability and intense specialization will discourage criticism. This, in turn, may jeopardize the open society that he seeks. When scientists invest so heavily in one framework, they will seek to reinforce that structure rather than remain critical of it. This will lead to bitter paradigm wars and the revolutions that Kuhn described. In Popper's world of multiple theories, such wars, if they occurred at all, would be far less hostile and far more intellectually rewarding.

Scientists, Kuhn's Promise of Progress, and the Implicit Contract

Understanding how these models shape the choices made by individual scientists is vital to understanding scientific practice.[87] Why would any scientist choose to work within the constraints of Kuhn's normal science? Wouldn't most prefer to be independent agents shunning narrow specialization and work across a myriad of theoretical and methodological fronts? Kuhn's scientists typically accept the confines of the dominant framework in exchange for small but steady increases in knowledge. To do otherwise is to risk isolation and obscurity, if not hostility. Loyalty to the paradigm will be rewarded with the gradual cumulation of knowledge. This implicit contract is one where scientists trade autonomy and theoretical breadth for a degree of security and narrow advances in knowledge. This promise of cumulation—and its realization—drive the conservative but progressive elements that characterize Kuhn's paradigms and Lakatos' scientific research programs.

This approach has yielded a great deal of steady progress in physics, medicine, and other realms where scientists engage in narrow puzzle-solving founded on concrete scientific achievements. The social goods gleaned from adhering to one theoretical framework are clear: Diseases are cured, bridges are built, and the public benefits. Since individual scientists can identify their efforts with the progressive nature of their research program, they can justify the conservative, normal science approach.

Many scientists may choose the comfort found in convention and community provided by the dominant theoretical framework. These are the sociological elements of Kuhn's paradigms. In sum, scientists may forego many interesting but risky questions that fall outside this framework, so long as their efforts continue to be rewarded by incremental advances. This risk-aversion by scientists is consistent with prospect theory, where individuals pursue lower payoffs that are certain rather than great payoffs that are less certain.[88]

For Kuhn and Lakatos, this implicit contract serves as a constitutional dictate that regulates scientific practice by issuing rewards and punishments in the form of acknowledgements or obscurity. This constitutional dictate is forged by some stunning scientific achievement. Without this scientific achievement, there is no consensus among scientists, are no paradigms, no scientific research programs, and is little chance of science progressing in the orderly and incremental fashion promised by disciplined inquiry as laid out by Kuhn and by Lakatos.

The stringent demands of consensus-forging achievements may be the reason why both Kuhn and Lakatos remain skeptical of their relevance to the social sciences. The paucity of universally recognized achievements in both political science and IR raises at least one important question. Why, in the absence of any stunning scientific achievements, would disciplines like political science seek to apply these constrictive models of science in the first place? In other words, why submit to the blinders of normal science without the promised cumulative growth in knowledge? Some argue that applying Kuhn helps lend legitimacy and status of science to the academic pursuits in political science. But as Kuhn noted, such efforts in the absence of a scientific achievement are misguided at best, and fraudulent at worst. Aside from the questions of motives, a more important question arises over the consequences of adopting a Kuhnian model in disciplines like IR. In the next sections I will explore how adhering to a paradigm mentality in IR has failed to bring much progress. Instead, on several fronts, the misapplications of these models of science may have hindered progress.

Realism's Dominance and the Decline of Theoretical Vision in International Relations

References to Kuhn's philosophy of science began during a period of realist dominance in IR. If there has been a

dominant paradigm, political realism is it. With its emphasis on states as unitary actors interacting in a potentially hostile anarchic system, its stress on power, and its clean distinction between international and domestic politics, the realist paradigm has informed the vast majority of hypothesis testing in IR over the past half-century.[89] John Vasquez refers to Hans Morgenthau's *Politics Among Nations,* first published in 1948, as the Kuhnian exemplar for the realist paradigm.[90] Kenneth Waltz's *Theory of International Politics,* however, garners the most numerous references as the founding work of the neorealist scientific research program.[91] After frequent references to Lakatos and Kuhn, Waltz proclaims that neorealism is the only theory within IR that would be "recognized as theory by philosophers of science."[92] Many followed this line of reasoning and now treat realism as the dominant and exclusive paradigm in IR.

While realism may putatively be the dominant paradigm, it fails to meet any of the criteria set out by Kuhn, with the exception of its closed and constraining affect. First, realism fails to demonstrate any concrete scientific achievements or novel facts essential to Kuhn's model. No realist work approaches what Newton did for physics, Lavoisier for chemistry, nor Lyell for geology, to cite a few examples of consensus-forging works noted by both Kuhn and Lakatos. Such a bar may, admittedly, be too high for any research in the social sciences. However, realism has also failed to register the progress that Kuhn and Lakatos predict will come by adhering loyally to one paradigm or research tradition. This lack of progress is even noted by the most committed champions of realism. Robert Gilpin, for instance, questions "whether or not twentieth-century students of international relations know anything that Thucydides and his fifth-century compatriots did not know about the behavior of states."[93] Tossing aside the criteria of progress emphasized by Lakatos, Waltz admits that while "structural theory has at least helped focus people's minds on the theoretical problem . . . I don't think there's much increase in understanding. . . . I guess I think there's been very little progress."[94] These are hardly ringing endorsements of a progressive scientific research program. However favorably one would like to evaluate the accomplishments of political realism in IR, the framework falls well short of the progressive criteria that Kuhn required of paradigms or Lakatos required of research programs. And without progress, no framework can maintain its privileged status as a research program or paradigm. Realists like Waltz cannot have it both ways. They cannot claim a privileged status—or protective belt—of their research program *and* admit to the absence of progress. The rationale for a protective belt is to insulate a *progressive* research program from the distractions posed by outside critics.

Relying largely on the logic of Kuhn, realism became ensconced as the dominant paradigm for IR. As a paradigm, realism restricted the vision of IR scholars as Kuhn might have predicted. Kenneth Boulding draws a direct connection between the rise of Kuhn's influence in the field and a narrow conservatism in IR research, observing that in the late 1960s the field consolidated into "what Kuhn calls normal science." In this period, few "new ideas or new lines of development" emerged and, according to Boulding, "the quality of the research has suffered severely as a result."[95] Kal Holsti notes a similar decline in alternative theorizing in the late 1960s: "Writings about universal collective security, ways of improving international judicial institutions, peacekeeping, or other means of controlling violence continue to appear but they do not loom so important in the field as they once did."[96] The dominance of realism in this period has been well documented. Vasquez demonstrates that more than 90% of the hypotheses tested in the 1960s to the 1970s were informed by realist concerns.[97] As a result of realism's dominance, theoretical alternatives were largely marginalized, if not entirely ignored.

Examples of Conveniently Ignoring Anomalies and Criticism

Scholars working within a paradigm or research program concentrate on those variables fitting neatly inside the box. They will often look askance at findings outside the dominant framework. This practice of ignoring anomalies and refutations has become common in IR. The "democratic peace" thesis provides one example of a strand of thinking about which IR was slow to acknowledge important findings that did not fit within the dominant realist framework. Even though claims of the democratic peace can be traced back to Enlightenment figures such as Immanuel Kant and Thomas Paine, the possibility that democracy and democratic structures might alter a state's foreign policy falls outside the realist paradigm, which regards domestic political arrangements as irrelevant.[98] Dean Babst's original study of peace between democratic states was not even reported in an IR journal. Instead, Babst documented the apparent relationship between democracy and peace in two journals far from the realist mainstream—*Wisconsin Sociologist* and *Industrial Relations.*[99] Little is known about Babst by IR scholars, and his pioneering studies have been largely ignored. While David Singer and Melvin Small challenged Babst's findings in 1976, few others engaged the question for almost two decades.[100]

After languishing in obscurity for decades, the democratic peace thesis is now widely considered to be "as close as anything we have to an empirical law in international relations."[101] Still, many realists find it difficult to acknowledge this finding, as Kuhn would have predicted. Waltz, for instance, continues to argue that the regime type matters very little because the "functions of states are similar, and distinctions among them arise principally from their

varied capabilities."[102] For realists, a state's relative power—measured in terms of material capabilities—is what matters, not its domestic political institutions. In an effort to discredit the democratic peace thesis, Waltz resorts to a strained view of history. He first claims that the War of 1812 contradicts the thesis. Waltz argues that "the two most democratic countries in the world—there were only two—*fought a war*"(emphasis in original). He also claims that the American Civil War would have constituted another anomaly of the democratic peace, especially if Britain had recognized the Confederacy—thus making it an inter-state rather than a civil war.[103]

These efforts to cast doubt on the democratic peace thesis reflect the problems with paradigm mentalities. In both cases, Waltz ignores important facts that contradict expectations of his paradigm. In the first case, it is especially difficult to refer to Britain as a democracy before the Reform Acts of the 1830s and 1860s. Before these reforms, less than 5% of the British populace could vote. Hence, the War of 1812 was not a war between democracies. In the case of the American Civil War, similar problems arise. In the Confederacy, the most basic rights were denied to slaves who numbered almost 30% of the population. To consider the slave-owning Confederacy a democratic state is curious at best. As a leading realist, Waltz's effort to deny the democratic peace in such a strained manner exemplifies how Kuhn described scientists working within the confines of one paradigm.[104]

The debate over the balance of power, initiated by the historian Paul Schroeder, also exemplifies realists' attempts to dismiss evidence contrary to their paradigm's expectations. Although the controversy resulted in a widely discussed debate in the *American Political Science Review,* its origins may be less well known.[105] Schroeder published a massive study entitled *The Transformation of European Politics, 1763–1848.*[106] As part of a special issue of *The International History Review* devoted to *Transformation,* Jack Levy, a political scientist, celebrated Schroeder's work as one that "provides a superb illustration of how an emphasis on the interplay of theory and history can enhance our explanations of international politics."[107] In a subsequent article, Schroeder turned exclusively to the prevalence of the balance of power in European history from the Treaty of Westphalia to World War II. Contrary to core realist explanations, Schroeder demonstrated how balancing power is historically rare. Instead, states often ally with the emerging power. Other times, they seek to solve the problem and end the threat, according to Schroeder, by "transcending, i.e., attempting to surmount international anarchy and go beyond the normal limits of conflictual politics: through some institutional arrangement involving an international consensus or formal agreement on norms, rules, and procedures for these purposes."[108] Sometimes states "hide" by distancing themselves from the game of power politics. But balancing, according to Schroeder, was an infrequent pursuit during these 300 years of European history—long considered the golden age of the balance of power by realists.

Schroeder's historical evidence contradicts Waltz's unequivocal prediction that in anarchic systems, states will seek balances of power through alliance-making, military buildups, or both.[109] Schroeder concludes that the realist view of "the unchanging, repetitive nature of balance-of-power politics and outcomes throughout the ages, may make its theory on international politics simple, parsimonious, and elegant; they also make it, for the historian at least, unhistorical, unstable, and wrong."[110]

Such a direct attack on the dominant paradigm was met with the expected hostility. One pithy response to Schroeder's critique of balancing came from Waltz. Waltz disparaged Schroeder's historical claims as "a melange of irrelevant diplomatic lore." Making no effort to engage Schroeder's evidence, Waltz instead retreated to the privileged conservatism afforded by a Lakatosian model of science. Waltz dismissed Schroeder's claims because they lack a theory that subsumes realism: "It is because falsification is untenable that Lakatos proposes that we evaluate theories by the fruitfulness of their research programs. Ultimately he [Lakatos] concludes that a theory is overthrown only by a better theory."[111]

Waltz's response highlights several problems with employing Lakatos in International Relations. First, Waltz assumes (rather than demonstrates) that neorealism has legitimately earned its status as a scientific research program by exhibiting some novel facts. If this were indeed the case, a devotee to Lakatos might justifiably "shove aside" anomalies, as Waltz attempted. But realism has nothing approaching Lakatos' novel facts, so "shoving aside" anomalous evidence is unacceptable by Lakatos' standards. Second, by imposing Lakatos' criteria of subsumption, Waltz endorses the idea that science progresses most efficiently when guided by one overarching theoretical framework. Any criticism of a theory becomes pertinent only when it is packaged with a new research program that can subsume its standing rival. This tall order minimizes critical thinking. Finally, Waltz conveniently skirts the issue of "fruitfulness" or progress of his neorealist research program. As noted above, Waltz confides that with neorealism "there's been very little progress."[112] So Waltz's effort to dismiss Schroeder's evidence on these grounds is puzzling and inconsistent with Lakatos' criteria of progress. By ignoring strong anomalous evidence, Waltz's critique of Schroeder exposes the hazards of relying on paradigm mentalities to guide the study of IR.

In a second critique, Colin and Miriam Elman criticize Schroeder for failing to demonstrate a full appreciation of the "neorealist research program." In a classic example of stretching the domain of realism to incorporate nearly all state behavior, the Elmans charge that Schroeder "underestimates the extent to which his rendition of the historical

record is in fact consistent with a neorealist reading of international politics."[113] Much like Lakatos' process where one research program is "grafted" onto the dominant but inconsistent program, the Elmans seek to incorporate Schroeder's discordant evidence by grafting power transition and "peaceful accommodation, internal balancing, alliance formation, or preventative war," as well as other behaviors, onto the neorealist research program.[114] Indeed, with this long list of so-called neorealist behaviors, it is hard to imagine many things transpiring in the international system that run contrary to a realist explanation—even if they run contrary to one another. In a short reply, Schroeder notes how this critique "appropriates every possible tenable position in International Relations theory and history for the neo-realist camp. . . . They succeed, in fact, in rendering neo-realist theory immune to empirical falsification."[115] Popper would view this as a cardinal sin. Even for Lakatos, such grafting is one way of dealing with anomalies to *an established research program,* at least in the short term.

Rather than addressing the historical evidence or the differing conditions which may challenge Shroeder's claims, the Elmans retreat into the hard core—and protection—of a Lakatosian scientific research program. Like Waltz, they conclude their critique on a conservative note by citing Lakatos and calling forth the criteria of subsumption:

> Only better theories can displace theories, but we have yet to construct a competing research program that can account for both new facts and anomalies as well as past patterns of state behavior. Thus, Waltz's [neorealist] theory should not be discarded until something better comes along to replace it.[116]

By these criteria, Popper's idea of theoretical pluralism becomes unlikely, if not impossible.

Strained efforts to deflect criticism are common practices within paradigm mentalities. Heavy investment in one exclusive paradigm leads scholars to hold tight to that paradigm. Academic turf will be fiercely defended and rival explanations will be either ignored or attacked. This defensiveness, according to Popper, results in "the wrong view of science [which] betrays itself in the craving to be right."[117] Realist responses to Schroeder demonstrate this craving and all of its deficiencies.

In the end, the critiques by Waltz and the Elmans functioned just as Kuhn predicted—to silence criticism from outside the paradigm. In his rebuttal to the Elmans, Schroeder concludes that he "will not discuss neo-realist theory further, at least not in this journal. . . . I will devote myself from now on to the history of international politics, and leave neo-realists to deal with the results, or ignore them, as they see fit."[118] If the tenets of normal science stand—and the nature of these critiques indicate that they will—neorealists will continue to ignore any evidence that does not fit into their paradigmatic box. And their "paradigm" will become that much more narrow, self-contained, and divorced from the complexities of world politics.

As a result of realism's dominance, many scholars take on a cautious tone when presenting alternatives. In his edited volume challenging realism, Peter Katzenstein begins by acknowledging the challenges posed by paradigm mentalities. Katzenstein regrets that "scholars tend to shy away from conversations that pose fundamental disagreements, preferring instead to live in the comfortable cocoon of the like-minded." Then Katzenstein transitions to a Popperian exhortation: "Science is a social process that develops, refines, and rejects ideas. It is not a football game in which players protect turf—intellectual or otherwise."[119] While paradigm mentalities still reinforce "cocoons of the like-minded," awareness of their intellectual costs is growing. I will now turn to how paradigm mentalities can divide and fracture research communities.

The Balkanization of the Discipline: Paradigms as Both Theories and Methods

Paradigm mentalities manifest themselves both in broad theoretical frameworks like realism and in more methodologically focused communities like game theory. This proliferation of paradigms may have been aided by Kuhn's own multi-faceted definition of the term. Margaret Masterman counted more than twenty different ways Kuhn defines a paradigm.[120] One particularly troubling aspect of Kuhn's varied definitions—especially for IR—is his tendency to conflate both methods and theory as part of the communal aspects of a paradigm. Kuhn frequently defines a paradigm as a method/technique and as a worldview binding researchers to a particular theoretical tradition. No natural history, according to Kuhn, "can be interpreted in the absence of at least some implicit body of intertwined theoretical and methodological belief that permits selection, evaluation, and criticism." Kuhn also adds that when "learning a paradigm the scientist acquires theory, methods, and standards together, usually in an inextricable mixture."[121]

Kuhn's failure to distinguish methodological from theoretical aspects of a paradigm is especially unfortunate given the influence Ludwik Fleck had on Kuhn's early thinking. In the foreword to the first English translation of Fleck, Kuhn acknowledges Fleck's influence when forming the concept of a paradigm.[122] In *Genesis and Development of a Scientific Fact,* first published in 1935, Fleck differentiates the theoretical from the scientific-methodological aspects of research communities by introducing concepts of "thought collectives" and "thought styles." Thought collectives occur in any community of like-minded individuals. Fleck defined a thought collective "as a community of persons mutually exchanging ideas or maintaining intellectual interaction. . . . It also provides the special carrier

for the historical development of any field of thought."[123] Fleck stresses how a thought collective, like a paradigm, promotes exclusive training that helps unite a community of scholars: "The more specialized a thought collective is and the more restricted in its content, the stronger will be the particular thought nexus among members."[124] Fleck recognizes the dangers of how specialization and like-mindedness will "help ensure harmony within the system. . . . [It] will also preserve the harmony of illusions." This harmony of illusions results from the thought collective's "laborious efforts [that] are made to explain an exception in terms that do not contradict the system."[125] Fleck's thought collective, however, encompasses only the theoretical and communal dimensions of Kuhn's paradigm.

In his discussion of intellectual communities, Fleck makes a crucial point that can be used to distinguish *Esperanto* enthusiasts from physicists. The difference rests in their respective "thought styles," or the accepted *methods* and norms of transmitting and legitimizing knowledge throughout the community. Fleck identified the thought style of modern science as one with a "common reverence for an ideal—the ideal of objective truth, clarity, and accuracy."[126] Fleck's scientific thought styles are analytically distinct from his thought collectives. Thought collectives *can be* formed among any like-minded group. Thought styles, however, demand an adherence to transparent, falsifiable scientific methodologies. Fleck's valuable distinction between theory and methods in research communities was obscured by Kuhn. As a result, Kuhn's paradigms take on multiple meanings and this multiplicity has led to a proliferation of self-contained paradigmatic communities in IR.

Confusion persists over paradigm-based-on-theory and paradigm-based-on-method. In an essay inspired by Kuhn, Arend Lijphart argues that IR in the late 1960s experienced a Kuhnian scientific revolution. Lijphart's claim is based exclusively on a shift in methodology associated with behavioralism. For Lijphart, all traditional theorizing—by types as disparate as Rousseau, Kant, and Grotius—had been "governed by what Kuhn calls a paradigm. And this paradigm remained the basis of the research tradition that dominated the field until the 1950's."[127] Before the behavioral revolution, according to Lijphart, IR was unified under one paradigm encompassing both Kant and Rousseau.

Even if Kant's very liberal essay on *Perpetual Peace* could be placed within the same paradigm as Rousseau's very realist essay, *State of War,* it remains unclear how a change in method constitutes a change in paradigm.[128] Many behavioralists were asking the very same questions that traditional IR scholars had been asking, only with a different methodological approach. For instance, studies of the balance of power using historical evidence from one case were followed by studies of the balance of power using multi-dimensional measures of military capabilities often based on numerous statistical measures. Vasquez makes clear the distinctions between a method and a paradigm while arguing that the behavioral revolution did not constitute a Kuhnian paradigm shift since most behavioralists were testing realist propositions.[129]

The mistake of equating the behavioral revolution with a paradigm shift in IR persists. Waltz argues that "political scientists generally work from two different paradigms: one behavioral, the other systemic."[130] Here Waltz mistakes both a method and a level-of-analysis for a paradigm. Waltz's assertion raises two questions. First, could all theory at the systemic level be nested in the same paradigm as Waltz suggests? This would result in one systemic paradigm containing world system theorists' critique of world capitalism and neorealism. While both are systemic theories, they have very little else in common.[131] A paradigm must be more focused than a level of analysis. The second question posed by Waltz regards the methods used. Are the behavioral studies like those of Singer, Bremer, and Stuckey, which explore the relationship between war and the distribution of capabilities in the international *system,* not systemic studies?[132] In this early study, Singer et al. explored the relationship between the distribution of power in the international system and conflict. This behavioral study employed the same variables that are central to Waltz's concerns with system structure, stability, and war.[133] Large-n studies of the distribution of power and stability in the international system are part and parcel of the systemic theory of international politics pioneered by Waltz, not something distinct from it.

Some postmodernists also mistakenly equate a method with a theory. After a dizzying critique of neorealism, Richard Ashley suggests that within realism "structuralism, statism, utilitarianism, and positivism are bound together in machine-like self enclosing unity." He concludes that neorealism is a "theory of, by, and for positivists."[134] Ashley fails to recognize that positivist methodologies with their distinct ontologies can be applied to a variety of theoretical frameworks in IR. The democratic peace is one example where liberal, non-realist conjectures are studied using positivist methods. Even Ashley's own "dialectical competence model" could spawn an empirical, positivist research agenda. Samuel Barkin discusses other examples where studies erroneously combine methodological concerns with theoretical frameworks.[135]

Lijphart referred to a paradigm as both a method and a theoretical framework. Waltz equated paradigms to both methods and levels of analysis. And Ashley bound positivist methodologies to realism—as though liberal theory is bereft of positivism. Much of this confusion can be avoided by clearly distinguishing a theory from a method. Methods provide procedures and techniques of investigation. Theory, on the other hand, conceptualizes and defines the subject to be investigated. Theory provides a representation of the world, and it helps identify a range of possibilities within that world. In other words, a theoretical framework leads the researcher to certain important questions, while a

method—whether it be a formal deductive model or a historical case study—provides a way of answering those questions. Methods like behavioralism and game theory are vacant without a path to follow or some theoretical framework guiding their application. Even simple game-theoretic models, like the most basic hypothesis, cannot be attempted without some theoretical insight as to actors and their supposed preferences. To put it differently, game theory can be used to model the growing significance of the United Nations and the evolution of a system of world governance, just as it can be used to model coercive diplomacy and balance of power. In the first case, the model is informed by a cosmopolitan theoretical framework, while the second is informed by political realism. All methods are contingent upon but distinct from a theoretical foundation.

The appropriation of Kuhn's paradigm to mean both *method* and *theory* obscures this distinction. If we consider each method a distinct and incommensurable paradigm while also considering each theory a distinct and incommensurable paradigm, any discipline will become divided along a number of highly specialized and Balkanized research communities. By ignoring Kuhn's requisite scientific achievement, each methodological approach—formal models, case studies, classical theory, constructivism, post-positivist, large-n empirical—can form a distinct paradigm. Similarly, each theoretical approach—radical, cosmopolitan, liberal, realist, neorealist—could also be considered a paradigm. As their own paradigm each becomes an "esoteric, isolated, and largely self-contained discipline . . . its own exclusive audience and judge."[136] As a result, efforts to speak across paradigmatic boundaries become more and more difficult. Lee Sigelman's recent observation about political science is particularly true of IR:

> These days it is harder than ever to find a center of intellectual gravity in our discipline. More and more we are a confederation of narrowly defined and loosely connected, and even disconnected, specializations. . . . Most of us have little knowledge, understanding, or appreciation of what our colleagues in other subfields are doing.[137]

Few look to the sociology of knowledge to explain these trends. Yet the misapplication of Kuhn's and Lakatos' account of esoteric, isolated, and self-contained paradigms has surely contributed to the hyper-specialization and Balkanization that now characterizes the discipline of political science, and especially the subfield of IR.

Conclusion: Models of Politics in Models of Science

Thus far I have concentrated on how various models of science have been adopted by students of politics. Now I turn the tables and define these models of science in terms of their politics. This turn further highlights the conservative nature of Kuhn and Lakatos. This also demonstrates Popper's belief that an open society is vital for the dynamic growth of knowledge, and that the growth of knowledge will suffer from the dogmatic, esoteric, and authoritative claims inherent in paradigm mentalities.

The models of Kuhn and of Lakatos compare nicely to particular regime types. Because they are largely conservative and seek to suppress criticism and opposition, paradigms operate much like strict authoritarian regimes. Coexistence with rivals is highly unlikely. Even Kuhn likened the choice of paradigms to types of political institutions: "Like the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life."[138] Kuhn's paradigms resemble ideologically-based regime-types that vie for monopolistic control of the polity. Once established, these regimes restrict competition and perpetuate their narrow claims, even in the face of discordant evidence. The only way to overthrow a paradigm, according to Kuhn, is through total upheaval and revolution; hence his book's famous title, *The Structure of Scientific Revolutions.*

Lakatosian research programs might be likened to liberalizing authoritarian regimes. While Lakatos' conception of science tolerates some dissent, the incentive system in his model tends to discourage opposition. As noted above, few scientists will pursue research outside the dominant research program and risk having their findings "shoved aside," as Lakatos suggests. Lakatos shares with Kuhn the criterion of subsumption that calls for a complete revolution of paradigms and research programs. In both cases, but especially in Kuhn's, there are few efforts to sustain and encourage multiple methods, theoretical pluralism, or peaceful coexistence—if not exchanges—between rival theoretical approaches. Not unlike authoritarian leaders, Kuhn and Lakatos claim that their models are the most efficient way toward progress. While supporters of authoritarian regimes may boast that at least their trains run on time, Kuhn highlights how a regimented normal science results in an "observation-theory match that could be achieved in no other way."[139] This analogy, of course, can only be taken so far. A dominant theoretical framework does not smash and jail its opponents. It simply fails to acknowledge them. Or if it is forced to acknowledge them, it does so in a dismissive manner—as demonstrated in the realists' responses to anomalies that have challenged their paradigm.

The perestroika movement in political science was driven in part by dissatisfaction with these authoritarian developments within the discipline. One source of this movement was the widely shared frustration with "narrow parochialism and methodological bias toward quantitative, behavioral, rational choice, statistical, and formal modeling approaches in American political science."[140] Leaders of the movement argued that opposing voices, methods, and

theoretical visions were being suppressed by certain hegemonic powers within the discipline. In the broadest sense, perestroika was an attack on the dominant paradigm, and a call for an opening up of the discipline. This critique represented a repudiation of the paradigm mentalities encouraged by the appropriation of Kuhn and Lakatos. And it pointed towards a more Popperian commitment to vigorous theoretical debate.

If IR scholars continue to rely on paradigm mentalities without acknowledging that paradigms can only be justified by concrete scientific achievements, we risk two equally unattractive outcomes. The first involves a Kuhnian paradigm shift where the dominance of realism is replaced by an equally dominant alternative paradigm, such as liberalism. In this scenario, after the revolution is complete, theoretical pluralism again perishes and we turn exclusively to those puzzles relevant to the liberal paradigm. Reliance on one paradigm, however, will cause neglect of many global issues that deserve attention. A second possible outcome of this flawed reliance on paradigm mentalities is the proliferation of small but exclusive, esoteric, incommensurable, self-evaluating research communities that speak only to those working within the same paradigm. By ignoring the requisite scientific achievement, any group of like-minded scholars can call forth their exclusive paradigm and insulate themselves from outside critics. This would lead to the further Balkanization of the discipline due to the inability of scholars to engage across paradigmatic frontiers. As Kuhn noted, the "proponents of different theories are like the members of different language-culture communities."[141] While some courageous scientist may set out to transcend paradigmatic boundaries, "he will do so as a foreigner in a foreign environment." Or, as Almond lamented, these different groups "sit at different tables, each with its own conception of proper political science, but each protecting some secret island of vulnerability."[142] This scenario is hardly attractive to those seeking a broad and nuanced understanding of political phenomena.

This dissatisfaction brings us back to Popper. If Kuhn and Lakatos lean toward the authoritarian, Popper's views endorse an open society of scholars employing a diversity of theories and methods. A longtime critic of dogmatic thinking, Popper linked his ideas of intellectual transparency, broad accessibility, theoretical pluralism, and fallibility to the civic virtue of tolerance in politics.[143] For Popper, scientists who are proven wrong will learn more and be better prepared for future research. Similarly, when elected officials are proven wrong—and voted out of office—the costs are relatively low and they can return to run for election again. The scenarios sketched by Kuhn and Lakatos are different. Here we see scientists holding tightly to their authority and discouraging any challenge that might give rise to revolution. This is understandable given the fate of the old guard in the wake of revolution. Theoretical pluralism eases the fears of revolution and subsumption. A final distinction with Popper is his stress on fallibilism. The realization that theories can never be proven beyond critical doubt and that even our most reliable theories can be overturned will nurture a more humble spirit of inquiry. The idea of fallibilism does much more than promote mere tolerance of opposing methods and theories. It invites the active engagement of scholars working within different approaches.

The movement toward greater pluralism reflects what Peter Katzenstein and Rudra Sil recently termed "eclectic theorizing." Rather than being bound by paradigmatic constraints, this approach values "problem-driven rather than paradigm-driven research." Progress will occur when research trespasses "deliberately and liberally across competing research traditions."[144] For any of this pluralism to occur, as researchers we must strive to make our work widely accessible to those outside our specialization. As readers, we must cast our nets more broadly and intently. As Popper advised for working through drafts, "If a conscientious reader finds a passage unclear, it has to be rewritten. . . . [With clear writing] one can avoid some misunderstandings, assuming readers who want to understand."[145] This could prove beneficial in other realms. For instance, more accessible research might narrow the widening gap between political scientists and policymakers.[146] Second, more accessible research makes possible a well-ordered, democratic science that, in the words of Philip Kitcher, avoids "a theology of science that would insulate inquiry against moral and political critique."[147] Of course, it is easier to be understood—and published—when one is addressing a narrow, esoteric, self-contained, and self-regulated research community. However, the challenge in a discipline guided by Popper's principles is to make our work more accessible, reach across boundaries, and directly engage those with whom we may have fundamental disagreements.

This plea for pluralism and dialogue may suggest an academic utopia. However, dialogue between different schools in IR will not necessarily lead to synthesis and harmony.[148] In Popper's view, such a scientific consensus would indeed be undesirable. Popperian science involves unending intellectual disagreement. Meaningful pluralism, however, can help erode the pretensions nurtured by paradigm mentalities, which convert disagreement into incomprehension, hostility, and imperiousness. These pretensions, I have argued, impoverish inquiry in political science. Popper's model of bold pluralism would lead to more lively and engaging study of politics. It would put to rest fruitless debates over naturalism or intepretivism or postmodernism, where each side seeks to legislate appropriate and exclusive methods of inquiry. In Popper's view, metaphysical speculation can co-exist and even enhance hypothesis testing. But before a meaningful theoretical and methodological pluralism can emerge, students of

politics must abandon their faith in what Popper calls "the myth of the framework."

As the world grows more complicated and more interdependent, questions regarding politics can hardly fit a single paradigm or a single method. Popper's fallibilism points to the hubris of believing that one method or one theory is sufficient to address the multifarious questions that students of politics must address. If we are to better understand the complexity of our subject matter, we might heed the advice of Popper to "keep the flow of ideas running from all tributaries."[149] This would require a new appreciation for theoretical and methodological pluralism, and a willingness to engage a range of perspectives. While such engagement may be challenging and at times unpleasant, Popper envisioned such challenges as the best way to ensure the growth of knowledge. We can ill-afford the narrow, imperious impulses that arise from a faith in one dominant theoretical framework. Nor can we afford the mistaken belief that the models of science depicted by Kuhn and Lakatos provide appropriate guides for the study of politics.

Notes

1 Lakatos 1970, 93.
2 Kuhn 1970a, 167.
3 Popper 1945, 129.
4 For a short description of these discussions see Fox 1996, 335; 344, fn 10.
5 Kuhn 1970a, viii.
6 Kuhn 1970b, 245.
7 Lakatos 1970, 176.
8 See, for example, Lijphart 1974; Ball 1976; Polsby 1998; Elman and Elman 2002.
9 Truman 1965, 865.
10 Almond 1966, 875.
11 Polsby 1998, 200.
12 See Vasquez 1997 and 2003; DiCicco and Levy 1999; Dessler 2003; Elman and Elman, 2003; Waltz, 2003; and Chernoff, 2004.
13 See Hirschman, 1970; Sil, 2000; and Bennett, 2003.
14 George and Bennett 2004 (131) and McKeown 2004 (140) characterize Popper as a positivist, while Lakatos 1970 frames Popper as a naïve falsificationist.
15 Almond 1977 is one exception. Almond employed Popper's dichotomy between clocks and clouds to introduce the importance of indeterminacy. Bohman 1993 and Hardin 2003 also addressed how indeterminacy may play out in social science.
16 Philosophy of social science since Popper and Kuhn has developed and diverged in significant ways. Stephen Walker 2003 suggested that Laudan's 1977 *Progress and Its Problems* provides a more fitting guide to IR. Fred Chernoff 2005 argued that a version of Duhem's conventionalism would be a better metatheoretical guide. Alexander Wendt's 1999 discussions of scientific realism constitute yet another direction that philosophy of social science has taken. Andrew Bennett 2003 reviewed three post-Lakatosian views of science for their relevance to IR: the naturalists, the Bayesians, and Mayo's error-statistical approach. Bruno Latour's 1987 anti-realist, constructivist views stand in stark contrast to Philip Kitcher's 1993; 2001 appeals for a "modest realism" and a "well-ordered" science. In spite of these advances, political scientists still reference Kuhn or Lakatos as their primary philosophy of science guides.
17 See Monroe 2005.
18 This Balkanization can be characterized as one where political scientists come to "sit at different tables," as Gabriel Almond 1990 (13) lamented.
19 Kuhn 1970a, viiii.
20 Kuhn 1970a, 10.
21 Schram 2005, 112.
22 Kuhn 1970a, 24.
23 Ibid.
24 Kuhn 1970a, 135.
25 Kuhn 1970a, 178.
26 Kuhn 1970a, 24.
27 Ibid.
28 Kuhn 1970a, 151.
29 Kuhn 1970b, 254.
30 Kuhn 1970a, 150; 205.
31 Kuhn 1970a, 109; 42.
32 Kuhn 1970a, 64–65.
33 Kuhn 1970a, 52.
34 Kuhn 1970a, 24; 90.
35 Kuhn 1970a, 77.
36 See Polsby 1998.
37 Ball 1976.
38 DiCicco and Levy 1999, 676.
39 See Elman and Elman 2003.
40 Lakatos 1970a, 177.
41 Lakatos 1970, 178; 93.
42 See, for instance, Dessler 2003, 381; Vasquez 2003, 429; Chernoff 2004, 64.
43 See Kuhn 1970b; 1970c.
44 Kuhn 1970a, 23.
45 Kuhn 1970a, 158.
46 Kuhn 1970a, 169.
47 Kuhn 1970a, viii;178.
48 Kuhn 1970a, 24.
49 Kuhn 1970a, 147.
50 Lakatos 1970, 177.
51 Lakatos 1970, 135.
52 Lakatos 1978b, 212.
53 Lakatos 1970, 109.
54 Lakatos 1978a, 184.

55 Lakatos 1978a, 179.
56 Cited in D'Amour 1976, 88.
57 Lakatos 1978a, 179.
58 Lakatos 1970, 137.
59 Lakatos 1970, 137.
60 Lakatos 1978a, 179.
61 Lakatos 1970, 142.
62 Elman and Elman 2002.
63 Lakatos 1978b, 110.
64 Lakatos 1970, 157.
65 Lakatos 1978b, 218.
66 Lakatos 1978b, 218.
67 Lakatos 1978a, 112–114.
68 Feyerabend 1988, 162; 229.
69 Lakatos 1970, 116.
70 Almond 1990.
71 See Popper 1957 for the most concise discussion.
72 Popper 1994, 72.
73 Popper 1970, 52.
74 Popper 1994, 71.
75 Webb 1995, 89.
76 Popper 1994, 34.
77 Popper 1934, 38.
78 Popper 1974, 80; 1956, xxii.
79 Hacking 1983, 3.
80 Farr 1983.
81 Popper 1957, 129.
82 Popper 1994, 143.
83 Popper 1994, 34–5.
84 Popper 1970, 55.
85 Popper 1994, 109.
86 Popper 1994, 142.
87 Each model conveys both descriptive and prescriptive elements of how individual scientists are expected to behave. While some emphasize Kuhn's descriptive account and Popper's prescriptive exhortations, Kuhn acknowledges that the dichotomy between empirical and normative is not so easily maintained: "A number of contemporary philosophers have discovered important contexts in which the normative and the descriptive are inextricably mixed." See Kuhn 1970a (207). While Kuhn cited Cavell 1969, Searle's 1964 discussion is also relevant.
88 Kahneman and Tversky 1979, 265 refer to this as the "certainty effect."
89 For realism's dominance, see Vasquez 1998. For the recent decline of realism, see Walker and Morton 2005.
90 While Vasquez 1998 (Chapter 1) argued that Morgenthau provided a Kuhnian exemplar, he overlooks Kuhn's required concrete scientific achievement for an exemplar.
91 See Elman and Elman 1997; Vasquez, 1997; and Keohane, 1986.
92 Waltz 1998, 385.
93 Gilpin 1981, 227.
94 Waltz 1998, 385–6.
95 Boulding 1990, 38.
96 Holsti 1985, 36.
97 Vasquez 1998.
98 See Walker 2008 for Paine and Kant's respective views on the democratic peace.
99 Babst 1964; 1972.
100 See Small and Singer 1976.
101 Levy 1988, 662.
102 Waltz 1990:42.
103 Waltz 1998, 378–79.
104 Kuhn 1970a, 24.
105 See Vasquez 1997 and the various responses that followed in the same issue.
106 Schroeder 1994b.
107 Levy 1994, 744.
108 Schroeder 1994a, 117.
109 Waltz 1979, 125.
110 Schroeder 1994a, 129.
111 Waltz 1997, 914.
112 Waltz 1998, 385–6.
113 Elman and Elman 1995, 183.
114 Elman and Elman 1995, 186.
115 Schroeder 1995, 194.
116 Elman and Elman 1995, 192.
117 Popper 1934, 281.
118 Schroeder 1995, 195.
119 Katzenstein 1996:xiv.
120 Masterman 1970.
121 Kuhn 1970a, 16; 109.
122 Kuhn 1979.
123 Fleck 1935, 39.
124 Fleck 1935, 107.
125 Fleck 1935, 38; 27.
126 Fleck 1935, 142.
127 Lijphart 1974, 42.
128 See Kant 1795 in comparison to Rousseau 1756.
129 Vasquez 1998, 21.
130 Waltz 1997, 913.
131 This point is further explored in the introduction of Wendt 1987.
132 See Singer, Bremer, and Stuckey 1972.
133 Waltz 1979.
134 Ashley 1986, 268; 280.
135 Barkin 2003.
136 Kuhn 1970b, 254.
137 Sigelman 2005, 324.
138 Kuhn 1970a, 94.
139 Kuhn 1970a, 65.
140 Monroe 2005, 1.
141 Kuhn 1970a, 204–5.
142 Almond 1990, 13.
143 Popper, 1945; 1994.

144 Katzenstein and Sil 2008, 110.
145 Popper 1974, 83.
146 George 1993.
147 Kitcher 2001, 182.
148 On this see Hellmann 2003 and the adjoining responses.
149 Popper 1994, 143.

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