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Joseph Agassi

Popper and His Popular Critics

Thomas Kuhn, Paul Feyerabend and Imre Lakatos

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The unfairness of which I complain is that you do not distinguish between mere disputation and dialectic: the disputer may trip up his opponent as often as he likes, and make fun; but the dialectician will be in earnest, and only correct his adversary when necessary ….
I would recommend you, therefore … not to encourage yourself in this polemical and controversial temper, but to find out, in a friendly and congenial spirit...

Plato, *Theaetetus* 167e–168b
Jowett translation

For Ian C. Jarvie

Preface

The papers that appear here are new or new versions of previously published ones. I am always ready to rewrite, partly or fully, but never in order to report changes of opinion. These deserve fresh separate papers, not grafts. The new versions of older papers here are mostly abbreviations of and amplifications. Of the new papers, some are written as background information for the dispute. Two papers were rejected by the encyclopedias that had commissioned them. Two others were addresses delivered in conferences in memory of Thomas S. Kuhn and of Paul K. Feyerabend (see details on the next page).

At the background of this volume stand perennial attempts to navigate between dogmatism and relativism. Recent classical discussions of relativism, especially those of Ernest Gellner (e.g., Gellner 1986) and of Ian Jarvie (e.g., Jarvie 1984), are exhaustive. I will not repeat their arguments here. (I did so elsewhere.) My proposal here, if I have any, concerns my Popper-style attitude. I find it unnecessary to block dogmatism, as it is no temptation for the curious. Relativism is too great a constraint on criticism, but it is appealing as it dismisses the absolute truth, which admittedly is inaccessible. Yet as an ideal, as a regulative principle, it is essential for realism. Popper improved his philosophy as increasingly realist. We may further develop his philosophy in this vein.

Popper declared all attempts at criticism valuable, even ones that rest on misunderstandings. Does this hold for malicious distortions too? Yes, after they are cleansed of their malice. This volume centers on Popper's recent popular critics, whose presentations of their target look less faithful than those of his older ones, but deceptively so. His old critics ascribed to him their Wittgenstein-style philosophy (improperly but with no ill intent). His new critics are different, as they share his rejection of all justification, especially inductive (Nola and Sankey 2000, ix). Regrettably, they also belittled rationalism in the style of Michael Polanyi (Kuhn expressly so, Feyerabend against his expressed dissent from Polanyi, and Lakatos wavering). Kuhn supported the scientific leadership, Feyerabend disapproved of it, and Lakatos declared his wish to take over (Holton 1974). They use against Popper arguments that he had invented without saying so. Nevertheless, their contributions are significant and deserve less offensive and less exaggerated paraphrase.

Finally, here I overlook the criticism that W. W. Bartley, III, has launched against Popper, as well as mine. This is due to their different character: they are both within Popper's minimalist program.

Some of the chapters that comprise revised and abridged versions of invited papers are mentioned below:

4-"Rules against Excessive Defensiveness" is a revised and abridged version of "Popper's Popular Critics", an invited paper, read at the conference of *L'Associazione Fundazione Karl Popper* in Milan in January, 1997, published in full in *Anuar*, 7, 1999, 5-25.

5-"Against the Bouncers in the Gates of Science" is a revised and abridged version of "The Philosophy of Science Today" published in S. Shanker, ed., *Routledge History of Philosophy, IX, Philosophy of Science, Logic and Mathematics in the 20th Century*, 1996, 235-65.

6-"Duhem, Quine and Kuhn" ends with a revised and abridged version of "Comparability and Incommensurability", published in Stefano Gattei, ed., *The Kuhn Controversy, Social Epistemology*, 17, 2-3, 2003, 93-4.

7-Karl Raimund Popper (1902-1994) is an encyclopedia article first commissioned and then rejected.

8-"Kuhn's Way" is a revised and abridged version of the paper by the same name from *Philosophy of the Social Sciences*, 32, 2002, 394-430.

9- "Feyerabend's Proposal" is a revised version of my "The Politics of Science", *J. Applied Philosophy*, 3, 1986, 35-48.

10-Imre Lakatos is an encyclopedia article first commissioned and then rejected.

11-"A Touch of Malice" is a revised and abridged version of "A Touch of Malice" (the Feyerabend-Lakatos correspondence) published in *Philosophy of the Social Sciences*, 32, 2002, 109-21.

12- "The Essential Popper" is a revised and abridged version of the paper by the same name that appeared in Raffaele De Mucci and Kurt R Leube, eds., *Un austriaco in Italia, Studi in onore di Dario Antiseri*. Rome, Rubbettino, 2012, 149-66.

13-*Kuhn on Pluralism and Incommensurability* was an invited paper read in Tai Pei (Taiwan), in a conference called "Incommensurability 50" on 1-3 June 2012.

14-"Paul Feyerabend and Rational Pluralism" was an invited paper read in the International Feyerabend 2012 Conference in Humboldt University, Berlin, in September 2012 and found unsuitable for its proceedings,

15- Lakatos on the Methodology of Scientific Research Programs includes a revised and abridged version of my "The Methodology of Research Projects: a Sketch", *Zeitschrift für allgemeine Wissenschaftstheorie,* 8, 1977, 30-8.

Herzliya, Spring 2014 Joseph Agassi

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Contents

Part III In a Nutshell

Abstract

We, critical rationalists, are obliged to offer and invite criticism as a matter of course, not to try to convert and not to impose criticism: proper debates are voluntary and should remain so. Popper's popular critics—Kuhn, Feyerabend, and Lakatos—replace his older, Wittgenstein-style critics, now defunct. His new critics did not quite advocate the defunct doctrines (confirmation and relative truth are the perennial candidates), but they played with the idea of criticism as beneficial, in vain search of variants of these that could better appeal to the public, be interesting, and possibly even feasible. Some of their criticism of Popper is valid, yet it is marginal for the dispute about rationality. He endorsed fallibilism; they hedged about it. He viewed learning from experience as learning from error; they were unclear about it. His view resembles Freud's reality principle; they hedged about this too, as they defended the idea of constructive criticism (hold on to your belief in a refuted theory until you can replace it). He stressed his criticism of the current views of science as inductive; they endorsed it with the qualification that go with their demand for constructive criticism, or so they hoped. The intended readers of this volume are those who are willing to assume (at least for the sake of the argument) that Popper's (Hume-style or rather Russell-style) criticism of inductivism is valid, since his popular critics attacked him on different points. Their central criticism of him was off target, but successful nonetheless. They differed from him significantly regarding their intended readers: he had addressed those who readily admit criticism (willingly or reluctantly) as beneficial and his popular critics addressed those who find it hard to admit openly that criticism upsets them somewhat, so that they are disposed to evade criticism furtively. Current popular criticism of Popper's ideas shows yet again the logical relation between the critical attitude and liberalism: obviously, it is possible to endorse liberalism without being critically minded, but scarcely the other way around. Hence, we better read the objection that Popper's popular critics have launched against him not as criticism proper, but as somewhat reasonable protest against his use of the highest standards in his relentless advocacy of liberalism and of criticism in his valuation of science and of democracy alike.

N. B. Popper's Wittgenstein-style critics saddled him with the view of "the language of science" as the only metaphysical framework for discussing science. It is hilarious; ascribing it to him is an insult to his intelligence. Now that their view is waning, so does their criticism of Popper. His new popular critics are more subtle: their distortions of his views are harder to refute as they are more exaggerations than distortions, or at least it is friendlier to view their criticism this way.

Part I
Prelims

Chapter 1
On Human Rules About God's World

The short story "El Aleph" of Jorge Luis Borges concerns aesthetics—how the rules of art relate to superior poetry (or to superior art in general; it is hard to judge). On the spur of the moment, Borges suggests, poor poets make up rules to justify whatever goes into their art—to no avail. To be good, art must be kaleidoscopic: it must present its object in diverse perspectives and thereby reflect the whole world, no less, and in many ways. Borges envisioned a materialization of this kaleidoscope; he named it an aleph—after the first letter of the first alphabet, the numerological name for the First, the kabalistic name for the origins of the universe; the name that arch-mathematician Georg Cantor gave actual infinity.

Borges wrote prose and poetry. His prose excels; hardly his poetry. His prose pieces are concise sketches, outlines of veiled secrets, barely told tales, and abstracts of philosophical treatises. One of these, "Borges and I", is (a précis of) a fabulous essay of less than one page that reports about two selves of his, one humble one vain. He confesses there ignorance as to who is more real. His ascription of vanity to himself is possibly an expression of his fear that since the quality of his poetry is not the highest, his publication of it rested on vanity. We may then read his confessed ignorance here as a silent prayer that the esteem for his poetry will rise.

The plot summary of "El Aleph" is this. The deceased sweetheart of the narrator had lived in one house with a relative of hers who is an inept poet working on an ambitious project of writing a poem about everything while making up rules to justify his poor choice of words. The narrator befriends him in order to frequent the home of his deceased sweetheart. One day the relative sees an aleph in his cellar. He fears losing his mind. The narrator volunteers to verify the strange fact. Down in the cellar he feasts his eyes on the marvel. Upon his return, the inept poet asks him anxiously: did you see it? No, he replies cruelly. Later the inept poet gains honors, moves to a better house and demolishes his old one. The narrator reports about another manifestation of the aleph.

This expresses obliquely Borges' fears that his poems are poor (and of his having published them as vain) as his painful awareness of his ability to see but not

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to articulate his vision. The story must have more readings: it is kaleidoscopic—like all of Borges' prose that is poetic.

The story's center is the contrast between the poor artist who makes rules after the facts and the inexhaustible richness of existence. Are all rules after the facts? Are some rules helpful? Borges raises these questions regarding art. Others did so regarding evolutionary and developmental psychology, learning and education theory, generative grammar and philosophy, learning and research: can the best system of science possibly contain an image of the whole cosmos? Russell said (1914), of course not. Popper and Polanyi agreed: the cosmos is too rich for that. Kuhn followed Polanyi; Feyerabend followed Popper. A posthumous book of his is *Conquest of Abundance: A Tale of Abstraction versus the Richness of Being*. As science is limited, does it nonetheless reflect reality? How?

The Age of Reason replaced traditional religious skepticism and took it for granted that science can encompass God's world. Even then some of the greatest thinkers doubted that, particularly David Hume and Immanuel Kant. In the middle of the nineteenth century William Whewell challenged traditional views about the rules of scientific research to make room for creativity. At the turn of the nineteenth century Pierre Duhem did the same more radically to allow freedom in research. Soon Popper developed the idea—critical rationalism—that the rules for scientific research do not depend on the world. To insure that this was the case he chose the smallest subset possible of the traditional set of rules for research: suffice it for scientific theories that they should be open to empirical criticism. This fits with Einstein's view: research is putting questions to Nature; She can say no and She can say maybe; She never says yes. The standard objection to Popper is naturally that his set of rules is too poor. Other oppositions were that we cannot articulate the rules (Kuhn), that there are no rules (Feyerabend) or that we need new rules (Lakatos).

Borges plays here the terrific role of bridging between traditional religious skepticism and the modern view of reason that is more humble than that of the Age of Reason but is rationalist all the same. This way he began the project dear to the heart of Feyerabend—of bridging between the arts and the sciences. Borges shunned capricious whims; as he has put it, he preferred naturalistic explanations to mystic ones. His naturalism is remarkably realist:

If I could live again my life,
In the next—I'll try,
—to make more mistakes,
I won't try to be so
perfect, I'll be more
relaxed…
I'll take fewer things seriously…
I'll take more risks …

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Chapter 2
In Search for Rules

A traditional philosophical problem appears in diverse contexts and situations: by what rule is navigation possible between dogmatism and relativism? Russell confessed he struggled with it all his life in frustration. He found absolutism too dogmatic and too stringent, relativism too lax and too superficial. He always followed his tremendous common sense but he sought a rule. He observed that science demolishes the *naïve* version of realism and wished to replace it with commonsense. Now he objected to the commonsense of commonsense philosophy as too highbrow. This raises the question, what commonsense did he seek? Answer: he sought a scientific worldview, one that is as near to the best of science as possible, as free of dogma and realist. He was right, especially in considering all this a challenge rather than a satisfactory solution.

Nowadays philosophers of science tend to avoid expressing their preference for science over the competition. Nor is the preference of science enough: scientific philosophy concerns not only science: it is a worldview that values science as a human achievement, though as one of the highest, because it is eminently rational. This is part of traditional western philosophy. Now what is the right worldview? Ernst Mach expressed a widespread view when he said, my worldview is the sum total of current science. Willard van Quine expressed a widespread view when he said, as science tells us what there is, discussion of this question is redundant. These are examples of efforts to apply the rules of scientific inquiry everywhere. There remain thus only two serious philosophical questions: What are the rules of scientific research? What is their end? Traditionally, the chief aims of the rules were first to prevent error and second to reveal the truth. This is an excess. Fallibilism tries to rectify the situation.

The central problem that Fallibilism raises is, what error is allowable, reasonable, or even fruitful? What rule, then, helps prevent errors that we find improper, careless, irresponsible? Finally, given alternative options, which should we examine critically first? The law of the land offers rules concerning these matters. Judges apply rules about duties according to law-books. Juries decide on questions of fact; they often have to decide with no reasonable doubt. What rule renders doubt unreasonable the law does not say. Philosophy says, all doubt is reasonable, even doubt about our very existence. The law deems a corpse and a

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smoking gun sufficient evidence for murder. Johnson's story, "The Man Who Shot Liberty Valance" (1949), describes a case of a very reasonable a mistake in such circumstances. Her story is impressive, but it does not lead to a legal reform. It does help see why traditional philosophy was not content with the law: it looked for a perfect rule. Fallibilism allows for satisfaction with the law but only while trying to improve upon it (Popper 1945, Chap. 17). How?

The rules characterizing improper error should allow for error in the rule and in its application. An allegation of impropriety may require official inquest. As Mark Twain noted, improper error whose outcome is not disastrous escapes such treatment. This holds also for silly scientific errors that regrettably pass severe tests by some sort of fluke. As against this, the literature on the philosophy of science centers almost exclusively on the question, what judgment is proper, as if every judgment that is not obviously proper is condemnable. This makes life intolerable. Thus far, fortunately, all efforts to characterize propriety—of validation—have failed. And yet the literature on the philosophy of science still considers the absence of such a rule a disaster, allegedly since the absence of such a rule dooms us to an inability to discriminate between ideas. This is easy to refute by observing that we usually judge—propriety or beauty or many other qualities—heeding no rule.

The search for rules is laudable nevertheless. It is obviously hard to find one for differentiating between valid and invalid conduct. It is easier to seek a rule for differentiating unimpeachable from impeachable error. Most philosophers of science seek rules for validation, considering the invalid improper. They are in error: in modern society only the obviously invalid is judged improper. Most philosophers of science condemn all action that rests on any not-yet-validated idea; fallibilists deem permissible acting on what has not been declared invalid, stressing that validation may turn out to be erroneous. It often is: there is no *utopia*. Rules for propriety, as given in law-books, are thus open to criticism. As neither relativism nor dogmatism accounts for ideas entertained tentatively—of the law or of anything else for that matter—philosophers ignore the tentative. Democratic legal systems decree rules as to what kind of error is improper; that renders them inherently fallibilist. They employ diverse rules for judging error improper, to apply with increasing measures of stringency to citizens, to the press, to government officials, the police, the district attorney and law courts, and above all to legislatures.

As to science, whatever is improper in civil society is improper there too, but not the other way around. Reporting unrepeatable observations is improper only in science: it is quite impossible to apply that rule universally. Repeating old errors is likewise improper only in science—regrettably not in politics. Applying repeatedly small modifications to an old error is permissible, but science may dismiss it as scholastic.

The philosophical literature treats information as unproblematic and as providing empirical support for theories. However, just how this support works is deemed an open problem. We need support to prevent arbitrariness as we deem the arbitrary improper: the propriety of an assertion is its "warranted assertability". In some inquests warranties for some assertions are required—usually in order to exonerate people who relied on them with disastrous results. Philosophers of science want to warrant successful scientific assertions and they take success as warrantee. This is very much after the event; if we only knew in advance the rule that leads to success then we would be unbeatable.

Philosophers of science deem science a success story and seek its rules for total success. Taking this seriously is hardly imaginable. Paul Feyerabend exposed this seriousness as a serious error; he challenged peers to articulate it. Moreover, since the freedom of speech includes the freedom to assert whatever one wishes, there is no need for permission to speak, from Wittgenstein or from any other philosopher. Yet they seriously seek reasons for trusting predictions that rest on scientific theory.

Different people, from Hume to Wittgenstein and Popper, said this is impossible. In response most philosophers of science say, in observed fact observation reports support theories that serve as grounds for trustworthy predictions. They study the question, what is support? How does it raise the credibility of predictions? They still do not know.

Approach then the problem from the opposite direction: what makes some error fruitful? Of course, the idea that science is admirable makes this problem include as a special case the problem of the demarcation of science, which has fascinated many great modern philosophers. They said science is certitude but failed to show the rules for attaining it. Peirce and Popper declared science fallible. Peirce's answer to the problem of demarcation is unclear. Popper has offered the rule to consider scientific all the theories that are open to empirical criticism. Most philosophers of science judge his rule insufficient: they want assured rules for assurance and annoyingly he offers none.

Most philosophers of science want perfect assurance. This does not exist. Yet assurance does exist. How do we achieve it? This question concerns facts, and so it is not exactly what philosophers of science have in mind: they seek perfection rather than observe. We who observe see that different people are assured in different ways and with different degrees of response to assurances. We likewise observe that to circumvent this fact in problematic cases we appeal to socially received rules about assurance. These rules are imperfect and so they undergo reforms repeatedly.

Science does seek perfection: the absolute truth. Technology does not: it receives its ends from its developers. The end of the test of whatever technologists test is to find some fault in constructions. Finding faults is context dependent: faults that science may find may be too small to matter to technologists. The search for faults is often deemed cantankerous. This never holds in science; in technology it does hold, regarding immaterial faults. Better find serious faults, important ones, like the defects that we expect diagnosticians to find in the state of health of their patients. Ignoring them is an error, often judged improper. Patients dislike being ill; they may resent their diagnosticians for telling them that they are. Alberto Coffa remarked on this when he discussed the philosophy of science of Moritz Schlick, who had said, people resent being told that they were in error. Not always, responded Coffa: we are glad to hear refutations of gloomy diagnoses. (Coffa 1991, 421)

Science takes all errors seriously as its end is to trace God's blueprint of the universe, to use Einstein's metaphor. And for this strictness tradition offered strict rules, for observations leading to theories (bottom up) and for theories leading to observations (top down) but not both. For, the two sets of rules may clash. As science was supposed to be infallible, clash was unthinkable. Fallibilists may endorse both methodologies, playing them one against the other, as already Democritus of old has suggested. In politics, the unreliability of an institution leads to limiting the reliance on institutions by applying other institutions against them in a system of checks and balances. Likewise, the political system and the free market limit each other. The same may hold for the foundations of science. We may try to emulate Kant's dialectic of pure reason that played one metaphysical system against another. His aim was to prove metaphysics futile. As Popper has suggested, it is better to pitch different lines of reasoning against each other and to pitch against each other the two methods, of reasoning and of observing, and to do this systematically and fruitfully. It is also possible to pitch metaphysics against science. A metaphysics that can conflict with science should be considered friendly to science. The rules that Imre Lakatos has offered are laudable despite their shortcomings, as he took seriously the contribution of metaphysical systems to scientific research. Alas, he ignored conflicts.

The chief common argument against metaphysics in my younger days was Wittgenstein's positivist theory of meaning that ousted metaphysics as meaningless. Later on his school has reluctantly granted some metaphysics rehabilitation and gave up discussion of meanings. Grand Oxford logician Dummett thus spoke disparagingly of the positivist "theory of meaning—more accurately, their proposal for the construction of a meaning-theory" (Dummett 1993, 211). So back to the central metaphysical discussion of philosophy: what rule will help science avoid the regrettable, excessive indifference to criticism that dogmatism and relativism share?

We have thus far left unanswered the question of assurance. Do fallibilists recognize the commonsensical everyday assurances that are all round us as, say, when we avoid unnecessary risk? The answer is in the affirmative; it is the majority of philosophers of science who do not recognize commonsense assurance: recognizing its shortcomings they pretend to replace it with the perfect assurance that they hope science grants us. Their hopes for such assurance are forlorn. Science can at best improve our systems of assurances by correcting some of its errors; it offers no guarantees (Agassi 2014). If anything, it does the opposite. As Russell observed (Russell 1948), if what science tells us is anywhere near the truth, then life is more precarious than it was ever envisaged.

It puzzles me that I had to explain all this. That some people want rationality to prescribe rules of conduct that absolves them of the need to take responsibility is obvious; but there are people who do not fear taking responsibility and who nevertheless have difficulty to hold a fallibilist view of human conduct. It is a tribute to Popper's recent popular critics that they do not concern themselves with all this: they discuss the possibility of rules, especially of conduct regarding matters scientific. They should have said, Popper proposal is of less rules than any of his competitors: they wish to have a complete set of rules and he declared this impossible. (It would comprise a solution o the problem of induction, of course.) is his proposal nonetheless too stringent? Possibly. This suffices for looking at them with appreciation that they deserve more than their predecessors.

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Chapter 3
Rules Against Mock-Criticism

Popper's popular critics made use of the truism that every criticism is answerable, provided that even unsatisfactory rebuttals count as rebuttals. By the same token, every thesis is refuted, provided even unsatisfactory refutations count as refutations. This is a standoff. Hence it is advisable to set some standards for criticism, to bar arguments—refutations and rebuttals—that are too obviously unsatisfactory. Before offering comments on the advocacy of poor arguments that Popper's popular critics offer as criticism of Popper's philosophy of science, let me discuss first the Establishment's view on the matter.

Learned treatises criticizing alternatives to official doctrines naturally win establishment incentives. Establishments need not endorse them; they are content to invite attention to them, critical if need be. The demolition of such treatises takes time and that suffices to replace them with newer ones, good or else poor. That keeps heretics on the defense. This may matter little, as establishments take the defense of their doctrines as luxury: they stay in power anyway, and helping them to maintain a semblance of intellectual hegemony is but an added bonus. Obviously, since developing ideas is the *raison d'être* of intellectual establishments, the maintenance of a semblance of intellectual hegemony is more important for them. And so, critical works against their opponents are above average, and at some cost. Gertrude Stein has noticed this ("How Writing is Wrote"): they endorse new ideas to block newer ones. Thus, the nineteenth century scientific establishment backed the heresy of Roger Joseph Boscovich (his compromise between Newton and Leibniz) to block the field theory of Michael Faraday (that is even more in the wake of Leibniz). This does not hold for the Academy (especially the philosophy and the history of science departments there), because its task is not only to conduct research but also to validate. Thus, established historian of science Williams declared Faraday Boscovichian. This distortion of history of his still puzzles me.

Heretics are here on a sticky wicket. The learned treatises that mock-criticize their views do not merit attention yet overlooking them looks dogmatic; answering them sounds defensive; appreciating them lowers standards. They should make demands on the literature that is critical of their views and approach it with a

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check-list before responding to it. That way they may help institutionalize standard forms of initial response to poor criticism. The check-list should include such questions as, does the new critic represent heretics properly? Does the new critic recognize that the heretic offers significant new ideas? Do these comply with the standard requirements of answering the questions that heretics purport to answer? How do they respond to standard criticism of establishment ideas?

As to critics of Popper's critical rationalism, the check-list should include the question, is Popper's requirement of a scientific theory that it should be refutable a minimal requirement? (Critics should recognize that all rationalist philosophers of science endorse the rule for criticism in science but that most of them see it as necessary-but-not-sufficient.) Further, does the new critic agree that Popper has offered a theory of scientific progress (be it true or false) and does the new critic admit that this theory of scientific progress is impervious to Hume's critique of induction (and thus that it is new)? Here my disposition is to ascribe to Popper's current popular critics the right answers so that the discussion can continue profitably. There is a limit to this benevolent attitude, though. It is hard to apply it to Popper's older popular critics, much less to their contemporary heirs.

Moritz Schlick said, Popper's refusal to view observation reports as certain makes him an idealist (Ayer 1959, 213). This makes little sense, as idealists since George Berkeley do view them as certain and as realists do not—ever since Robert Boyle and up to Bertrand Russell. Still, this untruth is no violation of etiquette. What is such a violation is that although Schlick's friend Otto Neurath and Popper shared the view that he criticized, he directed his criticism against only one of them.

Rudolf Carnap then claimed that Popper's criterion of demarcation is [not of science but] of the language of science. This made him look very silly (as the negation of an item in language is still in language whereas the negation of an item in science is not). This was silently dropped. Hempel first agreed with Carnap and later suggested that Popper demarcated as scientific all and only tested but not refuted theories (Hempel 2000, 279–280). This renders Newton's mechanics unscientific. Perhaps Hempel deemed scientific as credible. Popper never spoke of credibility except to say, grudgingly, that the best available hypothesis for credibility is that the severely tested yet not refuted theory is the best candidate for credibility. Popper himself preferred the suggestion that it is a candidate for further critical examination. The establishment ignored tests as intensional: they viewed credibility as a relation between sentences. To this they added that credibility is a kind of probability. They thus viewed the credibility of observation reports as maximal. Popper differed and followed the scientific tradition that declares an observation report scientific iff it is repeated, deemed repeatable, and generalized. He discussed acceptance only as intent to test.

Popper could have insisted that only tests—attempts at refutations—enter the discussion of the empirical basis of science. Instead he said more. For the sake of the argument he waived his demand to limit discussion of observations to outcomes of tests; for the sake of the argument he allowed all observations; even then, he proved that empirical support is no probability. His proof was dismissed as misunderstanding. He offered more proofs. One of them he developed and published with David Miller. To no avail. Let me select as an example one famous text by the late Wesley Salmon (1979) as an example of an excessive maltreatment of Popper's ideas that still persists. It is still hailed as an utterly successful refutation of Popper. Salmon claimed to have refuted Popper's disproof of the view of empirical validation as probability. Happily, Salmon admitted that Popper's discussion concerns tests, not validation. He declared, however, that Popper was concerned with plausibility *malgré lui* (22, 27, 115). This he could do as he ignored Popper's view that a test that ends in refutation is successful. What Popper and Miller proved yet again is that even were the aim of science failure to refute, the success in achieving this aim is no probability. Salmon denied then that Popper's and Miller's proof help validate. Indeed it does not. Rather than praising this for its consistency with Popper's views, Salmon disapproved of it for its inconsistency with established views. This kind of move is traditionally known as begging the question.

In democracies Establishments cannot purge free debate. Improved public education increases scope for it and raises its quality and its usefulness. Improved public education raises the recognition that the Establishment's rules for credibility are not too credible. The level of critical attention has risen: in the rationalist camp arguments that used to pass muster not long ago are now *passé*. So let us ignore the Establishment whenever possible and appeal to public good sense instead. Not all critics of Popper are Establishment, and not all who are violate etiquette blatantly. I salute them all, since my aim is to promote (not Popper but) the standard of critical engagement. Fair play will benefit us all.

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Chapter 4
Rules Against Excessive Defensiveness

4.1 Let a Thousand Flowers Bloom

Toleration is obligatory, not criticism: we may demand toleration but not the critical attitude. We may however try to encourage it. The best way to do so is to help people learn to distinguish between appropriate and inappropriate criticism. This will make them learn to enjoy it. This is tricky, however, since appropriate criticism may turn out to be invalid, and some of the worst diatribes may inadvertently include pearls (as Stefano Gattei's has noticed in his review of the Lakatos-Feyerabend correspondence). Valid criticism of the classical theory of rationality as proof left rationalists with no theory of rationality. Two candidates compete for this position. One is the relativism that views rationality as relative to intellectual frameworks and thus blocks criticism of frameworks and thus makes criticism insignificant. The other is Popper's critical rationalism that identifies rationality with the critical attitude. Critical rationalism shares with classical rationalism the appreciation of criticism and the view of the truth as absolute. Critical rationalism shares with relativism the readiness to entertain diversity of opinions. Classical rationalism takes scientific opinion as compulsory, since the truth is one and morally compulsory. Critical rationalism allows for diversity out of ignorance—out of learned ignorance, that is. Relativism denies that the truth is one. Classical rationalism is frivolous as it lacks a theory of empirical proof; relativism is more frivolous as it blocks criticism of intellectual frameworks.

As Plato noted (*Gorgias*, 506c), hostility to criticism is similar to hostility to medicine. Hating it is beside the point; welcoming it, gladly or reluctantly, is due to its value, intellectual or practical respectively. Criticism is valuable as it eliminates error; those who hate it apply it furtively. Furtive change is self-deception; it often confuses and wastes time. It is not clear where Popper's popular critics, Kuhn, Feyerabend and Lakatos, differed from him. It seems they differed from him in the way they addressed the public: he addressed people who appreciate criticism and they addressed people who hate-criticism-but-refuse-to-say-so-out-loud—the furtively anti-critical. Popper's older critics had distorted his views (as do their current heirs, such as Putnam 1975, 281). Popper's popular critics did

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not. Rather, they used against him furtively arguments that he had devised. This is understandable: these critics had no clear criticism of what Popper said, they had to put their objection to his high standards of criticism as if it were criticism, but without admitting that they shun criticism. Their new idea is thus that the admission that some criticism is valid is required only under some conditions and discussing at length these conditions and offering historical examples.

4.2 The Evasion of Criticism

Many critically-minded people found Popper's popular critics interesting. They deserve to learn that there are better things to do, for example, to try one's hand at serious criticism of Popper. He has effectively criticized his predecessors' solutions to some standard problems, and offered alternatives to them that do not suffer from that criticism. By contrast, Popper's popular critics did not discuss his criticism nor did they approach his ideas as solutions. They offered criticism of his views with no reference to their background.

To begin with, no criticism is ever final: no criticism is unanswerable (Popper, *The Logic of Scientific Discovery*, 1959, §22; Wettersten 1992, 160, 164). Anyone knows this who knows something about scholasticism. The question is not, is some criticism answerable? It is, how good is the extant answer to a given criticism? Wise critics tend to ignore answers that are too poor. If parties to a critical debate feel that they have to go on as long as they can, it will never stop. They may thus engage in single lifelong debates. In our society debates usually stop when they bore participants. Debates can be suspended: it is always permissible to answer to criticisms saying, I have forgotten my answer and I need time to look for it, or I am tired or distracted, or I want to consult my friends. Here is a wrong answer: I have to consult my authority. It may be a priest, a guru, or a party organizer; it is regrettable as it is the surrender of autonomy. Nevertheless, we rightly consider some people authorities: they are fallible, but as learned and judicious they may help us improve. Upon an encounter with some strange astronomical assertion, it is wise to seek the response of the Astronomer Royal to it before rushing to condemn it in public. One may dismiss what the Astronomer Royal says, but not off-hand. To conclude, in many cases it may be wise to suspend a debate, but not indefinitely unless stagnation is preferred.

All this pertains to personal responses to criticism. The social dimension of criticism is different. It needs moderators of sorts, and thus some responsible leadership. Today the irresponsible conduct of the leadership in the history and philosophy of science invites responsible response.

4.3 Popper for Criticism in Science

As browsing in the literature easily suggests, the attitude of the philosophical leadership towards Popper is unfriendly. He objected to the demand for conclusive empirical proof that most of them advanced. They ignored that demand as they objected to his view by claiming that no argument is conclusive. Although this appears in Popper's first book (Popper 1935, 1959), leaders repeatedly used it against him as if he was ignorant of it. He used it against the naturalism that (following Ludwig Wittgenstein) they were cultivating; he disagreed, noting that naturally the choice to accept or reject criticism is always open. They overlooked this criticism.

To repeat, Popper always opposed the idea that the leadership of scientific philosophy took for granted: in principle, they said, the truth value of every statement is decidable—logically or empirically. Those who note this brand him a conventionalist. This rests on the ancient classical theory that there are two and only two kinds of truth, truth by nature and truth by convention and that only proven truths are on the agenda for serious discussion. Popper opposed both naturalism and conventionalism. In his first book already (1935) he argued explicitly against both. Naturalist philosophers demanded that we prove what we say yet they had no proof procedure. The conventionalist philosophers advocated the following procedure: valid criticism of valuable theories should lead to altering them minimally—as little as the criticism requires. Against both schools Popper suggested rules that encourage criticism and discourage making light of it. He suggested going for maximal openness to criticism. True, criticism is not always valid; likewise, the validity of valid criticism in itself need not impose it, as it may rest on falsehoods. Thus, when Newton's theory was taken to be true, all criticism of it was carefully checked for validity, and when it turned out to be valid, its premises were declared false. That is, whenever some observations were found to be in conflict with the theory they were declared erroneous. This was not dogmatic, however, as they had to reveal the error, and they did—once as the result of an optical effect previously ignored (aberration), and once as a result of the oversight of a planet that caused the observed deviation. This way an unknown planet was discovered. Popper's popular critics take this as evidence that science is dogmatic, that it resists criticism. This is rather silly, since all it shows is that scientific criticism is open to criticism.

4.4 Lakatos Against Criticism in Science

The classical *Proofs and Refutations* by Lakatos applies Popper's critical philosophy to mathematics. He never withdrew that book or his early philosophy that ridicules the rule that advocates use of small amendments to rescue refuted ideas. His later works advocate this rule. By his terminology, he had two different philosophies, Lakatos$_1$ that promoted criticism and Lakatos$_2$ that did the opposite.

Lakatos$_2$ said, since any criticism of any thesis is answerable, no theory is really open to criticism. He discussed this in detail though it is obvious. Some commentators, for example Noretta Koertge, responded by seeking inescapable criticism. This is both impossible and unnecessary. Let me paraphrase the Lakatos$_2$ argument archeologically. Since broken pottery is repairable, no pot is really breakable. Query: is a thoroughly crushed pot still repairable? This query is a distraction. The very need to repair a pot is proof enough that it is broken and thus breakable.

Lakatos$_2$ disagreed (Lakatos 1978, 34–35); he sought constructive criticism (Lakatos and Musgrave 1970, 179); refutation, he said, only works then. Over a century earlier Dr. William Whewell went much further (Fisch 1991, 149, 195): refutation is a disappointed expectation, a counter-expectation. Disappointed expectations deduced from a clearly stated theory are refutations. Lakatos would not deny that expectations based on theories may disappoint, and that therefore the theories should be rejected. He said, a scientific theory is not a fixed set of statements (as Whewell envisaged), but a series of such sets, with a small set of shared statements, the metaphysics behind them, which he called their hard core, and these are too uninformative to be open to refutation. He nevertheless provided them with "protective belts" (Kampis 2002, 61). His image of science is a variant of Kuhn-style paradigms—except that the paradigm is not explicitly stated and the hard core is. Kuhn presented examples of paradigms from the history of science; Lakatos viewed his idea of the hard core not as a description but a rational reconstruction. Kuhn and Lakatos agreed that modifications of a theory in the light of valid criticism are unpleasant and their accumulation initiates a search for a new paradigm.

Query: is the criticized theory deemed false before it undergoes modification? Is it the prerogative of the metaphysical idea alone to be deemed true until it is replaced? Why? What causes a change—be it small or big—if not refutation? Is science empirical? Does science learn from experience? How does it progress? Where is the novelty of scientific discovery? Lakatos died young. Some of his followers stepped in and offered a theory of novelty for him: novel facts are unexpected. This idea belongs to Sir Francis Bacon. Since Bacon demanded that research should start with the deletion of all unproven theories, he had no room for the counter-expected, so that he was left with the expected and the unexpected. He then rightly viewed the expected as not new and concluded that the new is the unexpected. Whewell criticized Bacon, saying, the genuinely unexpected is not noticed. He said the new is expected on the basis of a new hypothesis that it verifies. The refutation of classical theories refutes him. Popper's theory suggests that the novelty of facts is their being counter-expected (Agassi 1975 Chap. 3). Modern perception theory, says John Wettersten, rests on ideas of Whewell or of Popper: it is nearer to Popper than admitted. Thus, bumping into people unexpectedly is common, and not surprising except on the assumption that one cannot bump into them—say, because they are celebrities or because they are reported dead.

4.5 Feyerabend Against Method

In his early phase Feyerabend offered serious criticism of Popper's ideas. He said then, since factual information is conjectural, the choice between theory and contrary evidence is given to decision, contrary to Popper's proposal of a rule for it. To keep with my example, we do not use our meeting with an allegedly dead friend as refutation of the theory that death is final. This example of mine is not good as the evidence is not repeatable. Feyerabend mentioned repeatable evidence, especially the refusal of Copernicus to take the evidence against his theory as refutations, a refusal that Galileo and Popper had praised. This criticism is easy to take care of. Popper suggested that the preference for declaring counter-examples true rather than the theories they contradict was meant as the default option: he was anyway careful to allow factual claims to be conjectural and to allow that the choice was subject to discussion. Feyerabend, however, suggested that there are no rules here.

He then changed; he toyed with the idea that there are no rules for research. This is an exaggerated way to saying, all rules are subject to reconsideration, of course. Yet he insisted on his exaggerations: to keep with my example, he allowed for the option that our allegedly deceased friend who walks down the street has come back from his grave. Some readers found this stimulating; others found it funny; still others found it dangerous; only few took him to mean it literally. What did he mean? He said he meant this literally. He also said, he did not mean anything in particular; whatever he said, he meant only as therapy. I do not know what his diagnosis is, nor what disease he was diagnosing, or whose. He said that the claim of science for superiority is imperialist; science is a culture like any other. In a published letter to me he reported that, being a chronic patient, he personally preferred alternative or folk physicians over scientifically qualified ones. He did not say there, but he added to me personally, that he never allowed these to treat him.

Rejecting scientific method, Feyerabend allowed replacing science with any alternative (Feyerabend 1975, 1993, Introduction to the Chinese Edition). Thus, meeting our allegedly dead friend, he suggested, we may suppose that he has come back from the dead—if we want to: there are no valid rules against this as there are no valid rules.

Popper's popular critics reason thus: no criticism is unanswerable, as no observation is free of a theoretical bias and as all theory is fallible. This argument is redundant as all observations are possibly false anyway. The bias signifies as it renders unreliable the use of observation as support to theory; the use of observation biased in favor of a theory against that theory is a *tour de force*. Other options exist. Bunge said, it is advisable to accommodate criticism by making first a small concession to it, in the hope of refuting it, and repeating the exercise in the hope of having to intensify the modification, thus ushering in a scientific revolution.

4.6 Kuhn for Constructive Criticism

What rules do/should help decide between conflicting theory and observation? Duhem and Poincaré said, decisions should be agreeable, as theories are useful tools. This sounds like Feyerabend's view, but it is not. He said, decision is arbitrary; they offered a rule. Their rule is not easy to use. They said, simplicity is usefulness. Yet useful aerodynamics is complex and useless general relativity is simple. To be useful, they both insisted, theory must be trimmed to fit evidence. They were in error: practitioners apply theories while using their refutations to avoid known errors. Finally, unlike Popper's popular critics, Duhem and Poincaré rightly abhorred the indiscriminate rescue of theories from criticism; their advice to cling to Euclidean geometry was more reasonable though still an error.

Enter Kuhn. He took Einstein for granted. He said, Einstein has offered an alternative to Euclid. Poincaré had disregarded the alternative because of his rule of choice of a theory: he deemed Euclidean geometry the simplest. Kuhn learned from Michael Polanyi that the choice of a theory need not and cannot obey explicit rules: they are tacit. This sounds like Feyerabend's view. It is not: he says choice is arbitrary; Polanyi says, scientific leaders choose responsibly following tacit rules.

Popper followed the tradition that deems all criticism enlightening (Christakos 2010, 66, 115, 122). By Popper's popular critics, refuted theories live on borrowed time till alternatives replace them. This is the theory of constructive criticism often attributed to Lenin though it is older (Hartung 1945, 121). It was Lenin, however, who dismissed criticism lightly, demanding alternatives and attacking them: The best defensive is the offensive method, he said (Laski 1923, 47). He thus clung to his harmful practices. Now admitting criticism of a practice leads to the search for a better alternative to it. At times the practice is suspended with no alternative and at times it persists till replaced by legal reform. Popper's popular critics thus confuse theory and practice. They say, new alternatives, not refutations, cause change of scientific opinion. They discuss prevalent beliefs and their changes as if they apply a theory of it. They have none.

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Chapter 5
Against the Bouncers at the Gates of Science

Current philosophy of science is peculiar in that it is a prestigious, specialized section of an academic department and yet its members endorse views current among scientists, especially physicists: rather than show the way, they follow. The major non-scientific worry of physicists concerns funding expensive experiments of pure research. They often justify this funding by reference to the potential applications of pure research. Thinkers of the Age of Reason were very different. They valued science as the search for the truth. They also valued it for its potential to promote peace and prosperity, but only as derivative value: science is useful, they taught, only as long as its practical aspects are by-products of the search for the truth. Modern philosophers of science often value truth and truth-surrogates for their utility (following Carnap 1950).

Philosophers of science consider themselves the bastion of traditional rationalist philosophy. What they share with their classical predecessors is the identification of science with rationality and the hostility to metaphysics that the latter promotes. For, standard discussions of the philosophy of science ignore the philosophy of science that obscurantist philosophers expound, largely because many pro-science philosophers of science reluctantly agree with the obscurantist philosophers about the importance of science as merely practical. This view that they share is not to be taken seriously. There is nothing practical about the scientific view of the dinosaurs yet it is an important ingredient in the formation of the worldviews of all people with the slightest familiarity with western ways of life and it is a typical bone of contention between rationalists and irrationalists. Current philosophy of science opposes obscurantism and irrationalism with no reference to usefulness. On this they follow the tradition of analytic philosophy, especially the "Vienna Circle" (Carnap included), of opposing churches that supported some of the worst political movements ever.

The battle of the "Vienna Circle" against obscurantism is laudable *tout court*. Their strategy is not: it insured failure. They battled evil by denouncing metaphysics—as violating the rules of grammar. This way they included in the target of their criticism many decent people: they could not distinguish between liberal and reactionary clergy. This is no accident. The greatest difference between them and the Enlightenment concerns openness. Enlightenment-anti-metaphysics opposed

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official Aristotelian philosophy, thus boosting openness; the "Vienna Circle" advocated whatever scientists were advocating and condemned the rest, thus playing bouncers. Although their view of metaphysics as ungrammatical has been tacitly dropped, philosophy of science is still struggling with its anti-metaphysical tradition. It was always unnecessary. As Paton (1951, 13) said, the business of philosophy is to be synoptic. This contrasts with Ernst Mach's claim that the sum of all extant scientific theories was his synoptic philosophy. The inadequacy of this idea matters less than its inadvertent advocacy of the elitism that invited bouncers to enforce as rational only scientists and/or uncritical promoters of science.

Valuing science as practical, philosophers of science deplore views that defy current scientific consensus as metaphysics. They defend the consensus despite their ignorance of it that is scarcely avoidable since familiarity with all science is hardly possible. It looks safer to combat religion and magic. (It is better but harder to argue against the use of religion for reactionary ends.) Feyerabend (1975, Chap. 5, n. 25) based his misguided advocacy of magic on his just complaint that philosophers of science know too little about religion or magic. Only a few philosophers, he observed, discuss them by the standards of current social anthropology (i.e., without hostility). He exaggerated: philosophers have no trouble recognizing scientific texts. Nevertheless, things are worse than he indicated: most philosophers cannot distinguish the properly scientific from the pseudo-scientific: they cannot help editors of scientific periodicals decide whether a manuscript deserves publication, or appointment committees decide on the competence of candidates, or grant committees decide how to distribute their funds. Rather, they take as proof of scientific character past decisions on such matters. This conduct wins them their academic jobs, as it pleases scientists on academic appointment and tenure committees. Ignorance and incompetence of candidates do not matter then, since the job description of bouncers permit these.

This flouts the public interest. Philosophers-bouncers who rally public disapproval of what scientists consider the competition tends to lower the standards of science. Popper's popular critics strengthen this tendency. Kuhn and Lakatos explicitly advocated bouncing. Following Michael Polanyi (Nye 2011, 305), Kuhn expected leaders to act as bouncers (Nye 2011, 235, 252). He did not say who they are. His writings convey the impression that these are famous researchers, yet they are actually the philosophers of science who influence academic administrators. Lakatos was more direct: he appointed himself arch-bouncer. Gerald Holton exposed him (Holton 1974, 76) and thus reduced his influence. Feyerabend opposed all bouncing, posing as an anarchist (Feyerabend 1975, Preface). Anarchism is notoriously ineffective: its extremism makes it inapplicable.

Even bouncers bump into the question, what is science. It is a body of knowledge; it is what scientists do; it is a tradition; it is any empirically involved research activity; it is a faculty in the university. All these are true and meet the question. Hence, the question is vague. What then is non-science? It is dogma; it is magic; it is what traditional shamans do; it is a rearward way of life. What differentiates science from non-science? This question invites serious study: it is more social than intellectual. The study of alien cultures is highly recommended;

bouncers will not condemn it. Controversy about alien cultures abounds; it is thus hard to contrast them with the scientific culture. Worse, the severest bouncers, the "Vienna Circle", could not block all suspects from entry. Can we leave them for a while and look at science rather than at the competition and find there some clear-cut characteristic that sets it clearly apart from all else? How? (Case 2007, 72, 77, 141–142).

Tradition supports science as a body of empirically backed theories. With the (alleged) use of modern logic, it underwent a radical change: scientific character shifted, as logic handles not theories but sentences and not the proven but the provable. Tradition rejected the negations of scientific theories as a matter of course; Ludwig Wittgenstein decreed the negation of a scientific sentence also scientific, demanding not proof but proof or disproof, namely, decision, and not only as regards theories but also as regards every sentence. The inclusion in science all sentences and their negations should have relaxed the rules that bouncers use. Instead it tightened them: bouncers declared theology worse than non-science: its sentences are non-sense. And then Kurt Gödel proved that even in mathematics decidability is not always attainable. In computer science decidability may be empirical (in a new sense of empirical, though): a computer may decide the truth or falsity of a sentence; decision on some sentences may take a very long time or forever; and so it is not known whether a problem is decidable or not (Harel 1987, Chap. 8.) This is a knockout for the "Vienna Circle"; as its members were all bouncers, they refused to acknowledge this verdict (Rodych 1999).

A recent sociological tradition takes a different road, viewing science as people—as a prestigious social class (Shapin 2009). Are scientists then like shamans or prophets? Polanyi and Kuhn clinched the difference between the traditional and their sociological view, by admitting that science is esoteric: scientists are indeed more like shamans or prophets than common people: science is ineffable personal knowledge, a trade secret that master transmits to apprentice. Scientists are not like shamans and prophets, however, as they do deliver the practical goods.

This is not good enough: appreciating science only due to its successful forecasts is insufficient: not all true forecasts are valuable. Appreciating all true forecasts is incompatible with the telling observation of the prophet Jonah: terrible forecasts are better unrealized. Also, successful forecasts may mislead. Most philosophers of science disagree: they share Bacon's forecast: science is power; it puts us in control. This is comforting scientific-establishment talk that ignores the threat of destruction from the proliferation of nuclear weapons, from pollution, from the population explosion, and from the ever-increasing gulf between rich and poor nations. Admittedly, more science is needed to reduce the threat and more democracy (Agassi 1985, final chapter). Scientific establishment talk is reassuring: more grants will bring the desired solutions. Philosophers of science boost this assurance. This makes them false prophets. Where does the success of science come from? Is it that successful? Research is guesses, tests, and eliminations: science is not as successful as it looks. Philosophers of science systematically conceal its failures in their wish to be useful bouncers. The vulgar view of science as success leads to treating it as successful magic. The view of research as esoteric opposes the traditional idea of research as open to public scrutiny. We have then traditional philosophers who advocate openness on the strength of the view that science follows simple assured rules, and those who reject both openness and simple assured rules. Kuhn has suggested that although there is no simple rule open to everyone to apply, puzzle-solving is something like this: the people who can apply it are good, professional researchers, but not the very best. This is an improvement on Polanyi that should reduce bouncing. It does the opposite: Kuhn followed Polanyi in advocating total autonomy to science with no democratic controls, internal or external. He did not specify beyond demanding no government intervention and no internal democracy of any kind. Bouncing comes then naturally and grows with the prestige of science in society at large (Reif 1961).

Sociological observations reveal that science follows rules. But it also permits breaking a rule—openly and at one's own risk. It is advisable to begin the sociological research by admitting ignorance. No one knows how science grows and whether the increased stress on practical, technological aspect of research improves matters or not. Technological end-products do bring research nearer to common experience than basic research can, but the great complications that technological research unavoidably adds to basic research make it go the opposite way. Observing what technological research is judged scientific is insufficient, as decisions are too often too poor and often motivated by accidental factors. With no sufficient knowledge of what research is to be judged scientific, it is never clear enough what paper deserves publication in the scientific press and what rules that editors use are right, what appointment is proper, what grant-decision is: more information and deliberation and experimentation are required. The traditional problems in the field are now open to empirical study: the sociology of science can help develop our philosophy of science. This should be exciting.

The sociological approach must first oust the psychological approach. Kuhn has caused much confusion (Bird 2002) by claiming that the failure of traditional philosophy of science leads to the search for a psychology of research (Kindi and Arabatzis 2012). This is very odd, since tradition was psychological almost all the way, laying great stress on the credibility of scientific theories. Do researchers have to believe? Kuhn says they must, that their change of belief is a religious conversion, and that those who fail to believe in the dominant scientific dogma lose their jobs. Why is it not enough that they solve puzzles? Obviously, competing intellectual frameworks, competing paradigms, offer researchers options, and liberal philosophy says, the more options the better. This should free the theory of scientific research from its current obsession with rational belief. Sir Francis Bacon's superbly intelligent and highly influential doctrine of prejudice says, false beliefs poison research. To become a productive researcher, then, one must give up all one's preconceived beliefs. This theory is magnificent; also, it is amply refuted. Now while belief matters little for research, it possibly matters for action. This is empirically refuted too: the applications of new technologies follow the laws of the land, and these do not refer to beliefs (Agassi 1985, Chap. 1).

The study has hardly begun of science as a central item in culture and of its interaction with other items in it. Bouncers isolate it. Nothing human should be alien to any philosopher, philosophers of science included. Certainly the idea of the siblinghood of humanity that is at the root of all science is not scientific and yet it is inextricably linked to the very possibility of science as a human enterprise. Whichever way we look at it, we do better to avoid bouncing anyone from the halls of science.

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Chapter 6
Duhem, Quine and Kuhn, and Incommensurability

The popular critics all thought the famous Duhem-Quine problem/thesis was a fatal objection to characterizing science as falsifiability. What is the Duhem-Quine problem/thesis? This question has no obvious answer. Roughly, it relates to an observation of Duhem (1906, 1954) included in a brief rider to his discussion of the inability of crucial experiments to act as proofs: at most they disprove. But, he added as a rider to his rider, even that is not clear-cut.

A universal hypothesis never follows from particular information, since it goes beyond the given; its negation can follow. This is a part of classical logic. Bacon and Popper stressed it; Duhem gives the impression of having rejected it but he did not; he clung to logic, and even emphatically so. A scientific prediction rests on some hypothesis plus working hypotheses about instruments. These latter may be faulty (say, regarding calibration): *a hypothesis cannot be tested in isolation from them*. Perhaps the *Duhem-Quine thesis* is this. And then, the deduction of a false prediction proves only that at least one premise is erroneous. It is for the researcher, Duhem added, to decide which premise is false.

The literature offers this thesis in six variants: it is impossible to test/confirm/refute an isolated hypothesis in physics/science. So perhaps it is better to speak here not of the thesis but of the problem. Roughly, it is the question, *how can we coax experiments to yield clear-cut verdicts*? The answer to this is the Duhem/Duhem-Quine thesis/Quine's under-determinacy thesis/paradox. It says, there is no way to do this. Is Duhem's thesis identical with Quine's? We have it from Quine's pen that they differ. Now Quine repeatedly declared that perfect translation is impossible, and thus perfect synonymy (translation within one language) is impossible too. Duhem's and Quine's versions then must differ. Hence, in this discussion he waived the no-perfect-synonymy thesis. Hence, his claim is, the two versions are even not nearly the same (Ariev 1984). It should then be easy to show this by putting the two side-by-sides. It is not. Some commentators say, the variance is in the range of applicability: Duhem limited the thesis to physics and Quine stated it generally. This is misleading since Duhem too allowed for the thesis's broad application. Moreover, as this does not make their theses differ about physics, possibly there they are (near-) synonyms. The difference lies

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elsewhere. To show this, let us spread our net wider. This may sound evasive. So let me observe that we can simplify and broaden the thesis while raising no objection. The expanded thesis is scholastic: *every criticism is answerable*. It is familiar since antiquity. It sounds surprising because we unthinkingly rule out some responses to criticism as illogical. The thesis becomes much less surprising once we realize that it ignores all such exclusions. (Adolf Grünbaum rightly noted (1960, 79) that limiting answers to reasonable ones changes the picture totally. Indeed, Duhem agreed when he relied on "good sense" for the endorsement of some refutations even though it is not obligatory.) *Every criticism is answerable, but not every criticism is adequately answerable*. Inadequate answers are excuses or evasions.

What are rules for the adequacy of an answer to criticism? Following Socrates, let us notice some popular inadequacies. One is to repeat the criticized assertion. Another is to deny the counter-example because it contradicts the hypothesis in question. The response that Bacon has repeatedly condemned is to declare the counter-example irrelevant — say, it comes from another field. Also, it is possible to dismiss critics as aggressive. What is the right response to these faulty moves in debates? Privately, it is best to cut it short. Publicly, violation of the rules of debate may merit exposure. The adequate discussion of the rules for (adequate) dialogue has hardly begun. When parties to a dialogue cannot agree about rules they may engage in a meta-dialogue. This may break down and stop the dialogue. Judges and legislatures try to avoid this. At times they offer inadequate answers in lieu of adequate ones.

Minor amendments to save positions are more reasonable in (scientific) technology than in science proper. The reasonable use of them in science is in its technological aspects: experiment involves instruments and (remember Duhem) these may be inadequate. Hypotheses about instruments are called working hypotheses. When prediction fails, it is always permissible to declare the relevant working hypothesis faulty. Faraday looked for the quadratic electro-optic effect (Kerr's effect), failed, and repeatedly put the blame on the insensitivity of his instruments. After he died John Kerr retried the experiment with success. Was this an improper evasion that luckily turned into success? Possibly. When a researcher offers an excuse or an evasion, peers show patience and wait for some further move. If none turns up, then they expect a withdrawal or ignore those who cling to their hypotheses. As Duhem noted, this logical situation renders verification impossible—even for crucial experiments. As full empirical certainty is impossible, and as science is utterly certain, he declared that science must be merely applied mathematics. Duhem then described relative certainty, namely the utter certainty of a hypothesis within the domain of its application. This was a mistake: utter certainty exists nowhere. Einstein has observed a more interesting kind of relative certainty: having to give up some hypothesis, researchers try to blame the shakier hypothesis first. (This proposal is central to Popper's philosophy. The only comment on it in the literature, it seems, is that Peirce said it first. Indeed, he did; Peirce 1879.) Thus, one should test one's working hypotheses before using them:

they are insignificant except for their assigned tasks. This indeed is a common practice.

The difference between Duhem and Quine is about history. Duhem admired scholasticism and suggested following its methods: try to stay as close to your refuted hypotheses as logic permits; deviate from them minimally. Quine did not suggest this rule but noted it as an option. In practice researchers try whatever they can in the hope for progress. Thomas Kuhn assumed the existence of turbulent revolutionary periods and periods of calm. This assumption does not concern people who always work within dictated grooves, those whom he was pleased to call "normal scientists". Kuhn's term "incommensurability" designates Duhem's view (Agassi 2003). Two scientific theories may contradict each other, as Kepler's and Newton's do, but they should not be put side by side; they should be taken as separate mathematical systems, and as such they constitute implicit definitions of their terms, and so they comprise different languages. (The idea of implicit definition is Duhem's; it entered the mainstream via the writings of Poincaré and Hilbert.) And as sentences from different languages, they cannot contradict each other. One may object to this, claiming that when they are translated into one language they do contradict each other. This, Duhem responded, is impossible: "*traduttore, traditore*": perfect translation is impossible. Duhem was not against comparing theories, of course. Viewed as applied mathematics they have degrees of (certain) truth determinable by their domains of applicability—and these are (partly) comparable.

Applicability is context-dependent and this requires that Duhem's theory should undergo modification. He did not deny the concept of the absolute truth; he only divorced it from science. Reluctant to call a scientific theory false, he had to make do with relative truth. Admitting the equation of rationality with proof, he had to allow for both demonstrability and fallibility. Now, the absolutist need not deny the existence of relative truth. It is the relativist who, by definition, rejects the absolute truth. The admission of the absolute truth as a regulative idea (in Kant's sense), then, permits the reinstatement of Duhem's relative truth within Popper's system. Popper could allow for this as he has replaced the equation of rationality with demonstrability with its equation with openness to criticism. Kuhn objected to this, as he objected to the very concept of the absolute truth. The rationality of science, he added, lies in its conformism. This makes it difficult to know what he thought of logic. It also raises the question, could he be a realist, a non-idealist, and this question is difficult too. Finally, as there is conformism outside science, and it is not usually seen as virtuous, what is scientific conformism and why did he consider it a virtue?

Kuhn said, he was wrongly accused of having denied that comparison between theories can ever take place (Agassi 2003), although he explicitly and unequivocally said, of course theories are comparable. In what sense? This question invites a theory of comparison. Consider the original incommensurability: the length of the diagonal of a square and its side are easily comparable by rotating one of them by half of a right angle and seeing that the diagonal is longer than the side; their lengths are not comparable if comparison is restricted to the use of whole numbers the way Pythagoreans wanted. Two pictures can be compared as to area and as to the price they may fetch; the one with a smaller area may fetch a higher price. A theory may be a better approximation to the truth yet be less well applicable in most practical situations: we often prefer to apply Kepler's laws or Galileo's laws to Newton's. Kuhn said that Newton's and Einstein's theories cannot be compared as the word "mass" is used in each in a different way. Why should we endorse the restriction to the use of the one terminology rather than to similar ones? Kuhn never explained.

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Part II
Popper and His Popular Critics

Chapter 7
Karl Raimund Popper (1902–1994)

Karl R. Popper is "the outstanding philosopher of the twentieth century" (Magee 1997, 199). He felt affinity with thinkers of the Age of Reason and developed a new version of rationalism—critical rationalism, as he called it (Popper 1945, Chap. 24). He was a very influential philosopher of the post-WWII era—as a champion of both science and democracy. He was a close follower of Bertrand Russell and of Albert Einstein. Insofar as Russell adumbrated Popper's philosophy, (as Hattiangadi and Wettersten have argued) it may be fair to view it as a streamlined version of Russell (the way both Berkeley and Hume viewed their philosophies as streamlined versions of Locke). Both Russell's and Popper's contribution to philosophy, each in its own way, amounted to a critique and a modification of the whole Western rationalist tradition. Russell raised the level of rational discussion in philosophy, but remained within the empiricist tradition; Popper did more than continue and consolidate Einstein's and Russell's philosophical achievements: he set new ways of philosophizing. Many philosophers sought to find a *via media* between rationalism and irrationalism, between individualism and collectivism, and between radicalism and traditionalism. Many philosophers of science sought to find a *via media* between empiricism and intellectualism. Popper's fallibilist-reformist philosophy is the only known, viable and comprehensive rationalist suggestion of this sort of *via media* (although it is open to modifications, of course). Popper has thus achieved a new and intensified common sense philosophy, and the only one that is integrated. "My theory of knowledge, my philosophy of science and my political philosophy are original only in their interdependence," he said in a 1976 interview (Hacohen 2000, 505).

This integration or interdependence differs from what traditional philosophers claimed for their systems, as it rests on fallibilism rather than on axioms. His fallibilist commonsense makes his influence pervasive and thus hard to detect. His brand name remains the particular ideas of his views on science and on democracy. His views of science are elaborations on Einstein's; they have impressed his peers

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most. His views on democracy are elaborations on Russell's; they have influenced the educated public at large much more than his philosophical peers, who still largely support elitism, paternalism, and technocracy. Regarding science, he was uncompromising in rejecting the view that the method of science is inductive in any way (endorsing a view of Einstein's) and he proposes the alternative idea that *we learn from experience by refutations*, so that the empirical character of scientific theories is shown by their clashes with experience (if they are false), by their refutability. To this Popper added as a corollary to his idea of refutability the demand (of Einstein) that new scientific theories should incorporate older successful ones as approximations and as special cases, so that scientific theories may hopefully serve as series of approximations to the truth. Among peers, Popper is still reputed as a philosopher of science and as the chief detractor from the analytic or linguistic or "logical" positivist movement, criticizing their claims to be progressive and exposing them as conservative or worse. For the educated public he is the champion of democracy and liberalism, whose specific proposal is to replace the conventional effort to ground them in the sovereignty of citizens with efforts to improve them. Governments are imperfect but indispensable, hence in need of watchfulness. Democracy is the public's ability to overthrow the government by peaceful means; it is a right that characterizes democracy best, he said. The richness of his ideas comes into sight with the unfolding of the impact of these specific ideas on diverse areas of human activity. Finally, a word on his style: he is one of the clearest and most readable writers of his time.

7.1 Life and Works

Karl Popper was born in 1902 in Vienna to a respectable Jewish family turned Protestant. His father was a learned, well-to-do lawyer who lost his means after World War I. His mother was a talented amateur pianist of a Jewish family of prosperous merchants distinguished in music, the academy, and the professions. Both parents influenced him greatly throughout his life. The atmosphere of Vienna then also impressed him for life, with its enormous variety of conservative and radical culture, *avant-garde* art, psychoanalysis, "logical" positivism, Marxism, nationalism and anti-Semitism. Although for a brief while during his adolescence Popper was a Marxist and *avant-garde*-art follower, he soon developed a lifelong distaste for them. He disliked as vulgar the celebrated Vienna coffeehouse chic. (He associated with it the style of Wittgenstein's first book.) And he never forgave Austria for absolving herself from her enthusiastic welcome of the *Anschluss* and the *Führer*. Yet he loved Vienna passionately and tried to return there late in life. Though he was received with open arms, he could not stay; he soon returned to England where he died in 1994.

His early career was checkered. He dropped out of high school in 1918. He tried out briefly engagement in voluntary work for the poor and in manual labor. He matriculated in the University of Vienna in 1922, obtained a cabinet-making diploma in 1924, and a primary-school teaching diploma in 1925. He studied mathematics, music, psychology, physics and philosophy in the University of Vienna, where he received a Ph. D. in psychology in 1928. He received a secondary-school mathematics and science teacher diploma in 1929 and worked as a teacher in 1930–1936. In 1930 he married Josefine Anna Henninger, known as Hennie (1906–1985). They had no children. She was a great help to him till her death. Between 1925 and 1931 he published a few papers on education and on the psychology of learning. Two short notes appeared in 1935 in the "logical" positivist periodical that miraculously encapsulate the grand philosophy of science that he had expanded (1934) in his first vintage, his *magnum opus*, *Logik der Forschung* (1935). Its English translation appeared in an extended version as *The Logic of Scientific Discovery* (1959).

Popper visited England in 1935 in a vain search for a job, fearing that Austria would soon turn Nazi. The Poppers left Austria for good in 1937 for a senior lectureship in philosophy at Canterbury University College, Christchurch, New Zealand, where he stayed till after the war, writing there his *The Open Society and Its Enemies* (1945), on the strength of which the London School of Economics (part of the University of London) offered him a post as Reader in Logic and Scientific Method (1946), which he most gratefully accepted. He received a personal chair in 1949 and knighthood in 1965. He was elected Fellow of the Royal Society in 1976, and made a Companion of Honour in 1982. He retired in 1969, remained active and received many honors until his death.

Although he made contributions to all fields of philosophy, as well as to the history of ideas, the classics, and mathematics, the philosophical establishment showed him indifference and hostility. He nevertheless won a tremendous reputation with the general public and in the scientific community, mainly due to his two first-published books, *Logik der Forschung* and *The Open Society and Its Enemies*. He was a prolific writer to the very end, and his output remained highly significant with almost no exception. He preached individual autonomy, responsibility, and clear language; always concerned with public affairs, he advocated democratic control and moderate liberalism laced with Keynesian interventionism.

7.2 A Fallibilist Theory of Rationality

What is unique to Western philosophy, Popper said, is the establishment of the critical tradition that encourages critical examination of different answers to any given question. Behind this stands the recognition (a) that to depend on tradition and a traditional way of life is to depend on an accident, and (b) that this is unreasonable. This recognition raised the most central problem of Western philosophy: the general problem of rationality: what way of life is reasonable? What views and values deserve endorsement? That presupposes the availability of different options to choose from, perhaps with the help of thinking. Recognition of the importance of thinking translates the general problem of rationality into the special problem of rationality: what is the way to the truth? Thus, Descartes said, whatever I think merely because I am French and not Chinese, I do not want to think. How then do I decide what is the truth, what idea to endorse? Socrates said, I do not know the truth, but I do know some falsehoods to be false. Also, I should go on examining my opinions, since the unexamined life is not worth living. This is Popper's solution to the problem of rationality. His theory of rationality then is, (a) we should shun irrationality and admit the falsehood of refuted ideas, and (b) this is rational enough, as it permits progress. More rational is seeking answers to problems and examining them. Thus, in particular, there is no need and no possibility to reject traditions *en bloc*, but it is advisable to examine them piecemeal. It is not known how much of this Socrates saw, and whether he sought a solution to the problem of rationality. Possibly he did. His erstwhile pupil Plato ascribed satisfaction with negative solutions to problems to the sophists and he declared them all unserious. Following Parmenides he taught that all and only proven ideas are worthy of rational endorsement. Popper answered Plato on behalf of the sophists, observing that unlike Plato they supported democracy and that Aristophanes described Socrates as a sophist.

Plato's theory of knowledge is inadequate, since it comes with no theory of proof. Throughout the history of Western philosophy, almost all rationalist thinkers advocated Plato's idea that only proofs render theories rational, while, strangely, having no theory of proof. It was self-understood that some self-evident axioms need no proof and that inferring other propositions from these axioms comprises proving them. One may perhaps take this as the traditional proof theory. Euclidean geometry was the paradigm for this. For historical reasons, mathematicians considered Euclid's parallels axiom not quite self-evident. This led to the rise of non-Euclidean geometry and this in turn kindled proof theory. It began in the 1890s (Pierre Duhem, Henri Poincaré, David Hilbert), continued on into the 1930s (Kurt Gödel, Gerhard Genzen), and is still growing. What proof achieves, however, was agreed upon since antiquity: the proven is obviously true and thus in no need of modification or qualification; it comprises perfect, true knowledge.

In the traditional context, thus, science is the body of proven theories. Newton's theory was considered scientific in this sense but no it is longer so, since Einstein's theory has superseded it. As the body of theories taught in the faculty of science of any respectable institution of higher learning, however, science includes Newton's theory as a matter of course. What makes it still worthy of this distinction? This question is known as the problem of demarcation of science. Popper ascribed this problem to Kant; this is strange, since Kant stressed repeatedly that all and only proven theories are scientific. Popper's solution to the problem clashes sharply with tradition and seems counter-intuitive: a theory is scientific, he said, if (and to the extent that) it is vulnerable to empirical criticism.

Why counter-intuitive? Because traditional methodology stresses the positive. Popper showed that what we consider positive and what we consider negative is not iron-clad: the [positive] demands for simplicity and for explanatory power follow from the [negative] demand for openness to criticism. Under his unacknowledged influence, some philosophers stress explanatory power, others stress simplicity.

And then they argue that before Einstein came along Newton's theory was the best by their criteria. They accused Popper of skepticism. To this he offered two replies. First, (sharpening an idea of David Hume, William Whewell and of Henri Poincaré) he suggested that support stems from resilience in the face of severe criticism. Second, he said, in science endorsing an idea amounts to treating it as an object for critical study. (In technology, acceptance amounts to license to apply and readiness to do so.) Belief is irrelevant as it is personal and subjective. (The adoption of heretical belief systems may contribute to research more than shared beliefs.) All this diverges from tradition and so it seems counter-intuitive.

On further consideration, Popper's criterion of openness to criticism is not so counter-intuitive. As it turns out, it is minimal in the sense that all other criteria include it. (Hence, Popper appears more often in works that are critical of irrationalism than in those that advocate rationalism.) A common obstacle to viewing critical rationalism as commonsense is easy to remove. There is the confusion of refutability with refutation. Recognizing that some ideas are refutable in principle without being refutable in fact should remove it. It is easy to imagine some experimental setup that would refute a true theory were it false. It is even easier to imagine a true observation-report refuted. Thus, fallibilism is possibly not as counterintuitive as the idea that current science is perfect. Quite a few researchers said that they found it hard to view their ideas as perfect rather than as way stations. Perhaps Newton was the only one who insistently and constantly claimed perfection for his ideas. And although Descartes and Spinoza, for example, accepted the demand for proof as a matter of course, they invited criticism and claimed only that they did their best to clean their works of error. It is thus hard to admit and also hard to deny that possibly one's ideas are false. Charles Sanders Peirce declared himself a fallibilist (he invented the term) yet he deemed science infallible and could not overcome this obvious inconsistency. The first who did that was his contemporary Pierre Duhem, who declared science perfect only when fully divested of its empirical information (to become applied mathematics); when applied it becomes informative, he said, and then it is open to correction. (He nevertheless was no fallibilist: strangely, he deemed commonsense infallible.) Things changed radically under Einstein's influence. He said, and Popper cited him to say, insofar as geometry is certain it is not about reality and insofar as it is about reality it is not certain. Russell took fallibilism for granted, allowing certitude only for the uninformative formulas of logic. (Notice a sad terminological vagueness here: Russell endorsed skepticism as fallibilism and Popper rejected skepticism as the psychological doctrine that evinced no sympathy for science. Both were correct.) Einstein reported that fallibilism was essential for his research. This became the cornerstone of Popper's philosophy; his contribution was of immense significance just because it was a fallibilist theory of rationality as critical mindedness that he presented as an alternative to Plato's theory of rationality as proven truths. Plato viewed reason as infallible; Popper viewed reason as a means for approximation to the truth.

The difficulty of developing a fallibilist theory of rationality is evident in the popular theories that Popper tried to replace. They all offer certitude surrogates, usually “soft” certitude, usually probability in the sense of the calculus of probability. (This includes even Russell!) For, although a certitude-surrogate is no guarantee for truth, it is nearest to certitude and so it is (allegedly) the next best thing. As John Watkins has wryly observed, better proximity to truth than to certainty. Popper said, the theory of science as high probability implies that most extant scientific theories are true. Old science textbooks refute this. Also, that theory claims certitude for the questionable assertion that some evidence renders probable some universal theories. Such certitude, ascribed to a philosophical theory, is less defensible than the certitude that tradition ascribed to scientific theories. Also, that theory claims certitude for the questionable assertion that empirical information is certain: the probability of a given theory in the light of evidence is possible only if some evidence is given. Consider Carl G. Hempel’s objection to Popper’s claim that scientific theories are open to empirical refutation. Since evidence is uncertain, refutation too is uncertain. (On this Popper and Hempel agreed.) Hence, Hempel added, symmetrically, both probability and improbability come to replace proof and disproof. This is a howler. Take Eddington’s evidence that simultaneously refutes Newton and supports Einstein: Hempel’s criticism rests on the claim that the support it provides holds regardless of its uncertainty and the refutation that it provides holds only tentatively. That peers take this seriously indicates that they find Popper’s negative rationality too hard to take.

Thus, the theory that science is perfect and the theory that it is vulnerable to criticism are both hard to take: intuition deceives. It is especially difficult to explain the observed fact that claims for certainty and their justifications follow simple, familiar patterns. For, obviously, some certitude is misplaced. This is where ancient Greek philosophy stepped in with the question, when is assent rational? Plato went too far: he required that assent be given to all and only the proven: he demanded infallibility. His excess raised a strange problem, known as the problem of error. As error is avoidable, we should expect that people avoid it; why then do people err? The answer was a central part of Francis Bacon’s views of science that the whole Enlightenment Movement shared, from René Descartes to Immanuel Kant: people err because they deviate from the right method: they cut corners: they speculate and deceive themselves that their pet speculations are proven: they become prejudiced and they spread their prejudices and thus impede the natural growth of knowledge. Russell rejected this traditional view: total freedom from prejudice is impossible, he said; he called it humbug. The battle against prejudice is thus endless.

7.3 A Fallibilist Theory of Logic

In line with this Popper presented logic as the theory of dialectic, of the art of conjecturing speculations and then seeking criticism of them. He viewed the theorems of logic as the margin, as the statements that happen to be immune to criticism because they represent the canon of criticism in parallel to the anti-theorems of logic that are statements immune to endorsement because they are false due to the same canons: a contradiction is defined as false in all of its substitutions, so that any dialogue that ends with any contradiction has reached its goal, i.e., exposure of falsity. If a statement is consistent, then it may be false and a critical dialogue may come to test it, to find contradictory statements, ones that contradict the statement under examination yet are endorsed, so that the view under scrutiny cannot stand. This situation is rare; it is more likely to happen with a theory that is a set of statements rather than an isolated statement (although this is not a matter of principle since a theory is equivalent to the conjunction of its axioms). And if we find a very rare theory that happens to be consistent with all the available information, then the dialogue about it has to go in the direction of testing it further, of seeking new information to contrast it with. This, said Popper, is empirical science. Thus, although the growth of ancient Greek ideas was admirably dialectical, as the ancient Greek theories developed in a series of admirable dialogues, most of them were not scientific in this sense: as it happened, few of the ancient dialogues referred to new empirical findings.

(This raises a question about the content of Popper's theory. As it evidently considers Newton's theory both scientific and false, how does it view refuted myths and folk theories? This question is intriguing but seems hardly important. Few empirically refuted primitive views should count as scientific; the flat-earth theory that Eratosthenes of Cyrene refuted should.)

Popper considered science a rare bird: most theories we have are too vague for scientific tests, and then criticism of them is not definitive. Among these are some metaphysical theories, some political ones, and more. (Even the noble theory that all humans are siblings (the rational unity of humanity), that is at the root of science, is obviously irrefutable.) Science then is the peak of critical activity and critical activity is the peak of thinking. Creative thinking may lead to new criticism of received opinions and it may be triggered by extant criticism (the hen and the egg). All this contradicts sharply the idea that all meaningful assertions are scientific, an idea that the "logical" positivists of Popper's time found in the *Tractatus Logico-Philosophicus* of Ludwig Wittgenstein. Popper noted that this is their solution to the problem of the demarcation of science, and that for it to hold water it must, at the very least, demarcate meaning very sharply (as Wittgenstein declared in the famous closing sentence of his book) or else the problem of the demarcation—of science and of meaning—would reemerge. This criticism was deadly, since the "logical" positivists noted that as universal theories do not follow from observation reports they are not certain and so they had to grant theories a lesser degree of meaning and so a lesser degree of certitude. The problem of demarcation then reemerges as the question, what degree of certitude marks a theory as scientific?

7.4 Between Science and Metaphysics

Enlightenment philosophy cultivated hostility to metaphysics as speculative and thus as possibly misleading. In the future, said Bacon, as science matures, it will generate a scientific metaphysics. Before that, premature metaphysics is prejudicial. In line with this, intellectualists oppose not metaphysics but speculation; Descartes declared his metaphysics proven; Kant declared all speculation contraband and recommended their suppression (Preface to *Critique* A). He proscribed speculation to eliminate his antinomies or paradoxes, namely, the contradictions that he claimed to have proven. Following him, Ernst Mach proscribed involvement in metaphysical controversy. He violated this rule when he argued against Newton's theory of absolute space instead of dismissing the question, does absolute space exist? This was a boon for Einstein and thus for us all. Nevertheless, Mach's taboo on metaphysics won popularity and even encouragement from Einstein's critique of Newton. Fortunately, Einstein did not share this view, not even in his early days, much less when he found that avoiding metaphysics impeded progress on his study of gravity.

Popper first endorsed Mach's taboo on metaphysical controversy. His *Logik der Forschung* excelled this way. In particular, he offered his theory of observation as indifferent to the dispute between realism and idealism (declaring his partiality for realism personal). In *Logik der Forschung* Popper also avoided discussing truth and instead spoke of contradictions that are unquestionably pernicious. Popper changed his view on this and reported repeatedly that the change was due to Alfred Tarski's theory of truth that deeply impressed him. This would have chimed with Popper's early philosophy if and only if Tarski had managed to close the dispute about truth. He did not. In the meantime, Popper changed his philosophy after his move to London. He was dismayed to learn then that (under Wittgenstein's influence) most English philosophers rejected realism as metaphysical. It made him advocate realism and thus make peace with metaphysics.

Already in his *The Open Society and Its Enemies* of 1945 he observed that dismissing metaphysics as meaningless is an error that nourishes superficiality. Thus, he said, those who declare the word "soul" meaningless should nonetheless endorse the rule to care for their souls and discuss this rule while avoiding using that word. He had also said, determinism is a discouraging metaphysics, but not necessarily so, as long as its adherents uphold the commonsense idea that they can choose to act responsibly. This renders their opinions inconsistent, but he would not object to their holding an inconsistent metaphysics as long as they behave responsibly. This is very thoughtful, although Socrates might have disapproved. Still, Popper was still deprecating metaphysics yet already advocating commonsense metaphysics. Classical rationalists would object that this is dangerous as leading to prejudice; Popper would answer that both commonsense and metaphysics are and should remain open to criticism.

Popper developed his indeterminism into grand-scale metaphysics. First, he criticized reductionism, the identification of theories about humanity with theories about computing machines of sorts. He then developed his *Postscript* to his *The Logic of Scientific Discovery* to criticize determinism in three volumes that suggest repeatedly that determinism is problematic and has no argument in its favor. This is still problematic, as Popper's chief asset is his taking scientific theories literally—as true or false (*à la* Frege)—as objective; probability in general and quantum theory in particular are objective as well. He viewed the probabilities of the theory of probability as propensities of the physical world that are more general than forces. His applied realism to the mind-body problem and declared the mind real. This is not quite real in the Cartesian sense, since the doctrine of the substance is by now *passé*. Popper should have admitted diverse kinds of reality and discussed them. He never did. He then surprised his positivist peers most by speaking of objective knowledge. This is odd, since we all speak of the fund of knowledge and we have no trouble recognizing ideas that were utterly forgotten and rediscovered in archeological digs, in ancient libraries, in attics, and in law books. Ideas do not vanish when forgotten. The immediate response to this claim for abstractness is the identification of an idea with its concrete (usually written) representation. This turns out to be highly problematic. Popper then spoke of three worlds, the material, the mental and the world of ideas. It is no accident that the idea of three worlds appears, say in the writings of Frege and of C. S. Pearce, and that it wants repeated rediscovery. Yet it is doubtful that ideas will survive our possible extinction.

Popper's idea of objective knowledge ties in with his view of social institutions. In his *The Open Society and Its Enemies* he criticized psychologism, the theory that the social is reducible to individuals so that the human sciences are all psychology at heart. He recommended avoiding the question, do social institutions exist? In his later phase he took it for granted that they do exist, in World 3. He insisted that institutions have no aims. And as he always considered science a social institution, he took it to be not merely a set of theories, as he did in his early days. This is one example of the challenge that Popper's thought has left for the future.

7.5 Philosophy of Science

Popper's first vintage, his *magnum opus*, *Logik der Forschung* (1935), presented two questions. The first is what demarcates science from non-science? Here the objects to characterize are theories, namely, sets of statements within a given language. The answer is, they are characterized by their methods. This leads to the second question, what is the method of science? Popper added a new answer to the two traditional ones. We can see this in the following presentation that is a bit more historical than in Popper's original book and more consistent with his later views. (In the original Popper erroneously identified as pseudo-science all non-science, including all metaphysics and much commonsense.)

Viewed traditionally, science is a set of proven theories, namely, ones known to be true. And their truth, we remember, are truths by nature, since truths by convention are not binding. The way to demonstrate a theory is by the use of either intuition (intellectualism) or experience (empiricism, inductivism). Kant said it is a scandal that we claim to have proof and yet we disagree about what it is. He suggested a mix. The fear of using a mix is the fear of landing in a contradiction. Kant took care of this by suggesting making the intellect responsible for theory and the senses for raw experience, adding that theory has the last word, as it imposes itself on raw experience to generate scientific information. This is ingenious but it makes every human being intrinsically familiar with Newtonian mechanics. It made discovery impossible. Modern conventionalism (Duhem, Poincaré) overcame this kind of difficulty: it rendered Kant's system relative to different axioms and observed that already in Kant's system prior to their applications the axioms possess no informative content. Each axiom system is then valid within its domain of application. We thus have two traditional views of scientific truths: they are true by nature or true by convention. Einstein created a new category; scientific truth, he said, need not be true. Popper then elaborated. Any given theory is initially true-or-false, and it has empirical character in the sense that it can explain some extant observations and is refutable by other extant ones, or else the search for new refuting observations is underway. Scientific truth, then, is an explanatory theory that is tested and not refuted, and not all scientific theories are scientific truths. Truth is timeless; scientific truth is not. To admit this is to break away from the justly revered ancient dichotomy between truth by nature and truth by convention, the doctrine of truth by convention is arbitrary and thus not binding: scientific truth is the nearest we can bring truth by convention to resemble truth by nature. This requirement for resemblance to the truth, for verisimilitude, Einstein characterized as the requirement that the new theory explains the success (success as explanation and success as having passed severe tests) of its predecessor by presenting it as a special case and a good approximation. Popper offered a more precise theory of verisimilitude that was devastated by criticism and he withdrew it. Commentators, especially Hempel, took as Popper's solution to the problem of induction his theory of positive evidence or empirical support, his view that support is failure to refute. This is a serious error: this solution is to the traditional problem of rational belief, one he refused to recognize. The problem of induction is, how is empirical theoretical knowledge possible? His solution is, empirical theoretical knowledge is possible as refutations, since these are theoretically informative.

We may read it as a proposal and as a description. In either reading its great asset is its fallibilism, of course: it takes as final nothing but the falsehood of (formal) contradictions and it offers the exhortation to be critical as a replacement for the traditional demand to avoid all error and the traditional view of error as evil. As a proposal it is easier to take than as a description: such critical activity is rather rare. Yet the reading of Popper's view as descriptive should enhance the value of what we value by showing the contribution of criticism to it. Popper gave as examples thinkers who enjoyed modest success but, not pleased with their results, applied severe criticism to them and then did much better. Clearly, dissatisfaction and the resultant criticism can be very enlightening. Is this always the case? We do not know.

What is peculiar to Popper is not taking criticism as a lofty activity: this is rather traditional; what is peculiar to Popper is the view of science as critical, as dialectical. Maimonides had said, human language is not fit to describe the attributes of the divine, yet it behooves humans to try to do so and to acknowledge the limitations of the results of their efforts. Combining the dialectic of the Maimonidean negative theology with the Spinozist replacement of natural theology with natural philosophy amounts to the negative science that Einstein and Popper envisaged. How adequate is it as a description? Are all refutations of reasonable/great theories reasonable/great discoveries? Is every reasonable/great discovery a refutation of some reasonable/great theory? We do not know. The greatest and still most impressive discovery is Eddington's refutation of Newton's theory of gravity that set Popper on his long quest. Was Einstein's theory of gravity due to refutation? It is very hard to decide. All this is an elaboration on the first two chapters of Popper's *magnum opus*. The other chapters compare his criterion of demarcation of science with other criteria. He dismissed the "logical" positivist equation of scientific character with meaning. This part of his work can safely be here ignored since that idea had no value and no longer has advocates. He argued that explanatory power and simplicity come together with refutability so that there is no need to seek for them separately. This is an error: technology is full of highly refutable, well-tested theories that are of little scientific worth, and science struggles with some metaphysical ideas in efforts to render them testable by raising their content. Here Popper's philosophy is extremely useful, as his theory of content as improbability led to the suggestion to compare contents of some theories and to the suggestion to enrich the contents of meager ones. Popper also developed his theory of degrees of testability that has merit but is in need of further development. His theory of probability led him to an improvement of the axioms of the theory of probability and to a careful study of quantum theory. The suggestion that Heisenberg's uncertainty principle puts a limit to tests evinced a comment from Popper that is a true eye-opener: the principle should be put to test. Popper tried to devise a thought experiment to do that. It was erroneous: the experiment allowed a quantum particle to pass through a filter, thereby smuggling in a new uncertainty. Einstein, Podolsky and Rosen offered a new and more decisive thought experiment. In it the quantum particle encounters not a filter but another quantum particle, pitting the two uncertainties against each other. This is quantum entanglement. Its empirical applications are very exciting and very puzzling, and their philosophical import is still under debate.

7.6 Social and Political Philosophy

In the Age of Reason, when all researchers were philosophers, philosophy was understood in the broadest sense. German idealists, particularly Fichte and Hegel, introduced philosophy in the narrow sense of the word, not so much because they were professors of philosophy (Kant was that too) but because they were ignorant of science and hostile to reason. The rationalist wing of philosophy in the narrow sense soon developed too. It reached its peak with the new philosophy of science that in Popper's days turned into a philosophy of language under the impact of Wittgenstein. He had one thesis: he axiomatically rejected all philosophical problems. The application of this to parts of philosophy, the philosophy of science, of history, and of the social sciences, not to mention social philosophy, amounted to dismissing their problems. This rendered them scholastic, irrelevant to the genuine problems that Wittgenstein's disciples left for science to solve. Popper, on the contrary, glided into social philosophy as well as to the philosophy of the social sciences on the wings of his social and political concerns. At the time salon political discussions turned on the question, what do you prefer, Fascism or Bolshevism? This question obviously rests on despair over democracy. Popper sought the factor common to both options, the tools with which to choose between them. It was historicism, the doctrine of historical inevitability, the idea that history has a meaning, a divine plan for humanity. The religious version of the theory of the divine plan relates to history loosely: on the Day of Judgment, divine intervention will reveal the divine plan. The version of it that claimed scientific status was the idea that History goes through stages that end with the attainment of the end of history. Popper had a theory of scientific status. He applied it to historicism and found it wanting.

Many commentators still repeatedly say, he has refuted historicism. Not so: it is the scientific versions of historicism, he said—all of them—he refuted: the versions that claim to be explanatory and thus testable. The mere assertion that historical laws exist he found irrefutable and thus unscientific (as all purely existential assertions are). Irrefutability, he said, is not a virtue but a vice. This he explained in his classic *The Poverty of Historicism* that he began working on soon after he finished his *Logik der Forschung*. He had his first results published during WWII (1944–1945), to reissue as a book in Italian (1952–1953, 1954) and French (1956) translations and finally in English (1957) and then in other languages.

7.7 Optimism

Popper always deemed his philosophy optimistic; later in life he increasingly stressed this fact. His philosophy is skeptic as it is fallibilist, yet he said he was not a skeptic, meaning that he greatly differed from traditional skeptics, particularly the Pyrrhonists, known for having preached against optimism. He lamented the popularity and disingenuousness (*Verlogenheit*) of much of the pessimist philosophy (the existentialists and the mock-progressive Frankfurt critical school and their likes). He saw the pessimism associated with reactionary irrationalism more reasonable, except that he considered irrationalism disingenuous—apart from the irresponsibility of its advocates. For, he assumed that responsibility requires traditional skepticism, but not its traditionally passive attitude and not its pessimism that he staunchly rejected.

His arguments for optimism were diverse. First and foremost, the world is beautiful. (“The propaganda for the myth that we live in an ugly world has succeeded. Open your eyes and see how beautiful the world is, and how lucky we are who are alive!”) Second, recent progress is astonishing, despite the Holocaust and similar profoundly regrettable catastrophes. The clinging to life that victims and survivors of the Holocaust displayed despite all horrors, he observed, stirs just admiration that bespeaks optimism. Most important, however, is the moral aspect of the matter: we do not know if we can help bring progress but it is incumbent on us to try. This is the imperative version of optimism. Popper clung to philosophical optimism although he was fairly pessimistic in personal disposition: he clung to philosophical optimism all the same. He never dreamt he would have so much influence and he always felt his endless efforts would fail to eradicate the vicious myth that he had once sympathized with the (anti-)philosophy of the “Vienna Circle”. As things turn out, his fame has outlived theirs, and it is his friendly remarks on some of them that today stand out.

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Chapter 8
Kuhn's Way

> Anything printed is *ipso facto* out of date. (Whittaker 1913, 26).

This review of the posthumous collection of essays by Thomas S. Kuhn (2000) is my personal obituary. I am not neutral, since I fancy myself a rival. (He was my senior by a few years.) We wrote on the quantum revolution (Agassi 1967; Kuhn 1978) and on the historiography of science (Kuhn 1962a; Agassi 1963, 2008). His second book was the first on that topic; my first book came second. We reviewed each other's book (Kuhn 1966; Agassi 1966). Buchdahl (1965, 69) reviewed both and noted a trend. The trend was mostly Kuhn. His success is immense. His book "influenced … scientists, … economists, historians, sociologists and philosophers, touching off considerable debate. It has sold about one million copies in 16 languages and remains required reading in many basic courses in the history and philosophy of science." (Gelder 1996)

He good-humouredly indulged me my crude manners. Our meetings were few and casual but pleasant. He invited me to speak to the Princeton departmental graduate seminar. He then received me at his home. We crossed swords in meetings. His book on the quantum revolution (Kuhn 1978) had many reviews, and he answered all of them (Kuhn 1984) but mine (Agassi 1983). We met last at the 1985 Berkeley international history of science meeting. I talked there about willful distortions (Agassi 2008, 245–253), using as examples works of Henry Guerlac (Agassi 1987, 102). There and then Kuhn broke off relations with me. Guerlac was a friend, he briefly explained. This was our last meeting. He ignored my efforts to appease him.

Historians of science considered open criticism hostile; they concealed their criticism. Guerlac told me that his review of Donald McKie (Guerlac 1954) contains criticism that caused hostility. I find none there. Both pour scorn on the false phlogiston theory and praise Lavoisier's alternative to it as if it is true, masking the refutations of it, allowing only that his terms need updating (McKie 1952; Guerlac 1961; Agassi 2008, 237). Kuhn (1963, 139–143, 173) noted rightly that some distortion is unavoidable and thus excusable. He ignored willful distortions. In his review of my book he dismissed my examples of distortions as dated (Kuhn 1966). Here he reports on his having discovered distortions and on his

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having learned to avoid distortion due to up-to-date readings of old texts (276–277, 291, cf. 276, 278).

His histories are above average, as he did not conceal controversy and error. Regrettably, he played them down. Controversy is a vital and regular factor in the scientific tradition. He did not do it justice. He said, for most of the time leading scientists rightly shield the ruling scientific idea against criticism. This limited his vision. "I am never a philosopher and a historian at the same time", he claimed (316). Not so. We are all victims of philosophical limitations. His chief limitation was his opposition to criticisms of scientific leaders.

8.1 Glossing Over Criticism Creates Confusion

I first met Kuhn in 1962, at Guerlac's Cornell international history of science congress. My paper for the occasion concerned simultaneous discovery. Historians of science often blur differences between distinct ideas by identifying them with their up-to-date variants (Agassi 2008, 204 note 29; see also notes 34, 40 and 80 there; Fuller 1989, 130). Genuine simultaneity is rare. It comprises simultaneous tests of one theory. Kuhn's 1959 essay (Kuhn 1977, 66–104) depicts it as due to time being ripe. This is obscure and useless. I showed Kuhn my paper. He pleaded with Guerlac to ask me to scrap it. This puzzled me.

I once postponed commenting on a lecture of Kuhn from the public discussion period to a private chat. He thanked me—as a gentle hint, I suppose. Again, it puzzled me. After all, he was a skillful contestant. I learned later that he regularly placed the consensus on his side, declaring dissent from him as merely verbal variance. "Inevitably, the term 'cross-purposes' better catches the nature of our discourse than 'disagreement'" he said in response to Karl Popper's criticism. "There is not a great deal to choose between us." (126, 136, 141) He rejected Popper's choice of words as too harsh, his criticism as too explicit (126).

Unfortunately, Kuhn viewed dissent increasingly as verbal variance. Had he rewritten his famous book, he confessed, he "would emphasize language change more and the normal /revolutionary distinction less." (57) This renders merely verbal the conformity that he required of researchers. This is good. It also renders all revolutions merely verbal. This is not good. Rudolf Carnap had advocated a "principle of tolerance", allowing for disagreement if it is verbal and the varied wordings are mere synonyms (Wedberg 1975, 163). W. V. Quine criticized this: perfect translation is impossible and evidence cannot decide the choice of theories uniquely (46–47, 279, 306). As Kuhn endorsed this critique, he could not endorse Carnap's principle of tolerance. It seems he did (104). He was a positivist *malgré lui*.

Kuhn deemed general assent to him essential for his becoming the leader. He repeatedly voiced accord. He agreed with Hempel (208, 309), with Popper (133, 135), and with Margaret Masterman his nemesis (137, 169n, 300). He agreed with me on the historiography of science, our dispute on scientific controversy notwithstanding (Kuhn 1966) (108). He deemed controversy a communication barrier (124). "When I received the kind letter in which Carnap told me of his pleasure in my manuscript, I interpreted it as mere politeness, not as an indication that he and I might usefully talk. That reaction I repeated to my loss on a later occasion." (227) This loss was not due to false views but to his ignorance at the time of a "deep parallels" his views had with Carnap's late views. This "deep parallel" is unknown.

Kuhn enjoyed a "very considerable rapprochement" with Hempel (247) who was a friend (209–210, 224–226). Their views "were perhaps not quite so different as we both then thought" (225): Hempel learned to agree with Kuhn! "A few years later" Hempel "implicitly adopted a developmental or historical stance." (226) Implicitly. He then put things "in a sort of historical developmental perspective." (309) Sort of. They enjoyed a sort of agreement yet Hempel agreed and Kuhn disagreed with Carnap about the possibility of observation not theoretically biased. Despite reluctance, Kuhn expressed dissent, even from Popper and from Carnap. but he played it down (127). He dissented from the traditional inductivist view of scientific theories as resting on perfect evidence by inductive inferences. On this he was "an unrepentant Popperian." (128) Hempel told me, his philosophy was always inductivist and the changes it underwent were comparatively insignificant.

8.2 The Scientific Tradition Encourages Glossing Over Criticism

Plato expressed admirably the right view on criticism (*Laws* 635a, *Gorgias*, 506c). "If you refute me, I shall not be vexed with you … but you shall be enrolled as … my benefactors." A more popular view curbs criticism drastically by immunizing principles to it. *Contra principes non disputandum est*. Presumably, Aristotle endorsed it (Met., 1006a7; Anal. Post., II, 3, 90b; Popper 1945, II, 287–288). So did Wittgenstein. So did Kuhn. Following Michael Polanyi (Polanyi 1958, Chap. 6, §5), he declared obligatory the endorsement of dogmas of scientific leaders. He saw science as a profession that makes great demands on its affiliates; among these he did not include the demand to respect rivals.

Einstein said, science progresses as newer theories meet the empirical criticisms that had hit older ones. This idea is plain and powerful. Public notice of it lagged behind. Schlick returned from it to verification; Polanyi defended science and religion on a par—as traditions. Both were physicist-philosophers. Kuhn offered an austere version of Polanyi's view, with no theory of tradition and nothing on religion.

8.3 Kuhn Used Commonsense to Fill Gaps in His Philosophy

Kuhn overlooked tradition and religion to avoid controversy. Any rounded view on the rise of science takes notice of the great contribution of religion to it. Even leading anti-religious positivist Otto Neurath had admitted that although he followed Duhem, who had separated the history of science from that of religion. Kuhn's Paradigms are social beings. To discuss these with no sociology of science is bad; to do so with no reference to religion is worse. But he had to ignore what undermines the authority of science (Finkelstein 1984, last pages).

His image of science fits the new situation, when the authority of science grew vastly. It is a rounded, convincing insider's view. He offered it whole, he said, minus the technical stuff whose comprehension requires years of hard training. He allowed for one kind of change only: paradigm shifts. Observations influence paradigm shifts, but only partly; they resemble religious conversions (108–109, 174–175) (Cohen 1987, 464, 468; Fuller 1989, 67). Controversy may help, but a new paradigm imposes a new consensus: (108, 169n; 223, 288) paradigms are "what consensus was about." (299) They were "traditionally models, particularly grammatical models of the right way to do things." (298)

Little of Kuhn's philosophical output comprises expositions. Much of it is of old ideas, especially that there are no "pure" observations (107, 311). Most of his philosophical texts comprise examples from the history of physics. Most of the rest is corrections of misreading of old scientific texts. The rest is corrections of "damaging misrepresentations" of Kuhn (156). He complained and showed surprise (53–54, 106, 123–124, 133–135, 156–157, 160, 228, 307–308, 311, 315, 322 and more). He was surprised to hear, "Well, Tom, your biggest problem now is showing in what sense science can be empirical." (159n) He did not name his source; mentioning that she had written a favorable review of his book he targeted Hesse (1963). The story reappears later, with her name (186). She repeated her message over a lunch we three had one day. His view of the leaders as mediators between data and research troubled her, as she held the traditional view that research serves seekers after the whole truth. Kuhn dismissed it as "fossils" (120).

He later allowed two simultaneous paradigms, but did not make the change throughout his system. He allowed then many paradigms and small revolutions (143). Norwood Russell Hanson said, Kuhn had good case histories, but no idea for them to illustrate (Hanson 1964, 180–181). After Kuhn had caught the public eye, he took back all that he had ever said, Hanson added. He told me he saw nothing remarkable in Kuhn except good public relations. He was quick to notice Kuhn's way, yet he exaggerated. Kuhn always said, in science leaders impose a shared belief on peers. True, he also took this back once, but this was a mere slip. He said, science requires dogma, as some dogmatic conduct is beneficial (Kuhn 1963). This justification will not do: when dogmatic conduct is useful, one can fake it (Bendix 1970, 68; Agassi 1977, 338). At one point Kuhn said so too (141). This is a mistake as it amounts to his relinquishing his central demand for shared belief and then his philosophy collapses. Abner Shimony has ascribed to Kuhn the "sleight of hand" of a systematic "abortion of a viable line of reasoning at exactly the moment that it became embarrassing to the author!" (Shimony 1993, 309) Shimony disliked my using this quotation. His reason is obvious: it is drastic: it requires of all the serious followers of Kuhn to give up his philosophy or else to attempt to present a consistent canonic version of it.

8.4 Conant Influenced Kuhn Significantly

Modern science comprised a loose network of amateurs. The American and French Revolutions and the industrial revolution precipitated change. In the mid-nineteenth century technical universities appeared. Interest in applied science grew. Yet few academics performed research before World War I. The chemical industry employed only a few researchers and research institutes employed fewer. The military stepped in only during World War II and the Cold War. "… for good or ill, the cold war is in large measure a war of the laboratories." (Danhof, 1968, 1) Kuhn viewed all research as professional, linked to political power in a "necessarily permanent" manner (149, 252). Leaders then oozed authority and boasted top reputation (and security clearance). A lively passage in Kuhn's book on the quantum revolution (Kuhn 1978, 215) pictures young, hardly known Einstein visiting a famous university, the professor showing him pronounced respect, and the students realizing that he was a somebody.

Kuhn collapsed quite a few distinctions. Here are some: proficient versus dilettante; professional versus amateur; qualified versus unqualified; polymath versus specialist; reliable versus sham; trade specialist versus academic specialist; specialism versus sub-field (Zuckerman 1988, E 4b); research activities versus research projects (Bunge 2001, 170); preference versus dogmatism (Bendix 1970, 68); intellectual leadership versus socio-political leadership.

Harvard University president Conant made new conditions for academic jobs. He demanded professional authority and political conformity (Hershberg 1993, 391–554; Danhof 1968, 281, 316, 320). Polanyi defended this authority cautiously. Authority "grows out of mutual control and criticism", he observed. It "enforces scientific standards and regulates the distribution of professional opportunities". Above all, it is imperfect (Polanyi 1969, 44–46, 53–55, 94–95). "For, scientific opinion may, of course, sometimes be mistaken." (Polanyi 1962, 61) Not so Kuhn. Science is "in certain circumstances the most authoritarian", he said (308)—always except for inter-paradigms times.

Conant was Kuhn's mentor. He had academic, political, and military standing (Hershberg 1993, Chap. 28; Lipset and Riesman 1975, 302, 305 *ff*.). Busy and burnt out, he opened a program for teaching popular science, planning to improve it with "overall direction and planning." (Conant 1964, 51) The rigorous science teaching by reputed physicists (266) left popular teaching for duds. He tried to alter this (Conant 1964, 4). He lacked a "nationwide policy adequate to meet the challenges of the new and awesome age in which we live." (Conant 1964, last sentence) He moved to teaching the history of science—with notable success (Hershberg 1993, 409–411). Kuhn joined it as a rising star with a fresh doctorate in physics. As the history of science was barely a profession then, he had some difficulties settling down. Conant assured him of a career (278).

8.5 Conant's View of Criticism Is Conservative

"At the risk of incurring the everlasting hostility of the American Association of University Professors, I suggest that the time is more than ripe for lay boards to ask searching questions of the experts. These questions … should be addressed to the faculties through the presidents and the deans." (Conant 1963, 110) Controls thus flow from boards through presidents and deans. Kuhn said, in science controls start at the top—presumably leading intellectuals, not administrators. The absence of democratic controls makes imposition administrative, not scholarly (Danhof 1968, 298) Kuhn ignored democratic proposals. He advocated rigid instruction, ignoring the view that "Scientific education should be particularly careful to avoid this dangerous rigidity." (Ziman 1968, 70–71) He ignored Robert Merton on egalitarianism in science (287–288) (Zuckerman 1988). Derek J. de Solla Price spoke of "Diseases of Science" (Price 1961, Chap. 8); Harriet Zuckerman discussed deviance in science (Zuckerman 1988, V, C and D); Popper said, science thrives on training for criticism (Popper 1945, Chap. 10, n. 71). Kuhn wanted efficiency.

The Cold War initiated a social revolution (Weinberg 1963; Kowarski 1977; Agassi 1988). The academy offered to its members worldly success and that became increasingly valuable (Zuckerman 1988, V: C, D). Competition in the academy increased (Burke 1988, 114–132). But old wounds are now healing. Interest in nuclear weapons is waning. The need for democratic control over the public institutions of higher learning is gaining recognition. The republic of science needs reconstruction. Kuhn is outdated.

8.6 Hempel Failed to Reconcile Kuhn with Rationalism

Kuhn's frank authoritarianism invited the charge of irrationalism. The scientific leaders are rational, he retorted, and so are their edicts. He used commonsense, nor a theory of rationality.

In a symposium in honor of Hempel at the meeting of the Eastern Division of the American Philosophical Association, Boston 1983, Wesley Salmon and Kuhn paid him homage (Chap. 9). He was the commentator with Israel Scheffler in the chair. In the discussion period I criticized the hostility to metaphysics. The positivist hostility to metaphysics is excessive, Hempel admitted, but their hostility to religious dogmatism he declared beneficial. Later I casually reported this and elicited a hostile denial from Adolf Grünbaum. Scheffler sided with me. I checked it with Hempel. He said I had misheard him. On another occasion I heard him say that Kuhn endorsed either irrationalism or the common rules for the choice of theories. On this too he said I had misheard him. Later Kuhn took responsibility for the "damaging misrepresentations" of him as an irrationalist: some of the difficulties with my published accounts of theory choice would be avoided if *desiderata* like accuracy and scope, invoked when evaluating theories, were viewed not as means to an independently specified end, like puzzle solving, but as themselves goals at which scientific inquiry aims (209–210).

That is, had Kuhn admitted that, had he offered rules for theory choice as determining the aims of science, then the charge that he was an irrationalist would die down. This he did not admit. The charge of irrationalism stands (Sankey 1997, 306–307; Toulmin 2001, 215–216)

Hempel attempted to help Kuhn out (Hempel 1979). To that end he had also to discuss Kuhn's demand for conformity. He said, Kuhn limited this demand for where reason fails (Hempel 1983, 87–88) So do the best irrationalists. Kuhn said, conformity was necessary in order to "maximize efficiency" (209). Bohr wanted "crazy" ideas; Popper wanted respect for criticism. Kuhn wanted efficiency.

Kuhn advocated group rationality to combat the classical rationalism that viewed science as a "one-person game" (243). Most rationalist philosophers today regrettably emulate Carnap, Hempel and Grünbaum who view rationality as individual deliberations on extant evidence in search of wise choices of hypotheses to believe in. Kuhn tried to do without an explicit view on rationality. He said science is "a language game", "intrinsically a community activity" (215). He said, "the observed norm" is rational (209) but he dismissed "older, more comprehensive modes of practice" as "fossils" (120).

8.7 Kuhn Borrowed Traditionalism from Polanyi

Kuhn ignored his debt to Polanyi (296–297). Earlier he had admitted it, offering his "paradigm" as synonymous with Polanyi's "tacit knowledge" (Kuhn 1963, 392; cf. Kuhn 1970a, 44n, 191, 1977, 340–351). It is not. Newton's system is the paradigm of a paradigm (Kuhn 1963, 356) and it is explicit (Cohen 1956; Bunge 2001, 170). Kuhn admitted Margaret Masterman's criticism: "Paradigm was a perfectly good word until I messed it up." (298) "I seldom use this term these days, having totally lost control of it." (221)

Polanyi left small room for dissent in science (Polanyi 1969, 80, 93); Kuhn left none: science aims at unique optimal solutions (209). Polanyi said, "I can accept the … [conception of] Kuhn only as a fragment of an intended revision of a theory of scientific knowledge." (Polanyi 1963, 380)

When Kuhn expressed blanket agreement with Polanyi (Kuhn 1963, 392) he agreed on the authority of leaders, not on the freedom to criticize them. Polanyi criticized them for their radicalism. Kuhn granted them unchecked power. He dismissed their philosophies of science silently. This increased the "damaging misrepresentations" of his views.

8.8 Kuhn Borrowed Incommensurability from Duhem

Kuhn ignored his debt to Duhem while respecting his leading followers (286–287). Responding to a query of mine on this, he said he had never read Duhem. Bernard Cohen said then, this is impossible: members of Conant's team were familiar with Duhem. Here Kuhn hardly mentions the Conant team, and he mentions Duhem as an inventor of a term (235). he treats Whewell similarly (212). This is a common token tribute that inadvertently is an insult (Agassi 2008, 134). Kuhn's expression of gratitude to Popper for the advice to read a book by a Duhem fan is more impertinent (286). (As a student, Kuhn attended Popper's seminar in Harvard.)

Kuhn's image of positivists does not fit Duhem. He derided them for their lack of historical perspective. Duhem was a positivist historian of science. Kuhn borrowed incommensurability from Duhem. He said, "the notion still seems to me the central innovation introduced by" his famous book (228). It is an important idea that Duhem has expounded in some detail. It is the rule, do not forget old theories even after they are dated. He said this in opposition to realism, the view that the aim of science is a comprehensive image of the world (Duhem 1954, 81, 103, 171, 173, 176), that he rejected as naïve (Duhem 1954, 31–32) as it allows no more than one member of a set of alternative theories to be true. As we continue to use old theories, we should overrule realism. And then theories cease to compete (Duhem 1954, 101, 294). Kuhn endorsed this reasoning. The error in it is Bacon's refuted hypothesis that usefulness goes with truth. This error permeates the writings of Duhem as well as those of Kuhn.

If we view alternatives as languages they cease to compete—since perfect translation is impossible. Choice between different theories is then between languages. No amount of information suffices to settle with finality this choice (Duhem 1954, 187–188). Crucial tests do not either, as they carry no assurance. Possibly a faulty working hypothesis (say, about measuring instruments) is involved in the deduction of the tested predictions (Duhem 1954, 185, 187–190, 220).

8.9 The Consensus Is a Complex Matter

Popper encouraged troublemakers. Kuhn discourages them. This is the chief difference between them. David Budworth said, reading Popper made him regret that he had moved from research to administration, and reading Kuhn made him glad that he had (Budworth 1981, 177).

Confusion on the consensus abounds. Inductivists see it as given; Kuhn said, it is decreed; both confuse consensus with unanimity. Dissenters recognize the consensus while destroying unanimity. Philosophers of science often rely on the consensus in the wish to be right on ideas that are beyond their skills (Laudan 1983, 118–119). They forget that the consensus is complex.

Consider Polanyi's valiant struggle for scientific freedom (Polanyi 1958, 145, n., Chap. 6, §5); it is admirable. Future historians will write about the incredibly great and important influence that his fight for the freedom of science and of culture has exerted. Had he fought against the American bureaucracy too, he might have had success in that venture too. We do not know. We do know that they managed to intimidate him by silly, groundless accusations.

8.10 Kuhn's Incommensurability Is Redundant at Best

The most famous Kuhn-style paradigm is Newtonian mechanics. And Newton had met opposition, mainly from Leibniz. Kuhn blamed Leibniz for insubordination to the ruling paradigm (290). He did not blame Einstein for his siding with Leibniz (Einstein 1954) as his was a different paradigm. Thus, much depends on how Kuhn demarcated between paradigms. He had no rule for it. He viewed this as a serious setback (187n) and as no setback (142–143).

The imposition of incommensurability is due to "the primacy of the community over its members" (104). Fortunately, "groups do not have minds." (103, 242) So, the imposition is by leaders. They impose uniformity, not incommensurability. It is redundant. Viewing theories as languages merely blocks conflicts between them. For that end suffice it to give a term different senses when they appear in different competitors.

Duhem said, we compare systems by comparing their domains of application. This reintroduces comprehensiveness as the aim of physics. Duhem viewed comprehensiveness as universal applicability, as the ideal. His view of systems as empty shells is thus redundant too. He has ascribed to theories relative truth—depending on their domains of applicability. His admission that the relatively true is (absolutely) false allows perfecting his philosophy by noticing that relative truths are relative falsehoods. His system and Popper's will then merge. Kuhn added imposition to all this. The consensus can do without it. The crux is, there is no objection to relative truth as long as it does not oust absolute truth. Kuhn did oust it. To see why, we have to consider his theory of truth.

Kuhn ignored error since he deemed obedience to paradigms error-free. "Paradigms had been traditionally models … of the right way to do things." (298) So his view explains success (129, 132–133). Is it incommensurable with the view of science as inductive? Should contrasting them lead to crucial tests? Kuhn wanted incommensurability to be grammatical (211): "Paradigms had been traditionally models, particularly grammatical models of the right way to do things" (298). Can rules of grammar explain history? Is Kuhn's grammar commensurable with its standard alternative or should they undergo crucial tests? (44, 77, 200)

8.11 Kuhn's Critique of Approximationism Is Disappointing

Approximationism is the only viable variants of realism extant: science approximates the truth. A theory should outdo the explanatory success of its predecessors. This follows from Popper's view: the explanatory success of a theory refutes competitors unless they share it. A new competitor thus invites a crucial test (Popper 1972, 200, 358).

Kuhn denied all that (188–189)—even while stressing that in some sense science progresses (74). Yet Newton's laws as a part of Einstein's system are not the same as the original "at least they are not unless those laws are reinterpreted in a way that would have been impossible until after Einstein's work." (Kuhn 1970b, 101) Of course. This, Kuhn admitted, is no argument for incommensurability. He then explained why it is. The argument for approximationism, he said,

> has still not done what it purports to do. It has not, that is, shown Newton's Laws to be a limiting case of Einstein's. For [this] … we have had to alter the fundamental structural elements of which the universe to which they apply is composed (Kuhn 1970b, 102)

Here Kuhn says, approximationism rests on the assumption that competing theories apply to the same universe (Schiebe 1997, 338–339). This Duhem said: realism renders competing theories mutually exclusive. Kuhn agreed and rejected realism. This landed him into relativism. He tried to wriggle out of it. He failed.

Kuhn invented a new argument against approximationism (106, 161, 188–189, 243, 280): a new theory may resemble less its immediate predecessor than an older one. Now Kuhn was satisfied with any progress in any respect. Yet he demanded of approximationism to progress on all aspects (189). This is rather unfair. When a new theory outdoes its predecessors, verisimilitude increases. That should do. Though as an argument Kuhn's new point is unfair, as an observation it is true and significant. A theory may serve many ends. Progress proliferates. Kuhn and Popper are thus somewhat reconciled. Change is generally a mixed blessing, and this should hold for scientific change too. Yet approximation to the truth is central to the life of science and Kuhn's objection to it rests on his rejection of the absolute truth. Science explores the real world, he agreed, but there is no thing-in-itself (7, 71, 207, 245, 264). "My point is rather that no sense can be made of the notion of reality as it has ordinarily functioned in the philosophy of science." (115) He was a positivist *malgré lui*.

He praised Hempel as "a man who intends philosophical distinctions to advance truth…" (208) He said.

> some people, to the extent that surprises me and others, simply say, 'in the Ptolemaic system the planets go round the earth and in the Copernican system they go round the sun.' But that's an incoherent statement! (312)

Pace Kuhn, the statement is consistent, as Tycho Brahe proved.

Of the extant alternatives to relativism and approximationism, the more detailed their presentation, the more apparent their troubles become, unless they collapse into relativism or approximationism. The editors of this book write as if Kuhn had developed his alternative to absolutism and relativism and as if he had criticized in detail diverse alternatives to it (6–8). He did not.

8.12 Kuhn Had No Theory of Truth

One philosophical problem fascinated Kuhn: what is truth? (278, 312) He had the choice between physics (273), history of science (276), and historian of philosophy (316); but he was a born philosopher (314). He sought a new theory of knowledge. Scientific theory cannot both ignore the external world and describe it. Kuhn wished to do both by limiting semantics to "intra-theoretic applications." (162) He wished competing theories to be separate-but-equal. He hoped to do that by calling them languages. This idea fails as the mathematical theory of embedding allows full embedding of some older theories in newer ones, thus permitting perfect translations (Vuillemin 1986, notes 28 and 34; Scheibe 1997, 341).

Duhem opposed granting theories truth-values since it makes them probably false. Considering theories implicit definitions renders them vacuously true. We may then try giving them different meanings. This way Duhem combined (mathematical) certitude with (scientific) doubt (Duhem 1954, 174, 181). This is a splendid achievement. Popper's admission of false scientific theories supersedes it, however. It still is active in the study of the foundations of mathematics. Kuhn has ascribed it to a critic of himself and dismissed it casually (249).

8.13 Kuhn Had No Theory of Meaning

Kuhn claimed that he had linked incommensurability, meaning and translation. He did not. He wrongly understood Quine's view on translation as limited to nouns and descriptive phrases (48). Quine viewed dictionaries as sets of loose, circular definitions. This is hardly contestable, least of all by Kuhn. Dictionaries employ theories, Kuhn rightly added, hinting that Quine would disagree, whereas this is an argument that Quine used against the positivist theory of meaning. Kuhn unjustly derided "Quine's conception of a translation manual" (47, 74, 165).

Though all classifications are legitimate, they may smuggle theories, and these may be problematic. These theories may be hard to detect, as they often appeal to intuition. Ernst Mayer told me proudly that he managed to convince Popper that the dispute among biologists about classification is significant. Later, David Hull expounded on this significance (Hull 1999, 496–499). The literature that he refers to ignores commonsense. It thus also ignores Kripke, Putnam, and Hacking—not to mention Wittgenstein. Where Kuhn stood on all this no one can tell. Hilary Putnam follows Quine on meaning. Kuhn puts them down differently. A book by Quine is "going off the rails"; "there isn't much of an argument" in it (279–280). As to Putnam, "nobody could reasonably show anything but respect for" him. His book is not exactly Kuhn, but it is "a big step" (312–313). Clearly, Putnam is a friend.

Kuhn expressed broad agreement with arch-conservative Charles Taylor. He charmingly confessed ignorance. Interest in social affairs had cured him of positivism (216–217). Some leaders in social studies approved of him, wishing to impose unanimity. They ignored his view of their fields as too arid for raising paradigms (57, 223). Unanimity is insufficient for this:

> … the Greek heavens were different from ours. … … the heavens remained the same while the search was under way. Without that stability, the search … could not have occurred. But stability of that sort cannot be expected when the unit under study is a social or a political system. No lasting base for normal, puzzle-solving science need be available to those who investigate them …. (223)

This is a moving speculation. Despite esteem for Koyré, Kuhn ignored the neo-Platonism of early modern science. He was a positivist *malgré lui.*

Kuhn was admirably candid as he admitted that he refused to play guru, as "it scared the shit out of me." (321) He should have said, "It is beneath me." His fame allowed him to be a power broker like Conant. Laudably, he did not care for it. He was far too decent to drive his ambition to success. His wanted recognition as serious, not merely as popular. I confess I did him injustice by ignoring his ambition while considering his views a mere vulgarization of Polanyi's. A leader in the history of science, he wished to be a leader in philosophy. He failed in this. He was much more subtle than he appears, but also much less systematic. He did not need me to remind him of his shortcomings. I must have been a thorn in his side, I now realize. I regret this.

May he rest in peace.

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Chapter 9
Feyerabend's Proposal

Feyerabend raised furor when he declared science an intellectual system that competes with other intellectual systems, and that its claim for superiority to all competitors is but an expression of its cultural imperialist tendencies (Feyerabend 1987, 163; Oberheim 2006, 22). Some found all this atrocious; others found it intriguing. It is hard to see why, as it is the idea that most post-modern writers take for granted (Hamm and Smandych 2005, 63). The option that science is not intellectually superior to magic is but a teaser. He clearly offered it as a mere challenge: he did not consider it seriously. In his discussion of it he did offer one unusual idea: he proposed legislation of a separation of science and state (Priority for this idea goes to Haberer 1969, 988). This proposal merits some exposition and some examination (Fuller 1997).

Before discussing this proposal, let me notice its advantage: it is a discussion within the politics of science. This is quite unusual: most writers about science prefer to pretend that science and politics do not mix. This is a part of the myth that there is no politics in higher matters—cultural, intellectual or scientific. It is dangerous as it blocks the important and urgently needed design of democratic controls of the existing political system of all cultural institutions, especially the commonwealth of learning. Feyerabend's attack on science makes sense only when understood this way.

9.1 Preliminaries

Every human system displays as aspects of itself items that belong to different subjects: psychological, sociological, economic, political, legal, communicative, and whatever else comes to mind. Moreover, usually these aspects of human systems have their institutional expressions. Thus, the academy has diverse internal institutions—economic, social and political. Nevertheless, there is almost no literature on cultural politics proper. The little that exists is incidental to stories, to the sociology of science, and such. The field of science policy, in particular, differs from the politics of science: taking for granted the results of extant projects,

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it considers their place in national politics. Most writings on cultural politics express some unintelligent moral indignation that rests on the denial of the claim that there is any politics within the commonwealth of learning. Indeed, academics and administrators deny its existence. Evidence to the contrary leads to protest: it should not exist. We admit war as a part of political life but we do not admit academic politics. We even dismiss the "science wars" as science politics (Goswami 1996, 46, 219). This renders the level of discussion unusually low.

An interesting exception is discrimination—religious, social, ethnic, and gender. Democratic societies allow for religious discrimination within communities, not within the nation. Religious discrimination in universities was practiced openly—even after they were secularized. They still practice discrimination against women, although in the civilized world it is almost everywhere illegal these days. Some philosophers differentiate between masculine and feminine science, thereby offering a new justification for discrimination. This is a regrettable folly (Agassi and Agassi 1987).

Under the pressure of evidence the popular claim that intellectual politics does not exist switches temporarily into the indignant response that it should not exist. Now indignation is gratuitous, dogmatic, pig-headed and harmful. It is a standard conservative defense of the *status-quo* and the tacit claim that the injustice in question is deviant and can/should be eliminated from the system accompanied with no other change; it is thus the top-dog's way to tell the under-dog that there is no *status-quo* and no top-dog, that the road is open to the very top for anyone who is very good. Whereas the view of intellectual politics as non-existent is *naive* and ignorant, indignation about it, as all indignation, is hypocritical and plainly self-serving.

The *naïve* take it for granted that the common valuation of members of the scientific community and of their output is reasonable (while allowing reasonable time-lag and understandable error and so on). It is not. Take any period in the history of any science and ask for the list of five or ten top-dogs there. Then conjure the people whose lives were spent in those periods and ask them, whom they would nominate for the same list. As an example, our top-dogs list in astronomy in the first quarter of the seventeenth century includes Kepler, Galileo and perhaps Borelli. For contemporaries Kepler would not count, Galileo would, but as second to Grassi or some other individuals. Historians of science mention this seldom, and then with indignation and while explaining this fault as due to prejudices. This is not so. The people who were over-valued were very powerful and respected and they lost their power when they left the scene. Over-valuation is commonplace; it matters here since the scientific big shots were respected as great thinkers. Appreciating powerful people is politics; appreciating them as thinkers is intellectual politics. At present, allegedly leading researchers are more likely than not to be members of the Ivy League; often they are good, and sometimes they are very good. Equally often the excellent, the truly leading researchers, do not come from the Ivy League. Their influence is known to their leading peers, but not to the rank-and-file. Ernest Gellner has called them the underground leaders. Of course, if you pick at random some Ivy League researchers and say, they are not the very best, people will disagree: people exaggerate the value of the research of Ivy League researchers. Such are not leading scientists in the sense of Michael Polanyi and Thomas S. Kuhn, of being the originators of new leading ideas. Nor are they rank-and-file researchers or, in Kuhn's terminology, normal scientists. Polanyi and Kuhn assumed that the top intellectuals are the most appreciated ones: Kuhn says they are top dogs first and they make the revolutions second. And if not, he added as an after-thought, then they are the Young Turks, and if they do not take over at once, then give them time. The idea that field theory was studied by a heretic group that won only after Einstein achieved a monumental victory is beyond the Polanyi-Kuhn view, and the fact that not Einstein but Bohr was the leader refutes this view.

That science and politics should not mix is then a demand—the very reasonable demand for impartiality and the less reasonable demand that impartiality should prescribe unanimity and preclude scientific schools. Yet these exist and contribute to the growth of science. Hence, science is inherently political. The more it integrates in the military-industrial complex, the less democratic its politics becomes (Price 1962).

9.2 The Ethics of the Matter

We all have ideal images of people; we idealize politicians and scientists, and even taxi-drivers. Ian Jarvie has noted that the ideal New York taxi driver is a Hollywood product that is close enough to the real thing for New York taxi drivers to emulate it willingly (Jarvie 1970, 221). This is understandable, helpful, and fairly innocuous.

Jean-Paul Sartre criticized sharply the waiter who tries to play the ideal image of the waiter, as if to say, I am not really a waiter, I only play the part (Sartre 1956, 103). Yet, mostly, playing a part by some familiar guidelines usually makes life easy all round.

The ideal scientist may be a Weber-style ideal-type. The role of such an ideal type includes a standard of conduct, an ideology, and views and values. Most scientists believe in science and in its method and they take it for granted that there is no disagreement between active researchers about the right method. They do disagree about religion, politics, departmental affairs, at times even about scientific conjectures. But wherever scientific method applies controversy quickly settles down; what controversy does not quickly settle, scientists should not speak of as scientists but as private citizens. However erroneous this ideal type—any ideal type—may be, its prevalence is an observed fact. Yet, like most popular beliefs, it is refuted. It refuses to go as it serves a function. Unfolding the self-image of waiters, Sartre unwittingly shows this. Forced to be servile, the self-respect of Sartre's waiters is hurt; so they remind themselves that their servility is a part of their job. Sartre rightly interprets their impeccable performance as self-distancing. We better ignore his censure of waiters as nasty. The case of scientists, oddly, is the reverse. They accentuate the perfection of the image they project in order to identify with it. They know that ideal scientists do not exist yet they try hard to be.

By this reasoning, playing the scientist's role is quite possibly immoral since criticism of it leads not to rethinking but to enhanced efforts to follow it. Not that scientists are clear about their ideal: they simply follow vague gossip about it. Alas, the gossip is manipulated by intrigue-mongers. Now in science these are more intelligent, knowledgeable, and respectful toward the truth than their equivalents in other walks of life. Even so, gossip is never honorable, following it is not rational, and we wrongly dismiss it, as if it does not count. It hurt, especially novices.

9.3 Science as a Social Phenomenon

The Enlightenment Movement had high aspirations that rested on the false assumption that people are autonomous. The peak of the reaction to its failure to deliver the goods was Nietzsche's unbecoming expression of contempt for the masses as having slave morality and admiration for intellectual leaders as super-human masters. The hope that intellectual leaders will reinstate the high aspirations of the Enlightenment Movement led to unbecoming contempt for them too, and also as lacking autonomy. In the quasi-autobiographical novel *Martin Eden* of 1909 Nietzsche fan Jack London describes a writer's painful struggle of to get published. The moment he was noticed he could not prevent his becoming a celebrity. "He was overwhelmed by requests from editors" who had rejected his manuscripts and now "wrote to him telling him to name his own terms." The readiness of literary opinion leaders to join bandwagons—due to their inability to judge—disappointed him deeply and drove him to suicide.

Bernard Shaw, the greatest Nietzsche fan, discussed the bandwagon effect in his 1911 comedy *Fanny's First Play*. A theatre critic in it explains how to judge a play: "If it's by a good author, it's a good play, naturally." This makes the audience laugh wildly. Jack London's bitter complaint and Shaw's sardonic joke are one. Shaw explained: it is indeed more likely that masterpieces are products of accredited artists than of unknown ones and this causes the bandwagon effect. Critics on the bandwagon are harmful parasites. Unlike London, however, Shaw did not resent them. Every band-wagon, he said repeatedly, has its joiners from among the rabble and the mixed multitude; critics and other commentators are no exception. It is unwise to trust them on art—on science too, let me add—before learning to discriminate: one must learn to judge for oneself. Credulity was the target of Shaw's ridicule throughout his life.

Sociologist Robert K. Merton found the bandwagon effect in science. Politely, he renamed it "the Matthew effect" (Merton 1968). Unknown scientists, he observed, have to struggle for space in the learned press and accredited scientists get published with relative ease. Unlike Shaw's *exposé*, Merton's causes no laughter. It should: it indicates that commentators can be, and at times are, as slavish in science as in art.

(In a private discussion Merton stressed that the time lag in the arts can be much longer than in science; this is questionable. He refused to discuss this in print.)

The role of the bandwagon effect is to keep establishments in power. This allows them to induct into their ranks those they consider best, and use this as evidence that you cannot keep a good originator down. But at least they try. In performing its duties the scientific establishment is better than other establishments. My intent is therefore (not to attack it but) to observe *the apologetic symptom*—scientists' inability to keep the image of the calm rational scientist and to listen to the empirical facts presented here. This makes them more culpable-in-their-own-eyes than I would deign to presume. As Walter Kaufmann has described (Kaufmann 1975) it, their apologetic symptom expresses a sense of guilt that leads its bearers (not to critical self-appraisal and improvement but) to trying harder, to repeating old mistakes with a vengeance.

Sigmund Freud said, your fate is sealed by the time you have learned your parents' characteristics and are trying to emulate them. Polanyi substitutes teachers for parents. Moreover, both had in mind not the individuals and not ideal types but ideal projections in young inexperienced minds. That a graduate student should be viewed as a mere child is quite remarkable, yet this is Polanyi's view of the graduate student as an apprentice. It is increasingly popular, because it does recognize the important fact that most of the powerful scientists are past apprentices of powerful scientists. He also justified this by declaring that powerful scientists are powerful because they are original. They are not; they are often mere bandwagon joiners: originality is quite rare.

Polanyi wins acclaim, mainly through popular accounts of his ideas, chiefly by Thomas S. Kuhn, simply because he wrote in a vacuum: no one wrote of leaders in science before, due to the popular prejudice that science is utterly rational and so free of leaders. Polanyi's rejection of this popular prejudice enabled him to present science as a closed club.

9.4 Science and the General Public

Polanyi's view does not reflect the situation. He has emphasized its inadequacy, thus rendering his view much more sophisticated and reasonable than that of the apologists for science such as Kuhn. Polanyi viewed science as an autonomous system—not democratic, but one functioning freely within democracy (Polanyi 2000). This freedom must give way to interaction as long as the scientific community thrives on outside prestige that rests on cooperation: for physics it rests on the excellence of its contributions to the military, for mathematics on computer science, and so on. Polanyi desired facts to better approximate his ideal: for the sake of the freedom of science he hoped for less interaction between science and society or national politics. He was not interested in the influence of science on society; he wanted the influence in the other direction to diminish. Feyerabend was concerned with the excess influence of science on society, better known as the imperialism of science.

Despite the scorn and ridicule that Feyerabend regularly encounters, his popularity is rising for half a century. However just the scorn is, the resilience of his views under attack deserves attention. They are popular because of his complaint that science exhibits intolerance to levels that should cause concern. Let us join Polanyi, Feyerabend and many others, and refuse to ask what exactly science is and who exactly is a scientist. Let us admit as a scientist or a leading scientist anyone publicly known as such. Let us join Feyerabend and ignore arguments in favor of militant conduct and the negative overtone of words like 'prejudice' and 'superstition', 'charlatan' and 'pseudo-scientist'. Let us notice the aggressiveness of scientists towards others. Consider an astronomer forcing a commercial publisher not to publish a pseudo-scientific astronomical book and somehow succeeding in doing so (http://en.wikipedia.org/wiki/Immanuel_Velikovsky#.22The_Velikovsky_Affair.22). What makes this different from the conduct of any other pressure-group, say religious leaders forcing a publisher not to publish Tom Paine or Albert Schweitzer? Of course, we the scientific public cherish science. But then religious people cherish their religion the same way. Hence, religious and scientific conduct may be intolerant, and recently science has been increasingly intolerant towards folk-medicine, folklore, religion, etc. Feyerabend said, science is harsh to all its competitors, and every activity that may enter the curriculum, for example, or compete for a research grant, competes with science for a place in the curriculum or for the grant money. Hence, the tyranny of science must be checked before it goes too far. Anyone who cares for pluralism must help control the aggression of science (Velikovsky 1983).

This is a possible reading of Feyerabend. It makes him popular. It makes him a champion of the under-dog—all alternatives to science such as non-science, pseudo-science and para-science. The philosophical literature proves that Feyerabend's claims suffer excessively from excess. His admirers gladly indulge him his excesses. Let us accept Feyerabend's complaint after it is cut down to size. How, then, should we curb the aggressive conduct of science?

9.5 Feyerabend's Proposal

Feyerabend made a proposal: separate state and science. This proposal wants clarification. The question, 'Is religion inherently tolerant?' is very difficult: the religious and the anti-religious may differ about it along party-lines. A much better question is, how can religious intolerance be prevented? For, those who consider religion tolerant will not oppose curbing its intolerance; those who consider it regrettably intolerant may be glad to curb its intolerance; and those who advocate religious intolerance should be curbed. The separation of state and church has done the trick. Since science is aggressive towards increasing areas traditionally in the domain of religion and folk-lore, it may seem reasonable to effect a similar separation so as to restore a sense of balance.

The separation of state and church does not imply any separation of politics and religion. Political action may rest on religious convictions and religion should not ignore politics. State and church represent nation and congregation; these overlap, thus linking politics and religion. The authorities are separate in the formal, technical sense, but interact via the overlap of nation and congregation. To make the parallel between religion and science fit, we need to know what it involves. Consider the triad nation, nationality and state; compare it to the triad congregation, religion, and church; and seek the scientific triad. The first element, the human grouping, should be the scientifically educated or the scientifically inclined. The second element, the characterization, in parallel to nationality and religion, should be science. This is quite unorthodox, since traditionally philosophers of science consider science a body of rationally binding doctrine. Many scholars admit that it is not clear what religion is, and this more than legitimizes Feyerabend's parallel claim that we do not know what science is. The third element is more troublesome: what is the scientific parallel to state and church? (Using the notation of Claude Lévi-Strauss, we may put it thus: nation : nationality : state :: scientific community : science : state.)

Science does have authority; it exercises power, and inevitably at times it does this unjustly. This is hardly avoidable: knowledge is power, power corrupts, and hence, knowledge corrupts. Anyone who is disagrees is excessively *naïve* or simplistic. Anyone who agrees has already endorsed Feyerabend's view cut-down to size. His advice, then, is to separate those authorities from the state. Which authorities? Among the candidates are universities, scientific societies, and national scientific research and development institutions. We can go further and add to the list: the suppliers of atomic bombs to the military, of nuclear plants to the government agencies in charge of the supply of energy, and the medical corps in the armed forces. Medical schools are powerful political institutions. Since they grant diplomas that are prerequisites for the joining of the medical corps of the armed forces, they have strong ties with the military. Is this healthy? No. Is this avoidable? Yes.

Feyerabend's idea is inapplicable. The need to control cultural authorities and prevent them from misusing their power is the need to institute specific controls, not separation. For example, it is admittedly a mistake to allow heads of national medical bodies to decide for the nation how many citizens should become medical practitioners each year, and who exactly should become a practitioner. To change this there is a need to set up a public body to discuss admission politics and to grant medical degrees and so on. The public body will have to use medical personnel, of course, but this will be progress, since the interest that members of public bodies represent is of the public. Legislation should control guilds' abuse of excessive powers.

The need to institute organizations that should look after the public interest in science is not the same as the separation of authorities. The separation will benefit both sides. When British universities became excessively dependent on government funding British legislators demanded the establishment of a University Grants Committee whose major function was the prevention of governmental intervention in university affairs. In the meantime, under Thatcher and others, it has been integrated into the government machine. The Research and Development funds in the United States were used from the start and almost openly as means to bribe university administrators and enlist their greed in the cause of inducing professors to work for the military. It was also used to enlist the same greed as means to fight segregation and discrimination. At the time, saying all this out loud was considered treason; now that it is past history and no one fears indictment, it is common knowledge.

The authority of a chief medical officer is military, not scientific; and that of a chief medical research unit administrator is scientific. A chief medical research unit administrator is often scientifically ignorant: a complaint is regularly made that these are either professional administrators or second-rate or superannuated scientists. This is as irrelevant to Feyerabend's case as the same claim made about religious leaders. The separation of authorities imposes indifference to such claims.

To see the possible benefit of a separation, let us examine a case of fusion—in the present case, a fusion of scientific and political authority. Consider a system whose political and scientific authorities legitimize each other. When cardinals made kings and kings appointed cardinals, intolerance was not in check; when college presidents appoint premiers and premiers appoint college presidents Feyerabend's proposal sounds reasonable; it throws light, for example, on the organization of science in the Soviet Union. Does Feyerabend's proposal make sense for a system like it? I do not know. The question shows that rather than pour scorn on Feyerabend it is better to render his messages clear so as to render it possible to implement them to the extent that would be democratically judged reasonable.

Nearer to home, consider Feyerabend's suggestion that inflicting cruelty on others may be instructive (Lugg 1977, 755–775, 767). This is true, or else we would not need the Oslo Convention to forbid it. Will the separation of science and politics help here? The separation of church and state does not allow for human sacrifice, female genitalia mutilation, and such; it does allow the claim of a religious community to be the chosen people. Feyerabend objects to the claim of scientists that they are the chosen people, not to the damage they cause. His remedy is useless.

9.6 Towards an Institutional Analysis of Science

Who is a scientist? Kuhn says, normal scientists are small-scale researchers. If their research is industrial, then their employers, not the scientific leaders guide them. Consider then science professors. They are scientists. Are they researchers? If they are community-college employees, then no; if they are fellows of Ivy

League universities, then, probably yes. Are Ivy League researchers engaged in research? Some are; definitely not all. Perhaps this is all wrong. With so much sociology of science around, we do not even know who is a scientist and who is a scientific researcher. This fact is truly amazing.

In practice such things are determined by scientific leaders. Who are they? We do not know in theory; we do know in practice. We do not know why Einstein was not a leader and Niels Bohr was. In part this is due to self-selection that may rest on a sense of responsibility, personal ambition, self-assessment as a good organizer or as a burnt-out researcher or both. Leaders are chosen *ad hoc*, but not arbitrarily. The decision is *ad hoc* because the accepted ideology refuses to recognize leaders; it is not arbitrary because in democracy leaders cannot be extremely high-handed, arbitrary, and impervious to criticism.

The recruitment of leaders is from a limited pool. The first step to scientific leadership is registration in an introductory science course, as Polanyi and Kuhn observed. As long as research is a profession, the prerequisites for which include a degree, it may petrify. In a piece that is largely still science-fiction, Isaac Asimov describes a situation in which leading scientific periodicals accept only papers with reference to some grant. (This reflects his experience as a professor whose refusal to apply for a grant caused him much trouble, as he told me in detail.)

When a racist essay appears in an Ivy League periodical, it gains notoriety and can even be called a report: the Jensen Report, in *Harvard Educational Review*. (Jensen complained of discrimination on the pretext that his paper is scientific. His view of skin-color and intelligence as given by birth and as unproblematic disqualifies it; inept critics examined his correlations instead of his assumptions.) The only way to reduce gullibility is education. But who will educate the educators? Recruitment and job placement spell political power, and it is in the hands of the Ivy League universities. They take themselves to constitute the standards of excellence. The view of Kuhn's position as a justification for this sad practice is the right reading of his texts.

When Kuhn wrote, the United States led the way, for better and for worse. Things have changed since—even in the United States—and this changes the flavor of his texts. Information about Russia and the East is not easily available, but the European community is more open and the study of the rapid change its academic system undergoes needed study.

Science political leaders (controllers of the science media, journal editors and conference organizers) may abuse their power. This raises unstudied background questions. Proposals are most urgently needed for the reform of the current cultural institutions, especially scientific ones. Such reform should establish and recognize the contribution of civilized controversy to culture, especially science. The canons of conduct of the commonwealth of learning have served admirably well yet they are no longer adequate, and the cost of their inadequacy is mounting. The feeling that radical reform of this sort is needed makes Feyerabend understandably popular. We should improve upon him.

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Chapter 10
Imre Lakatos

Lakatos was born in 1922 to a Jewish family in Debrecen, Hungary and died in London in 1974 of heart failure at the age of 51. His father emigrated when he was a child and he grew up with his mother and grandmother. They died in Auschwitz. He belonged then to a small communist clandestine group. He pushed a young member of that group to suicide on a flimsy excuse, possibly for no better reason than that it boosted his ego. This was a secret that hovered over him to the rest of his life. He was probably denied the coveted British citizenship because of it. In 1948 he graduated (mathematics and physics) in Debrecen University. Already in 1947 he became a senior official with much power. In 1949 he studied in Moscow State University and was arrested while on vacation in Hungary, perhaps because his victim's sister had enough power to ruin him. He was incarcerated for 3 years and released in 1953, able to flee the country in 1956 soon after the Hungarian revolt. In the last 3 years of his stay there he was employed as secretary and translator in the Mathematical Institute in Budapest. After his escape he registered in the University of Cambridge, where he received his second doctorate in 1961. By then he already had moved to the London School of Economics, where he stayed about a decade, till the end of his short life as he experienced a meteoric rise from obscurity to world fame.

10.1 Works

The works of Lakatos are in three areas, the philosophy of mathematics, the philosophy of science (especially economics) and a political pamphlet or two on the students' uprising at the London School of Economics (regrettably ignored here). A superior philosopher of mathematics, he is better known for his philosophy of science despite its questionable value. It consists of little more than an

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incoherent collage of slogans from two great philosophers of the time, Karl Popper and Michael Polanyi. He moved gradually away from the former towards the latter. He died too young to formulate his views on science definitively. Their novelty lies in his scintillating terminology and witticisms; his true intellectual contribution was to the philosophy of mathematics.

His work was highly innovative and stimulating and has had great influence. He deviated from the traditional presentation of theories of method by rejecting both the deductive method and the inductive method. As a historian he suggested that Newton used and tradition followed a different option: a combination of both the deductive and the inductive method that allegedly secures science doubly. These two methods usually clash. Creating clashes and resolving them repeatedly is what as a methodologist Lakatos considered the correct method in mathematical research. He offered an example, published in his epoch-making *Proofs and Refutations*: there were series of proofs of the Descartes-Euler theorem about polyhedra that met repeatedly with counter-examples that in turn inspired better proofs. In the process the concept of the polyhedron was repeatedly altered. Lakatos observed that the comprehension of the final definition of polyhedra as it appears in handbooks and textbooks requires familiarity with the history as he has narrated it.

Lakatos broke new ground on the role of mathematical intuition in research. Some recommend it as infallible. Others denounce it as fallible. Lakatos recommended it as it and its products are improvable. Uncertain, it is a means for developing ideas, a heuristic. Only strictly formal systems are certain, he observed, and even this certitude evaporates upon application. Lakatos cited the famous discovery of Abraham Robinson to argue that even the application of formal arithmetic to ordinary arithmetic is uncertain.

Formal mathematics aside, we do not know what proof is. Yet we can prove—by deduction. For, we do know that the consequence of the proven is proven. But for proof by deduction we need proven premises. Where do we find these? Logicism restricts the axioms of mathematics to tautologies since these are provable by the method of analysis: tautologies are true by virtue of their logical form alone. Logicism failed. Formalism may be the view of mathematical systems as empty, as devoid of meaning. W. V. O. Quine has shown that this view, being all too easy to apply, invites excessive arbitrariness. What system then deserves formal treatment? The preference for one axiom system over another is either arbitrary or justified by intuition. But all intuition is fallible. Following Popper, Lakatos took the initial steps as beginning with putatively proven assertions and the proof as their deductions that may lead to their refutations. The difference between refutations in mathematics and in science, he explained, is that the scientific ones but not the mathematical ones need empirical corroboration. This is true but unsatisfactory: we have a good idea as to what counter- example we want to be real, and what is sufficient when imagined. What rule supports it?

Even without an answer, Lakatos' achievement helps throw much light on the history of mathematics.

Lakatos suggested that a series of conjectures and refutations end up in a purely formal system. He could not explain this. This is a defect of his philosophy that he presented as a rule: formalization should not be forced: it should be a natural end-point of a series of developments. This is a sleight of hand. To clinch it he offered a witticism: avoid premature formalization. Does all mathematical research lead to a "natural" axiomatization and then to formalization? When is it time to axiomatize and to formalize? How is Euclidean geometry axiomatizable even though it describes physical space? As an admission of his inability to answer these questions, Lakatos distinguished between progressive and regressive changes. Feyerabend noticed that Lakatos had no rule for differentiating between them: it is easy to intuit the difference, but hard to articulated it. Progressive change is desirable and also interesting, but Lakatos needed a rule that governs this. He had none.

What is missing is a comprehensive idea. *Proofs and Refutations* is most powerful in the ease with which each move in the story takes place. Yet the very first line in it is puzzling and too difficult for many readers. It presents the Descartes-Euler theorem as a conjecture in a move that is too abrupt, contrary to what elsewhere in his system he so magnificently resisted. There is irony in this, since Lakatos' philosophy of science centered on just the metaphysical background to research that is here so blatantly missing. He sought to present the framework within which any scientific research project takes place. His followers, particularly Peggy Marchi and Michael F. Hallett, tried to supplement his theory this way.

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Chapter 11
A Touch of Malice

This book's hard core is Lakatos' last lecture-course on scientific method. It is about 90 pages long; seven of eight lectures are reproduced nearly in full. (Decades ago I heard a clear and complete recording of these lectures.) His thesis is that Popper is in error: scientific theories are irrefutable, because it is possible to protect them by explaining away refuting evidence. In the focus of these lectures, then, evasive hypotheses linger. This is sad: obviously, if a hypothesis may need evasion from refutation, then it is vulnerable. Lakatos knew this, of course. When an alternative to a refuted hypothesis is found, he said, it is time to recognize the refutation. This is very sad. In his early writings on mathematical method he took refutations seriously.

Most of the book is a selection (about 250 pages; the editor says nothing about his filter) from the correspondence between two famous philosophy professors. It began in 1967 and ended in 1974, with Lakatos's sudden death at the age of 51. Young Lakatos held high political office in Communist Hungary that ended with four years in jail (Long 1998). He graduated in Cambridge in 1961 and in 1965 he achieved great fame in the philosophical community. Feyerabend was slightly younger, was severely wounded serving in the *Wehrmacht,* and achieved greater fame earlier. He survived Lakatos by two decades. The letters provide a portrait of two interesting famous academics. They also offer some insights into their philosophies. Only fans will find philosophical interest in them. The letters articulate an absorbing hostility (to Popper's idea that criticism is the hallmark of rationality, particularly in science). They illuminate some dark recesses of the souls of their authors. They also contain some valid criticisms of Popper. The hostility should be ignored; the criticism is better expressed elsewhere.

Lakatos and Feyerabend (1999)

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11.1 The Lectures

Lakatos' lecturing style was rightly praised for its drama, humor and excitement (20). The style of the lectures reproduced here is disappointing in its dramatization (189), pretension, and derision. Some of Popper's texts, Lakatos says, "bear the sign of mental senility" (92). His comment on Popper's endorsement of the traditional ban on evasion of criticism is mean: he "never goes into this deeply…. Anybody who has read anything about the methodology of economics knows that this is always discussed, but exactly the same happens in physics too" (88). The demand to avoid evasion, he said, "is too stupid, and of course Popper never said anything like it" (89). The nadir here is a passage in which a famous scientific idea is declared an important inconsistency (82–83), suggesting that the demand for consistency is a unique characteristic of Popper's philosophy. The editor reports that Lakatos loved contradiction as he never abandoned Hegel (15). Not so. He approved of Popper's ridicule of Hegel's *Naturphilosophie* (23). The image of Popper here is a caricature.

The lectures have an unusual, excellent framework. It is non-justificationism Bartley-style (35), here presented as the combination of the views of Sextus Empiricus and of Popper. Sextus viewed as the most fundamental philosophical dispute the one between skeptics and dogmatists, as he deemed dogmatists both the empiricists and their *a priorist* opponents. Popper viewed methodology as comprising three rival schemes, the old dogmatic two, naturalism and conventionalism, and his new (non-dogmatic) refutationism (33). This framework barely sustains Lakatos's early refutationism; it bans his later dogmatic position (95), not to mention his last irrationalist one (108, 253). His opposition to conventionalism is facile, and he repeatedly fell back into it (see below). The quest for truth that lends skepticism dignity is here slighted by the demand to postpone recognising just criticism. Feyerabend rightly says, there is but a "difference in *rhetoric*" (116) between his own total anarchism and Lakatos' division of research-moves into progressive and regressive. He poked fun at this division (248, 333). This way both of them repressed what they once admired, namely, Popper's dazzling view of scientific progress as due the refutations of extant theories plus the invention of new replacements for them. This view, Popper said, rests only on this: science is the search for the truth and hence the rejection of contradictions (as their falsehood is uncontroversial).

Lakatos repeated a valid criticism, by Feyerabend and me, of the traditional view of explanation as deduction. He nastily ascribes this view to Popper alone. It is refuted whenever an explanation modifies what it explains: the explanation negates then the unmodified version of what it explains (84ff.). The paradigm here is Newton's explanation of Kepler's laws: taken literally, there is a contradiction here: Newton said, planets interact and thus perturb their motions along their Keplerian ellipses. It is the success of Kepler's laws that follows from Newton's law: it makes them hold approximately in our system, whose sun is very massive. Whewell discovered the inconsistency between Newton and Kepler; as Lakatos observes, it is still strongly resisted (47)—giving a reason that was already invalidated by Popper when he restored the old picture as approximately correct. Lakatos presented approximations as a part of his own methodology—with no reference to truth (101).

Lakatos treated a difficulty concerning the most basic rule within science: when a hypothesis and some empirical information clash, the information should win. As scientific information must be repeatable, it is hypothetical and so the rule is not obvious. Robert Boyle instituted this rule and raised this difficulty about it. Popper has overcome it with magnificent ease: the rule follows from the demand for refutability. Lakatos followed Kuhn and explicitly rejected the rule. They replace it with the demand that critics should offer an alternative to the idea which they wish to oust. This is otherwise known as "constructive" criticism. It is repugnant. Criticism *tout court* is creative and helpful. It leads to the search for alternatives to whatever it deposes. The openness of science demands frank and prompt admission of the impact of criticism.

11.2 In Praise of Destructive Criticism

The rejection of destructive criticism must be constructive, of course. What treatment, then, does destructive criticism deserve? Kuhn and Lakatos recommend taking defensive measures against it. This leaves refuting observations unexplained. Usually, attempts to explain rest on the assumption that what is to be explained is true. This does not preclude attempts to view it as an observation-error. These are better when done without defensiveness, as, for example, when both a hypothesis and its refutation are considered false. (In 1900 Planck did so with great success, 206). It is often said that since classical physics is still in use it is true. This is absurd, since this is to affirm mutually exclusive ideas. Duhem's alternative is consistent: in his view scientific theories are empty: they are neither true nor false. The alternative of Einstein and Popper is still better: the demand to disdain or ignore refuted hypotheses should go: classical physics is a false part of a proud heritage. The excellence of Popper's legacy lies in its explanation of the admiration for some false ideas as well as for their valid criticism.

The received rule against evasive hypotheses is strict; its application is flexible: efforts to appraise new situations are valuable, and they require time out. Copernicus is not censured for his tentative introduction of new epicycles as a part of his effort to cancel the persistent, old ones. And Galileo expressed admiration for Copernicus's distrust of his senses (regarding the orbit of Mars). Popper's harsh ban on evasive hypotheses gave way to his patience for non-trivial ones used as stopgaps (Popper 1966). Lakatos adds to this local gossip spiced with a touch of malice (89–92).

Feyerabend offered a historical hypothesis: all valid criticism was constructive (238, 330, 332). It is false: valid destructive criticism occurs regularly. Moreover, since it is harder to defy refutations than to give up the ideas behind them, constructive criticism is often accepted half-heartedly as it helps destruction. Popper viewed this as admissible, as the validity of criticism is indifferent to its pedigree. Nevertheless, Feyerabend had a point: good alternatives may point at overlooked refutations, and they are useful tools for criticism. Lakatos had a terrific *bon mot*: if you cannot refute a hypothesis, try to explain it first. Here he displays a totally different mood.

Already Bacon complained that the evasive rescue of refuted hypotheses is a common practice. Being a naturalist he prohibited it; being a conventionalist, Duhem promoted it. Kuhn and Lakatos allowed it temporarily—until a better alternative shows up. They also allowed the conventionalist perpetuation of the survival of rescued hypotheses: as Duhem suggested, placing competing alternatives into different systems prevents inconsistency. "Incommensurability" is Kuhn's term for this technique. He violated logic as he sanctioned both the overthrow of theories by scientific revolutions and their survival. He resolved the contradiction by calling the same item a paradigm when he deemed it dead and a theory when he deemed it still alive. Feyerabend violated logic otherwise: following Evans-Pritchard (Douglas 2013) he used Duhem's suggestion to rescue superstitions as incommensurable with science. This way he made superstition permanent though he allowed it only as a temporary measure, as an antidote to "the imperialism of science" (Feyerabend 1976). Lakatos was better off: he allowed rescue operations for the sole purpose of upsetting Popper (82). Otherwise his preferred alternative is the authority of research programs: his infamous LMSRPDQ [Lakatos Methodology of Scientific Research Pretty Dammed Quick].

Duhem misused the possibility that refuting observations harbor some unspecified systematic error: he embraced it as a blanket excuse against all refutations, thus robbing theories of their empirical character. It is better to restrict discussion to sufficiently specific excuse that may be testable and that should be put to test. The paradigm here is the discovery of an additional planet whose presence accounts for some perturbations previously unaccounted for (69). Logic requires no more than the removal of contradiction between observation reports and hypotheses. Neurath recommended that one statement be deleted, possibly the observation report. This deletion must be temporary, of course. A search is then afoot for alternatives to hypotheses and to observations that might lead to crucial experiments. Anyone who approves of science approves of tests; and anyone who approves of tests approves of crucial experiments—with the amazing exceptions of Kuhn and Lakatos. They hid behind Duhem's back. But he described some crucial experiments that he rated highly. They ignored this and perpetrated instead verbal confusion between Duhem's old terminology and the current terminology of Einstein and Popper. In the current sense of the word, any simultaneous test is crucial. The old sense restricts it to tests that result in verification. Denying the possibility of verification, Duhem rightly denies the existence of crucial experiments in the old sense (Adam 1992). The Kuhn-Lakatos ban on tests proper together with their distortions is better ignored.

11.3 The Correspondence

In the letters discussions are naturally unfocused. The breezy references in them to serious goings-on demand amplification and comparison with other texts. References here to politics, international, national and academic, are clearer. The bulk of the correspondence is naturally daily affairs: plans, work in progress and daily troubles, women, money and ill-health. The drama here is the increased distance from Popper and the mutual encouragement in that direction, together with some malice, perhaps in jest, repeated expressions of contempt for truth, hopefully tongue in cheek, and a preference for browbeating and ridicule over rational argument. Lakatos brags that he is the Grand Inquisitor of the Popper Church, that he has evaluated a student's intellectual ability by judging her appeal (to both of them) as a sex object, and that he has excommunicated me but does not ban my writings (150). Here is a charming quote, from a letter of introduction, Feyerabend to Lakatos: "all the little Popperians, cryptopopperians, parapopperians, popperopopperians, lakatopopperians should profit from her presence…. Beware, however, for she is a witch (truly). She is a good witch, so do not be afraid" (198). Feyerabend calls Lakatos "pop-Hegelian" (184, 194); he calls Popper our "*lapis irae*" and author of the "*biblis pauperum*"; he ridicules the "Popper-Lakatos commonsense"; he is "the grand master of the Neo-Bakuhnian [sic] Church of Intellectual Freedom and Self-Expression" whose father figure is "Pegel or Hopper" (192–196, 199).

The two share conservative politics and philosophy (194, 219, 242, 343, 356). This is puzzling as Feyerabend, a fan of Trotsky (151), repeatedly praised Lenin and Chairman Mao; he declared himself an anarchist. And they were in discord. Feyerabend's "sudden absence" from the Brandeis University philosophy colloquium where he was expected to cross swords with Lakatos (169, 178–180), had me step in as his stand in. I contrasted his hostility to method with Lakatos' advocacy of one (271). Lakatos reported to Feyerabend on my performance. The editor has left it out. Here, at times the two delight in disagreement (226, 252). Feyerabend's *Against Method* is dedicated to the memory of Lakatos as a "fellow anarchist", while referring to their disputes. So possibly they agreed on basics only. The correspondence begins with Feyerabend's assertion that he is "thoroughly Popperian: Karl always started his lectures on scientific method… with the remark that 'there is no scientific method'." (120, 126, 272) Soon Feyerabend dissuades Lakatos from preparing a volume in honor of Popper (147).

They allied themselves with Kuhn (94, 129, 181, 327, 351): "to take on a trio like US (Kuhn, you, me)—that takes guts" (359), says Feyerabend. (Kuhn expressed agreement in public meetings with Lakatos, with Feyerabend, and even with me. Popper's "Replies", 1974, 1144, contains praise for Kuhn as a student in his seminar.) Kuhn deemed scientific unanimity imposed in controlled mass conversion. The three agreed on this, but whereas Kuhn backed the scientific establishment, Feyerabend rejected it as imperialist. Lakatos amassed power, using slogans that he freely borrowed and distorted. He repaid his sources with betrayal

(224; Long 1998, 298). One day he will be powerful and impose proper education on the public, he told me; and then he will rectify his distortion and injustice, he promised.

Success came easy as scorn at Popper was in demand. The establishment distorted his ideas, craved criticism of them, and praised all attempts to supply it. The trio hit the jackpot. They claimed that Popper's portrait of science is idealized. His demand is minimal; they deemed it excessive; they dismissed the truth that unlike the established idealization of science, Popper's is moderate and commonsense. Their demand for constructive criticism made them offer alternatives to his views; these are not serious. As we never are really clean, they suggested, we better roll in the muck. Duhem advocated holding on to conflicting systems while keeping them separate and empty. A Roman Catholic, he dismissed all accounts of reality as metaphysical. Lakatos and Feyerabend did not; they used his view as false feathers (135, 138).

11.4 Inaccuracies

Lakatos reports that young Popper was a materialist and hence a non-positivist, "actually following Lenin, whose philosophical book he translated from Russian to German in 1919." (33) Possibly Popper did help a translator of Lenin to improve his German. Otherwise this account is misleading. Old versions of positivism were materialist. When Lenin viewed Mach an idealist, he prompted a protest. Even some "logical" positivists, notably Neurath, were materialists. Young Popper was a positivist, though never a "logical" one. He admired Mach and followed his advice to avoid both materialism and anti-materialism. Lenin may have heard a faint echo of Einstein. Even if matter is mere energy, he added as an afterthought, at least it is objective. (Feyerabend's praise for Mach was sincere, unlike his praise for Lenin.)

The book is full of minor inaccuracies. Penn, where Popper resided, is here Penn State University (260). Two wrong dates are given for my stay in the London School of Economics (131, 356). Feyerabend reports (147) having refused to co-edit a journal with me. His refusal was to be a member of my editorial board: he refused to have any official role, even a fictitious one. (He later joined another editorial board.) We learn here (401) that Lakatos's given name is Imre. It is Avrum. The debt to Popper that both Feyerabend and Lakatos owed are here both admitted and denied. The editor makes no comment on the many slips in Lakatos' lectures, such as that Sorel was a "right-wing extremist" (61) and that photons are atoms (72). For good measure he refers to a note of Popper "in which he [Popper] recognizes his debt to Duhem" (47). He relies on faulty books on Neurath (45). He falls for Lakatos's propaganda for a criterion of novelty of his disciple Zahar (108, 111) that belongs to Bacon (Bacon 1620, Aph. 109 *et seq.*). Lakatos' conjecture that Einstein was ignorant of the secular motion of the perihelion of Mercury during his research on gravity shows much ignorance. Lakatos finds important that the support that this fact lent the theory was due to (alleged) ignorance: it thus became "an unintended by-product of Einstein's programme" (111–112), whatever this means. This is not true: Mach had noted the evidence in question in his history of mechanics that Einstein admired. Well before Einstein studied gravity, Mach referred to Paul Gerber's 1898 calculations of the size of the secular motion of Mercury's perihelion and to Wien's 1900 discussion of it (in a book that Einstein was also familiar with). As to his program, it was to reduce all physics to sets of well-behaved partial differential equations subject to invariance to some transformations (and so to some rules of symmetry). He developed it in his late years.

11.5 Conclusion

The book's appendices introduce a ray of light. Both authors advocate here education for intellectual freedom. Lakatos did so in an early paper, originally in Hungarian. In the lectures he repeatedly denounces élitism and brands Popper élitist *malgré lui*. In letters he presented himself as an élitist. During the students' revolt he was bluntly reactionary. Yet his response to the Prague spring revolt is moving (149). The editor charges him with racism (64) and I would defend him.

The image that emerges is of two soul-mates, intelligent and educated, hyperactive and aggressive. They repeatedly expressed pleasure from and longing for each other's letters and company, exchanging much-needed moral support, in repeated efforts to cheer up each other, driven by unbounded ambition. "Moving the World Spirit? Not for me. I am a private citizen, not a general", protests Feyerabend in utter self-deception (351). They worked hard despite pain and anguish. They reported regularly on extended periods of deep depression. Chronic depression bespeaks poor self-image. The two resented Kuhn's having outdone them (210). All three sang to the gallery and won tremendous applause and came for curtain calls. Lakatos did (rightly, Hersh 1978) appreciate his work on mathematics; for the rest, they all knew, the worth of all their productions was limited. They worked hard, carefully deliberating about impact. They hoped to narrow gaps between repute and worth. They knew better. They could justly blame their peers for their discontent: better ones reduce such gaps. This is as good an excuse as they could find, and it did not comfort them.

The two were my close friends. They stayed in my home frequently. Things changed, perhaps due to my frank indifference. Feyerabend called Popper Kronos (210) and the editor notices that Kronos used to eat his children but Zeus was saved as his mother hid him in a cave and replaced him with a stone. This made me shudder.

I closed my philosophical correspondence with Feyerabend, as what he wrote about me in the learned press was not too frank. He responded with regret, writing that he had never meant what he had written about me. Lakatos used means more effective than the press: he ruined my prospects of an offer of a chair in the University of London. We discussed this three months before he died. He said he had to do it. So do feel free to take this essay as my settling of accounts.

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Part III
In a Nutshell

Chapter 12
The Essential Popper

Let me describe the essential contributions of Sir Karl Popper who was my teacher. It is generally agreed that thinking should be trusted or else it should not rank very high. *Popper's recommended rating thinking very highly without trusting it*. Some philosophers, preachers, artists and others do not trust reason. They recommend that we accord it some secondary or tertiary role in human life, second to faith or to art or to gut feelings. Popper denies that reason is trustworthy, yet he considers it one of the most important things we have and also the best guide we can ever have: reason is unreliable in the sense that it can never be assuredly free of error, but there is nothing better to rely on. This is what Churchill said of democracy, and Popper agreed: he did not consider democracy good, but he found nothing better to take its place among the regime that had been tried out.

Does Popper's philosophy have an essential characteristic? he did not think so. He objected to putting his philosophy in a nutshell. Nevertheless, I regularly put it in a nutshell. It is this: *use your reason as best you can but do not trust it;* do not defend your views: improve upon them. Elaborations on this comprise Popper's teaching. The whole of his output was in effort to answer critics who could not share his idea that our heritage consists of old noble errors. He tried to use his suggestion (that we should think but not trust our thoughts) as means for explaining our partiality for science and for democracy. Scientific progress is the elimination of errors; democracy is the peaceful overthrow of objectionable government. Now the partiality for democracy is much broader than the expressed reasons for it.

12.1 Fallibilism

Perhaps it is useful to name these ideas. The first is fallibilism: we are never assuredly free of error. The second is rationalism: reason is the only guide in human affairs. These ideas are neither striking nor new; their conjunction is unusual and problematic. Popper is the first to have succeeded in rendering this conjunction—fallibilist rationalism—surprisingly comprehensive and applicable

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to science and to democracy alike as systems that admit their shortcomings and invite and welcome criticism.

Popper applied his central idea to many questions, and his followers tried to do the same regarding other questions and somewhat differently regarding his questions. Their efforts to apply rationalist fallibilism makes them his disciples. (This is why it is useful to call it the essential Popper, as David Miller did earlier.) Here is one brief example. It is the work of Imre Lakatos, whose ideas about science differ from those of Popper. He should not count as a disciple of Popper in his philosophy of science, as it is not in the vein of fallibilist rationalism. He counts as Popper's disciple, however, in his philosophy of mathematics. He disagreed with Popper there too, but while retaining fallibilist rationalism. A major traditional dispute in mathematics concerns mathematical intuition. The intuitionists say that properly used mathematical intuition is utterly reliable. They try to base mathematics on it. Those who do not find any mathematical intuition utterly reliable recommend ignoring it altogether. This is scarcely possible, as Jacques Hadamard has stressed (Hadamard 1949). Lakatos said, we should employ mathematical intuition as best we can, be critical of it, and try to educate it. Lakatos developed a philosophy of mathematics around this idea and he won thus the position of a significant philosopher of mathematics who has raised many questions to explore (Lakatos 1976, 110–111, 121, 123). To elaborate on his philosophy of mathematics and to explain its importance is a different task. So let me return to Popper and his discussions of scientific progress of democracy. Popper disliked putting his philosophy in a nutshell. He also wrote against discussing questions that begin with the expression "What is…" and end with a noun phrase. What is scientific progress? What is democracy? What is freedom? Justice? Life? And so on. And what is the importance of science? And of democracy? Such questions, Popper said, what-is questions, are invitations for definitions, and definitions are verbal and arbitrary; they settle nothing. Let me explain and justify Popper's hostility to what-is questions and my dissent from it.

Consider an example. A new concept appeared on the political horizon, that of people's democracy, soon after World War II. What kind of a regime is it? This question matters little now, when we agree that people's democracies were not democratic, even though we do not quite know what democracy is. For, whatever it is, tyranny it is not; and whatever people's democracies were, tyrannies they were; hence, a people's democracy is no democracy. Today no one says that the regimes that called themselves by the name of people's democracy were democratic. Today their having called themselves democracies does not impress anyone. Similarly, calling divine the right of Christian kings to rule their people does not make it so. We may still speak of the divine rights of kings, but we do not think of them as divine. It is not different from calling a child King. It sounds funny that the boy next door, King, is not a king but a common child around the block. But this does not cause any concern, since no one mistakes King for a king. Also, calling Elvis Presley the King does not confuse anyone: the word is used metaphorically. There are even jokes made on the possible confusion involved, such as the name of Robin Hood's friend, a big fellow called John Little, or the name of a once famous film, "A Nameless Film".

Briefly, the confusion—about what democracy is—is not about a word, since we may call any regime by any name. Yet until Popper cleared matters we were confused, or at least in obvious error. At least I was. Let me elaborate.

"Truth emerges quicker from error than from confusion", said the great Sir Francis Bacon. The mistakes that are easy to eliminate and are not eliminated, then, may well be confusions. If so, then it is useful to know the difference between the two: when are we in error and when are we confused? This question was important for at least one famous modern philosopher, Ludwig Wittgenstein, because he took it upon himself to free his fellow humans of confusion, as he considered it the task of philosophy. He ignored the task of correcting errors, as he left it to science. And he did so explicitly. How did he decide about the difference between confusion and error? Some errors are obvious and yet they are repeatedly made and not corrected only because they hide behind confusion. Some of these are created by words. Wittgenstein said all philosophy is verbal confusion. When Popper developed his philosophy he was not understood because his closest associates in philosophy tried to find out what confusion he was trying to clear although he was developing a philosophy that Wittgenstein had declared *verboten*.

Wittgenstein's views on verbal confusion are very strange. He spent much of his life in efforts to eradicate traditional philosophy by clearing the verbal confusion on which he said it rests. He discussed these, but offered no general theory of verbal confusion. Perhaps he found no need for it, considering verbal confusion is any verbal mix-up. A mix-up is indeed confusion, but perhaps not the other way around. Thus, when we mix up Tom with Dick it is not that we think that Tom is Dick but that we know that they are different yet at times we behave as if we do not. We are confused here, as we are unclear about matters. If we say clearly, yes, Tom is the same (person/word/grammatical form) as Dick, then we may be right or we may be making a mistake, but either way we are not confused. If we do confuse Tom and Dick, then we are confused regardless of whether they are the same or not. If we mix them up, if we confuse them, then this is so because once we think that they are identical and once not. If this is true of all confusions, then confusion is a kind of contradiction. I do not know if all confusions are contradictions; it is impossible to find out what Wittgenstein thought. So let me see if I can describe my own confusion about democracy that in my student days made me think that people's democracies are democratic in some sense. Let me see how Popper cleared this for me not in the way Wittgenstein recommended but by discussing a genuine difficulty and offering a distinct philosophical idea, which, to repeat, Wittgenstein forbade.

12.2 Democracy

The word "demos" means the people, or the simple people, or the poor. "Democracy" means then that simple people govern or partake in government. They obviously do not. The government governs, the rulers govern, or the bureaucrats, not the people. Perhaps we should ask, what is the action that is called "to govern" and how can simple people govern? What is the act of governing? The word "govern" meant initially steer, navigate. In politics it means, both to administer and to make some political decisions, to implement justice and to legislate. People do not administer: administrators do. What decision is political we need not ask, as we know that most people do not make political decisions either. And so on. Hence, there is no democracy and that is all that there is to it.

Many people are confused when they are told that, since they know that France is a democracy. This confusion, I propose, is expressed in the ambiguity: people who are confused may admit, perhaps reluctantly, that what they say is not quite true, not stated with sufficient precision. They blame the imprecision of their expressions for their falsehood, taking for granted that with some attention they could restate their view well and then it would be seen that it is true. They thus take it for granted that certain false statements are poor substitutes for true ones, even though they cannot reword them properly this way. They would observe that full precision is impossible anyway. This observation is true, and it insures that clarity cannot always and fully be achieved, that reasonable comprehension should usually do. Yet not always is imprecision tolerable; not in the present case, for example.

Incidentally, Wittgenstein too knew that it is not reasonable to demand full precision in all discourse. But he wanted people to talk precisely when it matters. On this he was right. How do we know what matters and what not? Who is to say? This did not interest Wittgenstein except when it comes to philosophy. How did he know that? He did not; he was simply very confused. The rule is, disagreements about the importance of precision should lead to debates.

So let us return to the case at hand, the important case of democracy. We know that governments govern, not the people, and it seems we should put our view of democracy precisely—without denying this. Effort in doing this often failed, and so confusion about it still prevails. Now the very conspicuous fact about democracy is that it is not tyranny. In tyranny most people have no political influence. Hence, in a democracy people have some influence. Hence, the more influence more people have, the more democratic the regime in which they live. This idea is very famous and has a great appeal. It has a name: it is populism. Populism is the idea that as many people as possible should partake in political decisions. To make this possible, incidentally, it seems clear that politics has to be very simple, that decision-making should be very simple—in parliament, in the workplace and in the neighborhood. This idea was expressed in a captivating image by Vladimir Illich Lenin, the father of Soviet Russia and the first head of a populist tyranny: he said, a soviet system is so very simple that a simple cook can run a factory or sit in a parliament. This idea was basic for the soviets as he imagined them: representatives, large groups of them, participating in politics in legislation and in running the country in a just manner. As a university student I fell for this idea. I did not know that very soon after the Soviet Union was established Bertrand Russell visited it and reported that Lenin was a cruel person and that the Soviet Union was hell. I wanted to believe that the concept of people's democracy is more democratic than the British. Even had I known all this, however, it would not suffice to clear my confusion: I do advocate participatory democracy, that is to say, I endorse the populist idea that the more government is shared by common people the better. Yet I oppose populism. In my early days I simply confused populism with participatory democracy; had I known of Lenin's tyranny, I would possibly have disapproved of his practice, without dissenting from his theory. As it happened, I realized before I learned about Karl Popper that the Soviet regime was tyrannical and not democratic, but I needed to know more about it; I even suspected that populism is not democratic, but I did not have a sufficient explanation for this.

And so, not having a clear idea about democracy and about why it is so important, I was confused, and did not think properly, and instead of trying to listen to Russell I listened to Lenin. Of course, I was not alone in this: it was no accident that I read Lenin's books and did not even know of Russell's book about Russia: Lenin's books were translated into Hebrew, which is my mother tongue, but Russell's book still is not. When Chairman Mao Tze-tung behaved in a tyrannical manner comparable to that exhibited by his Russian comrades, my colleagues in Boston University, where I was teaching then, taught a course on the thought of Chairman Mao. This looked to me so significant that I interrupted my own lectures and read in class sentences from Chairman Mao's *Little Red Book*, which is a very straightforward expression of his penchant for tyranny and terror. I did not succeed to make any difference. Many philosophers in the United States at the time admired Chairman Mao, as they earlier admired Lenin and his heir, Joseph Stalin. Some thinkers, such as George Orwell, the author of the famous *Animal Farm* and *1984*, said, intellectuals admire power (Orwell 1946). He suggested that it was a character defect in intellectuals. I hope this is an error. Perhaps it is confusion. Perhaps people who expect too much are led to contempt. If so, then populists known as left-wingers express the view that common people are trustworthy, and then, when they should admit that they were in error, they prefer to blame common people and show contempt for them. As a result, populism is a mix of admiration and contempt for common people: it is sheer confusion.

12.3 Methodological Esssentialism

Confusion, said Popper, often rests on the search for the "real" thing. When we ask what democracy is or what is justice or what is science, we mean real democracy, real justice, and real science. Taking this seriously, one would say, quite possibly only democracy exists, not real democracy; possibly only justice exists, not real justice; only science exists, not real science. But they do not say that; they think democracy at heart is real democracy, justice at heart is real justice, and science is at heart real science. This "real" means ideal, and to say that at heart x is the real x is to say that at heart x is ideal, that what we have is not quite ideal but nearly so. This is a confusion that supports anti-reformist established orders, democratic or any other.

We are misled by the theory of Aristotle that says, definitions are certain and provide true knowledge of the real world. They are certain because they are stipulations of what we mean; alternatively, they are about facts and so not certain. When we ask what democracy is, we often mean, what regime will we approve of? The idea is that names are at times true but not always, as we show when we say, this is what I call a friend or this is what I call a real friend. Real names are the original names, the names that our mothers gave us. The etymology of a word is revealing: etymology is the search for the true meaning of a word, etymologically this is what etymology is: the etymos logos, the true meaning; the oldest is the truest. This is Plato's metaphysics: the oldest is the best, like the coin that exhibits its image best when it comes fresh out of the mint: the mint is Plato's Heaven, and things are matter impressed by ideas, coins shaped by the mint. Amazingly many people still take it for granted that clear thinking is assured by definitions that should offer the true meanings of words. Modern logic, nearly two centuries old, has proven this a logical error and confusion.

So much for Popper's reasons for his objection to what-is questions. He was in error about its scope. He said, a what-is question is a question about meaning. This is an answer to a what-is question: the question, what is a what-is question, is itself a what-is question. Popper was right in suggesting that we should not worry about words: we should try to find out what is a good regime, why Sweden is a better place to live in than China, for example. We may agree that China is a true democracy and Sweden is not a democracy at all, as long as we also agree that Sweden is better governed, and ask what is better about its government as compared with that of China. This is a better way to put Popper's just critique of Aristotle. Socrates admitted his own ignorance and said, he did not know what justice is, but, he added, we can more easily find out what justice is not. Thus we do not know what democracy is, but we know that it is not tyranny. Why? Because, said Popper, in Russia the people could not get rid of Stalin and in Britain people could and did get rid of Churchill. Popper called democracy this very ability. This ability matters; calling it democracy does not.

Popper thus opposed what-is questions in the platonic-Aristotelian mood. They are questions of whatness or of quiddity, to use a clumsy jargon. Now that Quine has discussed (many examples of) this matter (Quine 1987), it has become kosher, as everything Quine did. I do not know what it is about him that is so successful. Whatness is exactly that, the effort to find what makes a thing what it is. Things possess essences and accidents: it is an accident that humans have two legs each, since one with one leg or three still will count as human, but the ability to think is of the essence of being human. Accordingly, it is having a body and a soul: without a soul we will be asses and without a body we will be angels. Hence what makes us what we are is our being body and soul. This is convincing and confusing. It is the confusion of what makes humans human with the fact that we call ourselves human. Moreover, surely we will not call aliens human!

Popper knew he exaggerated when he saw all what-is questions as questions about quiddities. Some are not, he knew. (His counter-example was the what-is questions in exams, that are invitation to show that we have retained what our teachers have told us.) Moreover, not all quiddity-questions are equally worthless. Popper was impressed with Erwin Schrödinger's *What is Life*? He noted that, taken literally, the question is about a quiddity. He brushed this aside as mere trifle: it is better worded otherwise. Words, Popper always stressed, should not matter; what matters is that Schrödinger asked a question about a quiddity: what is it about life that makes it what it is? Popper seems to have rightly found worthwhile some examples of essentialism (when freed of Aristotle's confusion and his claim for certainty.)

12.4 Anti-metaphysics

The scientific revolution took place in rebellion against Aristotle. He had a very strong hold on intellectual life then, both because the academic establishment swore by him and because he had logic on his side. So the scientific revolution was hostile not just to Aristotle but to metaphysics in general: they abhorred it and tried to anchor their discourse in experience. The hostility to metaphysics found its justification in Bacon's doctrine of prejudice (Agassi 2013): when one endorses a metaphysical system one gets biased and sees facts in accordance with it. Every idea, however untenable, can be confirmed by facts because facts are seen as fitting it. There is thus no use arguing against its holders as they are prejudiced. They must give it up voluntarily: the unprejudiced see facts as they really are and then they can deduce from them the true scientific theory—like wine out of grapes.

Descartes corrected Bacon by adding mathematics to his image of science. And so metaphysics was admitted on the condition that it be demonstrated. Kant divided the metaphysics into the demonstrated foundations of science, and the undecided, concerning which one should have no opinion. Mach endorsed this view too, considering all metaphysics prejudicial. Following Gottlob Frege, Popper said, to have meaning, a statement should be true or false; science means what it says, he added, so that it theories are true or false; older ones are usually false, newer ones are putatively true and have to be tested. They usually are. Following Bertrand Russell, Popper said, total freedom from prejudice is impossible. Identifying science with ideas open to empirical criticism, he rejected metaphysics as the escape from criticism. And quiddity is a metaphysical entity. Now quiddity is a poor form of metaphysics: the view of reality that it offers is fragmented. The question, what is wood? is an example; not so the question what is life? Answers to that question belong to metaphysical systems; different systems hint at different scientific hypotheses, and these can be put to test. Thus, Popper's objection to metaphysics does not hold even on the basis of his own argument. Indeed, he gave up his moderate hostility to metaphysics and developed his own metaphysics, of which he was very proud. And as any other good metaphysical system, it offers a suggestion for a comprehensive system of quiddities.

Finally, following Frege's idea that the meaning of a sentence is its having a truth value, i. e., its being true or false; Popper concluded that scientific theories mean what they say; he followed Albert Einstein and Bertrand Russell in advocating realism and in admitting that science cannot be free of metaphysical assumptions. Towards the end of his life he developed his own indeterminist and dualist metaphysics.

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Chapter 13
Kuhn on Pluralism and Incommensurability

> The whole of science is nothing more than a refinement of everyday thinking
>
> (Einstein, 1936, 59).
>
> No fairer destiny could be allotted to any physical theory than that it should of itself point out the way to the introduction of a more comprehensive theory in which it lives on as a limiting case
>
> (Einstein, 1920, 78).

Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 1962, was an outstanding success: it was published in 16 languages and more than a million copies of it were sold. Like many a success story, it is not quite obvious why. Often a story is of success just because its vagueness invites different people to enjoy reading it differently and to feel good about it. This holds for Kuhn's book with a vengeance. Opinions differ even on the question, what is the message of his book? (Authors of obituaries on him have answered this question in different ways.) Three years after its publication a distinguished symposium devoted to this question took place; a volume of its proceedings appeared five years later still (Lakatos and Musgrave 1970) and was a big success too. Much of it presented Kuhn's views in a poor light. One contributor to it went far: Margaret Masterman claimed (Lakatos and Musgrave 1970, p. 59) there that the book was hopelessly ambiguous: the keyword *paradigm* that Kuhn had introduced there as his central technical term he used in more than two dozen different senses, she claimed (Lakatos and Musgrave 1970, p. 77). He subsequently tried to drop this term, but it stuck. The conclusion of that book is a famous response of Kuhn to his critics, in which he tried to explain his view better and to clear some of the worst misunderstandings of it. He found particularly disconcerting the view of him as an irrationalist or a relativist. He disliked even the view of him as having skated near irrationalism. He saw no irrationalism in his justification of the dogmatism of normal scientists, namely, those who performed research within the received framework of the day. Einstein and Popper had already noted this fact (in different words) with some measure of displeasure; Kuhn viewed it as essential to high efficiency and high efficiency as essential for science. Now efficiency requires a better classification of researchers into normal and leaders—even on the odd supposition that for the study of the structure of scientific revolutions Kuhn's description of leaders as the makers of new paradigms. Of normal scientists he also said that they undergo religious

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conversion with every paradigm-shift. Those who refuse to convert, he added, are excommunicated. This is odd. When Schrödinger failed to develop a theory of the electron following Einstein's paradigm he followed Newton's (his celebrated equation is Galileo invariant, not Lorenz invariant), he won applause. This part of Kuhn's teaching is simply better ignored as a minor slip. Now there is no religious conversion without soul-searching, personal crisis, and a change of identity. Yet paradigm shift is but a change of the way (the "grammar") of carrying out the job of research that Kuhn called "puzzle solving".

Not that all normal scientists engage in "puzzle solving": there are many science-based administrative and technical routine jobs. This is obvious in cases in which such scientists are compensated by the allotment of a day a week for research proper. On that day the scientist can show a degree of autonomy, not function (*à la* Kuhn) as "normal", i. e., as subservient. Let me leave all this for now.

A conference called "Incommensurability 50" in honor of Kuhn's book took place in National Taiwan University on 1–3 June 2012. I was honored with an invitation to participate even though I am known as one of its harshest critics. Going there just to repeat my criticism was pointless: participants were already familiar with it. I took the opportunity to present the book as favorably as I could. Making use of the opportunity that I had as the last speaker there, I also integrated into my paper as much as I could the lessons that other members of that conference said we may draw from it. I am grateful to the organizers and hosts of the conference for the opportunity and to all the participants in that conference for the opportunity to integrate their observations in this paper. Here it is, more-or-less as it was delivered—with the usual corrections and supplementations that such an exercise requires. It is, to repeat, as favorable an interpretation of Kuhn's views as I could gather.

By a traditional rule, an interpretation should enlighten. Reading one's opinions into some text does not (as Vladimir Nabokov has illustrated in his 1962 *Pale Fire*). And yet generations of Bible interpreters read up-to-date science textbooks into the ancient holy texts. Their reason is that they consider authoritative both science textbooks and the Bible and then they justly feel the need to harmonize them. And so another traditional rule is that an interpretation that is not enlightening may be valuable for some specified reasons. (Galileo and Kepler truncated the tradition that demanded of scholars to harmonize the latest scientific ideas with the Bible, relegating it from science to theology.) The worse tradition is that of reading some current philosophy into some old philosophical texts. The paradigm case for this is the reading of new philosophy of science into the philosophy of science of Immanuel Kant, a point repeatedly made in the Tai Pei meeting. It is called Kant with moveable categories or Kant on wheels. Already Hegel suggested this move. Ignoring him for a while, we can see that after Einstein such a move seemed necessary. Even before that, a greater need appeared: to give up Kant's view that Aristotelian logic is the last word in logic. This was taken in stride, until Euclidean geometry and Newtonian physics were superseded too. Moritz Schlick and Hans Reichenbach realized this before they encountered the ideas of Ludwig

Wittgenstein. They had both advocated that revision of Kant's philosophy of science, observing that any revision deprives it of all claims for certitude. And then they joined Wittgenstein in renewing the demand for certitude. Their suggestion for revisable *a priori* proposed ideas—for hypothetical, revisable foundations of theoretical physics—is quite right and will be taken for granted here, since Kuhn never doubted it.

Three years after the publication of Kuhn's *opus magnum* Lakatos gathered a conference of great moment in which Kuhn took a major place. One of the four volumes of its proceedings was devoted to Kuhn (Lakatos and Musgrave 1970). It was a best seller. In that 1970 volume Feyerabend said (198),

> More than one social scientist has said to me that at last he knows how to turn his field into a 'science'… The recipe according to these people is to restrict criticism, to reduce the number of comprehensive theories to one, and to create a normal science that has this one theory as its paradigm. Students must be prevented from speculating along different lines and the more restless colleagues must be made to conform and 'to do serious work'.

Kuhn was appalled: he appreciated conformity only as means for rendering normal scientific activity efficient. Yet unlike his predecessor Michael Polanyi he ignored the less favorable option (that Feyerabend reported common).

Here then is a benefit from the study of interpretations: they may offer some information on the interpreters if not on the texts they interpret. Thus, if exegeses on Kuhn are interesting, then the focus should be on his popularity: even if these exegeses tell us little about him, they still may tell us much about ourselves, especially on the diversity of our views today. Possibly this may be reassuring in that the possibility to read one's own view into Kuhn possibly boosts one's confidence due to the esteem of Kuhn as authoritative (not as a religious leader but as a wise scholar). I intend to present here a view that Einstein has originated, and that both Kuhn and I have endorsed. It is not quite popular, at the very least because no other commentator advocates it consistently. In times of stress commentators do take recourse to it, though. It is thus half-believed, or at least possibly half-believed. This is a well-known quality of all myth, and science and rationalist philosophy can serve as myths, and at times they do—if only to relieve pressure. (This is not to recommend viewing science as a myth system, even though as a myth system it is admittedly the very best available; it is to observe that at times it does serve as a myth-system.)

The paradigm case is the recourse taken to instrumentalism: scientists do not like the instrumentalist view of theories as mere *façon de parler*: they are too curious to allow that science is merely an instrument for prediction. Nevertheless, when they have to give up the hope that a given good theory is a true description of some laws of nature, they say, at least it is a good predictor. This assertion, regrettably, they take as an expression of instrumentalism. As to Kuhn, he was no instrumentalist, but he did use an instrumentalist idea, and even centrally so. Pierre Duhem said, since when we understand a scientific theory literally it is refutable and since science is demonstrable, it is better to empty theories of their contents and view them as no more than instruments for prediction. For that we need to keep them consistent, but as putting together two variants of a theory from different periods violates the rule of consistency, we should insure the avoidance of mixing variants. This, Duhem suggested, is easy to achieve by considering different theories to be different languages. To that end he added at this point his invention: the theory of implicit definition. That invention is very valuable: David Hilbert soon rendered it a fixture in mathematics. Yet for science it is redundant. Hilbert's move was both homage to Duhem and an exposure of a great weakness: his proposal transforms all theoretical science into applied mathematics although we do regularly and easily distinguish between any branch of science and its application that belongs to applied science or to applied mathematics.

What threatens Duhem's separation of scientific theories by viewing them as languages is the ability to translate from one language to another, thus permitting putting together two conflicting variants of the same theory. To prevent this inconsistency Duhem claimed that precise translation is impossible. This idea is variably known as Quine's thesis of radical untranslatability or as Kuhn's (or Kuhn's and Feyerabend's) thesis of incommensurability. Of course, neither Quine nor Kuhn (nor Feyerabend) was an instrumentalist. This is no trouble: the thesis that Duhem offered in defense of instrumentalism is obviously independent of it: there is no need to be an instrumentalist to share it. One may likewise ignore it, unless it is shown to play a significant role. Still, at this stage suffice it to claim that we need not oppose Kuhn's thesis of incommensurability when we try to read his views as much in accord with Einstein's as possible: we can identify Newton's mass within Einstein's theory, but only approximately so, not absolutely. This is reading incommensurability into Einstein's view of science.

Is this reading correct? Was Kuhn in agreement with my Einsteinian views? Possibly: he has published his claim that he hardly disagreed with Popper (my mentor) or with me, not to mention Einstein. But then he was too generous with such claims, expressing agreement with parties who were in dispute with each other. Let us ignore this and center on the main question: what are (Kuhnian) paradigms? All we know for sure about them is that they are incommensurable ideas that serve researchers. It is here that the reputed internet *Stanford Encyclopedia of Philosophy* contrasts Kuhn's view with Einstein's (Art. Kuhn). Is that right? This depends on the exact meaning of incommensurability. If Margaret Masterman was right in finding dozens of meanings for his terms (Lakatos and Musgrave 1970, p.77), then it is very hard to decide on this question.

This is no censure. Kant, the most pedantic philosopher ever, did not much better. Leafing through any issue of the quarterly *Kantstudien* suffices to reveal that many possible readings of his texts are available. Their very availability indicates that Masterman's charge against Kuhn holds better against Kant. And yet we do not dismiss Kant on this ground. Indeed, formal logic shows that almost all philosophies are open to a variety of readings. This is a great risk: we may easily jump to the wrong conclusion here and declare every reading legitimate which commentators tend to ascribe to Jacques Derrida (Dawkins 1998). To repeat, the exercise is then too easy and quite useless, and the more detailed the worse. To avoid this, let me say what limit of my reading of Kuhn I offer. I still disagree with him, his assertion to the contrary notwithstanding. If I have any claim to have contributed anything to current philosophical discourse, it is that I have tagged to the problem, what theory is rational? the problem, what debate is rational? I have proposed that we debate the rationality of debates. First and foremost, I say, to be rational a debate should follow the standard rules of proper, polite debate. Disputants are too often impolite, but good debates can be recast in accord with the rules. Hence, this is insufficient: questions under debate must signify. By contrast, scholastic debates may be properly conducted, but they concern antiquated questions that signify no longer. What makes a question significant? What is required of a reasonable debate? I do not know but this does not matter here, as we all agree that Kuhn discussed significant matters. Let me conclude this preliminary discussion then by repeating that Kuhn claimed that in science all—or almost all—controversies are marginal and I claim that they are central. Let this ride.

The exegesis of any text should start with the question, what question does it answer? For, as R. G. Collingwood said, unless we have a question, we do not quite understand the text that answers it (Collingwood, 1939, p. 24). Perhaps we need more: perhaps we have to indicate, however loosely, the context of our discourse. In the present situation the context is within the philosophy of science, and, specifically, with post-Kantian philosophy of sorts that is known as Kant liberalized or Kant on wheels, since his own categories are Newtonian and thus admittedly out-of-date. The idea that his categories should be liberalized is already in Hegel, who wanted them historicized. Hegel saw no problem here, since he deemed truth relative and claimed that every doctrine that once dominated scholarly opinions is true, and that hence contradictions are true. This is all irrelevant: Kuhn repeatedly and insistently declared himself a rationalist. If we want our exegesis to stay within some limits then we must admit that Kuhn advocated a view that he considered rationalist. We must allow for some differences, but they must stay within reason. There are then slight differences, there are differences that are complementary—like the famous differences in views of the elephant as having legs, or as having a trunk, etc.—and there are differences that are contradictory and so they cannot all be parts of a whole unless somehow tampered with (in order to remove the contradictions).

The most important preliminary point here is that our discussion thus far concerns comparisons of ideas without grading them—for their importance or for anything else: we compare them in order to understand, and so, when we compare Kuhn, say, with Duhem or with Einstein, we imply no compliment and no censure. Another preliminary point is that all parties to the present discussion take it for granted that observations are theory-laden. Who discovered that all observations are theory-laden? Some say it was Russell Norwood Hanson, others say it was Kuhn, or perhaps Duhem. The idea was sufficiently forcefully argued in the writings of Galileo already, four centuries ago: without a theory, he said, while you are strolling in a moonlit street you might as well view the moon as jumping from rooftop to rooftop like a cat. Perhaps we should ascribe the idea to Galileo and to Bacon, both of whom came upon it, each in his own way, more-or-less simultaneously. If we want a thorough discussion of this idea, then we have to go to the works of William Whewell, the nineteenth-century philosopher. The reason that so often philosophers take for granted the opposite idea is that they seek inductive justification of theories by reference to facts, and it is too easy to use theory-laden observations to defend the theory that is used to describe them. But here as exegetes we are on safe grounds: Kuhn's opposition to inductivism was never under dispute. So let me leave this now.

A brief observation may be in place here. In my youth I learned that Einstein's relativity is so difficult that only a handful of people understand it. I was surprised to learn it in the university in a course that was so relatively very easy. The difficulty to understand relativity, I found, is that it conflicts with Newtonian mechanics. Newton said that time is absolute and Einstein disagreed. Once we look at the situation with an open mind and say, we do not know if time is absolute or relative, it becomes easy to see why Einstein declared it relative—be he right or not. Understanding a thesis and assenting to it are different: understanding a thesis—at least to some degree—must precede judging it. The idea that sufficient understanding of an opinion renders judgment on it obvious implies that I do not understand all the opinions that I have not decided to endorse or reject. Once we separate understanding from judgment, we find many difficult ideas not difficult at all, especially those that are too stupid to entertain.

This holds particularly for scientific revolutions. These are difficult to understand because we are told that science is certain and that hence there can be only one scientific revolution, the one that started science going, since every achievement of science is supposedly final. This idea belongs to Francis Bacon if not already to Plato. Let me stress that before Einstein most researchers took it for granted that Newton's mechanics will stay forever. Hume thought so, Kant thought so, Whewell thought so. The first who doubted it was Solomon Maimon (who died in 1800), and that has led to neglect of his works and of his many interesting ideas. Faraday too was of the opinion that Newton's mechanics is not the last word on its subject matter. But then he never said so openly. Once we clearly withdraw the claim that successful scientific theories are here to stay (as Faraday explicitly did), then the very idea of recurrent scientific revolutions becomes not difficult to understand. It does raise many new problems, and some are fascinating and exciting and quite welcome. Perhaps here we can spot Kuhn's major public service: he made popular the very idea of scientific revolutions, regardless of who was the first to advocate it. This idea is still somewhat frightening but already not taboo. And therefore many intellectual leaders wish to refer to them but are still wary of doing this too openly. So we have a comfortable Kuhnian synonym for scientific revolutions that is nowadays very popular: paradigm shifts.

When discussing Kuhn's philosophy, what we want to know most is, how did he manage to avoid both relativism and dogmatism, as he said he did. Here let us suppose that he did. After all, there is no proof that he failed, and so we may assume that he succeeded. His chief problem then was, how can science display the pluralism that since Einstein it does, yet avoid controversy? To come to grips with this question, we should dismiss some common superstitions first and see what remains then of Kuhn's celebrated teachings.

Whatever paradigm-sifts are, the important observation about them is that they do not occur in Bacon's way. He assumed that you build solid foundations and then add to them abstractions of higher levels, with each layer secure before the next layer comes on top of it. Newton adopted this idea and he had trouble with it, as he knew that rather than basing his theory on those of Kepler and of Galileo he refuted them both. Newton was obviously troubled by the fact that his and Galileo's theories are not in full accord, whereas Bacon's theory of science says that they should be. Indeed, he took pride in observing that his theory explains the irregularities—the devations of planets from their Keplerian ellipses—that were observed as Jupiter and Saturn came near enough to each other for their mutual gravity to have visible effects. Yet he also declared Kepler's laws true. The same holds for Galileo's theory: as gravity is smaller on a mountain than on the seashore, so the speed of a pendulum clock differs on the mountain from on the seashore. However small this difference is, since it is cumulative, it can be measured. Galileo said bodies fall; Newton said, no, they are in mutual attraction with Earth. This is an instance of incommensurability. People found it mysterious. Kuhn had said, two different paradigms are incommensurable and people argued with him, saying, we do compare different theories and even regularly. Kuhn devoted a session in an American Philosophical Association 1983 conference to a discussion of this point—which he made in his own name as well as in the name of Feyerabend: of course theories are comparable, he said then in the discussion of his paper ("Rationality and Theory Choice"), but in only one specific way. The comparison of results of observations according to Galileo's and Newton's and Einstein's theories of gravity is well known. But we cannot compare a theory of action at a distance with a theory that denies its very possibility. Theories are abstract pictures of the world and we do not know how to compare pictures, much less abstract ones. This is incommensurability with no mystery. Einstein, in particular, stressed it repeatedly. The mystery is still there: Newton's third law cannot be endorsed by Einstein, as it is of action at a distance, and what exactly replaces it is no easy question. (Kuhn found some allure in the mystery: researchers who follow different paradigms, he said, "live in different worlds" (Kuhn, 1962, p. 150; Fuller 2000, p. 267; Nickle 2003, p. 2).

Einstein overcame with ease obstacle that had troubled Newton. During the whole of his career he provided for contrasts between theories and their successors, at times possibly too arbitrarily, such as in his unified field theory, where he added a factor ($g^{-½}$) just to that end (Einstein, 2004, Appendix II. Relativistic Theory of the Non-Symmetric Field). Otherwise, we might look at the theory not as a unification of two theories (of gravity and electromagnetic) but as their mere conjunction—which is what was said against the unified theory of Hermann Weyl (Dusek 2006, p. 224). This is all fairly clear these days, but it was not clear at all when Kuhn struggled with his material, and, naturally, he was often unclear and developed his ideas and became clear about them only slowly.

This too some find difficult to understand—those who will not let go of the opposite idea of the clarity of all proper thinking. This idea I would like to call Wittgenstein's superstition. The superstition is, whatever can be said at all can be said clearly and what cannot be said clearly should not be said (Wittgenstein 1922, p. 7). As Popper responded (in the name of his friend Franz Urbach, Popper 1963, 71n), this is where things get interesting. For, Wittgenstein prohibited stuttering, and this prohibition blocks learning to speak.

A variant is the idea that disagreements are objectionable, the idea that when reasonable people disagree then, clearly, there is some misunderstanding between them that should take little effort to clarify so as to reach agreement. I would like to call this Carnap's superstition. Michael Friedman is these days the leading exponent of the philosophy of Rudolf Carnap and he ascribes to him this idea (Friedman 2000, pp. 148–49). Hans Reichenbach too viewed clarification as the way to agreement (Reichenbach 1978, p. 4). This superstition is common and it renders the rise of scientific revolutions incomprehensible. This is erroneous for a very deep reason: disagreements are between ideas, not between people. This was significant for Friedrich Nietzsche: "He could be inconsiderate only towards ideas; not towards the persons who had the ideas" (Gilman 1987, p. 93). Disagreements are not personal and clarifying them only separates the real disagreements from the seeming ones and sharpens them. Kuhn always tried to be realistic and so he tried to offer an image of science from the outside, but he pooh-poohed disagreements. This is a distortion that most historians of science are guilty of. (Alexandre Koyré is perhaps the first influential historian of science who took seriously disagreements between scientists. Kuhn admired him but ignored this.) Kuhn stressed that some distortion is unavoidable (Kuhn, 1962, pp. 139–143, 173). This is important, because most criticisms of Kuhn's views are that he idealized; this criticism, valid or not, is better overruled unless shown that the idealization should be avoided. Thus, the claim of Imre Lakatos that the distortions that historians are making are often the results of anachronistic clarifications justifies the demand to avoid anachronism as much as possible. (It is permissible as marginal remarks, though.) This is not the end of it, since a historical study may be of a clarification, where a historian has to cite and compare for clarity a few variants of a theory. These must be all within one paradigm, since incommensurability of paradigm prevents comparing two texts from two different paradigms. Here the difference of opinion between Kuhn and Reichenbach is of a principle, as Reichenbach claimed to have shown Einstein clearer than Newton. It was an obstacle to progress.

This renders most significant the decision as to what two variants of the same idea belong or do not belong to the same paradigm so that they are or are not commensurable. I do not know if there possibly is a rule for that. But there is a partial rule. It is a combination of Collingwood's suggestion not to clarify a statement before deciding what question it answers and Charles Sanders Peirce's suggestion not to debate the truth-or-falsity of a statement before examining whether it is an answer to the question on the agenda.

Then there is the wish to present clearly an answer to a give question. Galileo said, discuss with its advocates its possible implications before offering them criticism. And, of course, in all cases of placing stages of a procedure in a time-sequence some movement back and forth should be tried to avoid dogmatism (Laor and Agassi 1990, p. 152). Such movement confuses people as the rules demand that we say what we are doing at every stage yet we are not always careful about it. Consequently, some people say, during a discussion of the second stage, when we examine an idea, we can always change its meaning so as to dodge criticism, to avoid admitting a refutation. Even the marvelously clear Pierre Duhem made this mistake. He wanted people to be clear about their moves and he found painful the inconsistency that avoiding doing this creates. Yet he himself, though not inconsistent, recommended defending ideas against refutation by always dodging criticism. The idea that this can always be done is all too obvious ever since antiquity, and we find Plato describing Socrates fighting it like a lion. Yet philosophers of science repeatedly find this a great discovery and a commendable technique. Kuhn was realistic here. He said, scientists are defensive, and their defensiveness is tolerated up to a point. This point he called a revolutionary phase. This is all a bit too fuzzy in his theory and I think this fuzziness is easy to rectify—and make more realist, while stressing that the defense of a theory should always be viewed with suspicion and with the aim to educate scientists to become less tolerant of their own disposition to be defensive. Lakatos was right to say that playing with the precise meanings of terms is to be encouraged in the heuristic stage in search of an answer to a given question and avoided in the debate about sufficiently clearly stated answer to it. (The move from one of these stages to the next can also be questioned, of course.)

To conclude, once we eschew defensiveness it becomes obvious that two paradigms, one of which replaces the other, are incomparable as images of reality but comparable quantitatively in crucial experiments. This is what Einstein had hoped we learn from the revolution that he started. It is a position that allows for improvement; this improvement, when it comes, is a surprise. Here is the challenge of incommensurability. But incommensurability is also a boon: before Einstein the demand to forget false theories was dominant—largely because observations are theory-laden and false theories distort observations. Einstein's revolution discredited this idea: we will not forget Newton's theory. Therefore, say the relativists, Newton's theory is not false. No: false theories are not to be forgotten: our heritage is a series of errors, some shameful, some noble.

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Chapter 14
Paul Feyerabend and Rational Pluralism

Paul Feyerabend said in his autobiography that we were friends of sorts (Feyerabend 1995, 96). Let me tell you of what sorts. We enjoyed each other's company tremendously in private while in public he said about me things that were less than friendly, telling me in private to ignore them since he did not mean them. Let me get off my chest one item, the very worst, and do it at once, and then be relatively comfortable.

Feyerabend declared in his *Erkenntnis für freie Menschen* (Feyerabend 1979, 92) that the Führer was "*eben ein intelligenter Mensch, intelligenter als meisten kritische Rationalisten, mit einem klaren Blick für die Komplexität historischer Prozeßen*"—an intelligent man, more intelligent than most of the critical rationalists, with a clear view of the complex historical processes. Feyerabend did not think the monster was more intelligent than most critical rationalists. He could not possibly find anything that indicates that the monster had a clear view of anything, let alone the complexity of historical processes. The Catastrophe that the monster has caused hardly indicates intelligence or a clear view of historical processes; nor does his *Mein Kampf* testify to any. In a tragic error, it is well known, German Jews took that book for a joke. Feyerabend could not consider the monster more intelligent than critical rationalists like himself. Nor could he consider Popper "just a tiny puff of hot air in the positivist teacup". If Popper responded to this, he did so only obliquely, and only in a private letter to Hans Albert (Morgenstern and Zimmer 2005, 207): "Heute ist er Anarchist; morgen ist er Fascist"—today he is an anarchist, tomorrow he is a Fascist. Still, Feyerabend had a point even though as usual he exaggerated enormously. The point is that of the banality of evil, as Hannah Arendt has christened it in the title of her famous book (Arendt 1963) and as Feyerabend repeated with approval. Her thesis is her way of telling you not to hasten to condemn the Nazi philosopher Martin Heidegger, since you too could fall for the Nazi vision as he described it. As far as Heidegger is concerned, she may have been right: Heidegger had little understanding and less imagination. His follower and fan Herbert Marcuse told him he had no idea what the Shoah was when he said in response to it, in war people get killed. All this hardly holds for the Nazi monsters. True, morality requires that we should still treat them as humans; but as a courtesy, as saying, we are not nearly as evil as they were. Yet they truly

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were evil and their appearance should make us rethink many issues; but we can learn nothing about morality form observing them. That should do.

Feyerabend was an academic star. He was a charismatic teacher and speaker. He joins the many remembered stars whose contributions were major or minor, light or heavy, whose reputation and appeal were ephemeral or lasting, vain or justifiable. All of these categories are occupied so that the test of time is but a default option, not always reliable, although the light, vain reputation is usually less likely to be durable. I say "usually", since some durable human weaknesses perpetuate some vain reputations. One of these human failings is power worship and so some cruel tyrants have managed to retain renown for generations on end. The same holds for the seriousness of a contribution. The exceptions here are some empty works durable due to their pomposity. Some folly may supplement a serious contribution to our heritage and may be better remembered. My standard example here is Imre Lakatos, friend and colleague of the both of us, known less for his lasting contributions to the philosophy of mathematics and more for his prattle about the philosophy of economics. As to Feyerabend's fame, it is too early to know how durable it is. How serious is it?

In 26–29 September 2012 Humboldt-Universität zu Berlin held a conference in Feyerabend's memory. I was honored with an invitation to participate even though I am known as one of his harshest critics. The conference organizers said this. His provocative proposals continue to excite an extensive assortment of heated debates across a range of disciplines; these have proven to be of interest to a broad audience. Thomas Nagel says, he was "consistently outrageous" (Nagel 1998). He was more than that. What? The conference, its organizers hoped, would encourage research on his philosophical proposals in efforts to do justice to the notorious complexity of his ideas and their relevance to ongoing discussions. They had a question, not an answer—which is fair. Let me take it from there.

I open where I have left off. In 2004 Friedrich Stadler and Kurt R. Fischer edited a volume about Feyerabend. I reviewed it *in Philosophy of the Social Sciences* (Agassi 2006). The book is full of expressions of praise of him as a serious thinker; yet all of its authors but one admit explicitly that he had contributed no interesting idea. The exception is Julie Floyd. She ascribed to him two or three important novel ideas. One is that observation is theory-laden. Galileo already stated it very clearly. Feyerabend learned it not from Galileo but from Popper. Another is pro-science pluralism. Feyerabend never expressed it; Popper did. Feyerabend did support pluralism but as anti-science, and with some justice. For, the classical idea of science was the set of proven assertions; Popper's replacing proof with openness to empirical criticism opened the road to pro-science pluralism. He developed pluralism before Feyerabend and I can testify to this as I sat at his feet when he did so. Popper's first vintage of 1935 was not pluralist. He introduced there his theory of degrees of testability (§30) in order to explain the alleged unanimity among physicists. The unanimity in question is limited, as researchers make efforts to break it (Agassi 1981, 164–191). Popper presented his pluralism in his magnificent "Towards a Rational Theory of Tradition" (1949, reprinted in Popper 1963), where he offered a theory that put on a par the diversity of tradition, legislation and scientific hypotheses. Marcello Pera has offered a penetrating analysis of that paper in his (Pera 2006, 277). In allusion to Feyerabend's exaggerated slogan, "anything goes", Pera contrasts Popper's pluralism with the assumption of "a very thin moral framework, within which almost everything goes" (Pera 2006, 279). Pera is a conservative, and he presents Popper as one, in a manner that commands the attention of those who do not see him that way. Nevertheless, Popper's reformism that rejects extreme social philosophies, radicalism and conservatism, allows for all options from *quasi*-radicalism to *quasi*-conservatism (Wettersten 1987, 339–341).

The conclusion of my review of Stadler and Fischer is, the book must be mistaken: Feyerabend surely did make a serious contribution. The fault is in the tradition that credits novelty only to a discovery of a new fact or of a new theory. Editors of periodicals and of encyclopedias, for a conspicuous example, contribute novelty in ways still unacknowledged and still unstudied. Let me now take up the thread where I have left it: what was Feyerabend's contribution?

It was of another kind of contribution that we all recognize and still do not acknowledge: that of a public teacher, of an author who shows the reading public how to apply new techniques or new ideas. There are good and bad teachers. Feyerabend was both. He taught good and bad things and things that were both. Like a Guru, he offered some obviously outrageous proposals intending his audience to think and find what interesting proposals he had in mind.

Example. The philosophy of science is taught regularly as ideas about rational belief. So Feyerabend taught it by calling Galileo a mountebank and by teaching scientific lies (Feyerabend 1975, 106n).

Another example. Dr. Helmut Mayer's review of the first volume of Feyerabend's posthumous works, *Naturphilosophie*, edited by our host Eric Oberheim and his colleague Helmut Heit, cites Feyerabend to say, "The Odyssey begins with Parmenides" (Mayer 2009). This aphorism is both profound and jocular. *Odyssey* is the story of a long, arduous homeward journey. Feyerabend's *Naturphilosophie* begins with Parmenides' doctrine of rationality as proof and ends with viewing scientific proof as impossible.

Third example. In response to my having called Feyerabend a Popper disciple he said this: he was not, although he did listen to Popper's lectures, sat in his seminars, visited his home, and talked to his cat. Were the cat able to respond verbally, it would have asked, how come Agassi both called you a Popper disciple and begged you on his knees to be one?

The name of one of Feyerabend's books is *Farewell to Reason*; it is obviously a declaration of irrationalism, yet Dr. Oberheim objects to the "popular misconception" of "Feyerabend's later philosophy according to which Feyerabend dropped *rationality* as the explanation of scientific advance, arguing that science's development is primarily guided by *power, propaganda, and prejudice*" (Oberheim 2001). We can easily clarify the misconception by taking seriously the book's tirade against "the alleged friends of Reason": it is not against the true friends of reason. Feyerabend says there, if this is reason, then farewell to reason. He does not say, farewell to reason. It only seems so. To be precise, his parting shot is this:

"Of course, to write such a book I shall have to cut the remaining strings that still tie me to the abstract approach or, to revert to my usual irresponsible way of talking, I shall have to say, FAREWELL TO REASON."

(In his presentation of Cardinal Ratzinger, later Pope Benedict XVI, on rationality Angelo Matera discusses Feyerabend. He notes that his expressions are exaggerated and yet declares him an irrationalist; Matera 2008.)

Ambiguity runs systematically through Feyerabend's later writings. He said, "I can hold opinions without either having or giving reasons." This looks like license for unlimited arbitrariness; it is in fact the (true) observation that we cannot avoid all arbitrariness. Most rationalists will mistakenly disagree and disclaim all arbitrariness. (Claiming that one is free of prejudice is humbug, said Russell 1956, 77.) Deviation from classical rationalism may be critical rationalism (or some other, still unknown version of rationalism) and it may be irrationalism (of all sorts). The variant terms, non-justificationism (that Willard van Quine or William Bartley invented) as well as non-foundationalism and anti-foundationalism (that Richard Rorty invented) are synonymous, yet they are used differently. "Non-foundationalism" denotes irrationalism; "non-justificationism" denotes critical rationalism. Feyerabend's assertion, "I can hold opinions without either having or giving reasons" when understood as license, is ambiguously either careless license to turn philosophy into sophisticated cocktail-party chat, or license to be seriously critical of any view.

Thus, Dr. Oberheim is right: Feyerabend complained that many scientists make science serve propaganda and prejudice; he did not praise them. The cause of the popular misconception that he complains about, then, is due to his style. It is one thing to write ambiguously—which to some extent is unavoidable—and quite another thing to introduce the systematic ambiguity that George Orwell called double-speak. Traditional religious double-speak is prevalent, on the popular (false) assumption that critical thinking is only for the educated. One reason I left my religious background is my dislike of double-speak; I admit I do not particularly like reading the older Feyerabend.

Consider his claim that he never was a Popperian. In his book on Feyerabend's contribution, Dr. Oberheim has commented on this. To say that Feyerabend distanced himself from the Popper school is one thing, and to say that he took Popper's ideas seriously in a pluralist spirit and examined them is quite another. Oberheim ascribes to Feyerabend the second option, which is Popperian, since his criticism is pluralist and Popper proposed to take seriously and examine critically any possible answer to an interesting question. Many earlier thinkers wished to present this very idea consistently. Feyerabend cited Helmholtz, Boltzmann, Mach and Duhem as examples. They all failed, as they held that rationality is proof. Feyerabend glossed over their failure; nevertheless, his omit. He chiding Popper for not having paid them homage. He may be right.

Feyerabend contributed some gems that appear in his three volumes of essays. Let me single out his "Problems of Empiricism" and "How to be a Good Empiricist". His thesis, as he has put it (Feyerabend 1999b, ix–x, 104–105), is, criticism $\Longrightarrow$ proliferation $\Longrightarrow$ realism

Popper said,

criticism $\Longrightarrow$ realism $\Longrightarrow$ proliferation $\Longrightarrow$ criticism

(realism makes explanations competing and invites their critical examination).

True, Feyerabend ascribes all this to John Stuart Mill. But even were his ascription correct (check his *A System of Logic*, book III, chap. XIV), it would scarcely belittle Popper.

Feyerabend's *Knowledge without Foundations* (Feyerabend 1962) is terrific. John Preston rightly views it (Feyerabend 1999b, introduction) as one of his two most Popperian works. There are then two Feyerabends, the young mature one and the old immature one. I appreciate young Feyerabend's work and have endorsed some of his criticism of Popper (Agassi 1981, 10). In his introduction to his first two volumes he refers to his own change of view, and rather than explain he observes that all ideas can be defended. This is true, yet only good ideas can be defended well. Popper began his thinking by taking seriously the fact that good ideas may be false: naturally, Newton's mechanics was his paradigm case. Classical rationalists took for granted that truth is absolute and that only true ideas are properly defensible. Disappointed rationalists shifted to relative truth whereas they should have shifted to relative goodness of defense—the option that Feyerabend and I deemed commonsense.

To return to Parmenides—to the Parmenides of Popper and of Árpád Szabó (1978), to be precise. The unique thing about western philosophy is in its Greek origin; the unique thing about Greek philosophy is its equation of the distinction between reality and appearance with that between durable substance and fleeting phenomena. To be precise, since all distinctions are valid, we should say, the dichotomy between the absolutely stable and the utterly ephemeral is peculiarly Greek: everything is either unchanging substance or elusive mirage. The ephemeral, Parmenides concluded, is but a dream. A subtlety of the Greek worldview invites highlighting. Everybody knows that things are not always what they look. Parmenides was the first to say explicitly the great dichotomy as a principle on which his worldview rested: the world splits into dreamlike appearances and immobile reality. Real things may be made of atoms and they may be made of the Four Elements of Aristotle, or of the Five Elements of the *Wu Xing*. This desk in front of you is unreal; what looks like a desk is really something else. It may be a swarm of atoms or a mix of the four elements or of the five elements. This puts the Greek and the Chinese cosmologies on a par: both fit the great dichotomy but the dichotomy is Greek. And there lies the difference: pushing a familiar idea to the limit leads to the theory of substance, and this has no equal in cosmologies that are not heirs to the traditional Greek. Objections to this theory have led to its overthrow about a century ago; Feyerabend teaches us how to live without it.

Example. Feyerabend felt strongly about the cleavage between art and science (Feyerabend 1967). Since art was traditionally not subject to the classical standards of rationality, views on art were traditionally pluralist; writings in the critical spirit proliferated in art criticism, especially in literary criticism. Feyerabend wished to see the histories of art and of science united. Were the two ready to learn from each other, he suggested, were historians of science pluralists, they might contribute to rendering science part of the heritage of the common educated modern citizen the way art is. This will also establish the study of the interaction between art and science. I will not elaborate here, having done so elsewhere (Agassi and Jarvie 2008, Ch. 5 (a)). Suffice it here to observe that the Enlightenment view of art as inferior, as a handmaid of science, is an immediate corollary to the dichotomy, as it dismisses diversity. In his defense of the right to diversity Feyerabend' supported magical worldviews (Feyerabend 1975, 14, 44, 65, 93, 205, 298, 304). He did not mean it, he admitted in his response (Agassi 1988, 405–416) to my criticism. His *The Conquest of Abundance* has a remarkable study of Brunelleschi's discovery/invention of perspective (Feyerabend 1999, Ch. 4). Brunelleschi solved a problem on the borderline: it belongs to the stories of art and of science, blurring the demarcation between them.

An impressive review (Clark 2002) suggests that Feyerabend had a vision of putting an end to "the long-lasting battle between philosophy and poetry" (Plato, *Republic* 607b), between science and art. That is impossible: they can never unite; but they may cooperate, and this is vision enough. This battle is at the center of the greatest philosophical cleavage today: Wittgenstein and his cohorts sustain one side and Heidegger and his cohorts sustain the other—with variants that make Wittgenstein change allegiance (Friedlander 2001).

The Greek view of proof as the criterion for rationality is anti-pluralist. It was a glorious idea, as it broke the hold of mythical-magical thinking and the xenophobia that it enhances. Popper broke new ground when he opposed the new irrationalism as the return to magic and tribalism and identified rationality with criticism (Popper 1945). Did Feyerabend agree? It is hard to say since he wished the multitude to read him one way and the *cognoscenti* another way. Steven Toulmin made a point similar to that of Feyerabend: he suggested refraining from applying the traditional criterion of rationality stringently as it is injurious. "The chief task of this book is to show what is needed if we are to treat that injury, and reestablish the proper balance between Theory and Practice, Logic and Rhetoric, Rationality and Reasonableness" he said (Toulmin 2001, 13). Recognizing "the collapse of Foundationalism" (Toulmin 2001, 137), of the theory of rationality as proof, he demanded (not to replace it but) to soften its application. Did Feyerabend agree? He equivocated; I think he did not.

The same goes for Rorty. What renders philosophies that advocate the diminution of reason attractive? Julien Benda has raised this question between the two world wars (Benda 1927). It invites discussion of taste and is thus psychological. And tastes diverge. They may however converge by institutionalized means that evoke similar emotions in large publics: the excessive demand to avoid error and take responsibility puts excessive pressure that makes it reasonable to purchase books that may offer a reduction of the pressure. This is better achieved by replacing the demand to avoid error with the demand to avoid irresponsible error. The laws of civilized countries recognize this; moralists still do not.

Feyerabend never tired of condemning the violence and tyranny that some people practice in the name of reason and their ascriptions of intellectual hegemony to some people—priests, scientists or teachers. The traditional opinions about rational methods, Feyerabend observed with some justice, boost their callousness. As Dr. Oberheim has noted in his review of Feyerabend's *Conquest of Abundance* (Oberheim 2001), Feyerabend "emphasizes that common sense and science tend to disguise the fact that worldviews are not forced on us but are formed by individual decisions, and accordingly he … challenges the status ascribed to contemporary science as sole provider of truth, rejecting science as an objective process of discovering nature's secrets in favor of a worldview according to which 'art is a product of nature as a work of art'." Since Popper was not engaged in such campaigns, the claim of Feyerabend for deviation from Popper is somewhat vindicated this way. The question arises, was Popper right to advocate science without warning against its dangers? I do not know; the choice of a cause to fight for is private since every fight of this kind is supererogatory. To the extent, however, that these matters are also open to public debate, Popper was right: irrationalism is politically more dangerous than rationalism. Feyerabend disagreed. He compared the advocates of the Vietnam War with the Nazis and blamed me for siding with these advocates. That was too much for me. But now we are here to praise Feyerabend, not to bury him. His last works were written in the spirit of conciliation, suggesting that no cultural differences are too big to overcome and no disciplinary differences are so big as to separate one discipline from another. Descriptively this is notoriously false: Popper cited the Nazi adage "shoot first and talk later" as evidence that efforts at conciliation are at times utterly useless (Martin Buber expressed the same view in his open letter to Mahatma Gandhi.). But then Feyerabend's latest text may be no more than the commonsense proposal not to give up hope too easily—sounding too optimist due to its exaggerated wording.

Feyerabend's work opened up debates to many new publics. The appraisal of the value of this effort invites preliminary research. Effort to render Feyerabend's proposals useful has to lead to rewording them more carefully and more honestly. Hopefully they will then be more conciliatory—but not all the way: there is a limit to toleration. Feyerabend denied this, and he supported this denial by odd arguments that point at the positive value of violence: "Violence is necessary to overcome the impediments erected by a well-organized society …, and it is beneficial for the individual, for it releases one's energies and makes one realize the powers at one's disposal" (Feyerabend 1975, 187). This repelled both Gellner (1975) and Popper. But, again, we come to praise Feyerabend, not to bury him.

All conciliations raise new conflicts as Georg Simmel has taught (Simmel 1922) and as Popper has stressed. The abolition of all conflict is an impossible, dangerous utopian dream. This is not to preach against utopian dreams: Ian Jarvie rightly criticized Popper on this point (Jarvie 1987, 227–243): he agrees that dreams may be dangerous, but, like conflicts, risks are unavoidable. In sum, those seriously intending to engage in Feyerabend's project of showing all cultures to be one culture and all disciplines to be one discipline should be ready to see that creating a unity carries in it the seeds of a new diversity: pluralism is here to stay.

Bas C. van Fraassen's review of Feyerabend's *Conquest of Abundance* (van Fraassen 2000) declares Feyerabend's study of Brunelleschi the peak of his output. He did that in an effort to put art and science on a par, hoping that science will thereby cease to be esoteric. (This, Daniel Cohen reminds me, is a starry-eyed view of the place of art in present society.) Feyerabend reminds us that the Renaissance view of mediaeval art as inferior is erroneous, just as the Enlightenment view of superseded science is: contempt for the outmoded is objectionable. Come to think of it, almost all contempt is. Of course, the more recent scientific theory is a better representation of facts; so is Renaissance art; except that representation is not everything. In particular, van Fraassen rightly notes, Feyerabend was fascinated by the dramatic aspect of the visual arts and, he adds, perhaps also of science. Scientific discovery can be dramatic and its representation can be artistic. The Age of Reason view of art as inferior to science is an error that still requires systematic correction.

Perhaps we should generalize this point to cover all relevant background information. After Hans Christian Ørsted discovered electromagnetism, he reported, he was dazed for three months. Historians of science ignore this story. The story that lectures at the Royal Institution in the nineteenth century were the talk of the town is familiar but analysis of it is hardly available. And, the reason why this kind of information is ignored, not to say suppressed, is that it is judged irrelevant, and by obsolete criteria. Moreover, even were these stories irrelevant to science, they have their obvious dramatic fascination that is ignored due to the idolization of science. Feyerabend said, all idolization is harmful. In his story of Brunelleschi's output he tried to combine the history of art (including the theatre) and of science (mainly geometrical optics) with the histories of their social settings.

To repeat, he meant to avoid harm, at least the harm of excess stereotyping. The avoidance of harm, let me stress, served as a criterion in his system. He certainly had a point, even if not a new one: already *Ecclesiastes* (1:18) noted that knowledge may cause pain. If Feyerabend made people see that even what they are sure is harmless is possibly harmful, if he made some of us more aware of our causing harm, then we must acknowledge this with appreciation.

Let me conclude with Feyerabend's gift to me, his posthumous contribution to one of the *Festschrifts* dedicated to me, "Universals as Tyrants and as Mediators" (republished in his 1999b). It deals with scientific revolutions—ancient and modern. He questioned there the search for regularity: it does not bring happiness. What does? He did not know. Science looks for the universal, and the universal is too uniform. How did it come about? Already Al-Farabi found this puzzling. Greek culture is special because it taught that no culture is special, he noted; he found this paradoxical. The search for the universal, said Feyerabend (note 7 there), is accidental: the motive for the search for generalities in mathematics, he said, is to satisfy the requirement of some ritual. Now it does not matter what the motive was. Popper's breathtaking classic "Back to the Pre-Socratics" (Popper 1963) describes the pre-Socratic revolution as the Geek toleration of disagreement. Why, however, did some Greeks want to dissent? Perhaps because they did not like the Master's thesis. Their motive was pre-rational and thus pre-revolutionary, or rational and thus post-revolutionary. Either way, this misses the revolution. Did rationality appear whole, as Popper and as Gellner say, or did it evolve? Was its motive rational or a-rational?

Did Feyerabend try to offer an alternative theory of the rise of Greek culture, science, rationalism? I do not know. He did wish to return to the pre-rational its dignity, just as we have returned the dignity to mediaeval paintings. Let us admit all this with no further ado. Does it deprive rationality of its dignity? Feyerabend's vulgar followers say yes, the rationalist ones say, no. And, of course, the rationalists win hands down simply because beautiful as the pre-rational is, surely the rational is no less beautiful and no less legitimate. Feyerabend loved it, perhaps too secretly but too well, and he complained only about its imperialism. We have conceded to him this point. The rest is history.

The question remains: how do changes of patterns of conduct occur? The rationality principle allows changes as due to changes of circumstances—material, individual and social. The changes can be imposed—according to people's aims and circumstances. This leaves inexplicable some changes—say, those that Feyerabend discussed, from pre-rationalism to rationalism or even from mediaeval art to Renaissance art. How are these explicable? Jarvie and I address this question (Agassi and Jarvie 1987, 382–383). We say, "The problem is not, then, 'how on earth can they believe in magic?'; it is rather 'can people with inefficient magical beliefs come to be critical of them, under what conditions and to what extent?' To us this seems the really urgent sociological problem posed by magic." Some profound changes may follow the change of circumstances. Brunelleschi's revival of ancient building methods (for building the cupola of the Florence cathedral of St. Mary of the Flowers) made people realize that they can revive antiquity, contrary to their older view that only the Second Coming will.

How did Brunelleschi arrive at this idea? His biographer (Antonio Manetti) provides strong evidence that this idea guided him from the start. (Brunelleschi and Donatello went to Rome and acted as the first archeologists.) Whatever the details of the right answer to this question are, it is clear that earlier circumstances led here to a new conclusion. More generally, say Jarvie and I, ideological changes are explicable by the discovery that given ends and worldviews yield to other ends and worldviews as superior—rightly or not but rationally. Feyerabend found that the error was of taking the modern dismissal of mediaeval art as the paradigm and that paradigm was one of facile dismissal. He took today's ideological and intellectual strife as in dire need of a healing process. He hoped that his battle against excessive stereotyping will help the production of such a process even though he knew we cannot avoid stereotyping and can only hope to reduce dogmas that cling to them. Yet he rightly demanded that we should not dismiss as *passé* even past dogmas. As Jarvie has put it (Jarvie 1975, 259), "It is only in relation to its past that a tradition of enquiry can regain a sense of progress."

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Chapter 15
Lakatos on the Methodology of Scientific Research Programs

15.1 The Need to Assess Research Projects and Programs

There is a significant tradition of assessing research projects and programs. Research grants go for those who propose convincing research projects. What makes them convincing? How do members of grants committees assess research projects? Such assessments should rest on some theoretical and historical studies. These are remarkably scarce. Budgets for research projects are ever increasing; this makes preparation and detailed planning as essential as the need for their assessments to accord with public guidelines. These should be examined, explained, and hopefully improved. Instead the public makes do with what Imre Lakatos was pleased to call *the Methodology of Scientific Research Programs*. Why does this need not invite more serious efforts?

15.2 Tradition Against Research Assessments

The first reason is the enormous popularity of inductivism or extreme empiricism. According to this theory, research projects are proper if and only if they are based on existing knowledge. In this case they are presumed in no need for further justification or analysis. Projects that rest on hunches or on theories that are not scientifically established this theory dismisses as they rest on prejudice. The opposition to prejudice that is central to this theory—inductivism—rests on the observation that prejudice blinds its holders to facts, especially to the observable facts that refute it.

Sir Karl Popper has offered labels to describe the attitudes to the competing theories of research that introduce very apt metaphors. The empiricist theory that presents proper research as the indiscriminate collection of all facts as they come he called the bucket theory of the mind. Its opposite theory he calls the search-light theory. Now the bucket theory excludes the problem of choice of facts or of experiments—they are all welcome and of equal value. It also excludes the

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problem of the choice of theory, as it leaves it to known facts to do that: when assessing a theory consider all facts available. The search-light theory offers a very different approach: theory guides the search for new, relevant facts. And what guides searches for theories?

The theory that is traditionally opposite to inductivism or extreme empiricism is *a priorism* or extreme intellectualism. It requires that all proper research projects should rest on *a priori* valid foundations. René Descartes and Immanuel Kant are the prototype intellectualists. According to intellectualism some principles impose themselves on our intuitions with finality. Let us take for granted that no proof of finality is possible in empirical science and assume some fallibilist combination of the bucket theory and search-light theory. Can this fallibilism guide the assessment of research?

Notice that the required fallibilist theory should present the process of research as two-way: theories help search for facts and *vice versa*. The best example is the *experimentum crucis*, the experiment that helps decide between at least two existing theories. But at best facts only "suggest" theories; they may stimulate the imagination, but then it is the imagination that produces theories, not observations. (The question, what stimulates the imagination, belongs to psychology, not to methodology.)

It should be obvious then that just as the search-light (that a theory is) helps us find facts as yet unknown, though some ideas about them are available, so there may be something, some super-search-light, that helps us find search-lights (theories) as yet unknown, though some ideas about them are available. Let me call this a power- house. The power-house will not light for us buckets full of facts; it will direct probing search-lights that will do so. Do we have a theory of the power-house, then?

15.3 The Pedigree Theory and the *Hic Rhodos* Theory

There is here a red herring to get out of the way. There are two competing views of the judgment of the worth of a theory, the pedigree theory (as Popper calls it) and the classical *hic Rhodos* theory. The pedigree theory says, a properly developed theory is good; the *hic Rhodos* theory says, we should judge a theory on its merit by examining it here and now (The label "*hic Rhodos*" comes with the Aesopian fable of the fellow who bragged that he once performed a great jump while in Rhodos. *Hic Rhodos, hic salte*, was the response: here is Rhodes and we want to see you execute the jump here and now).

Two pedigree theories are fused here; one is a criterion of quality, the other is an explanation of quality. By the snob theory, certificates of pedigree render further examination for goodness unnecessary. By the conservative theory pedigree is goodness. The snob's pedigree theory conflicts with the *hic Rhodos* theory; the conservative's view is consistent and may be refuted either by the low quality of some that possess good pedigree or by the high quality that goes with some poor pedigree.

Popper's classic "On the Sources of Knowledge and of Ignorance" (Popper 1963, 3–30) discusses validation: the classical pedigree theory says, theories properly constructed are all right, others are not. Popper's advice to researchers is, fall in love with a problem, and let your imagination go. Very important, of course; but not enough. What, for example, is Popper's attitude towards metaphysical frameworks? In his classic *Logik der Forschung* (Popper 1935) he says they are not meaningless, since they can become scientific; but as they are too abstract for tests, he advises ignoring them. Yet in that very same volume he transformed certain metaphysical ideas to rules of research, systematizing a suggestion thrown out by Russell (1900) and by Wittgenstein (1922). Rules of research, Popper said, are conventions of the scientific world, tested by their fruitfulness. The theory of research projects should take off from that. Further, we may propose alternative and competing sets of rules that rest on some power-house ideas.

15.4 Is Methodology Ineffable or Rational?

Enter Michael Polanyi, and on his coattail Thomas S. Kuhn. Polanyi begins by recognizing induction as the only plausible rational procedure of producing scientific theories while admitting that nevertheless it is impossible. He also recognized that research comprises performing some projects, yet this performance is not automatic: it requires discrimination (Polanyi 1958, e. g., Chap. 6.4, 6.6 and 12.2). There are, then, a power-house, search-lights, and discriminations. But there can be no theory of discrimination, no theory of the power-house.

There is an important subtlety here, regularly obscured. Neither Polanyi nor Kuhn rejects entirely the traditional or modern criteria of excellence of scientific theory, such as testability, confirmation, simplicity. These criteria, however, they found far from sufficient: when researchers run against accepted criteria they may override them (like the traffic policeman who may overrule traffic-lights). Polanyi insists that there is something ineffable operating here, tact or an intuition of researchers, or rather of leading researchers (who act as policemen); miraculously, researchers function in accord with peers. They agree on leaderships and on proper conduct. The accord between them is neither explicable, nor dispensable; it is the outcome of apprenticeship in the tradition of scientific research. Researchers may be critical of aspects of science but not of science as a whole.

Kuhn has added to Polanyi's theory the idea of the paradigm (Kuhn 1962, Introductory Essay). At times a paradigm is a scientific theory that serves (*pro tem*) as the model for researchers; sometimes it is a fragment, and sometimes a whole intellectual framework. It is the idea round which researchers rally, it is the core of their unity. Kuhn, like Polanyi, deemed crucial the ineffability of the scientific procedure. Normal science is the routine practice of normal researchers; it comprises the small tasks of puzzle-solving. The peculiar thing about these small problems is that somehow they integrate, in virtue of the existing paradigm. When their degree of integration is lower than expected it is time for a revolution, for a paradigm-shift.

All this is vague and unsatisfactory, but at least it exhibits recognition of fairly long-term research-projects.

15.5 The Rational Way to Assess Frameworks

Only a small minority of projects are judged good. By what criterion? Polanyi said (and Kuhn concurred), only experts can judge projects, only experts can judge experts. Moreover, judges of projects as well as judges of experts cannot fully articulate their standards. Furthermore, criticism is always partial, never total, so criticized experts usually stay in place and improve their practices. At times, said Polanyi, they fail, and then they should be simply left out of the game; this is the usual case. Kuhn seems to have strongly disagreed.

Both demanded that we leave things as they are. Now once a sociological study of all this proceeds well enough, Polanyi and Kuhn will have to endorse it by their own tokens, and so they may find themselves inconsistent in accepting and rejecting it simultaneously. Perhaps this is why Polanyi opposed efforts to articulate the rules thus-far not articulated. Yet Polanyi and Kuhn did have a sociological theory. It concerns observed uniformities in science. Uniformities, deep and superficial, over groups large and small, and regarding doctrines and rules of method, articulated to this or that degree, are all observed, they rightly noticed. In particular, we do observe research-practices that change. We can study these changes empirically.

Such researches have already started, and Price (1963), Snow (1969) Bell (1973/1999, 248, 404n4) and John Ziman provide examples of researches cluttered with irrelevancies. Such arguments can be further improved by replacing whenever possible poor arguments by realistic and critical ones. The articulation of paradigms, I conjecture (Agassi 1975, 209–210), is a metaphysical system, a powerhouse. The interaction of a metaphysical theory and a scientific one can go both ways: each may require a revision of the other and revisions may lead to tests. Also, a scientific theory and an observation report can go both ways—especially when facts confirm a hypothesis that conforms to a competing metaphysics. For example, electrostatic action-at-a-distance theory won many empirical confirmations; the field reinterpretation of these offers alternative observations, including di-electricity and electric and magnetic waves (Agassi 1971), and at times all this may demand modifications of empirical observations. A metaphysical theory may merge with the scientific hypotheses that conform to it. When all our hypotheses conform to one metaphysical system, and we claim in an additional—and possibly testable—hypothesis that the list of these hypotheses is complete, then and only then does a scientific theory entail a metaphysics proper.

15.6 Progressive and Degenerative Problem-Shifts

The claim that a scientific metaphysics is possible and desirable is historical. The version of it offered here utilizes Popper's refutability criterion of empirical character and runs contrary to the scientific tradition that sought verification rather than (mere) tests. Conclusive verification is never possible; tests are possible, but not for the framework—unless we take the whole set of testable theories and their frameworks and call them science. Particularly conducive to such a move is Mario Bunge's claim that testability is not enough; in his view, unless a theory fits our intellectual framework, all its testability and confirmation will not make it a part and parcel of science (Bunge 1967–1968). I am in great sympathy with this view since I do agree that testability is not enough, but here I wish to retain the distinction between the function of search-lights and of power-houses.

What causes a switch from one power-house to another? This question is not pressing: there are very few more-or-less adequate power houses; and we may use them all. Clearly, however, some power-houses have ceased to function. Why? Alexandre Koyré has noted that although both Descartes' and Newton's systems are outdated, the one is ignored and the other is still in regular use (Koyré 1968, III). Why?

Here the theory of Lakatos of problem-shifts saves the day. This theory preceded his discussion of power-houses. In his superb 1963–1964 *Proofs and Refutations* (Lakatos 1976) he showed that pursuing a solution to a given (mathematical) problem may raise a new problem, and that a new problem may be better or worse than the original—depending on objective criteria (such as generality) or on immediate interest. He calls these progressive and degenerative problem-shifts. In particular, a generalization of a given problem may be a progressive problem-shift. So is a shift to a problem with far reaching solutions. Lakatos argued with the aid of examples that may be interpreted in diverse ways.

What he brought to the discussion is an answer to the question, why do some intellectual frameworks offer no help or cease to offer help. His answer is, following an intellectual framework at times leads to progressive problem-shifts at times to degenerating ones.

As Feyerabend observed, all this is still too preliminary. It is most significant nonetheless. Problem shifts occur at both the large scale and the small. And one who wishes to realize a progressive problem-shift may well be frustrated by all sorts of committees inspecting research-projects and run by old-fashioned inspectors. Such things happen and slow down progress. The task facing methodologists, of overcoming such delays, is still open.

As Lakatos took the lead, we should examine his contribution. Some of it is decidedly less than useless as William Bartley has observed (Cohen et al. 1976): he called metaphysical frameworks scientific theories just in order to gain popularity and legitimize his shoplifting of ideas from colleagues. This is confusion. Rather than observe that metaphysical frameworks are too uninformative to be refutable, he demanded that they be protected against refutations. For example, atomism is irrefutable as it does not specify the sizes of atoms. A version of it that declares them to be of varying sizes, however, was refuted long before Dalton postulated that they all have some fixed size (Thus, Avogadro's or Loschmidt's constant is the number of atomic or molecular particles of a gas in one mol). Dalton's atomism was more informative than that of Democritus, and it survived the detailed atomic theory that he also proposed until the discovery of radioactivity. We see here that a theory may appear in variants with an increasing amount of information and resultant degrees of refutability. Which variant we call scientific matters not and there are diverse ways for making them refutable and of modifying them after tests. This surpasses the teaching of Lakatos. He died young and his followers tried in vain to develop his ideas. This is regrettable, because he did have something new and interesting to say.

What caught his attention was the observation of Popper that, ironically, observation can support but not undermine metaphysics, whereas it can undermine but not support science. Popper's point was that support free of risk permits clinging to error, so that metaphysics qualifies as pseudo-scientific. Lakatos noted that historians of science often present refuted scientific ideas while concealing their refutations; to do that they strip them of their details so that they become metaphysical. The confusion of science with metaphysics thus becomes endemic (Agassi, 1975, Chap. 11). Lakatos suggested that rather than expect the tradition of the history of science to align with critical rationalism, it is more reasonable for the latter to align with the former and allow for calling "scientific" those intellectual frameworks that have played a significant role in the history of science. He deemed this compromise reasonable precisely for the reason for which Bartley dismissed his contribution: it is largely verbal. It became in his hand a reasonable compromise between Popper and Polanyi. He could thus become overnight the equivalent of Kuhn in the Popper camp.

There is hardly a reason to fight him on this. Except that he botched things up. Rather than take the credit for ironing out differences between critical rationalists and established historians of science such as Kuhn and L. Pearce Williams, he claimed he had a new rational reconstruction of the history of science. Rather than offering peace between Carnap and his arch-opponent Popper, he ridiculed Popper's claim that Newton's theory was highly refutable. These were tactical errors that he could have corrected with ease had he lived longer. Let me do it for him and thereby have his spirit come to rest.

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Chapter 16
Epilogue: Civilization and Its Self-defense

Noble conduct may appear even under the most devastating conditions. The devastation may be physical, like the Shoah, when one of it victims, Primo Levi, resisted the urge to steal a slice of bread. And it may be moral, like the depravity of a Nazi like Martin Heidegger, who viewed the Shoah as just another unfortunate result of the war, when he displayed nobility as he refused to save his academic career by expressing regret about the Shoah when he felt none (Wolin 1993, Chap. 9). Of course, the Shoah was a barbarian deadly assault on civilization that called—still calls—for special measures of self-defense, physical, political and intellectual. Self-defense is violence permissible only when it is almost too late. Judging the moment for it precisely must be *ad hoc*. It is hard to see how rationalist philosophers who seek a principle to justify rational conduct do not admit that this renders their search of principle hopeless: both nobility and the need for self-defense may easily violate all normal expectation. The Socratic idea is more realistic: judging what is wrong is easier than judging what is right, since we normally live in the gray area, where at times we appear quite unexpectedly at our best, but only at times. Should we take the best as standard? Is it too high? Is it too rare? Should we always try our very best? If not, when is the time to respond aggressively in public to open moral violence? Is it right to absolve every conduct that is not clearly in the wrong?

These are tough questions. Popper implied that we should always seek the highest standards. His popular critics wanted to render his view more realistic. Kuhn said he disliked Popper's terminology as it is too violent. Feyerabend declared (in response to my criticism) a mere temporary measure his own verbal violence (that was much greater). This could apply to his assault on science, not to his assault on Popper, or rather on Popper's advocacy of maximal criticism. Readers often ignored this. Lakatos mixed his verbal violence with his special kind of wit that softened it temporarily. Other leading philosophers dismissed Popper as a myth-maker. Not so his popular critics whose criticism is here under scrutiny. Common to them is their courtesy to him: their having noticed his high critical standard in their very dissent from it and in their proposals to put constraints on scientific criticism.

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Semi-official public opinion acknowledges (still reluctantly, to our shame) Popper's mark as his advocacy of bold conjectures and ruthless criticism. It follows Bacon-style tradition in valuing only factual and mathematical discoveries that it deems unproblematic and those theories that passed successfully (Popper-style) tests. There are exceptions to this. Thus, at times semi-official public opinion recognizes theories that were refuted upon their very first tests, such as the Bohr-Kramers-Slater statistical theory of matter-light interaction. The rise of science-policy that Big Science imposed boosted the sociology and politics of science and this initiated some change: Marxist students of these new fields can hardly avoid presenting the contributions of Marx as serious, despite their metaphysical and obsolete character. This forces them, of course, to admit the existence of valuable, significant ideas that we must recognize as errors.

Popper's critical rationalism comes to replace Bacon's inductivism and so it requires replacement of Bacon's criterion of novelty. Bacon viewed as novel those items (factual and theoretical alike) that are independent of what is already known. Popper viewed as distinctly novel items that contradict received opinion. Popper's popular critics take all this as read, which is agreeable, and pretend otherwise, which is not. Popper took the encouragement of criticism as the hallmark of liberalism—in (scientific) research and in (democratic) politics alike. He claimed that his chief innovation is his view of science and democracy as integral parts of our liberal tradition. This claim of his deserves endorsement. On this his popular critics are utterly silent, but at least this silence is legitimate, as they concentrated on the philosophy of science. Within the philosophy of science, a striking innovation of his is his total avoidance of psychologism, especially of the questionable (since circular) application to it of the latest scientific perception theory (Carnap 1928). This is Popper's rebuff of the search for empirical foundation, of course. His theory of science thus parallels Freud's reality principle: hitting one's head against a real wall one bumps into the real. Most people usually ignore the wall, they observed. Freud viewed this as pathological; Popper viewed it as stagnation. (Thus, Freud was more radical than Popper.) Refutations then are not always valuable for survival but they are always valuable as intellectual assets: their upsetting important ideas renders them important. (This is the positive power of negative thinking.) This allowed him to recognize that scientific evidence is obviously worded in abstract terms. (Reports of the coordinates and spectra of stars result from sophisticated calculation; likewise, statistical observation reports comprise distributions of fair samples that are abstract and not observable.) This is Popper's theory of the empirical as refutable.

Semi-official public opinion recognizes (still reluctantly, to our shame) Popper's claim that he has offered solutions to the problems of induction and of demarcation. Lakatos claimed (Schilpp 1974, 262) for his own views of these two problems the status of modifications of Popper's—in the hope that Popper would endorse them, he added. Popper made mincemeat of the arguments of Lakatos in his "Replies" (Schilpp 1974, Replies, Sect. 12). Kuhn solved these two problems too. He followed Polanyi here and like him he appealed to authority. Feyerabend agreed with Kuhn's description but condemned the authorities of science as imperialist. Popper's solution to the problem of demarcation is his claim that we value science for its practicing of the traditional critical attitude. His popular critics agreed that criticism is valuable, but they wanted it to come in small measures and together with alternatives to what it destroys.

Popper's great philosophical innovation is his rejection of the dichotomy between truth by nature and truth by convention that is central to western philosophy since antiquity. He viewed scientific theories as putative truths, as tentative substitutes for truth by nature, and he viewed repeatedly improved conventions as not necessarily arbitrary. Reports of repeatable appearances are also tentative substitutes for truth by nature. Commentators view Popper as variably a naturalist and a conventionalist. They rudely ignore his repeated disavowal of both positions. On this his popular critics side with him—again with no acknowledgment, alas. It is what gives them an edge over other critics of Popper.

Popper and Robert Merton met the same criticism. Popular critics of both reported that some researchers ignore the high standards that they prescribe. True. As Bernard Shaw noted, successful movements suffer joiners from the rabble and the mixed multitude. Kuhn and Lakatos demanded getting rid of them; Feyerabend backed them as equals. My proposal is to be reasonably tolerant towards them. Possibly, they will take over. This will invite self-defense. Hopefully, things will never come to that; but we should be ready to defend civilization. Popper said, such defense is legitimate but it implies admission of defeat, of having let the critical spirit decline too far (Popper 1945, Chap. 19, ii). So far we are strong enough not to fear that demoralization by critics may threaten the very existence of the open society and of its chief institutions. And so, while rebutting our critics we should follow Popper's legacy and express appreciation for their attention and improve upon their efforts by improving the statements of their criticisms and by strengthening it as best we can.

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Appendix 1
The Biological Base of Dogmatism

The greatest weakness of Popper's popular critics is the theory of constructive criticism that they advocate. And the greatest weakness of this theory is not the prevalence of instances contrary to it but the simpler and more forceful argument that the adherence to it leads to no incentive for change and the absence of such incentive spells stagnation, contrary to the intent of these critics. Still, let me speak in its favor as much as possible.

To that end let me use Karl Popper's "Thought and Experience, and Evolutionary Epistemology; Or, How the Lynxes Got Their Sharp Eyes", a speech delivered in the Academia Nazionale dei Lincei, Rome, Italy, in May 9–11, 1984 (typescript in the Karl Popper archive held at the Hoover Institution, Stanford University). Popper united there neo-Darwinism and his own critical rationalism by grounding his idea of learning as trial and error in biology, and doing so by generalizing a few items of his arsenal. Thus, trial need not be human or even intended: it can be a random event, and in evolutionary biology the significant kind—but not the only kind—of new event is mutation. Error then has to be a disappointed expectation, this time decidedly intended. Further, we should view knowledge as either embodied—in an animal's genetic material, for example—or in verbal expressions that are ideas reified—as sound waves, as written pages, etc.—as long as it leads to expectations (that may be disappointed). For all this to be more than a nice analogy, it should raise points that might throw some new light on natural selection or on scientific method. Let me discuss only one of these points, one that concerns constructive criticism.

Clearly, some animals are plastic enough to overcome disappointed expectations some not. Konrad Lorenz describes borderline cases, where the plasticity is very poor, or, to echo Popper, where the disposition towards dogmatism is nearly disastrous. Clearly, if an animal approaches a situation with alternative dispositions, then we may view the disappointment in one of them as some sort of constructive disappointment, and view the other cases, says Popper, as the disappointment that leads to bewilderment. And this bewilderment, to echo Lorenz, expresses itself as the disposition towards dogmatism. The bewilderment that accompanies refutations with no ready-made alternative is an echo of the idea that only constructive criticism counts.

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Appendix 2
Popper on Explanation

Popper suggested that explanation is inference, and a satisfactory explanation is testable, namely, refutable, namely, scientific. Yet scientific technology has many severely tested and corroborated models. An alternative view is, testable explanations are empirical. In the opinion of Mario Bunge, to be satisfactory an empirical explanation must accord with the body of science. In my opinion, to be satisfactory an empirical explanation should accord with is some reasonable metaphysics.

What is to explain may be a singular statement of some factual report, and it may be a generalization of observation reports or any higher-level theory that is corroborated. An uncorroborated high level theory seldom attracts sufficient attention to seek an explanation. This is so because in science the aim is to explain observed facts; these may be singular observation reports, generalizations, or corroborations, and for corroborations we take the theories they corroborate that are treated as factual reports. The observation reports that are not corroborations are usually refutations.

An explanation, Popper observed, may explain not quite what was to be explained but something sufficiently similar to it to explain the fact that it was reported. This invites a crucial experiment. Thus, not only refuting instances interest researchers prior to the discovery of any alternative; in the crucial experiment the corroborated theory undergoes test on the same footing as its new alternative or else its corroborations should count as refutations of the alternative. Hence even a theory that is corroborated and not refuted loses its status as corroborated when an alternative to it appears. So remote is the theory of positive criticism from scientific practice.

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