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Paul Feyerabend and Thomas Kuhn

In this chapter, I want to discuss several aspects of the relationship between Paul Feyerabend and Thomas Kuhn.[1] There are seven sections. The next section will contain biographical information about the two, especially when, where, and how these two thinkers came together. Then, in the third section, I'm going to discuss aspects of Kuhn's and Feyerabend's theories that have become trademarks. By discussing these issues one can see the greatest parallels between their theories. In the fourth section, I'm going to show the large differences that existed in the 1960s between these two thinkers, which can be seen in Feyerabend's general criticisms of Kuhn's *Structure of Scientific Revolutions*. It is important to show these differences because Feyerabend and Kuhn are often seen as sitting in the same boat, as if their opinions in the philosophy of science were identical. This, however, is not the case, because in the 1960s they could not put aside their massive ideological differences. In the fifth section, I'm going to analyze Feyerabend's direct criticism of Kuhn—specifically of Kuhn's account of normal science. In the sixth section, I'll critically discuss Feyerabend's argument against Kuhn's account. Finally, I'm going to briefly show how, in the late 1980s, Feyerabend changed his opinion about his philosophical relationship to Kuhn.

In order to expose these differences, in addition to published works by Feyerabend and Kuhn, I'm going to use two letters that Feyerabend wrote to Kuhn sometime around 1960 or 1961, in which he evaluated and criticized a draft of Kuhn's *Structure*. These two letters are quite extensive and were recently published.[2] They comprise thirty-one single-spaced pages and were found in Feyerabend's *Nachlass*. He had forgotten their existence.

1. Biographical Remarks

Paul Feyerabend, born 1924, went as a guest professor to the Philosophy Department of the University of California at Berkeley in 1958. The year after, he re-

ceived a permanent position. Prior to that he worked at Bristol. Thomas Kuhn, born 1922, went as an assistant professor to the University of California at Berkeley in 1956. Later he became a full professor, primarily in the Department of the History of Science. Prior to that he was at Harvard. According to Feyerabend himself, he first read Kuhn's work in 1959. By the latest they were well acquainted by 1960, and their most intensive interaction was in 1960 and 1961. Their positions at the same university ended in 1964, when Kuhn went to Princeton. Since then, they only met a few times. Their last meeting was in June 1985 in Zurich when Kuhn, who was in Paris, was invited by Feyerabend to spend three days in Zurich. These were days of intensive academic and personal discussions. Kuhn gave a talk at the ETH Zurich to a full auditorium. The following day, there was a pleasant cruise on the Zurich lake with a dozen friends and acquaintances. Feyerabend always called this trip 'the intellectual cruise'.

Feyerabend became known noticeably earlier than Kuhn. He made a name for himself in the late 1950s, predominantly as a knowledgeable philosopher of physics who had some provocative opinions that were clearly formulated and full of sharp arguments. He traveled often, gave talks, and had many discussions. Kuhn, at this time, was only known by the inner circle of historians of science. This circle was small because in the 1950s the historiography of science had just begun to establish itself professionally in North America. In the early 1960s, Feyerabend was noticeably instrumental in helping Kuhn become more well-known among philosophers. In several talks from 1960 to 1961, Feyerabend mentioned that a book from Kuhn was forthcoming: a book that would enforce and support Feyerabend's own theoretically developed opinions by way of concrete examples drawn from the history of science. Even so, some time went by before Kuhn became generally well-known, even after the publication of his *Structure of Scientific Revolutions* in 1962. In the well-known *Encyclopedia of Philosophy*, published in eight volumes in 1967, and which has since undoubtedly been a standard reference work in the English-speaking world, there is not one hint of Thomas Kuhn. Furthermore, there is not even a single article about the history of science, although there are over 4,200 large, two-column pages. Paul Feyerabend, by contrast, was mentioned and discussed in an article about the philosophical consequences of quantum mechanics and he was the author of four articles about the physicists Boltzmann, Heisenberg, Planck, and Schrödinger.

What brought Feyerabend and Kuhn together, so that they were so often mentioned in the same breath? In those days, both had reservations about the dominant philosophical tradition in the Anglo-Saxon world—namely, logical empiricism. Both had a solid scientific background. Feyerabend had a masters in astronomy. Kuhn had a Ph.D. in theoretical physics (his supervisor was John Van Vleck, who won the Nobel Prize in 1977). Feyerabend developed his critical attitude toward the neopositivist tradition not primarily from the history of science, but from the discussion of the empirical basis of science, the so-called 'protocol sentence' debate, and from his intensive analysis of Popper. Kuhn's skepticism toward the philosophical tradition had its roots in his analysis of the history of science, which he began in 1947. It appeared to him that the *actual* history of science did not fit well with the *normative* image of science that philosophers had

developed. Most important to their philosophical similarity, in the eyes of their peers at least, was the fact that they both simultaneously introduced a new concept into the philosophy of science in their extraordinarily influential works from 1962: Feyerabend in his essay 'Explanation, Reduction and Empiricism' and Kuhn in *The Structure of Scientific Revolutions*. This concept later became the center of philosophical controversy that began immediately after the publication of these works and which continues even today, without loss of strength and without prospect for consensus in the near future. The conflict about this concept is now an established part of the philosophy of science, for example, in the controversy about rationality and realism, that Kuhn and Feyerabend also sparked or substantially intensified. This controversial concept is called incommensurability.

2. Incommensurability

The concept of incommensurability turns out to be extraordinarily difficult and controversial. Feyerabend once expressed it like this: "Apparently everyone who enters the morass of this problem comes up with mud on his head". I will not discuss, in what follows, the concept of incommensurability in all its details, but only give a somewhat schematic overview—a more precise analysis would need its own essay. First, I want to show what is meant by the concept of incommensurability and why it was so provocative for the preceding tradition in the philosophy of science. Then, I will talk about the main difference between Feyerabend's concept of incommensurability and Kuhn's.

The central point of incommensurability is that theories that replace one another, separated by a scientific revolution, do not work with exactly the same concepts. On the one hand, a new theory invents new concepts and throws away certain concepts of the old theory. For example, in Newton's theory of gravitation, the concept of gravitational force is introduced and the concept of the center of the universe that was essential for the older theory was rejected as unnecessary and useless. But the more interesting cases of incommensurability are when concepts of the old theory are used in the new theory as well, but with a somewhat different meaning. For example, the concept of mass is used in Newton's mechanics and in the theory of relativity as well, but it does not mean exactly the same thing in both theories. These changes of concepts, often called 'conceptual shifts', lead to a special relationship between the prerevolutionary and postrevolutionary theory. This relationship between two theories is what the incommensurability concept is about. As I said, Feyerabend and Kuhn differ in several respects, to which I will turn in a minute, as to exactly how the concept of incommensurability is characterized. But first, I would like to mention four central characteristics of the relationship between incommensurable theories that are valid for both Feyerabend and Kuhn's conceptions.

1. Incommensurable theories are incompatible, but their incompatibility cannot be transformed into a logical contradiction. This may not knock you out of your chair, but for the received neopositivist philosophical program it was extraor-

dinarily provocative. This program contended that philosophical insight can only be accomplished by logical analysis. But according to Feyerabend and Kuhn, incommensurability cannot be completely characterized with the tools of logic—at least not with the tools of existing logic. This led them to criticize logical analysis as the exclusive tool of the philosophy of science, and to promote hermeneutic or anthropological methods for the philosophy of science as well.

2. Incommensurable theories make different claims about what exists in the world, or more sharply, what the world is. Correspondingly, one can also say, as Feyerabend and Kuhn did, that the world changes with a scientific revolution. What this means exactly is another question and to answer this question one would need at least a separate essay.

3. Incommensurable theories are not literally translatable into each other. That means that with the concepts of the one theory, one cannot formulate the other theory completely. Correspondingly, one has to learn a new language to understand the new theory—at least a group of established concepts are strangely and weirdly altered. In order to have a good command of both theories, one must, in a certain sense, become bilingual. Even if one has accomplished this, one is not able to translate one theory into the language of the other theory, because to be bilingual is not identical with having the ability to translate.

4. Comparing two incommensurable theories with respect to their capabilities is substantially more complicated than comparing commensurable theories. With commensurable theories that employ substantially the same apparatus of concepts, one compares the predictions of the two theories with respect to their empirical precision. For the single predictions of the two theories this is not problematic, because for each prediction of the one theory there is a corresponding prediction of the other. Difficulties can only arise if one theory is better than the other theory in a certain area, while the other theory is better in another area. This case does not arise if one theory is a special case of the other, and this is how theory succession in modern science was understood, at least in physics. With incommensurable theories, one cannot compare them by superimposing their separate single predictions, because of the difference in their concepts and the untranslatability of both theories or, to put it differently, because they conceptualize differently more or less the same domain. That is why certain claims of one theory cannot find counterparts in the other theory, and what is from the perspective of the one theory evaluated as a substantial achievement is from the perspective of the other theory simply not understandable.

Let me illustrate. In chemistry in the first half of the eighteenth century, combustion was understood as a procedure in which a certain substance—'phlogiston'—was set free. That is not so implausible if one observes a candle flame, or if one notices that the ash of wood is much lighter than the wood that was burned. The question arose, how heavy is phlogiston, and different methods of measuring were suggested. After the so-called chemical revolution, combustion was understood as oxidation, as a process of binding with oxygen, and phlogiston was declared not to exist. Obviously, the question about the weight of phlogiston does not make sense any longer and quantitative results of the older chemistry about it

find no direct counterpart in the new chemistry. From the perspective of the new chemistry, all data about the weight of phlogiston are simply irrelevant, no matter how precise and well reproducible they were. Similarly, many claims in the new chemistry have no counterpart in the old. Although such claims must play a role in the assessment of a theory, they cannot compete directly with the corresponding claims of the other theory, and that is how the comparison of the theories becomes substantially more complex.

A certain part of the controversial discussion about the conception of incommensurability can be traced back to the fact that Feyerabend and Kuhn were often understood as wanting to deny the possibility of an achievement-oriented comparison of theories altogether. But with that, the development of science would be arbitrary, as it would lack any rational procedure of theory-choice. This interpretation does not fit with the intentions of Feyerabend and Kuhn (even if Feyerabend, in particular, often sounded this way). They primarily wanted to argue against the received and, in their opinion, oversimplified understanding of the relationship of replacing theories, where the choice of theories exclusively by comparison of their capabilities was made by the comparative evaluation of single predictions.

Now to the main differences between Feyerabend's conception of incommensurability and Kuhn's. Kuhn's conception of incommensurability has a larger scope than Feyerabend's. The reason is that for Feyerabend only *comprehensive* theories can be incommensurable and then only if they are interpreted in a *certain way*. Examples of pairs of incommensurable theories are, for Feyerabend: quantum mechanics and classical mechanics, or relativist and classical physics—all in a certain interpretation. Such comprehensive theories are part of the constitution of their objects. If one uses a theory that is incompatible with another comprehensive theory, one gets different objects and the claims of both theories can no longer be directly compared with each other. As a consequence of the restriction of Feyerabend's incommensurability thesis to comprehensive theories, there are some theory-pairs that are incommensurable for Kuhn but not for Feyerabend. For example, the Ptolemaic and Copernican theories of planets, which is for Kuhn one of the standard examples of incommensurability, is for Feyerabend, on the other hand, explicitly rejected as a candidate for incommensurability.

Now I turn to the controversy between Feyerabend and Kuhn about Kuhn's *Structure of Scientific Revolutions*.

3. Feyerabend's General Criticism of Kuhn's *Structure of Scientific Revolutions*

Feyerabend respected Kuhn a great deal. Modest as he actually was, contrary to the impression he could also give, he always thought that Kuhn was more important than himself. He began his famous 1970 essay on Kuhn entitled 'Consolations for the Specialist', contained in the Lakatos and Musgrave volume, with the following words:

> In the years 1960 and 1961 when Kuhn was a member of the philosophy department at the University of California in Berkeley I had the good fortune of being able to discuss with him various aspects of science. I have profited enormously from these discussions and I have looked at science in a new way ever since. (Feyerabend 1970, p. 197)

Despite this, Feyerabend had deep reservations about Kuhn's *Structure of Scientific Revolutions*. Although he acknowledged the problems Kuhn struggled with (especially the problem of the omnipresence of anomalies in science), he often disagreed with Kuhn's theoretical treatment of these problems. I cannot deal with the many detailed critical remarks that Feyerabend formulated in his two letters to Kuhn. Instead, I will concentrate on the global point of criticism with which Feyerabend began the first letter to Kuhn and which is the central point of his essay from 1970. Feyerabend suspects that underlying Kuhn's thinking is an ideology that "could only give comfort to the most narrow-minded and most conceited kind of specialism". For Feyerabend, this ideology "would tend to inhibit the advancement of knowledge" and "is bound to increase the anti-humanitarian tendencies . . . of . . . science". Such an ideology is, for Feyerabend, naturally a flagrant target open for attack. Feyerabend, on the one hand, sees himself as a strict opponent of all dogmatic tendencies—in science, in philosophy, and later in politics as well. On the other hand, he sees himself as a champion of humanitarian values such as liberty and the right to develop one's individuality. Thus, he is an opponent of all forms of oppression of minorities and other cultures. With respect to science, for Feyerabend it is "the most important question of all . . . to what extent the happiness of individual human beings, and to what extent their freedom, has been increased". This question is a result of his conviction "that the happiness and the full development of an individual human being is now as ever the highest possible value". How does it come about that Feyerabend suspects Kuhn of a hidden agenda, an ideology with antihumanitarian tendencies?

Feyerabend's accusation has, on closer inspection, three components that I will summarize by way of introduction before we consider them in detail. First, Kuhn formulates, as is well-known, a certain picture of how science in the basic disciplines typically historically develops. Kuhn's book about the structure of scientific revolutions formulates a general model of the stages of the development of science that should be valid for basic research. There are, in other words, certain regularities in the succession of different phases of science. Feyerabend first doubts the historical adequacy of some aspects of Kuhn's (in this sense) schematic description of the development of science. Thus, he contradicts Kuhn in a historical (or descriptive) respect. Second, beyond these descriptions, Kuhn discusses the functional role that different elements of science have for scientific progress. For example, Kuhn characterizes so-called 'normal science' as containing a certain dogmatic tendency and explains why this dogmatic tendency is not, paradoxically, an obstacle to the development of science, but rather conducive to it. Kuhn, thus, assesses certain aspects of science positively by explaining their function for further development. Secondly, Feyerabend doubts Kuhn's assessment of these elements of science. Thus, he contradicts Kuhn in methodological (or evaluative or norma-

tive) respects. Third, in Kuhn's presentation of his theory in the *Structure of Scientific Revolutions*, the descriptive and evaluative aspects are not cleanly separated, more often descriptive and evaluative elements alternate. The reason for this from Kuhn's perspective (at least in retrospect from 1969 and in reaction especially to Feyerabend) is that under certain assumptions, the descriptive claims about the development of science indirectly imply normative claims for the successful operation of science. Feyerabend, on the other hand, sees in Kuhn's mode of presentation an insidious way of taking the reader in with his own ideology without the possibility of critical distance; thus, he criticizes Kuhn with respect to his mode of presentation. To summarize, Feyerabend criticizes Kuhn in three respects: historical-descriptively, methodological-evaluatively, and with respect to his mode of presentation.

So now to the concrete details. At the heart of these criticisms is the so-called 'normal science' that Kuhn brought into the discussion. Normal science, following Kuhn, is a phase of the development of science that is marked by the broad consensus that the scientific community shares with respect to the basic questions within their discipline. This consensus is based on concrete scientific achievements which are so convincing and which have so much heuristic potential that further research in the corresponding discipline can model itself upon them and choose problems and solutions in analogy to them. Such exemplary concrete scientific problem solutions are called 'paradigms' by Kuhn. The concrete research activity in normal science has certain similarities with a totally different activity, puzzle-solving. The given characterization of normal science has as an assumption that there is no doubt about the exemplary character of the paradigmatic problem solutions—more strongly put, that the paradigms are effective dogmatically. They are effectively dogmatic in the sense that during normal science they cannot be called into question, thus they do not have to be justified or continually tested. Instead, in the practice of normal science one assumes that they are valid. But for Kuhn, normal science leads always to scientific revolutions, where the until then valid exemplary problem solutions are replaced by new ones. This happens in phases of extraordinary science where several different theories, the old theory, improved versions of it, and totally new ones, are tried out and compared.

First, Feyerabend doubts that the existence of normal science is a historical fact. Instead, he believes that there is no temporal differentiation of, as he calls it, periods of theory proliferation and monism, but that those are ways of doing science which coexist simultaneously. Feyerabend's reasoning for this assertion is not very convincing: he just gives one example that he thinks proves the coexistence of theory proliferation and the work inside one tradition. But this example, as so often in the history of science, could also be interpreted differently. It does not seem to me to be a specific weakness of argumentation by Feyerabend, but rather an expression of the difficulty of justifying or refuting basically statistical assertions about regularities in the history of science by historical material. I will not follow this criticism of Kuhn and its inherent problems here because, by comparison with Feyerabend's following second and third points of criticism, it is marginal.

Feyerabend's second criticism concerns the evaluation of normal science. For Feyerabend, normal science is, to put it simply, a horror, just as it is for the

other critical rationalists of the 1960s—especially Popper and Watkins. I group Feyerabend with the critical rationalists because in 1960–61, when he wrote the two letters to Thomas Kuhn, an essential part of his arsenal of arguments was indeed taken from the critical rationalist camp. The reason for his aversion to Kuhn's normal science is its dogmatic or quasi-dogmatic element, as Kuhn himself explicitly developed it. For Kuhn, this dogmatic element is not at all tied to a devaluation of normal science, but is functional for scientific progress. On the other hand, for a critical rationalist, a dogmatic element inside science is absolutely unacceptable. The reason is that for a critical rationalist science is, and must be, at heart critical. Science consists of producing highly falsifiable hypotheses and testing these hypotheses as rigorously as possible. One of the central tenets of critical rationalism is that if one decides not to test certain hypotheses, then one ceases to do science. If Kuhn evaluates the dogmatic element of normal science positively, he shows, in the eyes of the critical rationalist, a fundamental violation of the scientific ethos, namely to be critical and undogmatic. And for the enlightener Feyerabend, this violation is also antihumanitarian, because "[p]rogress has always been achieved by probing well-entrenched and well-founded forms of life with unpopular and unfounded values. This is how man gradually freed himself from fear and from the tyranny of unexamined systems" (ibid., pp. 209–210). That is why Feyerabend identifies Kuhn's 'normal' element of normal science as conservative and antihumanitarian. I will return to more details in the next section.

But what enraged Feyerabend even more is Kuhn's mode of presentation, as Feyerabend understood it, and this is his third main criticism. Feyerabend grants that Kuhn needs a point of view, or as he also puts it an "ideology", which provides the background for his account in the sense that it influences the interpretation of the historical facts. Without such an interpretation-generating point of view, any historical account "would be the most drab and uninteresting affair imaginable". In other words, Feyerabend allows a "methodological", that is, evaluative, point of view according to which Kuhn evaluates certain elements of science as rational and other elements as irrational. Feyerabend has especially in mind Kuhn's positive evaluation of normal science. However, he accuses Kuhn of not making this evaluative point of view explicit so that readers can take notice that there are also alternative points of view which can lead to other evaluations. Instead, with Kuhn it looks as if the evaluation of historical facts follows immediately from these facts themselves. Feyerabend summarizes this aspect of his criticism very concisely in his first letter to Kuhn: "What you are writing is not just history. It is *ideology covered up as history*." And, "It is this bewitching way of representation to which I object most, the fact that you take your readers in rather than trying to persuade them. This manner of presentation you share with Hegel and Wittgenstein." Feyerabend calls this aspect of Kuhn's account its "ambiguity": a vacillation between description and prescription. Feyerabend goes as far as to claim openly and in print in his 1970 essay that Kuhn's ambiguity is intended:

> I venture to guess that the ambiguity is *intended* and that Kuhn wants to fully exploit its propagandistic potentialities. He wants on the one side to give solid, objective, historical support to value judgements which he just as many other

> people seem to regard as arbitrary and subjective. On the other side he wants to leave himself a safe second line of retreat: those who dislike the implied derivation of values from facts can always be told that no such derivation is made and that the presentation is purely descriptive.

I do not know if Feyerabend felt good about this massive and in fact slanderous accusation. In any case, in his first letter to Kuhn, he asks at the end of a similar paragraph: "Or have I perhaps completely misunderstood you?" And in the printed essay from which the above quote was taken, he pleads for support for his own interpretation because he is fortified in his opinion "by the fact that almost every reader of Kuhn's *Structure of Scientific Revolutions* interprets him as I do, and that certain tendencies in modern sociology and modern psychology are the result of exactly this kind of interpretation." This passage is surprising as Feyerabend is not known as a philosopher who is very impressed by majority opinions, not to mention using them to legitimize his own position. A bit later in the same essay, Feyerabend asks if those readers misinterpreted Kuhn—and I do not think that for Feyerabend this was a purely rhetorical question.

4. Feyerabend's Criticism of Kuhn's Evaluation of Normal Science

In any case, at the end Feyerabend leaves open the question about the mode of presentation of Kuhn's book. But their deepest point of difference is the assessment of normal science to which I now, in my fifth section, turn again. How deep this difference is can be seen by the fact that Feyerabend is obviously unable even to describe normal science in neutral, that is, nonevaluative, terms. In passages where he intends to present Kuhn's view about normal science, he describes normal science as the "most boring and most pedestrian part of the scientific enterprise" that deals with "tiny puzzles" and that is based on "the monomaniac concern with only one single point of view". Briefly put, normal science is "professional stupidity". I remark, by the way, that these characterizations invite the same criticism of Feyerabend as Feyerabend made of Kuhn—the mixing of descriptions with evaluations. Such characterizations strongly inhibit Feyerabend's ability to handle the positive function for the development of science that Kuhn attributes to normal science. But apart from his emotional reaction, he tries to disprove Kuhn's argument for the functionality of normal science for scientific development with a sophisticated counterargument. He formulates this argument by stating "three difficulties" with which Kuhn's functional argument for normal science is confronted.

Feyerabend's argument against Kuhn's positive assessment of normal science has the following structure. First, he describes Kuhn's functional argument for normal science. Then, he shows that Kuhn's functional argument is based on two presuppositions that are both untenable. Finally, he denies the historical existence of normal science. Feyerabend's argument against Kuhn's positive assessment of normal science can thus be reconstructed in four steps.

Step 1: Feyerabend's description of Kuhn's functional argument for normal science: "Normal science, [Kuhn] says, is a *necessary presupposition of revolutions*". This is a functional characterization of normal science that aims at explaining why normal science is a good thing: because it has the function of leading to revolutions. The persuasiveness of this functional characterization of normal science rests on two presuppositions, namely that "revolutions are desirable" and that "the particular way in which normal science leads to revolutions is desirable also". Steps 2 and 3 analyze these two presuppositions.

Step 2: Analysis of the first presupposition of Kuhn's functional argument, namely that revolutions are desirable. The "first difficulty of [Kuhn's] functional argument" that Feyerabend identifies consists in his contention that the desirability of revolutions cannot be founded in Kuhn's theory. According to Feyerabend, it is impossible for Kuhn to assess the changes that a revolution leads to as improvements because of the incommensurability that Kuhn himself recognizes. If a scientific revolution brings only a change but not an improvement, then there is no argument for the desirability of revolutions.

Step 3: Analysis of the second presupposition of Kuhn's functional argument, namely that the particular way in which normal science leads to revolutions is desirable. The "second difficulty of [Kuhn's] functional argument" that Feyerabend identifies consists in his contention that there is a better route to revolutions than normal science. Feyerabend thinks that according to Kuhn, scientists would do normal science "until disgust, frustration and boredom makes it quite impossible for them to go on"; they would "suddenly [give] up when the problems get too big". Feyerabend confronts this route to revolutions with an alternative. According to this alternative, revolutions should be caused by following the principle of the proliferation of theories: creating competitors to a given theory. In light of the competing theories, the difficulties of the former theory will be emphasized and simultaneously the means for repairing or getting rid of the difficulties can be seen. For Feyerabend, this procedure also leads to revolutions but, as opposed to Kuhn, in a rational way. Thus, Kuhn's normal science route to revolutions is not desirable.

Step 4: The third difficulty of Kuhn's functional argument is that the historical existence of normal science is more than doubtful. And this is the end of Feyerabend's counterargument against Kuhn's positive evaluation of normal science.

5. Critical Discussion of Feyerabend's Argument

Let me now assess Feyerabend's argument with respect to how convincing it is, which brings me to my sixth section. I will discuss the argument's four steps in turn.

Step 1: First of all and most importantly, the presentation of Kuhn's functional argument for normal science is not at all appropriate. The positive assessment of normal science by Kuhn is not at all derived from the fact that normal

science leads to revolutions, as Feyerabend alleges. Rather, its leading to revolutions only makes it *acceptable* that normal science contains a certain quasi-dogmatic element. The main function of normal science is rather that it produces, in an extraordinarily efficient way, scientific knowledge, of which a certain part also survives the next revolution.

Step 2: Feyerabend's "first difficulty" is an immanent-critical argument based on a certain interpretation of Kuhn's concept of incommensurability. According to this interpretation, incommensurability implies that one can no longer talk of scientific progress through revolutions. But in truth, this implication is not part of Kuhn's theory: For Kuhn, incommensurability does not exclude scientific progress at all, but only precludes certain conceptions of scientific progress, to which the conception of the cumulative progress of knowledge as approaching truth especially belongs. The first difficulty that Feyerabend mentions is thus based on a misunderstanding of Kuhn's conception of incommensurability and is thus irrelevant. I point out, by the way, the deep irony that one of the inventors of incommensurability misunderstands the other inventor's conception with respect to a very substantial aspect!

Step 3: The "second difficulty" that Feyerabend mentions is that he sees a much better way to trigger scientific revolutions than Kuhn's normal science—namely the permanent proliferation of theories. At first, let there be no doubt that step 3, if it is correct, is not an argument that hits Kuhn directly because of the misdescription of Kuhn's functional argument. But even independently, Feyerabend's argument is problematic. The background of this argument is Feyerabend's conviction that sometimes a theory's substantial anomalies can only be discovered from the perspective of a competing theory. From the perspective of the original theory, these anomalies are invisible, according to Feyerabend. I call this thesis the *anomaly importation thesis*. This thesis has its own difficulties. They become visible when one asks in exactly what relationship the two theories in the anomaly importation thesis stand. Let us suppose that the two theories are *commensurable*. In this case, one theory can be articulated basically with the vocabulary of the other. Under these circumstances, it is difficult to see why a substantial anomaly for one theory is invisible from its own perspective, but visible from the perspective of the other theory. Let us therefore suppose things are the other way around, that the theories are *incommensurable*. Then one has to explain how it is possible that the defenders of the second theory who see a substantial anomaly for the first theory can ever convince the defenders of the first theory of this circumstance. Because of the incommensurability of the two theories and the resulting ontological disparities between them, the proponents of the first theory can always claim that difficulties of their own theory that are diagnosed from the perspective of the second theory are irrelevant. I do not claim that these difficulties with Feyerabend's argument are insurmountable, but in any case, they present a challenge for his position.

Step 4: The "third difficulty" of the functional argument concerns the question of whether normal science really exists historically. But that is not truly a *difficulty* of the functional argument (however it is formulated). Rather, the historical reality of normal science is a *presupposition* of the functional argument. It only

makes sense to ask the question of whether normal science is functional for scientific progress if normal science really exists. The functional argument would not be rendered problematic by the nonexistence of normal science, but it would become superfluous. The "third difficulty" according to Feyerabend, is thus no *difficulty* for Kuhn's functional argument, but at most a difficulty with this entire theory. But, as I mentioned earlier, Feyerabend's historical argument against the existence of normal science is not very convincing.

Let me briefly add a word about Kuhn's own defence of the existence of normal science against Feyerabend and the other Popperians. Kuhn attempted to show that the existence of normal science necessarily follows from the existence of scientific revolutions. This argument assumes that scientific revolutions exist, and this assumption Kuhn shares with his Popperian critics and can thus be used unproblematically in the given context. Revolutions presuppose normal science, according to Kuhn, in the sense that there must be stages *between* revolutions, thus nonrevolutionary stages of science—if talk about scientific revolutions or upheavals is to make sense.

However one assesses the strength of Feyerabend's arguments against normal science, Kuhn felt misunderstood by Feyerabend on the decisive points, and he found Feyerabend's arguments against normal science entirely insufficient. First, Feyerabend himself reports this candidly at the beginning of his 1970 essay. According to Feyerabend, Kuhn had often said this to him in their discussion in the 1960s: "On all these points my discussions with Kuhn remained inconclusive. More than once he interrupted a lengthy sermon of mine, pointing out that I had misunderstood him, or that our views are closer than I had made them appear" (ibid., p. 198). Second, in many places of Kuhn's published reaction to Feyerabend's essay from 1970 Kuhn's disappointment with the arguments against his theory is obvious. There Kuhn goes so far as to say that the reaction of his critics leads him to postulate the existence of two Thomas Kuhns. One Kuhn had published a book with the title *The Structure of Scientific Revolutions* with certain theses. The second Kuhn wrote a totally different book, but with the same title. And this book contains, according to Kuhn, many theses that are totally incompatible with those from the first book. It is from this second book that his critics, Feyerabend included, often cite.

6. Feyerabend's Reconciliation to Kuhn in the Late 1980s

In the 1960s, when Feyerabend wrote his essay, he did not allow his opinion that he understood Kuhn properly to be changed. Also, in 1977, he reinforced his conviction that he contested the idea of normal science from the beginning. But another ten years later, his tone changed. After he read a habilitation thesis about Kuhn, he wrote in 1988 in the revised English edition of *Against Method*: "[H]aving been made aware of the great complexity of Kuhn's thought I am not at all sure that our differences are as great as I often thought they were." In an essay that came out one year later, Feyerabend even went farther. He wrote there that the "ideas [in the present essay] are very similar to, and almost identical with,

Kuhn's . . . later philosophy". He reinforced this opinion in the third English edition of *Against Method* of 1993. In a posthumously published 1994 review Feyerabend discussed a reconstruction of Kuhn's theory as a "surprisingly coherent and powerful system of thought". Thus, it seems that Feyerabend reconciled with Kuhn in the last years of life. In this ironic way, the somewhat superficial but widespread opinion that these two extremely influential thinkers of the second half of the twentieth century were really close in their thoughts is justified, after all.

Notes

1. I wish to thank Eric Oberheim for translating this essay from the German.
2. 'Two Letters of Paul Feyerabend to Thomas S. Kuhn on a draft of *The Structure of Scientific Revolutions*', *Studies in History and Philosophy of Science*, vol. 26, 1995, pp. 353–387.

Reference

P. K. Feyerabend (1970) 'Consolations for the Specialist', In I. Lakatos & A. Musgrave (eds.), *Criticism and the Growth of Knowledge*, (Cambridge: Cambridge University Press), pp. 197–230.