Paul K. Feyerabend
Physics and Philosophy

Philosophical Papers Volume 4

This collection of the writings of Paul Feyerabend is focused on his philosophy of quantum physics, the hotbed of the key issues of his most debated ideas. Written between 1948 and 1970, these writings come from his first and most productive period. These early works are important for two main reasons. First, they document Feyerabend's deep concern with the philosophical implications of quantum physics and its interpretations. These ideas were paid less attention in the following two decades. Second, the writings provide the crucial background for Feyerabend's critiques of Karl Popper and Thomas Kuhn. Although rarely considered by scholars, Feyerabend's early work culminated in the first version of *Against Method*. These writings guided him on all the key issues of his most well-known and debated theses, such as the incommensurability thesis, the principles of proliferation and tenacity, and his particular version of relativism, and more specifically on quantum mechanics.

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Volume 4

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a close friend and appreciative colleague
of Paul Feyerabend

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Introduction

> What does not satisfy me in [the statistical quantum theory], from the standpoint of principle, is its attitude towards that which appears to me to be the programmatic aim of all physics: the complete description of any (individual) real situation (as it supposedly exists irrespective of any act of observation or substantiation).
>
> *Albert Einstein*

> The question is not whether by a subtle and highly scholastic argument we may continue to uphold an untenable position. The question is whether we should think critically and rationally in physics, or defensively and apologetically.
>
> *Karl R. Popper*

> Only be aware what you will face in Copenhagen and how careful you will have to be. You will face a metaphysics. And metaphysicians usually are very dogmatic; but they are even more dogmatic when they believe their metaphysics to be truly factual.
>
> *Paul K. Feyerabend*[1]

The present volume, the fourth collection of Paul Feyerabend's philosophical papers, is especially focused on physics, and more specifically on

[1] The sources of these three quotes are, respectively: Albert Einstein, "Reply to Criticisms", in Paul A. Schilpp (ed.), *Albert Einstein: Philosopher Scientist*, Evanston, IL: The Library of Living Philosophers, 1949, pp. 663–688: p. 667; Karl R. Popper, *Postscript to The Logic of Scientific Discovery*, edited by William W. Bartley, III, vol. III: *Quantum Theory and the Schism in Physics*, London: Hutchinson, 1982, p. 150; and Paul K. Feyerabend's second letter to Kuhn, in Paul Hoyningen-Huene, "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*, 26, 1995, pp. 353–387: p. 380.

quantum mechanics. All papers fall within 1948 and 1970, thus covering the first period of Feyerabend's production and his most elaborate products, beginning with his very first publication (prior to his having submitted his doctoral dissertation), an unsigned article in the *Veröffentlichungen des Österreichischen College*. They are significant for at least two reasons. First, they document his keen concern with the philosophical implications of quantum theory and its interpretations (a concern that never faded throughout his life, but that in the following two decades occupied a considerably lesser space in his published output). Second, they provide the crucial background to Feyerabend's critiques of Karl Popper (beginning with "Explanations, Reduction, and Empiricism," 1962) as well as of Thomas Kuhn ("Consolations for the Specialist," 1970). Indeed, some understanding of his less familiar works on physics here republished is necessary for understanding his more familiar concern with issues that soon became central to his later production. Although rarely considered by Feyerabend scholars, these early works of his represent the concerns that steered his work between the early 1950s and the late 1960s, work that culminated in the first version of his most famous book, *Against Method: Outline of an Anarchistic Theory of Knowledge* (1970), and that guided him to all the key issues of his most familiar and much-debated theses, such as his incommensurability thesis, his principles of proliferation and tenacity, and his peculiar version of relativism.

The present collection comprises all of Feyerabend's early works on the philosophy of physics with these exceptions: the works readily available in the previous collections of his papers, his doctoral dissertation (unpublished; it should appear as a separate volume, in the original or in translation), and two brief posthumous entries in *The Oxford Companion to Philosophy* (1995) that we could not receive permission to republish. Our making the bulk of his works on physics easily available – to scholars, philosophers, and historians of philosophy alike – should lead to a better general understanding of his heritage. To facilitate this, we offer here some historical, philosophical, and scientific background material that may help in putting his reflections into context, understanding his critical targets, and seeing their role in forging key elements of his later works.

The fundamental theories of nineteenth-century (classical) physics divided the agencies of the physical world into two distinct categories. There were material substances: the chemical elements, each comprising distinct, unchangeable atoms; the various compounds, molecules formed by the combination of atoms; and the macrobodies that comprise conglomerates of these molecules – as described by Isaac Newton, Leonhard

Euler, John Dalton, Augustin-Louis Cauchy, and their followers. To another category belonged electromagnetic fields that contain energy and radiate it as light waves, visible and invisible – as described by Michael Faraday, James Clerk Maxwell, John Henry Poynting, Heinrich Hertz, and their followers. The bridge between these two distinct categories was the ether that supposedly housed the energy of the fields. Faraday and his followers, including Einstein, dismissed the ether; also, Einstein declared that matter is concentrated energy. This inaugurated the era of relativist physics, namely of physics that abides by Einstein's principle of relativity rather than by Galileo's.

Another route that led to the collapse of classical physics came from a separate front that did not link up to the principle of relativity until 1925. In 1800 Joseph von Fraunhofer discovered spectral lines, namely, the fact that each element radiates and absorbs light differently. In 1859 Gustav Kirchhoff argued that, nevertheless, the ratio of the emission and the absorption that are characteristic to each element depends (not on the element but) only on the temperature of the radiating or absorbing atom. After a few failed attempts by various physicists to find what that dependence is, in October 1900 Max Planck advanced as a hypothesis a formula that fit experimental evidence. He then provided an explanation of that formula that included the assumption that the total energy in a field is made up of discrete quantities of energy, the size of each *quantum* being proportional to the frequency of the wave whose energy is concerned.[2] In 1905 Einstein offered the hypothesis that all radiation exists only in discrete energy packets, which he christened photons. (At the time Planck found this hypothesis much too wild.) According to classical electromagnetic theory, the fact that many metals emit electrons when light shines upon them (the photoelectric effect) can be attributed to the transfer of light energy to an electron in the metal; and the rate of this emission should depend on the amplitude or intensity of the light that falls on the piece of metal. Experimental results, by contrast, show that electrons are only dislodged if light reaches or exceeds a threshold frequency, below which no electron is emitted from metals. Einstein suggested that energy is exchanged only in discrete amounts, which perfectly fitted Planck's earlier discovery of the relation between energy and frequency.[3] The amplitude of the wave indicates the rate of the total amount

[2] Max Planck, "Über das Gesetz der Energieverteilung im Normalspektrum", *Annalen der Physik*, 309, 1901, pp. 553–563.
[3] Albert Einstein, "Über einen die Erzeugung und Verwandlung des Lichtes betreffenden heuristischen Gesichtspunkt", *Annalen der Physik*, 322, 1905, pp. 132–148.

of energy it conveys, but the frequency (or the wavelength) indicates the amount of energy that a single photon carries. Single photons, said Einstein, are responsible for single electron emission.

That was in 1905; in 1913 another bold step was taken. Building on the work of Joseph J. Thompson and his pupil Ernest Rutherford, the latter's pupil, Niels Bohr, published a revolutionary work on the structure of the hydrogen atom, according to which most of the atom is in its compact, central, positively charged nucleus with an individual negatively charged electron traveling in a circular orbit around it.[4] Arnold Sommerfeld improved Bohr's model, to render it compatible with the principle of relativity, thus also allowing for elliptical orbits as the effects of external magnetic fields[5] – the electrons are held in their orbits around the nucleus by electrical attraction, similar to the gravitational attraction that holds the planets in their orbits around the sun in our solar system (the so-called Rutherford-Bohr planetary atom, or old quantum theory). Unlike our solar system, however, Bohr's atom has discrete orbits: the energies of the electrons can occur only in certain fixed amounts, which correspond to certain fixed orbits, as the absorption of one photon should make the electron jump to a higher orbit, and the emission of one photon should cause a jump to a lower orbit. Contrary to classical electromagnetic theory, within the atom moving electrons do not radiate, except when they move from a higher to a lower orbit. This move was called quantum jump: an electron is not permitted to move between orbits but can disappear in one and simultaneously appear in another. Although the new quantum theory dispenses with these jumps,[6] they have survived to this day, having fired the imaginations of science fiction writers.

[4] Niels Bohr, "On the Constitution of Atoms and Molecules, Part I", *Philosophical Magazine*, 26, 1913, pp. 1–25. The paper was part of a trilogy, which also included "On the Constitution of Atoms and Molecules, Part II: Systems Containing Only a Single Nucleus", *Philosophical Magazine*, 26, 1913, pp. 476–502; and "On the Constitution of Atoms and Molecules, Part III: Systems Containing Several Nuclei", *Philosophical Magazine*, 26, 1913, pp. 857–875.

[5] Arnold Sommerfeld, "Zur Theorie der Balmerschen Serie", *Sitzungsberichte der mathematisch-physikalischen Klasse der K. B. Akademie der Wissenschaften zu München*, 1915, pp. 425–458; and "Die Feinstruktur der Wasserstoff-und der Wasserstoff-ähnlichen Linien", *Sitzungsberichte der mathematisch-physikalischen Klasse der K. B. Akademie der Wissenschaften zu München*, 1916, pp. 459–500. A revised version of these papers appeared as "Zur Quantentheorie der Spektrallinien", *Annalen der Physik*, 51, 1916, pp. 1–94 and 125–167, and later in his *Atombau und Spektrallinien*, Braunschweig: Vieweg, 1919.

[6] Erwin Schrödinger, "Are There Quantum Jumps?", *The British Journal for the Philosophy of Science*, 3, 1952, pp. 109–123.

The odd thing was not so much the jumps as the fact that two theories were here closely related but separate all the same: Planck and Einstein spoke of the quantized energy of fields, and Bohr spoke of matter absorbing and emitting these quanta. The unification of the two ideas began with Einstein's 1918 theory of absorption and emission (that was neglected until the advent of lasers). The most enigmatic aspect of the situation was the central formula of the theory, Planck's formula that correlates the energy of the photon with the wavelength associated with it: $E = h\nu$ (E is the energy of the photon, h is Planck's constant, and ν is the frequency of the wave). What is this association? In 1924 Louis de Broglie used relativistic considerations to suggest that not only fields of energy but also material particles, such as electrons, have wave-like features, quite like photons.[7] He combined Einstein's formula ($E = mc^2$), relating matter to energy, with Planck's ($E = h\nu$), thereby obtaining $mc^2 = h\nu$. Since, for every photon, $\nu = c/\lambda$, he derived $\lambda = h/mc$, and assuming the same equation to hold for a particle moving with speed v, he obtained $\lambda = h/p$. Testing this idea, Clinton Davisson and Lester Germer found that electron beams behave like wave-fronts and can show interference patterns. This seemed so bizarre that Einstein had to insist on taking their work seriously. With de Broglie, the wave-particle duality became universal in fundamental physics. The question is, what does this amount to? What does it mean? The simplest answer is that the particle is a concentration of field energy. The trouble with this answer is that the wave theory allows for no stability of particle-like energy-concentration, whereas the received information is that every kind of particle has its characteristic degree of stability. The next simplest answer goes the opposite way: the particles are real; what makes for their quantum characteristics are waves that guide them this way. This is the pilot-waves theory of de Broglie. The difficulty with this answer may be smaller, as the particles in this case need not dissipate, but the waves should dissipate, and they do not.

A breakthrough came in 1925–1926, with the appearance of both versions of the classical quantum theory: the matrix mechanics of Werner Heisenberg, Max Born, and Pascual Jordan,[8] as well as Schrödinger's

[7] Louis de Broglie, *Recherches sur la Théorie des Quanta*, PhD dissertation, Université de Paris, 1924; published in *Annales de Physique*, 3, 1925, pp. 22–128.

[8] Max Born and Pascual Jordan, "Zur Quantenmechanik", *Zeitschrift für Physik*, 34, 1925, pp. 858–888; Max Born, Werner Heisenberg and Pascual Jordan, "Zur Quantenmechanik II", *Zeitschrift für Physik*, 35, 1926, pp. 557–615.

equation for the electron.[9] Both versions offered precise descriptions of the energy transfer involved in quantum transitions (of emission or absorption of light). Most physicists were slow to cope with matrix mechanics, due to its abstract nature and unfamiliar mathematics; instead, they welcomed Erwin Schrödinger's alternative wave mechanics, since its equation was more familiar, as it did away with all quantum jumps and discontinuities, although the waves it describes are of a new kind, material waves that have no analogue in classical mechanics and that were introduced by de Broglie only a short time before.

The success of Schrödinger was in that he viewed Bohr's electron as a standing wave – of vibrations with fixed end-points, like those of a musical instrument – and drew on the fact that such waves are harmonic, namely that the higher frequencies of the vibrating string are multiples of the base frequency. The novelty in his equation is that he showed that in quantum mechanics the resonance that depends on frequencies is the law of conservation of energy, since Planck's equation equates the frequency of a photon with its energy. Schrödinger thus viewed Bohr's electron as a "matter-wave" except that each wave has a fixed quantum of energy: whereas in the vibrating string the energy varies with the strength of the vibrations (their amplitudes), in quantum mechanics it depends on frequency. Schrödinger's equation led to much easier calculations and more familiar visualizations of atomic events than did Heisenberg's matrix mechanics, where the energy was found in an abstruse calculation. Schrödinger published then a proof that matrix and wave mechanics gave equivalent results: mathematically, they were the same theory. Wolfgang Pauli, who calculated the matrix for the energy levels of the hydrogen atom using matrix mechanics, advocated the use of Schrödinger's equation as a shortcut for

[9] Erwin Schrödinger, "Quantisierung als Eigenwertproblem", *Annalen der Physik*, 384, 1926, pp. 361–376. In this paper he presented what is now known as the Schrödinger equation, and gave a derivation of the wave function for time-independent systems, also showing that it gave correct energy eigenvalues for a hydrogen-like atom (such as the one described by Bohr in his own model). He later published, under the same title, three follow-ups to this paper (*Annalen der Physik*, 384, 1926, pp. 489–527; 385, 1926, pp. 437–490; and 386, 1926, pp. 109–139), in the second of which he showed the equivalence of his approach to Heisenberg's. The last of these papers, in which Schrödinger introduced a complex solution to the wave equation, marked the moment when quantum mechanics switched from real to complex numbers, never to return. Nevertheless, the equivalence is only partial: whatever matrix mechanics explains wave mechanics explains too, but not vice versa, since the wave equation also applies to continuous systems.

calculations within matrix mechanics (as means of transformation known as the diagonalization of matrices).

The advocates of the matrix formalism of quantum mechanics nevertheless faced the question, what is the wave function doing in the theory? To this Max Born came with a new idea: the waves carry not energy but probabilities – quantum mechanics is essentially statistical.[10] Heisenberg then insisted that the discontinuous quantum transitions give his version an edge over Schrödinger's. The intense debates that followed showed that both interpretations of atomic events are unsatisfactory. Both sides started searching for a more satisfactory theory, and began with the interpretation of the quantum mechanics equations in line with their own preferences.

One obvious defect of the theory in all versions was that it operated within Newton's framework, rather than within Einstein's. Paul Dirac tried to remedy this: he created a variant of Schrödinger's equation that abides by the demands of the principle of relativity; Jordan applied this to both matter and fields, in unified equations known as "transformation theory": these formed the basis of what is now regarded as the orthodox quantum mechanics.[11] The task then became a search for the physical meaning of these equations, for the ability to show the nature of physical objects in terms of waves or in terms of particles, or both. This was called quantum electrodynamics; it was an ambitious effort to include classical and quantum effects, and to allow for both waves and particles.

Next came the most philosophically seminal part of the theory, the Heisenberg inequalities or the Heisenberg uncertainty principle: at any given moment, Δp times Δq is bigger or equal to h – that is, the product of the inexactness of the measurement of the momentum of a particle multiplied by the inexactness of the measurement of its position is proportional to Planck's constant, so that the smaller the imprecision of one of these two variables, the bigger the other.[12] Heisenberg offered a sort of

[10] Max Born, "Zur Quantenmechanik der Stoßvorgänge", *Zeitschrift für Physik*, 37, 1926, pp. 863–867; and 38, 1926, pp. 803–827.

[11] Paul A. M. Dirac, "The Physical Interpretation of the Quantum Dynamics", *Proceedings of the Royal Society of London A*, 113, 1926, pp. 621–641; Pascual Jordan, "Über eine neue Begründung der Quantenmechanik", *Zeitschrift für Physik*, 40, 1926, pp. 809–838, and "Über eine neue Begründung der Quantenmechanik, II", *Zeitschrift für Physik*, 44, 1927, pp. 1–25.

[12] Werner Heisenberg, "Über den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik", *Zeitschrift für Physik*, 43, 1927, pp. 172–198. The "observer effect", according to which measurements affect the measured items, is not under debate. Heisenberg went further and advanced the thesis that at the quantum level the effect cannot be

derivation of this principle and a popular explanation of it, both too inexact to count. The most direct way to derive it is, again, by taking the idea of a material particle as a wave of sorts, since the inexactness occurs in the general theory of waves.[13]

The uncertainty relations had far-reaching implications. First, Heisenberg advocated operationalism, the doctrine that in science every concept has a meaning only in terms of the experiments used to measure it, so that things that in principle cannot be measured have no scientific meaning. Thus, since the simultaneous values of a particle's position and momentum prescribe it a path, the uncertainty of the particle's path amounts to the concept of its path having no meaning. Operationalism is untenable, though: a basic assumption of modern physics, ever since Galileo and Newton, has been that the real world exists independently of us, regardless of whether or not we observe it. In Heisenberg's view, such concepts as orbits of electrons, or paths of particles, do not exist in nature, unless – and until – we observe them.

Heisenberg also drew profound implications for the concept of causality, or the determinacy of future events. Schrödinger had earlier attempted to offer an interpretation of his equation in which the electron waves represent the density of charge of the electron in the orbit around the nucleus. In his reading, every electron fills the whole universe, yet most of it is present in a reasonably small location. Born showed that the wave function of Schrödinger's equation represents not the density of charge or of matter but the probability of the location of the particle. In Born's reading, then, the results of quantum mechanics are not exact but statistical. Heisenberg took this one step further, challenging the notion of simple causality in nature: the future is not predetermined in the real world, not even the trajectory of an electron.

Schrödinger tried to refute this. Quantum theory asserts that a photon hitting a certain transparent filament has equal probability of passing and of not passing through it. Suppose that if and only if the photon passes through the transparent body, it triggers a gun that hits a cat. Suppose

reduced below a certain limit – in experiment or even in thought: the uncertainty principle is inherent in the properties of all quantum objects – of systems that are both wavelike and particlelike. In other words, Heisenberg postulated that the uncertainty principle states a fundamental property of quantum systems that can never be eliminated from science. Bohr viewed this as the destruction of the traditional barrier between us and the world, which he deemed a major philosophical consequence of quantum mechanics.

[13] Óscar Ciaurri and Juan L. Varona, "An Uncertainty Inequality for Fourier-Dunkl Series", *Journal of Computational and Applied Mathematics*, 233, 2010, pp. 1499–1504.

that all that happens in a closed opaque box. Then, according to Heisenberg, the cat is half alive and half dead until we open the box and look at the cat. This, said Schrödinger, is absurd.

In 1927, in the famous lecture he gave in Como, Niels Bohr stated his complementarity principle, which takes waves and particles as equally unavoidable in quantum-theoretical accounts: the wave and particle pictures complement each other. They are mutually exclusive, yet jointly both are essential for a complete description of quantum events; the uncertainty principle prevents them from coming together and conflicting with each other. By choosing either the wave or the particle picture, scientists influence the outcome of experiments, thereby causing a limitation in what we can know about nature "as it really is".[14] Complementarity, uncertainty, and the statistical interpretation of Schrödinger's wave function formed together the orthodox reading of quantum mechanics, known as the "Copenhagen interpretation".[15] In October 1927, at the Solvay conference in Brussels, its upholders went so far as to declare quantum mechanics complete, and the hypotheses upon which it rested as no longer in any need of modification. Dirac's austere and influential book, *The Principles of Quantum Mechanics*, first published in 1930 (fourth edition, 1958), provided the standard presentation of the theory for the decades to come. Philosophically its position was relatively clear; Dirac refused to ask the questions about the path of the electron that in principle theory and experiment do not answer. He also admitted that a central principle of the theory, the principle of superposition, does not yield to a simple, clear statement (p. 9; it is usually presented by examples).

Quantum theory discarded two central axioms of classical physics. First, it treated the basic material particles and energy as fields. Second, it rejected all "clockwork" pictures of nature: according to quantum mechanics, questions about future behavior of physical systems can be

[14] Niels Bohr, "The Quantum Postulate and the Recent Development of Atomic Theory", *Nature*, 121, 1928, pp. 580–590; reprinted in Niels Bohr, *Atomic Theory and the Description of Nature: Four Essays*, Cambridge: Cambridge University Press, 1934, pp. 52–91.

[15] Feyerabend said, "there is no such thing as the 'Copenhagen interpretation'" (see Chapter 6 of this volume). Indeed, Bohr and Heisenberg never agreed on all details of the reading of the mathematical formalism of quantum mechanics. The "Copenhagen interpretation" is a label that critics introduced, to denote Bohr's idea of complementarity plus Heisenberg's interpretation of the uncertainty relations, and Born's statistical interpretation of the wave function. At times, they added to this the correspondence principle that Bohr advanced in 1920: the behavior of systems described by (the old) quantum theory reproduces classical physics in the limit of large quantum numbers.

answered only statistically: Heisenberg's uncertainty principle replaces the rigid causality of classical physics with probabilities.

Physicists reacted to the new theories in different ways. To some, the abandonment of causality seemed a small price to pay for the great extension of understanding that quantum mechanics offers. Yet, as John von Neumann soon argued, there was an inescapable price to pay. In his 1932 *Mathematical Foundations of Quantum Mechanics* (originally published in German), he developed a mathematical framework for quantum mechanics, as that of special linear operators in Hilbert spaces. (Heisenberg's uncertainty principle, for instance, was translated in the noncommutability of two corresponding operators). Von Neuman's treatment allowed him to confront the foundational issue of determinism versus nondeterminism, and in the book he tried to dissuade researchers from seeking a causal system underlying the extant quantum-mechanical one without altering the system in some manner. (This search is known as the search for hidden variables; his argument was later refuted: see the following.) "It is therefore not, as is often assumed, a question of a re-interpretation of quantum mechanics", wrote von Neumann about his (alleged) proof, "the present system of quantum mechanics would have to be objectively false, in order that another description of the elementary processes than the statistical one be possible".[16] Physicists and philosophers of science readily and almost universally accepted von Neumann's claim.[17]

The question remained, and appeared in the following wordings: Is quantum mechanics complete? Does it apply to single particles or only to ensembles of single particles?[18] Einstein, dissatisfied with probabilities and, more fundamentally, taking for granted that nature exists independently of the experimenter, sought a theory that describes the behavior of particles as precisely determined. It is the task of research to uncover comprehensive yet nonstatistical laws of nature. He took quantum

[16] John von Neumann, *Mathematische Grundlagen der Quantenmechanik*, Berlin: Springer, 1932; English translation by Robert T. Beyer, *Mathematical Foundations of Quantum Mechanics*, Princeton, NJ: Princeton University Press, 1955, p. 325.

[17] See, for example, Max Born: "No concealed parameters can be introduced with the help of which the indeterministic description could be transformed into a deterministic one. Hence if a future theory should be deterministic, it cannot be a modification of the present one but must be essentially different" (*Natural Philosophy of Cause and Chance*, Oxford: Oxford University Press, 1949, p. 109).

[18] This refers to the fact that a single electron interferes with itself, that in an electron-beam that shows interference patterns the interaction between the different electrons with each other is negligible.

mechanics in Born's reading as satisfactory as far as it goes, but he denied that it goes all the way. In other words, he deemed quantum mechanics as incomplete. The debate was fierce. It was argued that the precise measurement of one particle in two different places and times allows conclusions about its past path. Heisenberg dismissed this argument, declaring only predictions, not retrodictions, to be of concern for science. One might object that the interest in dinosaurs proves this wrong, but Heisenberg's apologists might answer that this is in the macroworld where classical science applies, and thus is irrelevant to the quantum world.

In 1935 Popper's *magnum opus*, his *Logik der Forschung*, appeared and advocated the view of science as the set of empirically testable theories, to wit, refutable ones. In effort to present quantum mechanics as testable, Popper offered an attempt to refute it by planning an experiment that might go beyond the limit of precision allowed by Heisenberg's principle. Heisenberg, Einstein, and others found a mistake in Popper's plan, since in it an electron passes a barrier that thus reduces the precision of its position or momentum.[19] Very soon afterward, Einstein, Boris

[19] Karl R. Popper, *Logik der Forschung: Zur Erkenntnistheorie der modernen Naturwissenschaft*, Vienna: Springer, 1935, pp. 172–181; English translation, *The Logic of Scientific Discovery*, London: Hutchinson, 1959, pp. 236–246; see also Appendix *XI, especially pp. 444–445. Popper's thought experiment may be derived from the Einstein, Podolsky, and Rosen thought experiment by imagining a film with a hole in it that the electron goes through, assuming that the film's position and momentum remain unchanged. Popper's error – first noted by Carl Friedrich von Weizsäcker (see Popper and von Weizsäcker's exchange in "Zur Kritik der Ungenauigkeitsrelationen", *Die Naturwissenschaften* 22, 1934, pp. 807–808), by Heisenberg (in private letters), and by Einstein in a letter reprinted in Appendix *XII ("The Experiment of Einstein, Podolsky, and Rosen") of Popper's *The Logic of Scientific Discovery*, cit., pp. 457–464 – was in ignoring the fact that in transition the electron gets "smeared" unpredictably. As a consequence, Popper withdrew his thought experiment, later to propose improved versions of it throughout the 1980s, at times in collaboration with physicists. Beginning in 2000, Popper's thought experiment appeared prominently in several papers published in journals of theoretical physics, giving rise to a heated discussion that leaves it as one of the open questions in the contemporary philosophical debate on quantum physics. In fact, more than realism is at stake: alongside with Einstein, Podolsky, and Rosen (as well as Vigier, Bohm, and Bell), Popper strongly opposed to the claim to finality and completeness of the standard interpretation. In his opinion – just as in that of Feyerabend after him – such claim was anathema, as it clashes with the realism of the critical attitude as expounded in *Logik der Forschung*. His aim was not to provide "a crucial experiment of quantum mechanics but only of its (subjectivist) Copenhagen interpretation (which they call 'the standard interpretation')" ("Popper versus Copenhagen", *Nature*, 328, 1987, p. 675). Michael Redhead said ("Popper and the Quantum Theory", in Anthony O'Hear (ed.), *Karl Popper: Philosophy and Problems*, Cambridge: Cambridge University Press, 1995, pp. 163–176: p. 176), "Popper's carefully argued criticisms won the support of a number of admiring and influential physicists. He has done a great service to the

Podolsky, and Nathan Rosen proposed a similar experiment, in which the barrier is replaced with a particle and the effect of the collision between the initial particle and its obstacle is measurable. (Their argument is known as the "EPR paradox".)[20] They argued that the uncertainty principle forbids certain precise measurements of the two particles, because the theory allows conclusions from results of one measurement of characteristics on one particle on the other and vice versa, even though the measurements can be performed when the two are at a great distance from each other. In the imaginary experiment with two particles speeding away from each other, but with correlated properties, to be precise, an observer could choose to find the position of the first particle by merely observing the second, and the momentum of the second particle by merely observing the position of the first. This way, the observer will find the precise position and momentum of both without violating the theory, yet while violating the principle of uncertainty. Hence, the EPR paradox does not refute the theory but only shows, or is claimed to show, that the theory is incomplete.

Bohr answered Einstein, Podolsky, and Rosen. He reaffirmed – at least twice – the assertion that the uncertainty of the measurement of characteristics of quantum objects is due to "the impossibility of controlling the reaction of the object on the measuring instruments, if these are to serve their purpose";[21] in other words, Bohr declared the EPR thought experiment not performable. He argued not so much for this claim as against Einstein's realist views.

By 1939 most of the younger theoretical physicists were convinced that Einstein's objections sprang only from nostalgia for the apparent certainties of nineteenth-century physics. Since 1945, however, a few physicists

philosophy of quantum mechanics by emphasizing the distinction between state preparation and measurement and trying to get a clearer understanding of the true significance of the uncertainty principle, but above all by spearheading the resistance to the dogmatic tranquilizing philosophy of the Copenhagenists. Because some detailed arguments are flawed, this does not mean that his overall influence has not been abundantly beneficial".

[20] Albert Einstein, Boris Podolsky, and Nathan Rosen, "Can Quantum-Mechanical Description of Physical Reality be Considered Complete?", *Physical Review*, 47, 1935, pp. 777–780. Bohr's reply – Niels Bohr, "Can Quantum-Mechanical Description of Physical Reality be Considered Complete?", *Physical Review*, 48, 1935, pp. 696–702 – left Einstein, Podolsky, and Rosen unconvinced. See his "Reply to Criticisms", cit., especially pp. 666–674.

[21] Bohr, "Can Quantum-Mechanical Description of Physical Reality be Considered Complete?", cit., p. 697; Bohr's reply is repeated in his "Discussion with Einstein on Epistemological Problems in Atomic Physics", in Schilpp (ed.), *Albert Einstein: Philosopher-Scientist*, cit., pp. 199–241.

once again have begun to criticize orthodox Copenhagen view, arguing that the statistical nature of quantum mechanics implied that it is only really applicable to ensembles of particles. The question, then, is, can research go further and find causal theories that accord with quantum mechanics, exactly or approximately? This is the question of hidden variables. The best known hidden-variables theory was that of the American physicist and philosopher David Bohm,[22] who in 1952 offered a detailed version of de Broglie's pilot-waves theory of a quarter of a century earlier. This showed von Neumann's celebrated proof wanting. As Bohm presented it, he managed to avoid the presuppositions of the proof. Heisenberg was swift to respond: the theory is metaphysical and so irrelevant. This response is obviously a rescue operation: irrelevant though Bohm's response may be, it rendered Einstein's dream of a causal completion of quantum theory possible again. Bohm distinguished between the quantum particle and a hidden pilot-wave that governs its motion; whether these waves exist or not, they represent the possibility of hidden variables thus rendering the principles of quantum mechanics somewhat less durable.

Bohm claimed[23] that theoretical speculation about subquantum physics is called for, von Neumann's arguments notwithstanding; he marshaled physical, historical, and philosophical arguments for this claim. In his view, alternative theories about the subquantum world will give observably different results, particularly concerning very high energies or the internal structure of the atom's nucleus. In presenting his own outline of a possible subquantum theory, Bohm employed explanatory models of the sort that Heisenberg had rejected. He compared the wave/particles of the quantum world to clouds or tidal waves, thereby representing transient configurations with blurred edges, continually forming, dissolving, and traveling across an underlying substratum (or "field") of energy. Accordingly, the statistics of orthodox quantum theory can once again be treated as statistics of a familiar kind that do not preclude causality. Finally, Bohm suggested to reconsider certain assumptions on which all physical theories have rested ever since the seventeenth century: in his view, just as Einstein had rejected some of Newton's assumptions in the

[22] In his first book, *Quantum Theory* (New York: Prentice-Hall, 1951), Bohm defended the Copenhagen interpretation, but soon thereafter he rejected his own former view and became one of the leading defenders of the hidden-variables theory.

[23] David Bohm, *Causality and Chance in Modern Physics*, London: Routledge & Kegan Paul, 1957.

theory of relativity, so we are compelled to reject Cartesian assumptions about space and geometry, drawing new concepts from topology.

The EPR thought experiment appeared impossible to perform. In 1957 Bohm and Yakir Aharonov presented a variant of it that is performable.[24] Nevertheless, the situation was not very clear, and it was in 1964 that John S. Bell helped dispel much of the fog that Bohr, Heisenberg, and von Neumann had created.[25] He used a simple case of a theorem that Maurice Fréchet had published in 1935 (Fréchet's inequalities),[26] known as Bell's inequalities. He applied these to quantum cases on the supposition that Einstein, Podolsky, and Rosen had used for their thought experiment: the assumptions of locality (of the proximity of cause and effect) and realism (the physical system is independent of its observer). These assumptions, together, are called "local realism", or "local hidden variables". He showed that no physical theory of local hidden variables can explain all of the predictive success of quantum mechanics.[27] The assumptions of local realism then prove quantum mechanics incomplete. In a previous paper (published only later, in 1966), Bell addressed the impossibility proofs that hidden variables are impossible, as von Neumann's proof contains a conceptual error (it relied on an assumption that is inapplicable to quantum theory: the probability-weighted average of the sum of observable quantities equals the sum of the average values of each of the separate observable quantities).[28] Alongside Einstein, Schrödinger, de Broglie,

[24] David Bohm and Yakir Aharonov, "Discussion of Experimental Proof for the Paradox of Einstein, Rosen, and Podolsky", *Physical Review*, 108, 1957, pp. 1070–1076.

[25] "Bohm showed explicitly how parameters could indeed be introduced, into nonrelativistic wave mechanics, with the help of which the indeterministic description could be transformed into a deterministic one. More importantly, in my opinion, the subjectivity of the orthodox version, the necessary reference to the 'observer', could be eliminated": John S. Bell, "On the Impossible Pilot Wave", *Foundations of Physics*, 12, 1982, pp. 989–999; reprinted in John S. Bell, *Speakable and Unspeakable in Quantum Mechanics: Collected Papers on Quantum Philosophy*, Cambridge: Cambridge University Press, 1987, pp. 159–168: p. 160.

[26] Maurice Fréchet, "Généralisations du théorème des probabilités totales", *Fundamenta Mathematicae*, 25, 1935, pp. 379–387.

[27] John S. Bell, "On the Einstein-Podolsky-Rosen Paradox", *Physics*, 1, 1964, pp. 195–200; reprinted in John S. Bell, *Speakable and Unspeakable in Quantum Mechanics*, cit., pp. 14–21.

[28] John S. Bell, "On the Problem of Hidden Variables in Quantum Theory", *Reviews of Modern Physics*, 38, 1966, pp. 447–452; reprinted in John S. Bell, *Speakable and Unspeakable in Quantum Mechanics*, cit., pp. 1–13. The supposed flaw had already been discovered by Grete Hermann in 1935, but her refutation remained nearly unknown for decades, until Bell rediscovered it. The alleged theorem had a strong influence.

and Bohm, Bell rejected the received interpretation of quantum theory, and called attention to the fact that empirical evidence does not at all force us to renounce realism. The long and the short of it is that due to Bell's clarifications, the EPR thought experiment was performed and its result corroborates the incredible quantum prediction that both Einstein and Bohr had deemed impossible. This is known as quantum entanglement: no matter how distant the two entangled particles are, the choice of variable of the one to measure limits the possible choice of the other one to measure.

This is the background of the physical and philosophical debate to which the papers collected in this volume belong. Paul Feyerabend's interests in the physical sciences – particularly astronomy, mechanics, and quantum theory – were deep. As a teenager, he attended Vienna's high school (*Realgymnasium*), at which he learned Latin, English, and science. His physics teacher was Oswald Thomas, a famous astronomer known for his works on popular astronomy. He was widely read in Austria and in Germany, and triggered interest in physics, especially in astronomy. Helped by his father, Paul "built a telescope from a bicycle and an old clothing stand", and "became a regular observer for the Swiss Institute of Solar research".[29]

> I was interested in both the technical and the more general aspects of physics and astronomy, but I drew no distinction between them. For me, Eddington, Mach (his *Mechanics* and *Theory of Heat*), and Hugo Dingler (*Foundations of Geometry*) were scientists who moved freely from one end of their subject to the other.[30]

After the war he went back to Vienna with the intent to study physics, mathematics, and astronomy. Instead, he chose to read history and sociology, but he soon became dissatisfied with them and returned to theoretical physics. His teachers were Hans Thirring, Karl Przibram, and Felix Ehrenhaft. The last was a critic of all orthodoxy in physics; many physicists considered him a charlatan. Feyerabend much appreciated his fearless iconoclasm. He must have been successful as a student, since in 1948 and in 1949 he was offered grants to attend the international summer seminar of the Austrian College Society in Alpbach. In 1948 he met Karl Popper there, and impressed him sufficiently to receive his help

[29] Paul K. Feyerabend, *Killing Time*, Chicago–London: The University of Chicago Press, 1995, p. 29.
[30] Ibid., p. 30.

to attain a scholarship to go to England. Before reaching England, in 1949, he witnessed a much-expected clash between Ehrenhaft and representatives of "the orthodoxy". Years later, in *Science in a Free Society*, he reported:

> Ehrenhaft gave a brief account of his discoveries adding general observations on the state of physics. "Now gentlemen" he concluded triumphantly, turning to Rosenfeld and Pryce who sat in the front row – "what can you say?". And he answered immediately. "There is nothing at all you can say with all your fine theories. *Sitzen müssen sie bleiben! Still müssen sie sein!*".
>
> The discussion, as was to be expected, was quite turbulent and it was continued for days with Thirring and Popper taking Ehrenhaft's side against Rosenfeld and Pryce. Confronted with the experiments the latter occasionally acted almost as some of Galileo's opponents must have acted when confronted with the telescope. They pointed out that no conclusions could be drawn from complex phenomena and that a detailed analysis was needed.[31]

At the time, Feyerabend continues, such heated discussions had little effect on him:

> None of us was prepared to give up theory or to deny its excellence. We founded a Club for the Salvation of Theoretical Physics and started discussing simple experiments. It turned out that the relation between theory and experiment was much more complex than is shown in textbooks and even in research papers. ... We continued to prefer abstractions if the difficulties we had found had not been an expression of the nature but could be removed by some ingenious device, yet to be discovered. Only much later did Ehrenhaft's lesson sink in and our attitude at the time as well as the attitude of the entire profession provided me with an excellent illustration of the nature of scientific rationality.[32]

Ehrenhaft's lesson sunk in much later – after attending Popper's lectures and seminars at the London School of Economics, the best school for sharpening one's critical acumen – and then Feyerabend started publishing on the philosophy of quantum mechanics. He found the dominance achieved by the Copenhagen interpretation quite undeserved; he found it incredible that this interpretation was considered the last word on the matter – by scientists and philosophers of science alike. His early works are the products of his study with Popper, whose unorthodox views on the philosophical interpretation of quantum theory are the concern of chapter 7 of *Logik der Forschung* (1935) and more so in the *Postscript* to its English edition that Popper was working on then, as well as in a few

[31] Paul K. Feyerabend, *Science in a Free Society*, London: New Left Books, 1978, p. 111; see also *Killing Time*, cit., pp. 65–67.
[32] Paul K. Feyerabend, *Science in a Free Society*, cit., p. 111.

other works Popper published in the 1950s.[33] In his very first publications, Feyerabend focuses on quantum theory as one of the most interesting examples of the way in which philosophical speculation, empirical research, and mathematical ingenuity jointly contribute to the development of physical theory. He sides with Popper – as well as with Einstein, de Broglie, Bohm and Vigier – in challenging the orthodoxy of the Copenhagen interpretation and advocating a realistic interpretation of quantum mechanics. As its orthodox interpreters, especially the "logical" positivists among them, tried to strip it of its metaphysical features, they rendered it a mere prediction device, no longer requiring researchers to provide an account of the atomic world as it exists independently of observation and experiment. (Reichenbach went so far as to exclude from scientific theory events that take place between observations, which he called "inter-phenomena".) Feyerabend openly distances himself by most logical positivists.[34]

[33] Feyerabend made extensive annotations throughout a copy of Popper's *Logik der Forschung*, particularly in the chapters devoted to probability and quantum mechanics. Later, he had access to the manuscript of *Postscript* to *The Logic of Scientific Discovery* that was published in a completely reworked form some thirty years later, in three volumes. The first two volumes of the *Postscript* were in galley proofs in the mid-1950s and the third volume in the late 1950s. In the published version, the first two volumes would be devoted to realism and indeterminism – two of the key issues repeatedly discussed in the papers collected in the present volume, and the third volume to the quantum paradoxes and Popper's propensity interpretation of probability as applied to the interpretation of quantum physics. See his *Postscript to The Logic of Scientific Discovery*, edited by William W. Bartley, III, vol. 1: *Realism and the Aim of Science*, vol. 2: *The Open Universe: An Argument for Indeterminism*, and vol. 3: *Quantum Theory and the Schism in Physics*, London: Hutchinson, 1982–1983. The central theme of the third volume was presented by Popper at the Ninth Symposium of the Colston Research Society, in Bristol, which Feyerabend attended, too (see *PP1*, pp. 207–218, as well as Chapter 16 of the present collection: here Feyerabend introduces the traditional thesis that would be central to his later work, namely, that observations are theoretically biased, and inevitably so). Feyerabend's own work developed in close parallel with Popper's: in addition to the latter's *The Logic of Scientific Discovery*, London: Hutchinson, 1959, chs. 8–9, see his "The Propensity Interpretation of the Calculus of Probability, and the Quantum Theory", in Stephan Körner and Maurice H. L. Pryce (eds.), *Observation and Interpretation: A Symposium of Philosophers and Physicists*, New York: Academic Press Inc., Publishers, and London: Butterworths Scientific Publications, 1957, pp. 65–70 and 88–89, his "The Propensity Interpretation of Probability", *The British Journal for the Philosophy of Science*, 10, 1959, pp. 25–42; and his "Philosophy and Physics: The Influence on Physics of Some Metaphysical Speculations on the Structure of Matter", in *Atti del XII Congresso Internazionale di Filosofia (Venezia, 12–18 Settembre, 1958)*, Venice: G. F. Sansoni, 1960, vol. 2, pp. 367–374.

[34] Feyerabend ridicules Reichenbach's interphenomena thesis: see his "Reichenbach's Interpretation of Quantum Mechanics", *Philosophical Studies*, 9, 4, 1958, pp. 47–59; reprinted in *PP1*, pp. 236–246.

Most importantly, Feyerabend was struck by the attitude of orthodox physicists toward the gaps of quantum theory. Although they admitted that it would have to undergo some decisive changes in order to cope with some new discoveries and that the future new theories will introduce new concepts for the description of the new facts, they insisted that the basic elements of current theory would remain unchanged. The basic structure of the theory did not require a revision, and any modifications would not affect its indeterminist framework. By contrast, in Feyerabend's view the Copenhagen interpretation was but one possible interpretation of the quantum formalism. He upheld a pluralistic approach, as opposed to Thomas Kuhn's advocacy of conformism within a scientific research community. So Feyerabend came to defend the right of "hidden-variables" theorists, such as David Bohm, whom he admired. (They were both at the University of Bristol at the time.) Only a realistic interpretation, he said, can reveal the revolutionary potential of scientific theories.

Bohm called attention to some aspects of microphysics that he deemed problematic and most physicists deemed settled. This was a clash of ideas that intrigued Feyerabend. Whereas it is often assumed – both in philosophy and in the sciences, not to mention the community of scholars at large – that within the sciences theories are (almost) uniquely determined by facts, so that speculation and ingenuity have a limited role to play, Bohm's (and, later on, Bell's) questions indicated that the notorious divide between the sciences and the humanities is due to this very erroneous picture of science. Bohm opposed the received view

> that complementarity, and complementarity alone, solves all the ontological and conceptual problems of microphysics; that this solution possesses absolute validity; that the only thing left to the physicist of the future is to find, and to solve equations for the prediction of events which are otherwise well understood.[35]

The claim that the Copenhagen interpretation of quantum mechanics is the only possible interpretation allowed by experimental results, then, is downright dishonest. Feyerabend said this in 1960 in a few letters to Kuhn, upon reading the first draft of what would be published in 1962 as *The Structure of Scientific Revolutions*. The issue, in this case, is historical reconstructions, not interpretations of a physical theory, but the argument is exactly the same, and in his letters quantum physics is often referred to:

> What you are writing is not just history. It is *ideology covered up as history* . . . points of view *can* be made explicit, and it *is* possible to write history in such a

[35] Feyerabend, "Professor Bohm's Philosophy of Nature", *The British Journal for the Philosophy of Science*, 10, 1960, pp. 321–338; reprinted in *PP1*, pp. 219–235: p. 219.

manner that the reader is always aware of one's ideology or point of view *as well as of the possibility of an alternative interpretation of the historical facts*. That is, history can be written in such a manner *that what is factual and what is reasonable appear as two clearly distinct affairs*. ... What I do object to most emphatically is the way you present this belief of yours; you present it not as a *demand*, but as something that is an obvious consequence of historical facts.[36]

According to Popper, experiment does not impose the strange consequences drawn from quantum theory; an erroneous philosophical approach to physics does that. It is positivism: Bohr and Heisenberg, Popper claimed, were seduced by traditional positivists such as Ernst Mach as well as by the new ones, the "logical" positivists, including the members of the Vienna Circle. Their theory was not logically true but hypothetical, and erroneous. Feyerabend disagreed. He claimed that the Copenhagen theorists had some perfectly good physical arguments for thinking that their view alone was compatible with the observed results of experiments, and he offered a defence of their instrumentalist interpretation. Ultimately, however, he argued for the necessity that the observed results of experiments themselves be challenged, thereby using the case of quantum theory (as, in other contexts, he appealed to Galileo's case, or to other cases from the rich history of science) to push for a reconsideration of the methodological rules by which researchers are abiding or declare to be abiding. Here we may find, *in nuce*, Feyerabend's pluralistic test model,[37] in which theories are contrasted with one another as well as with experience: "the methodological unit to which we must refer when discussing questions of test and empirical content is constituted by a

[36] Paul Hoyningen-Huene, "Two Letters of Paul Feyerabend to Thomas S. Kuhn on a Draft of *The Structure of Scientific Revolutions*", cit., p. 355; see also pp. 356, 360, 367–368 and 379–380. In another letter, Feyerabend describes the Copenhagen interpretation not as a paradigm, as Kuhn did, but as "what remains of a former paradigm (the classical theories) when this has been freed from anything that goes beyond experience. ... They have not simply added *another* theory to the theories of the past which at some future time may be replaced by again another theory. ... From now on we have entered a new age of scientific activity. There will be no more revolutions, there will be only accumulation" (ibid., p. 379). See also Paul Hoyningen-Huene, "More letters by Paul Feyerabend to Thomas S. Kuhn on *Proto-Structure*", *Studies in History and Philosophy of Science*, 37, 2006, pp. 610–632.

[37] Feyerabend's theoretical pluralism (scientific progress is enhanced by the simultaneous presence of a sufficiently large number of competing theories), advocated in his early works, is not to be confused with his later methodological pluralism (science has no distinctive method, therefore anything goes). "Theoretical pluralism (that is, Feyerabend's pluralistic methodology)", writes John Preston (*Feyerabend: Philosophy, Science and Society*, Cambridge: Polity Press, 1997, p. 139), "is intended to be a single methodology for all scientific inquiry. It sponsors the proliferation of theories, but not of methods for evaluating theories".

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whole set of partly overlapping, factually adequate, but mutually inconsistent theories".[38] Otherwise, he suggested, there would be no more arguing or judging among disciplines: criticism, evaluation, and explanation would no longer be the aims of proper philosophical discourse. All philosophers would be left with, then, would be descriptions of the logics, grammars, or first principles of the various kinds of discourse, and the many sorts of language games and forms of life in which they are embedded. Philosophical critique would no longer be of content, but of criteria application; as Feyerabend put it, all that would be left are "consolations for the specialist".[39]

Stefano Gattei

Joseph Agassi

[38] Feyerabend, "How to Be a Good Empiricist: A Plea for Tolerance in Matters Epistemological", in Bernard Baumrin (ed.), *Philosophy of Science: The Delaware Seminar*, vol. 2, New York: Interscience, 1963, pp. 3–39; reprinted in *PP3*, pp. 78–103: p. 92. This was but an extension of ideas Popper had already formulated in *The Logic of Scientific Discovery* and elsewhere. In the 1962 original version of "Explanation, Reduction, and Empiricism" (in Herbert Feigl and Grover Maxwell (eds.), *Scientific Explanation, Space and Time*, Minneapolis: University of Minnesota Press, 1962, pp. 28–97: pp. 31–32), Feyerabend readily acknowledged this; later he withdrew the acknowledgement – after Popper had called attention to it. See Popper, *Objective Knowledge: An Evolutionary Approach*, Oxford: Clarendon Press, 1972, p. 205; and Feyerabend, "Explanation, Reduction, and Empiricism", in *PP1*, pp. 44–96: p. 47, footnote 6. This implies that Popper never advocated a monistic model, according to which a single theory is tested against "experience". Although there is hardly any passage in which Feyerabend explicitly associated Popper with this thesis, a number of Feyerabend scholars assume that he did.

[39] Paul K. Feyerabend, "Consolations for the Specialist", in Imre Lakatos and Alan Musgrave (eds.), *Criticism and the Growth of Knowledge*, Cambridge: Cambridge University Press, 1970, pp. 197–230; see also his "Kuhns Struktur wissenschaftlicher Revolutionen: Ein Trostbüchlein für Spezialisten?", in Paul K. Feyerabend, *Der wissenschaftstheoretische Realismus und die Autorität der Wissenschaften*, Braunschweig: Vieweg, 1978, pp. 153–204.

Editorial Note

Two volumes of collected philosophical papers were edited by Feyerabend himself and appeared in 1981; he published another collection in 1987. In 1999, five years after his demise, John Preston edited a third volume of his collected papers, and Bert Terpstra saw through the press Feyerabend's last (unfinished) manuscript, to which he attached a number of previously published essays dealing with its main themes. These books will be referred to as follows:

PP1 Realism, Rationalism and Scientific Method: Philosophical Papers, vol. 1, Cambridge: Cambridge University Press, 1981.
PP2 *Problems of Empiricism: Philosophical Papers*, vol. 2, Cambridge: Cambridge University Press: Cambridge 1981.
FR *Farewell to Reason*, London: Verso/New Left Books, 1987.
PP3 *Knowledge, Science and Relativism: Philosophical Papers*, vol. 3, edited by John M. Preston, Cambridge: Cambridge University Press, 1999.
CA *The Conquest of Abundance: A Tale of Abstraction versus the Richness of Being*, edited by Bert Terpstra, Chicago–London: The University of Chicago Press, 1999.

Discussions of specific issues and detailed analyses of problems related to contemporary physics are scattered throughout Feyerabend's works. They appeared in various forms: as journal articles, book chapters, reviews, and comments, as well as in book form. Unlike the previous volumes, which cover a variety of issues in the philosophy of science, the present collection focuses on Feyerabend's papers on the