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No theory ever agrees with all the facts *in its domain, yet it is not always the theory that is to blame. Facts are constituted by older ideologies, and a clash between facts and theories may be proof of progress. It is also a first step in our attempt to find the principles implicit in familiar observational notions.*

Considering now the invention, elaboration and the use of theories which are inconsistent, not just with other theories, but even with *experiments*, *facts*, *observations*, we may start by pointing out that *no single theory ever agrees with all the known facts in its domain*. And the trouble is not created by rumours, or by the result of sloppy procedure. It is created by experiments and measurements of the highest precision and reliability.

It will be convenient, at this place, to distinguish two different kinds of disagreement between theory and fact: numerical disagreement, and qualitative failures.

The first case is quite familiar: a theory makes a certain numerical prediction and the value that is actually obtained differs from the prediction made by more than the margin of error. Precision instruments are usually involved here. Numerical disagreements abound in science. They give rise to an 'ocean of anomalies' that surrounds every single theory.[1]

Thus the Copernican view at the time of Galileo was inconsistent with facts so plain and obvious that Galileo had to call it 'surely false'.[2] 'There is no limit to my astonishment,' he writes in a

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[1] For the 'ocean' and various ways of dealing with it, cf. my 'Reply to Criticism', *Boston Studies*, Vol. 2, 1965, pp. 224ff.

[2] Galileo Galilei, *The Assayer*, quoted in S. Drake and C.D. O'Malley (eds), *The Controversy on the Comets of 1618*, London, 1960, p. 185. The 'surely false' refers to the condemnation by Church authorities. But as will be explained in the course of the book and especially in Chapter 13, the condemnation was based in part on the 'philosophical absurdity' of the idea of a moving earth, i.e. on its empirical failures and its theoretical inadequacy. See also the next quotation and footnote. 'As to the system of Ptolemy', writes Galileo on this point (184), 'neither Tycho, nor other astronomers, nor even

later work,[3] 'when I reflect that Aristarchus and Copernicus were able to make reason so conquer sense that, in defiance of the latter, the former became mistress of their belief.' Newton's theory of gravitation was beset, from the very beginning, by difficulties serious enough to provide material for refutation.[4] Even quite recently and in the non-relativistic domain it could be said that there 'exist numerous discrepancies between observation and theory'.[5] Bohr's atomic model was introduced, and retained, in the face of precise and unshakeable contrary evidence.[6] The special theory of relativity was retained despite Kaufmann's unambiguous results of 1906, and despite D.C. Miller's experiment.[7] The general theory of relativity,

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Copernicus could clearly refute it, inasmuch as a most important argument taken from the movement of Mars and Venus always stood in their way.' The 'most important argument' and Galileo's resolution are discussed in Chapters 9 and 10.

[3] Galileo Galilei, *Dialogue Concerning the Two Chief World Systems*, Berkeley, 1953, p. 328.

[4] According to Newton the 'mutual actions of comets and planets upon one another' give rise to 'some inconsiderable irregularities . . . which will be apt to increase, till the system wants a reformation', *Opticks*, New York, 1952, p. 402. What Newton means is that gravitation disturbs the planets in a way that is likely to blow the planetary system apart. Babylonian data as used by Ptolemy show that the planetary system has remained stable for a long time. Newton concluded that it was being periodically 'reformed' by divine interventions: God acts as a stabilizing force in the planetary system (and in the world as a whole, which is constantly losing motion through processes such as inelastic collisions). One of the 'irregularities' considered by Newton, the great inequality of Jupiter and Saturn (*Principia*, transl. Motte, ed. Cajori, Berkeley, 1934, p. 397) was shown by Laplace to be a periodic disturbance with a large period. Then Poincaré found that the series developments customary in the calculations often diverged after they had shown some convergence while Bruhns discovered that no quantitative methods other than series expansions could resolve the *n*-body problem. This was the end of the purely quantitative period in celestial mechanics (details in J. Moser, *Annals of Mathematical Studies*, Vol. 77, 1973, Princeton). See also M. Ryabov, *An Elementary Survey of Celestial Mechanics*, New York, 1961, for a survey and quantitative results of various methods of calculation. The qualitative approach is briefly described on pp. 126f. Thus it took more than two hundred years before one of the many difficulties of this rather successful theory was finally resolved.

[5] Brower–Clemence, *Method of Celestial Mechanics*, New York, 1961. Also R.H. Dicke, 'Remarks on the Observational Basis of General Relativity', in H.Y. Chiu and W.F. Hoffman (eds), *Gravitation and Relativity*, New York, 1964, pp. 1–16. For a more detailed discussion of some of the difficulties of classical celestial mechanics, cf. J. Chazy, *La Théorie de la relativité et la Méchanique céleste*, Vol. 1, Chapters 4 and 5, Paris, 1928.

[6] Cf. Max Jammer, *The Conceptual Development of Quantum Mechanics*, New York, 1966, section 22. For an analysis cf. section 3c/2 of Lakatos, 'Falsification and the Methodology of Scientific Research Programmes', in Lakatos–Musgrave (eds), *Criticism and the Growth of Knowledge*, Cambridge, 1970.

[7] W. Kaufmann, 'Über die Konstitution des Elektrons', *Ann. Phys.*, No. 19, 1906, p. 487. Kaufmann stated his conclusion quite unambiguously, and in italics: '*The*

though surprisingly successful in a series of occasionally rather dramatic tests,[8] had a rough time in areas of celestial mechanics different from the advance of the perihelion of Mercury.[9] In the sixties the arguments and observations of Dicke and others seemed to endanger even this prediction. The problem is still unresolved.[10]

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results of the measurements are not compatible with the fundamental assumption of Lorentz and Einstein.' Lorentz's reaction: '... it seems very likely that we shall have to relinquish this idea altogether' (*Theory of Electrons*, second edition, p. 213). Ehrenfest: 'Kaufmann demonstrates that Lorentz's deformable electron is ruled out by the measurements' ('Zur Stabilitätsfrage bei den Bucherer–Langevin Elektronen', *Phys. Zs.*, Vol. 7, 1906, p. 302). Poincaré's reluctance to accept the 'new mechanics' of Lorentz can be explained, at least in part, by the outcome of Kaufmann's experiment. Cf. *Science and Method*, New York 1960, Book III, Chapter 2, section v, where Kaufmann's experiment is discussed, the conclusion being that the 'principle of relativity ... cannot have the fundamental importance one was inclined to ascribe to it'. Cf. also St Goldberg, 'Poincaré's Silence and Einstein's Relativity', *British Journal for the History of Science*, Vol. 5, 1970, pp. 73ff, and the literature given there. Einstein alone regarded the results as 'improbable because their basic assumptions, from which the mass of the moving electron is deduced, are not suggested by theoretical systems which encompass wider complexes of phenomena' (*Jahrbuch der Radioaktivität und Elektrizität*, Vol. 4, 1907, p. 349). Miller's work was studied by Lorentz for many years, but he could not find the trouble. It was only in 1955, twenty-five years after Miller had finished his experiments, that a satisfactory account of Miller's results was found. Cf. R.S. Shankland, 'Conversations with Einstein', *Am. Journ. Phys.*, Vol. 31, 1963, pp. 47–57, especially p. 51, as well as footnotes 19 and 34; cf. also the inconclusive discussion at the 'Conference on the Michelson–Morley Experiment', *Astrophysical Journal*, Vol. 68, 1928, pp. 341ff.

Kaufmann's experiment was analysed by Max Planck and found to be not decisive: what had stopped Ehrenfest, Poincaré and Lorentz did not stop Planck. Why? My conjecture is that Planck's firm belief in an objective reality and his assumption that Einstein's theory was about such a reality made him a little more critical. Details in Chapter 6 of Elie Zahar, *Einstein's Revolution*, La Salle, Ill., 1989.

[8] Such as the test of the effects of gravity upon light that was carried out in 1919 by Eddington and Crommelin and evaluated by Eddington. For a colourful description of the event and its impact, cf. C.M. Will, *Was Einstein Right?*, New York, 1986, pp. 75ff.

[9] Chazy, op. cit., p. 230.

[10] Repeating considerations by Newcomb (reported, for example, in Chazy, op. cit., pp. 204ff), Dicke pointed out that an oblateness of the sun would add classical terms to Mercury's motion and reduce the excess (compared with Newton's theory) advance of its perihelion. Measurements by Dicke and Goldenberg then found a difference of 52 km between the equatorial and polar diameter of the sun and a corresponding reduction of three seconds of arc for Mercury – a sizeable deviation from the relativistic value. This led to a considerable controversy concerning the accuracy of the Dicke–Goldenberg experiment and to an increase in the number of non-Einsteinian theories of gravitation. Technical details in C.M. Will, *Theory and Experiment in Gravitational Physics*, Cambridge, 1981, pp. 176ff, a popular survey including later developments in *Was Einstein Right?*, Chapter 5. Note how a new theory (Einstein's theory of gravitation) which is theoretically plausible and well confirmed can be endangered by exploiting its 'refuted' predecessor and carrying out appropriate experiments. Cf. also R.H. Dicke, op. cit.

On the other hand there exist numerous new tests, both inside the planetary system and outside of it[11] that provide confirmations of a precision unheard of only twenty years ago and unimagined by Einstein. In most of these cases we are dealing with quantitative problems which can be resolved by discovering a better set of *numbers* but which do not force us to make qualitative adjustments.[12]

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[11] Tests outside the planetary system (cosmology, black holes, pulsars) are needed to examine alternatives that agree with Einsteinian relativity inside the solar system. There now exists a considerable number of such alternatives and special steps have been taken to classify them and to elucidate their similarities and differences. Cf. the introduction to C.M. Will, op. cit.

[12] The situation just described shows how silly it would be to approach science from a naive–falsificationist perspective. Yet this is precisely what some philosophers have been trying to do. Thus Herbert Feigl (*Minnesota Studies*, Vol. 5, 1971, p. 7) and Karl Popper (*Objective Knowledge*, p. 78) have tried to turn Einstein into a naive falsificationist. Feigl writes: 'If Einstein relied on "beauty", "harmony", "symmetry", "elegance" in constructing . . . his general theory of relativity, it must nevertheless be remembered that he also said (in a lecture in Prague in 1920 – I was present then as a very young student): "If the observations of the red shift in the spectra of massive stars don't come out quantitatively in accordance with the principles of general relativity, then my theory will be dust and ashes".' Popper writes: 'Einstein . . . said that if the red shift effect . . . was not observed in the case of white dwarfs, his theory of general relativity would be refuted.'

Popper gives no source for his story, and he most likely has it from Feigl. But Feigl's story and Popper's repetition conflict with the numerous occasions where Einstein emphasizes the 'reason of the matter' ('die Vernunft der Sache') over and above 'verification of little effects' and this not only in casual remarks during a lecture, but in writing. Cf. the quotation in footnote 7 above, which deals with difficulties of the special theory of relativity and precedes the talk at which Feigl was present. Cf. also the letters to M. Besso and K. Seelig as quoted in G. Holton, 'Influences on Einstein's Early Work', *Organon*, No. 3, 1966, p. 242, and K. Seelig, *Albert Einstein*, Zurich, 1960, p. 271. In 1952 Born wrote to Einstein (*Born–Einstein Letters*, New York, 1971, p. 190, dealing with Freundlich's analysis of the bending of light near the sun and the red shift): 'It really looks as if your formula is not quite correct. It looks even worse in the case of the red shift [the crucial case referred to by Feigl and Popper]; this is much smaller than the theoretical value towards the centre of the sun's disk, and much larger at the edges. . . . Could this be a hint of non-linearity?' Einstein (letter of 12 May 1952, op. cit., p. 192) replied. 'Freundlich . . . does not move me in the slightest. Even if the deflection of light, the perihelial movement or line shift were unknown, the gravitation equations would still be convincing because they avoid the inertial system (the phantom which affects everything but is not itself affected). *It is really strange that human beings are normally deaf to the strongest arguments while they are always inclined to overestimate measuring accuracies'* (my italics). How is this conflict (between Feigl's testimony and Einstein's writings) to be explained? It cannot be explained by a *change* in Einstein's attitude. His disrespectful attitude towards observation and experiment was there from the very beginning, as we have seen. It might be explained either by a mistake on Feigl's part, or else as another instance of Einstein's 'opportunism' – cf. text to footnote 6 of the *Introduction*.

On the last page (p. 91) of his *Über die Spezielle und allgemeine Relativitätstheorie*, The second case, the case of qualitative failures, is less familiar, but of much greater interest. In this case a theory is inconsistent not with a recondite fact, that can be unearthed with the help of complex equipment and is known to experts only, but with circumstances which are easily noticed and which are familiar to everyone.

The first and, to my mind, the most important example of an inconsistency of this kind is Parmenides' theory of the unchanging and homogeneous One. This theory illustrates a desire that has propelled the Western sciences from their inception up to the present time – the desire to find a unity behind the many events that surround us. Today the unity sought is a *theory* rich enough to produce all the accepted facts and laws; at the time of Parmenides the unity sought was a *substance*. Thales had proposed water,[13] Heraclitus fire, Anaximander a substance which he called the *apeiron* and which could produce all four elements without being identical with a single one of them. Parmenides gave what seems to be an obvious and rather trivial answer: the substance that underlies everything that is is *Being*. But this trivial answer had surprising consequences. For example, we can assert that (first principle) *Being* is and that (second principle) *Not Being is not*. Now consider change and assume it to be fundamental. Then change can only go from Being to Not Being. But according to the second principle Not Being is not, which means that there is no fundamental change. Next consider difference and assume it to be fundamental. Then the difference can only be between Being and Not Being. But (second principle) Not Being is not and therefore there exist no differences in Being – it is a single, unchanging, continuous block. Parmenides knew of course that people, himself included, perceive and accept change and difference; but as his argument had shown that the perceived processes could not be fundamental he had to regard them as merely apparent, or deceptive. This is indeed what he said – thus anticipating all those scientists who contrasted the 'real' world of science with the everyday world of qualities and emotions, declared the latter to be 'mere appearance' and tried to base their arguments on 'objective' experiments and mathematics exclusively. He also anticipated a

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Brunswick, 1922, Einstein writes: 'If the red shift of the spectral lines caused by the gravitational potential did not exist, then the general theory of relativity would be untenable.' Does this conflict with Einstein's cavalier attitude towards observation as described above? It does not. The passage speaks of *the red shift* not of *observations of it*.

[13] The following account is highly speculative. Details in Vols 1 and 2 of W.K.C. Guthrie, *A History of Greek Philosophy*, Cambridge, 1962 and 1965, as well as in Chapters 1, 2 and 3 of my *Farewell to Reason*.

popular interpretation of the theory of relativity which sees all events and transitions as already prearranged in a four-dimensional continuum, the only change being the (deceptive) journey of consciousness along its world line.[14] Be that as it may, he was the first to propose a conservation law (*Being is*), to draw a boundary line between reality and appearance (and thus to create what later thinkers called a 'theory of knowledge') and to give a more satisfactory foundation for continuity than did 19th- and 20th-century mathematicians who had to invoke 'intuition'. Using Parmenides' arguments Aristotle constructed a theory of space and motion that anticipated some very deep-lying properties of quantum mechanics and evaded the difficulties of the more customary (and less sophisticated) interpretation of a continuum as consisting of indivisible elements.[15] Parmenides' theory clashes with most modern methodological principles – but this is no reason to disregard it.

A more specific example of a theory with qualitative defects is Newton's theory of colours. According to this theory, light consists of rays of different refrangibility which can be separated, reunited, refracted, but which are never changed in their internal constitution, and which have a very small lateral extension in space. Considering that the surface of mirrors is much rougher than the lateral extension of the rays, the ray theory is found to be inconsistent with the existence of mirror images (as is admitted by Newton himself): if light consists of rays, then a mirror should behave like a rough surface, i.e. it should look to us like a wall. Newton retained his theory, eliminating the difficulty with the help of an *ad hoc* hypothesis: 'the reflection of a ray is effected, not by a single point of the reflecting body, but by some power of the body which is evenly diffused all over its surface'.[16]

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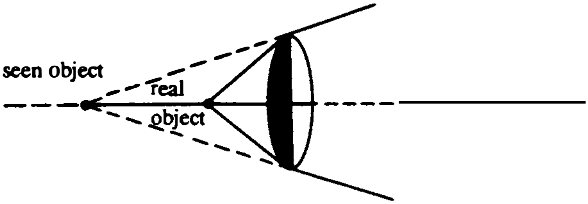
[14] A vivid description of the Parmenidean flavour of the theory of relativity is given by H. Weyl, *Philosophy of Mathematics and Natural Science*, Princeton, 1949, p. 116. Einstein himself wrote: 'For us who are convinced physicists the distinction between past, present and future has no other meaning than that of an illusion, though a tenacious one.' *Correspondence avec Michele Besso*, Paris, 1979, p. 312. Cf. also p. 292. In a word: the events of a human life are 'illusions, though tenacious ones'.

[15] For Aristotle cf. the essay quoted in Chapter 4, footnote 3. Modern attempts to get continuity out of collections of indivisible elements are reported in A. Gruenbaum, 'A Consistent Conception of the Extended Linear Continuum as an Aggregate of Unextended Elements', *Philosophy of Science*, No. 19, 1952, pp. 283ff. Cf. also W. Salmon (ed.), *Zeno's Paradoxes*, New York, 1970.

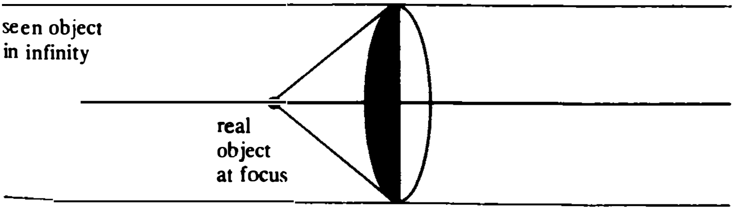
[16] Sir Isaac Newton, *Optics*, Book 2, part 3, proposition 8, New York, 1952, p. 266. For a discussion of this aspect of Newton's method cf. my essay, 'Classical Empiricism', *Philosophical Papers*, Vol. 2, Chapter 2.

In Newton's case the qualitative discrepancy between theory and fact was removed by an *ad hoc* hypothesis. In other cases not even this very flimsy manoeuvre is used: one retains the theory *and tries to forget* its shortcomings. An example is the attitude towards Kepler's rule according to which an object viewed through a lens is perceived at the point at which the rays travelling from the lens towards the eye intersect.[17]



The rule implies that an object situated at the focus will be seen infinitely far away.

'But on the contrary,' writes Barrow, Newton's teacher and predecessor at Cambridge, commenting on this prediction,[18] 'we are assured by experience that [a point situated close to the focus] appears variously distant, according to the different situations of the eye. . . And it does almost never seem further off than it would be if it were beheld with the naked eye; but, on the contrary, it does sometimes appear much nearer. . . . All which does seem repugnant to our principles.' 'But for me,' Barrow continues, 'neither this nor any other difficulty shall have so great an influence on me, as to make me renounce that which I know to be manifestly agreeable to reason.'



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[17] Johannes Kepler, *Ad Vitellionem Paralipomena, Johannes Kepler, Gesammelte Werke*, Vol. 2, Munich, 1939, p. 72. For a detailed discussion of Kepler's rule and its influence see Vasco Ronchi, *Optics: The Science of Vision*, New York, 1957, Chapters 43ff. Cf. also Chapters 9–11 below.

[18] *Lectiones XVIII Cantabrigiae in Scholio publicis habitae in quibus Opticorum Phenomenon genuinae Rationes investigantur ac exponentur*, London, 1669, p. 125. The passage is used by Berkeley in his attack on the traditional, 'objectivistic' optics (*An Essay Towards a New Theory of Vision*, Works, Vol. 1, ed. Frazer, London, 1901, pp. 137ff).

Barrow *mentions* the qualitative difficulties, adding that he will not abandon the theory. This is not the usual procedure. The usual procedure is to forget the difficulties, never to talk about them, and to proceed as if the theory were without fault. This attitude is very common today.

Thus the classical electrodynamics of Maxwell and Lorentz implies that the motion of a free particle is self-accelerated. Considering the self-energy of the electron one obtains divergent expressions for point-charges while charges of finite extension can be made to agree with relativity only by adding untestable stresses and pressures inside the electron.[19] The problem reappears in the quantum theory, though it is here partially covered up by 'renormalization'. This procedure consists in crossing out the results of certain calculations and replacing them by a description of what is actually observed. Thus one admits, implicitly, that the theory is in trouble while formulating it in a manner suggesting that a new principle has been discovered.[20] Small wonder when philosophically unsophisticated authors get the impression that 'all evidence points with merciless definiteness in the direction . [that] all the processes involving . unknown interactions conform to the fundamental quantum law.'[21]

A striking example of qualitative failure is the status of classical mechanics and electrodynamics after Boltzmann's equipartition theorem. According to this theorem energy is equally distributed over all degrees of freedom of a (mechanical or electrodynamic) system. Both atoms (which had to be elastic to rebound from the walls of a container and from each other) and the electromagnetic field had infinitely many degrees of freedom which meant that solids and the

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[19] Cf. W. Heitler, *The Quantum Theory of Radiation*, Oxford, 1954, p. 31.

[20] Renormalization has in the meantime become the basis of quantum field theory and has led to predictions of surprising accuracy (report with literature in A. Pais, *Inward Bound*, Oxford, 1986). This shows that a point of view which, looked at from afar, appears to be hopelessly wrong may contain excellent ingredients and that its excellence may remain unrevealed to those guided by strict methodological rules. Always remember that my examples do not criticize science; they criticize those who want to subject it to their simpleminded rules by showing the disasters such rules would create. Each of the examples of footnotes 3–17 can be used as a basis for case studies of the kind to be carried out in Chapters 6–12 (Galileo and the Copernican Revolution). This shows that the case of Galileo is not 'an exception characterizing the beginning of the so-called scientific revolution' (G. Radnitzky, 'Theorienpluralismus Theorienmonismus', in Diemer Meisenheim (ed.), *Der Methoden- und Theorienpluralismus in den Wissenschaften*, 1971, p. 164) but is *typical* of scientific change at all times.

[21] Rosenfeld in *Observation and Interpretation*, London, 1957, p. 44.

electromagnetic fields should have acted as insatiable sinks of energy. Yet '[a]s so often in the history of science, the conflict between simple and generally known facts and current theoretical ideas was recognized only slowly'.[22]

Another example of modern physics is quite instructive, for it might have led to an entirely different development of our knowledge concerning the microcosm. Ehrenfest proved a theorem according to which the classical electron theory of Lorentz taken in conjunction with the equipartition theorem excludes induced magnetism.[23] The reasoning is exceedingly simple; according to the equipartition theorem, the probability of a given motion is proportional to exp $(-U/RT)$, where U is the energy of the motion. Now the rate of work of an electron moving in a constant magnetic field B is, according to Lorentz, $W=Q(E+V\times B).\ V$, where Q is the charge of the moving particle, V its velocity and E the electric field. This magnitude reduces to QEV, which means that the energy and, therefore, the probability remains unaffected by a magnetic field. (Given the proper context, this result strongly supports the ideas and experimental findings of the late Felix Ehrenhaft.)

Occasionally it is impossible to survey all the interesting consequences, and thus to discover the absurd results of a theory. This may be due to a deficiency in the existing mathematical methods; it may also be due to the ignorance of those who defend the theory. Under such circumstances, the most common procedure is to use an older theory up to a certain point (which is often quite arbitrary) and to add the new theory for calculating refinements. Seen from a methodological point of view the procedure is a veritable nightmare. Let us explain it using the relativistic calculation of the path of Mercury as an example.

The perihelion of Mercury moves along at a rate of about 5600″ per century. Of this value, 5026″ are geometric, having to do with the movement of the reference system, while 531″ are dynamical, due to perturbations in the solar system. Of these perturbations all but the

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[22] K. Gottfried, V.F. Weisskopf, *Concepts of Particle Physics*, Vol. 1, Oxford and New York, 1984, p. 6.

[23] The difficulty was realized by Bohr in his doctoral thesis, cf. Niels Bohr, *Collected Works*, Vol. 1, Amsterdam, 1972, pp. 158, 381. He pointed out that the velocity changes due to the changes in the external field would equalize after the field was established, so that no magnetic effects could arise. Cf. also Heilbron and T.S. Kuhn, 'The Genesis of the Bohr Atom', *Historical Studies in the Physical Sciences*, No. 1, 1969, p. 221. The argument in the text is taken from *The Feynman Lectures*, Vol. 2, California and London, 1965, Chapter 34.6. For a somewhat clearer account cf. R. Becker, *Theorie der Elektrizität*, Leipzig, 1949, p. 132.

famous 43″ are accounted for by classical mechanics. This is how the situation is usually explained.

The explanation shows that the premise from which we derive the 43″ is not the general theory of relativity plus suitable initial conditions. The premise contains classical physics *in addition* to whatever relativistic assumptions are being made. Furthermore, the relativistic calculation, the so-called 'Schwarzschild solution', does not deal with the planetary system as it exists in the real world (i.e. our own asymmetric galaxy); it deals with the entirely fictional case of a central symmetrical universe containing a singularity in the middle and nothing else. What are the reasons for employing such an odd conjunction of premises?

The reason, according to the customary reply, is that we are dealing with approximations. The formulae of classical physics do not appear because relativity is incomplete. Nor is the centrally symmetric case used because relativity does not offer anything better. Both schemata flow from the general theory under the special circumstances realized in our planetary system *provided* we omit magnitudes too small to be considered. Hence, we are using the theory of relativity throughout, and we are using it in an adequate manner.

Now in the present case, making the required approximations would mean calculating the full *n*-body problem relativistically (including long-term resonances between different planetary orbits), omitting magnitudes smaller than the precision of observation reached, and showing that the theory thus curtailed coincides with classical celestial mechanics as corrected by Schwarzschild. This procedure has not been used by anyone simply because the relativistic *n*-body problem has as yet withstood solution. When the argument started, there were not even approximate solutions for important problems such as, for example, the problem of stability (one of the first great stumbling blocks for Newton's theory). The classical part of the explanans, therefore, did not occur just for convenience, *it was absolutely necessary*. And the approximations made were not a result of relativistic calculations, they were introduced in order to make relativity fit the case. One may properly call them *ad hoc approximations.*[24]

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[24] Today the so-called parametrized post-Newtonian formalism satisfies most of the desiderata outlined in the text (details in C.M. Will, *Theory*). My point is that this was a later achievement whose absence did not prevent scientists from arguing, *and arguing well*, about the new ideas. Theories are not only used as premises for derivations; they are even more frequently used as a general background for novel

Ad hoc approximations abound in modern mathematical physics. They play a very important part in the quantum theory of fields and they are an essential ingredient of the correspondence principle. At the moment we are not concerned with the reasons for this fact, we are only concerned with its consequences: *ad hoc* approximations conceal, and even eliminate, qualitative difficulties. They create a false impression of the excellence of our science. It follows that a philosopher who wants to study the adequacy of science as a picture of the world, or who wants to build up a realistic scientific methodology, must look at modern science with special care. In most cases modern science is more opaque, and more deceptive, than its 16th- and 17th-century ancestors have ever been.

As a final example of qualitative difficulties I mention again the heliocentric theory at the time of Galileo. I shall soon have occasion to show that this theory was inadequate both qualitatively and quantitatively, and that it was also philosophically absurd.

To sum up this brief and very incomplete list: wherever we look, whenever we have a little patience and select our evidence in an unprejudiced manner, we find that theories fail adequately to reproduce certain *quantitative results*, and that they are *qualitatively incompetent* to a surprising degree. Science gives us theories of great beauty and sophistication. Modern science has developed mathematical structures which exceed anything that has existed so far in coherence generality and empirical success. But in order to achieve this miracle all the existing troubles had to be pushed into the *relation* between theory and fact,[25] and had to be concealed, by *ad hoc* hypotheses, *ad hoc* approximations and other procedures.

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guesses whose formal relation to the basic assumptions is difficult to ascertain. 'I must . . . confess', writes Descartes in his *Discourse on Method* (Library of Liberal Arts, 1965, p. 52), 'that the power of nature is so ample and so vast, and these principles [the theoretical principles he had developed for his mechanical universe] so simple and so general, that I almost never notice any particular effect such that I do not see right away that it can [be made to conform to these principles] in many different ways; and my greatest difficulty is usually to discover in which of these ways the effect is derived.' Modern theoretical physicists find themselves in exactly the same situation.

[25] Von Neumann's work in quantum mechanics is an especially instructive example of this procedure. In order to arrive at a satisfactory proof of the expansion theorem in Hilbert Space, von Neumann replaced the quasi-intuitive notions of Dirac (and Bohr) by more complex notions of his own. The theoretical relations between the new notions are accessible to a more rigorous treatment than the theoretical relations between the notions that preceded them ('more rigorous' from the point of view of von Neumann and his followers). It is different with their relation to experimental procedures. No measuring instruments can be specified for the great majority of observables (Wigner, *American Journal of Physics*, Vol. 31, 1963, p. 14), and where specification is possible it becomes necessary to modify well-known and unrefuted

This being the case, what shall we make of the methodological demand that a theory must be judged by experience and must be rejected if it contradicts accepted basic statements? What attitude shall we adopt towards the various theories of confirmation and corroboration, which all rest on the assumption that theories can be made to agree with the known facts, and which use the amount of agreement reached as a principle of evaluation? This demand, these theories, are now all seen to be quite useless. They are as useless as a medicine that heals a patient only if he is bacteria-free. In practice they are never obeyed by anyone. Methodologists may point to the importance of falsifications – but they blithely use falsified theories, they may sermonize how important it is to consider all the relevant evidence, and never mention those big and drastic facts which show that the theories they admire and accept may be as badly off as the older theories which they reject. In *practice* they slavishly repeat the most recent pronouncements of the top dogs in physics, though in doing so they must violate some very basic rules of their trade. Is it possible to proceed in a more reasonable manner? Let us see![26]

According to Hume, theories cannot be *derived from* facts. The demand to admit only those theories which follow from facts leaves us without any theory. Hence, science *as we know it* can exist only if we drop the demand and revise our methodology.

According to our present results, hardly any theory is *consistent with* the facts. The demand to admit only those theories which are consistent with the available and accepted facts again leaves us without any theory. (I repeat: *without any theory*, for there is not a single theory that is not in some trouble or other.) Hence, a science as we know it can exist only if we drop this demand also and again revise our methodology, *now admitting counterinduction in addition to admitting unsupported hypotheses*. The right method must not contain

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laws in an arbitrary way or else to admit that some quite ordinary problems of quantum mechanics, such as the scattering problem, do not have a solution (J.M. Cook, *Journal of Mathematical Physics*, Vol. 36, 1957). Thus the theory becomes a veritable monster of rigour and precision while its relation to experience is more obscure than ever. It is interesting to see that similar developments occur in 'primitive thought'. 'The most striking features of Nupe sand divining', writes S.F. Nader in *Nupe Religion*, 1954, p. 63, 'is the contrast between its pretentious theoretical framework and its primitive and slipshod application in practice.' It does not need a science to produce Neumannian nightmares.

[26] The existence of qualitative difficulties, or 'pockets of resistance' (St Augustine, *Contra Julianum*, V, xiv, 51 – *Migne*, Vol. 44), was used by the Church fathers to defuse objections which the science of their time had raised against parts of the Christian faith, such as the doctrine of the corporeal resurrection.

any rules that make us choose between theories *on the basis of falsification*. Rather, its rules must enable us to choose between theories which we have already tested *and which are falsified*.

To proceed further. Not only are facts and theories in constant disharmony, they are never as neatly separated as everyone makes them out to be. Methodological rules speak of 'theories', 'observations' and 'experimental results' as if these were well-defined objects whose properties are easy to evaluate and which are understood in the same way by all scientists.

However, the material which a scientist *actually* has at his disposal, his laws, his experimental results, his mathematical techniques, his epistemological prejudices, his attitude towards the absurd consequences of the theories which he accepts, is indeterminate in many ways, ambiguous, *and never fully separated from the historical background*. It is contaminated by principles which he does not know and which, if known, would be extremely hard to test. Questionable views on cognition, such as the view that our senses, used in normal circumstances, give reliable information about the world, may invade the observation language itself, constituting the observational terms as well as the distinction between veridical and illusory appearance. As a result, observation languages may become tied to older layers of speculation which affect, in this roundabout fashion, even the most progressive methodology. (Example: the absolute space-time frame of classical physics which was codified and consecrated by Kant.) The sensory impression, however simple, contains a component that expresses the physiological reaction of the perceiving organism and has no objective correlate. This 'subjective' component often merges with the rest, and forms an unstructured whole which must be subdivided from the outside with the help of counterinductive procedures. (An example is the appearance of a fixed star to the naked eye, which contains the effects of irradiation diffraction, diffusion, restricted by the lateral inhibition of adjacent elements of the retina and is further modified in the brain.) Finally, there are the auxiliary premises which are needed for the derivation of testable conclusions, and which occasionally form entire *auxiliary sciences*.

Consider the case of the Copernican hypothesis, whose invention, defence, and partial vindication runs counter to almost every methodological rule one might care to think of today. The auxiliary sciences here contained laws describing the properties and the influence of the terrestrial atmosphere (meteorology); optical laws dealing with the structure of the eye and of telescopes, and with the behaviour of light; and dynamical laws describing motion in moving systems. Most importantly, however, the auxiliary sciences contained a theory of cognition that postulated a certain simple relation between perceptions and physical objects. Not all auxiliary disciplines were available in explicit form. Many of them merged with the observation language, and led to the situation described at the beginning of the preceding paragraph.

Consideration of all these circumstances, of observation terms, sensory core, auxiliary sciences, background speculation, suggest that a theory may be inconsistent with the evidence, not because it is incorrect, *but because the evidence is contaminated.* The theory is threatened because the evidence either contains unanalysed sensations which only partly correspond to external processes, or because it is presented in terms of antiquated views, or because it is evaluated with the help of backward auxiliary subjects. The Copernican theory was in trouble for *all* these reasons.

It is this *historico-physiological character of the evidence*, the fact that it does not merely describe some objective state of affairs *but also expresses subjective, mythical, and long-forgotten views* concerning this state of affairs, that forces us to take a fresh look at methodology. It shows that it would be extremely imprudent to let the evidence judge our theories directly and without any further ado. A straightforward and unqualified judgement of theories by 'facts' is bound to eliminate ideas *simply because they do not fit into the framework of some older cosmology*. Taking experimental results and observations for granted and putting the burden of proof on the theory means taking the observational ideology for granted without having ever examined it. (Note that the experimental results are supposed to have been obtained with the greatest possible care. Hence 'taking observations, etc., for granted' means 'taking them for granted *after* the most careful examination of their reliability': for even the most careful examination of an observation statement does not interfere with the concepts in which it is expressed, or with the structure of the sensory image.)

Now – how can we possibly examine something we use all the time and presuppose in every statement? How can we criticize the terms in which we habitually express our observations? Let us see!

The first step in our criticism of commonly-used concepts is to create a measure of criticism, something with which these concepts can be *compared.* Of course, we shall later want to know a little more about the measuring-stick itself; for example, we shall want to know whether it is better than, or perhaps not as good as, the material examined. But in order for *this* examination to start there must be a measuring-stick in the first place. Therefore, the first step in our criticism of customary concepts and customary reactions is to step outside the circle and either to invent a new conceptual system, for example a new theory, that clashes with the most carefully established observational results and confounds the most plausible theoretical principles, or to import such a system from outside science, from religion, from mythology, from the ideas of incompetents,[27] or the ramblings of madmen. This step is, again, counterinductive. Counterinduction is thus both a *fact* – science could not exist without it – and a legitimate and much needed *move* in the game of science.

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[27] It is interesting to see that Philolaos, who disregarded the evidence of the senses and set the earth in motion, was 'an unmathematical confusionist. It was the confusionist who found the courage lacking in many great observers and mathematically well-informed scientists to disregard the immediate evidence of the senses in order to remain in agreement with principles he firmly believed.' K. von Fritz, *Grundprobleme der Geschichte der antiken Wissenschaft*, Berlin–New York, 1971, p. 165. 'It is therefore not surprising that the next step on this path was due to a man whose writings, as far as we know them, show him as a talented stylist and popularizer with occasionally interesting ideas of his own rather than as a profound thinker or exact scientist,' ibid., p. 184. Confusionists and superficial intellectuals *move ahead* while the 'deep' thinkers *descend* into the darker regions of the status quo or, to express it in a different way, they remain stuck in the mud.

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As an example of such an attempt I examine the tower argument *which the Aristotelians used to refute the motion of the earth. The argument involves* natural interpretations *– ideas so closely connected with observations that it needs a special effort to realize their existence and to determine their content. Galileo identifies the natural interpretations which are inconsistent with Copernicus and replaces them by others.*

It seems to me that [Galileo] suffers greatly from continual digressions, and that he does not stop to explain all that is relevant at each point; which shows that he has not examined them in order, and that he has merely sought reasons for particular effects, without having considered first causes . . . ; and thus that he has built without a foundation.

DESCARTES

I am (indeed) unwilling to compress philosophical doctrines into the most narrow kind of space and to adopt that stiff, concise and graceless manner, that manner bare of any adornment which pure geometricians call their own, not uttering a single word that has not been given to them by strict necessity . . I do not regard it as a fault to talk about many diverse things, even in those treatises which have only a single topic . . . for I believe that what gives grandeur, nobility, and excellence to our deeds and inventions does not lie in what is necessary – though the absence of it would be a great mistake – but in what is not.

GALILEO

But where common sense believes that rationalizing sophists have the intention of shaking the very fundament of the commonweal, then it would seem to be not only reasonable, but permissible, and even praiseworthy to aid the good cause with sham reasons rather than leaving the advantage to the . . . opponent.

KANT[1]

As a concrete illustration and as a basis for further discussion, I shall now briefly describe the manner in which Galileo defused an important argument against the idea of the motion of the earth. I say 'defused', and not 'refuted', because we are dealing with a changing conceptual system as well as with certain attempts at concealment.

According to the argument which convinced Tycho, and which is used against the motion of the earth in Galileo's own *Trattato della sfera*, observation shows that 'heavy bodies . . . falling down from on high, go by a straight and vertical line to the surface of the earth. This is considered an irrefutable argument for the earth being motionless. For, if it made the diurnal rotation, a tower from whose top a rock was let fall, being carried by the whirling of the earth, would travel many hundreds of yards to the east in the time the rock would consume in its fall, and the rock ought to strike the earth that distance away from the base of the tower.'[2]

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[1] The three quotations are: Descartes, letter to Mersenne of 11 October 1638, *Oeuvres*, 11, p. 380. Galileo, letter to Leopold of Toscana of 1640, usually quoted under the title *Sul Candor Lunare*, *Opere*, Favoro, VIII, p. 491. For a detailed discussion of Galileo's style and its connection with his natural philosophy cf. L. Olschki, *Galileo und seine Zeit: Geschichte der neusprachlichen wissenschaftlichen Literatur*, Vol. III, Halle, 1927, reprinted Vaduz, 1965. The letter to Leopold is quoted and discussed on pp. 455ff.

Descartes' letter is discussed by Salmon as an example of the issue between rationalism and empiricism in 'The Foundations of Scientific Inference', *Mind and Cosmos*, ed. Colodny, Pittsburgh, 1966, p. 136. It should rather be regarded as an example of the issue between dogmatic methodologies and opportunistic methodologies, bearing in mind that empiricism can be as strict and unyielding as the most rigorous types of rationalism.

The Kant quotation is from the *Critique of Pure Reason*, B777, 8ff (the quotation was brought to my attention by Professor Stanley Rosen's work on Plato's *Symposium*). Kant continues: 'However, I would think that there is nothing that goes less well together with the intention of asserting a good cause than subterfuge, conceit, and deception. *If* one could take only this much for granted, then the battle of speculative reason . would have been concluded long ago, or would soon come to an end. Thus the purity of a cause often stands in the inverse proportion to its truth. . . .' One should also note that Kant explains the rise of *civilization* on the basis of disingenuous moves which 'have the function to raise mankind above its crude past', ibid., 776, 14f. Similar ideas occur in his account of world history.

[2] *Dialogue*, op. cit., p. 126.

In considering the argument, Galileo at once admits the correctness of the sensory content of the observation made, viz. that 'heavy bodies falling from a height, go perpendicularly to the surface of the earth'.[3] Considering an author (Chiaramonti) who sets out to convert Copernicus by repeatedly mentioning this fact, he says: 'I wish that this author would not put himself to such trouble trying to have us understand from our senses that this motion of falling bodies is simple straight motion and no other kind, nor get angry and complain because such a clear, obvious, and manifest thing should be called into question. For in this way he hints at believing that to those who say such motion is not straight at all, but rather circular, it seems they see the stone move visibly in an arc, since he calls upon their senses rather than their reason to clarify the effect. This is not the case, Simplicio; for just as I . . . have never seen nor ever expect to see, the rock fall any way but perpendicularly, just so do I believe that it appears to the eyes of everyone else. It is, therefore, better to put aside the appearance, on which we all agree, and to use the power of reason either to confirm its reality or to reveal its fallacy.'[4] The correctness of the observation is not in question. What is in question is its 'reality' or 'fallacy'. What is meant by this expression?

The question is answered by an example that occurs in Galileo's next paragraph, 'from which one may learn how easily anyone may be deceived by simple appearance, or let us say by the impressions of one's senses. This event is the appearance to those who travel along a street by night of being followed by the moon, with steps equal to theirs, when they see it go gliding along the eaves of the roofs. There it looks to them just as would a cat really running along the tiles and putting them behind it; an appearance which, if reason did not intervene, would only too obviously deceive the senses.'

In this example, we are asked to start with a sensory impression and to consider a statement that is forcefully suggested by it. (The suggestion is so strong that it has led to entire systems of belief and to rituals, as becomes clear from a closer study of the lunar aspects of witchcraft and of other cosmological hypotheses.) Now 'reason intervenes'; the statement suggested by the impression is examined, and one considers other statements in its place. The nature of the impression is not changed a bit by this activity. (This is only approximately true; but we can omit for our present purpose the complications arising from an interaction of impression and

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[3] ibid., p. 125.

[4] ibid., p. 256.

proposition.) But it enters new observation statements and plays new, better or worse, parts in our knowledge. What are the reasons and the methods which regulate such an exchange?

To start with, we must become clear about the nature of the total phenomenon: appearance plus statement. There are not two acts – one, noticing a phenomenon; the other, expressing it with the help of the appropriate statement – *but only one*, viz. saying in a certain observational situation, 'the moon is following me', or, 'the stone is falling straight down'. We may, of course, abstractly subdivide this process into parts, and we may also try to create a situation where statement and phenomenon seem to be psychologically apart and waiting to be related. (This is rather difficult to achieve and is perhaps entirely impossible.) But under normal circumstances such a division does not occur; describing a familiar situation is, for the speaker, an event in which statement and phenomenon are firmly glued together.

This unity is the result of a process of learning that starts in one's childhood. From our very early days we learn to react to situations with the appropriate responses, linguistic or otherwise. The teaching procedures both *shape* the 'appearance', or 'phenomenon', and establish a firm *connection* with words, so that finally the phenomena seem to speak for themselves without outside help or extraneous knowledge. They *are* what the associated statements assert them to be. The language they 'speak' is, of course, influenced by the beliefs of earlier generations which have been held for so long that they no longer appear as separate principles, but enter the terms of everyday discourse, and, after the prescribed training, seem to emerge from the things themselves.

At this point we may want to compare, in our imagination and quite abstractly, the results of the teaching of different languages incorporating different ideologies. We may even want consciously to change some of these ideologies and adapt them to more 'modern' points of view. It is very difficult to say how this will alter our situation, *unless* we make the further assumption that the quality and structure of sensations (perceptions), or at least the quality and structure of those sensations which enter the body of science, is independent of their linguistic expression. I am very doubtful about even the approximate validity of this assumption, which can be refuted by simple examples, and I am sure that we are depriving ourselves of new and surprising discoveries as long as we remain within the limits defined by it. Yet, I shall for the moment, remain within these limits.

Making the additional simplifying assumption, we can now distinguish between sensations and those 'mental operations which follow so closely upon the senses',[5] and which are so firmly connected with their reactions that a separation is difficult to achieve. Considering the origin and the effect of such operations, I shall call them *natural interpretations*.

In the history of thought, natural interpretations have been regarded either as *a priori presuppositions* of science, or else as *prejudices* which must be removed before any serious examination can begin. The first view is that of Kant, and, in a very different manner and on the basis of very different talents, that of some contemporary linguistic philosophers. The second view is due to Bacon (who had predecessors, however, such as the Greek sceptics).

Galileo is one of those rare thinkers who wants neither forever to *retain* natural interpretations nor altogether to *eliminate* them. Wholesale judgements of this kind are quite alien to his way of thinking. He insists upon a *critical discussion* to decide which natural interpretations can be kept and which must be replaced. This is not always clear from his writings. Quite the contrary. The methods of reminiscence, to which he appeals so freely, are designed to create the impression that nothing has changed and that we continue expressing our observations in old and familiar ways. Yet his attitude is relatively easy to ascertain: natural interpretations are *necessary*. The senses alone, without the help of reason, cannot give us a true account of nature. What is needed for arriving at such a true account are 'the ... senses, *accompanied by reasoning*'.[6] Moreover, in the arguments dealing with the motion of the earth, it is this reasoning, it is the connotation of the observation terms and *not* the message of the senses or the appearance that causes trouble. 'It is, therefore, better to put aside the appearance, on which we all agree, and to use the power of reason either to confirm its reality or to reveal its fallacy.'[7] Confirming the reality or revealing the fallacy of appearances means, however, examining the validity of those natural interpretations which are so intimately connected with the appearances that we no longer regard them as separate assumptions. I now turn to the first natural interpretation implicit in the argument from falling stones.

According to the Copernican view as presupposed in the tower argument the motion of a falling stone should be 'mixed straight-and-circular'.[8] By the 'motion of the stone' is meant not its motion relative to some visible mark in the visual field of the observer, or its

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[5] Francis Bacon, *Novum Organum*, Introduction.

[6] *Dialogue*, op. cit., p. 255, my italics.

[7] ibid., p. 256.

[8] ibid., p. 248.

observed motion, but rather its motion in the solar system or in (absolute) space, i.e. its *real motion*. The familiar facts appealed to in the argument present a different kind of motion, a simple vertical motion. This refutes the Copernican hypothesis only if the concept of motion that occurs in the observation statement is the same as the concept of motion that occurs in the Copernican prediction. The observation statement 'the stone is falling straight down' must, therefore, refer to a movement in (absolute) space. It must refer to a real motion.

Now, the force of an 'argument from observation' derives from the fact that the observation statements involved are firmly connected with appearances. There is no use appealing to observation if one does not know how to describe what one sees, or if one can offer one's description with hesitation only, as if one had just learned the language in which it is formulated. Producing an observation statement, then, consists of two very different psychological events: (1) a clear and unambiguous *sensation* and (2) a clear and unambiguous *connection* between this sensation and parts of a language. This is the way in which the sensation is made to speak. Do the sensations in the above argument speak the language of real motion?

They speak the language of real motion in the context of 17th-century everyday thought. At least, this is what Galileo tells us. He tells us that the everyday thinking of the time assumes the 'operative' character of *all* motion, or, to use well-known philosophical terms, it assumes *a naive realism with respect to motion*: except for occasional and unavoidable illusions, apparent motion is identical with real (absolute) motion. Of course, this distinction is not explicitly drawn. One does not first distinguish the apparent motion from the real motion and then connect the two by a correspondence rule. One rather describes, perceives, acts towards motion as if it were already the real thing. Nor does one proceed in this manner under all circumstances. It is admitted that objects may move which are not seen to move; and it is also admitted that certain motions are illusory (cf. the example of the moon mentioned earlier in this chapter). Apparent motion and real motion are not always identified. However, there are *paradigmatic cases* in which it is psychologically very difficult, if not plainly impossible, to admit deception. It is from these paradigmatic cases, and not from the exceptions, that naive realism with respect to motion derives its strength. These are also the situations in which we first learn our kinematic vocabulary. From our very childhood we learn to react to them with concepts which have naive realism built right into them, and which inextricably connect movement and the appearance of movement. The motion of the stone in the tower argument, or the alleged motion of the earth, is such a paradigmatic case. How could one possibly be unaware of the swift motion of a large bulk of matter such as the earth is supposed to be! How could one possibly be unaware of the fact that the falling stone traces a vastly extended trajectory through space! From the point of view of 17th-century thought and language, the argument is, therefore, impeccable and quite forceful. However, notice how *theories* ('operative character' of all motion; essential correctness of sense reports) which are not formulated explicitly, enter the debate in the guise of observable events. We realize again that such events are Trojan horses which must be watched most carefully. How is one supposed to proceed in such a sticky situation?

The argument from falling stones seems to refute the Copernican view. This may be due to an inherent disadvantage of Copernicanism; but it may also be due to the presence of natural interpretations which are in need of improvement. The first task, then, is to *discover* and to isolate these unexamined obstacles to progress.

It was Bacon's belief that natural interpretations could be discovered by a method of analysis that peels them off, one after another, until the sensory core of every observation is laid bare. This method has serious drawbacks. First, natural interpretations of the kind considered by Bacon are not just *added* to a previously existing field of sensations. They are instrumental in *constituting* the field, as Bacon says himself. Eliminate all natural interpretations, and you also eliminate the ability to think and to perceive. Second, disregarding this fundamental function of natural interpretations, it should be clear that a person who faces a perceptual field without a single natural interpretation at his disposal would be *completely disoriented*, he could not even *start* the business of science. The fact that we *do* start, even after some Baconian analysis, therefore shows that the analysis has stopped prematurely. It has stopped at precisely those natural interpretations of which we are not aware and without which we cannot proceed. It follows that the intention to start from scratch, after a complete removal of all natural interpretations, is self-defeating.

Furthermore, it is not possible even *partly* to unravel the cluster of natural interpretations. At first sight the task would seem to be simple enough. One takes observation statements, one after the other, and analyses their content. However, concepts that are hidden in observation statements are not likely to reveal themselves in the more abstract parts of language. If they do, it will still be difficult to nail them down; concepts, just like percepts, are ambiguous and dependent on background. Moreover, the content of a concept is determined also by the way in which it is related to perception. Yet, how can this way be discovered without circularity? Perceptions must be identified, and the identifying mechanism will contain some of the very same elements which govern the use of the concept to be investigated. We never penetrate this concept completely, for we always use part of it in the attempt to find its constituents. There is only one way to get out of this circle, and it consists in using an *external measure of comparison*, including new ways of relating concepts and percepts. Removed from the domain of natural discourse and from all those principles, habits, and attitudes which constitute its form of life, such an external measure will look strange indeed. This, however, is not an argument against its use. On the contrary, such an impression of strangeness reveals that natural interpretations are at work, and is a first step towards their discovery. Let us explain this situation with the help of the tower example.

The example is intended to show that the Copernican view is not in accordance with 'the facts'. Seen from the point of view of these 'facts', the idea of the motion of the earth is outlandish, absurd, and obviously false, to mention only some of the expressions which were frequently used at the time, and which are still heard whenever professional squares confront a new and counter-factual theory. This makes us suspect that the Copernican view is an external measuring rod of precisely the kind described above.

Let us therefore turn the argument around and use it as a *detecting device* that helps us to discover the natural interpretations which exclude the motion of the earth. Turning the argument around, we *first assert* the motion of the earth and *then inquire* what changes will remove the contradiction. Such an inquiry may take considerable time, and there is a good sense in which it is not finished even today. The contradiction may stay with us for decades or even centuries. Still, *it must be upheld* until we have finished our examination or else the examination, the attempt to discover the antediluvian components of our knowledge, cannot even start. This, we have seen, is one of the reasons one can give for *retaining*, and, perhaps, even for *inventing*, theories which are inconsistent with the facts. Ideological ingredients of our knowledge and, more especially, of our observations are discovered with the help of theories which are refuted by them. *They are discovered counterinductively.*

Let me repeat what has been asserted so far. Theories are tested, and possibly refuted, by facts. Facts contain ideological components, older views which have vanished from sight or were perhaps never formulated in an explicit manner. Such components are highly suspicious. First, because of their age and obscure origin: we do not know why and how they were introduced; secondly, because their very nature protects them, and always has protected them, from critical examination. In the event of a contradiction between a new and interesting theory and a collection of firmly established facts, the best procedure, therefore, is not to abandon the theory but to use it to discover the hidden principles responsible for the contradiction. Counterinduction is an essential part of such a process of discovery. (Excellent historical example: the arguments against motion and atomicity of Parmenides and Zeno. Diogenes of Sinope, the Cynic, took the simple course that would be taken by many contemporary scientists and all contemporary philosophers: he refuted the arguments by rising and walking up and down. The opposite course, recommended here, has led to much more interesting results, as is witnessed by the history of the case. One should not be too hard on Diogenes, however, for it is also reported that he beat up a pupil who was content with his refutation, exclaiming that he had given reasons which the pupil should not accept without additional reasons of his own.[9])

Having *discovered* a particular natural interpretation, how can we *examine* it and *test* it? Obviously, we cannot proceed in the usual way, i.e. derive predictions and compare them with 'results of observation'. These results are no longer available. The idea that the senses, employed under normal circumstances, produce correct reports of real events, for example reports of the real motion of physical bodies, has been removed from all observational statements. (Remember that this notion was found to be an essential part of the anti-Copernican argument.) But without it our sensory reactions cease to be relevant for tests. This conclusion was generalized by some older rationalists, who decided to build their science on reason only and ascribed to observation a quite insignificant auxiliary function. Galileo does not adopt this procedure.

If *one* natural interpretation causes trouble for an attractive view, and if its *elimination* removes the view from the domain of observation, then the only acceptable procedure is to use *other* interpretations and to see what happens. The interpretation which Galileo uses restores the senses to their position as instruments of exploration, *but only with respect to the reality of relative motion.* Motion 'among things which share it in common' is 'non-operative', that is, 'it remains insensible, imperceptible, and without any effect

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[9] Hegel, *Vorlesungen über die Geschichte der Philosophie*, I, ed. C.L. Michelet, Berlin, 1840, p. 289.

whatever'.[10] Galileo's first step, in his joint examination of the Copernican doctrine and of a familiar but hidden natural interpretation, consists therefore in *replacing the latter by a different interpretation.* In other words, *he introduces a new observation language.*

This is, of course, an entirely legitimate move. In general, the observation language which enters an argument has been in use for a long time and is quite familiar. Considering the structure of common idioms on the one hand, and of the Aristotelian philosophy on the other, neither this use nor this familiarity can be regarded as a test of the underlying principles. These principles, these natural interpretations, occur in every description. Extraordinary cases which might create difficulties are defused with the help of 'adjustor words',[11] such as 'like' or 'analogous', which divert them so that the basic ontology remains unchallenged. A test is, however, urgently needed. It is especially needed in those cases where the principles seem to threaten a new theory. It is then quite reasonable to introduce alternative observation languages and to compare them both with the original idiom and with the theory under examination. Proceeding in

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[10] *Dialogue*, op. cit., p. 171. Galileo's kinematic relativism is not consistent. In the passage quoted, he proposes the view (1) that shared motion has *no effect whatsoever.* 'Motion,' he says, 'in so far as it is and acts as motion, to that extent exists relatively to things that lack it; and among things which all share equally in any motion, it does not act and is as if it did not exist' (p. 116); 'Whatever motion comes to be attributed to the earth must necessarily remain imperceptible ... so long as we look only at terrestrial objects' (p. 114); '... motion that is common to many moving things is idle and inconsequential to the relation of those movables among themselves ...' (p. 116). On the other hand, (2) he also suggests that 'nothing ... *moves in a straight line by nature.* The motion of all celestial objects is in a circle; ships, coaches, horses, birds, all move in a circle around the earth; the motions of the parts of animals are all circular; in sum – we are forced to assume that only *gravia deorsum* and *levia sursum* move apparently in a straight line; but even that is not certain as long as it has not been proven that the earth is at rest' (p. 19). Now, if (2) is adopted, then the loose parts of systems moving in a straight line will tend to describe circular paths, thus contradicting (1). It is this inconsistency which has prompted me to split Galileo's argument into two steps, one dealing with the relativity of motion (only relative motion *is noticed*), the other dealing with inertial laws (and only inertial motion *leaves the relation between the parts of a system unaffected* – assuming, of course, that neighbouring inertial motions are approximately parallel). For the two steps of the argument, see the next chapter. One must also realize that accepting relativity of motion for inertial paths means giving up the *impetus theory*, which provides an (inner) cause for motions and therefore assumes an absolute space in which this cause becomes manifest. This Galileo seems to have done by now, for his argument for the existence of 'boundless' or 'perpetual' motions which he outlines on pp. 147ff of the *Dialogue* appeals to motions which are neutral, i.e. neither natural nor forced, and which may therefore (?) be assumed to go on for ever.

[11] J.L. Austin, *Sense and Sensibilia*, New York, 1964, p. 74. Adjustor words play an important role in the Aristotelian philosophy.

this way, we must make sure that the comparison is *fair*. That is, we must not criticize an idiom that is supposed to function as an observation language because it is not yet well known and is, therefore, less strongly connected with our sensory reactions and less plausible than is another, more 'common' idiom. Superficial criticisms of this kind, which have been elevated into an entire 'philosophy', abound in discussions of the mind–body problem. Philosophers who want to introduce and to test new views thus find themselves faced not with *arguments*, which they could most likely answer, but with an impenetrable stone wall of well-entrenched *reactions*. This is not at all different from the attitude of people ignorant of foreign languages, who feel that a certain colour is much better described by 'red' than by 'rosso'. As opposed to such attempts at conversion by appeal to familiarity ('I *know* what pains are, and I also *know*, from introspection, that they have nothing whatever to do with material processes!'), we must emphasize that a comparative judgement of observation languages, e.g. materialistic observation languages, phenomenalistic observation languages, objective-idealistic observation languages, theological observation languages, etc., can start only *when all of them are spoken equally fluently*.

Let us now continue with our analysis of Galileo's reasoning.

7

The new natural interpretations constitute a new and highly abstract observation language. They are introduced and concealed *so that one fails to notice the change that has taken place (method of anamnesis). They contain the idea of the* relativity of all motion *and the* law of circular inertia.

Galileo replaces one natural interpretation by a very different and as yet (1630) at least partly unnatural interpretation. How does he proceed? How does he manage to introduce absurd and counter-inductive assertions such as the assertion that the earth moves, and yet get them a just and attentive hearing? One anticipates that arguments will not suffice – an interesting and highly important limitation of rationalism – and Galileo's utterances are indeed arguments in appearance only. For Galileo uses *propaganda.* He uses *psychological tricks* in addition to whatever intellectual reasons he has to offer. These tricks are very successful: they lead him to victory. But they obscure the new attitude towards experience that is in the making, and postpone for centuries the possibility of a reasonable philosophy. They obscure the fact that the experience on which Galileo wants to base the Copernican view is nothing but the result of his own fertile imagination, that it has been *invented.* They obscure this fact by insinuating that the new results which emerge are known and conceded by all, and need only be called to our attention to appear as the most obvious expression of the truth.

Galileo 'reminds' us that there are situations in which the non-operative character of shared motion is just as evident and as firmly believed as the idea of the operative character of all motion is in other circumstances. (This latter idea is, therefore, not the only natural interpretation of motion.) The situations are: events in a boat, in a smoothly moving carriage, and in other systems that contain an observer and permit him to carry out some simple operations.

> *Sagredo:* There has just occurred to me a certain fantasy which passed through my imagination one day while I was sailing to Aleppo,

where I was going as consul for our country.... If the point of a pen had been on the ship during my whole voyage from Venice to Alexandretta and had had the property of leaving visible marks of its whole trip, what trace – what mark – what line would it have left?

Simplicio: It would have left a line extending from Venice to there; not perfectly straight – or rather, not lying in the perfect arc of a circle – but more or less fluctuating according as the vessel would now and again have rocked. But this bending in some places a yard or two to the right or left, up or down, in length of many hundreds of miles, would have made little alteration in the whole extent of the line. These would scarcely be sensible, and, without an error of any moment, it could be called part of a perfect arc.

Sagredo: So that if the fluctuation of the waves were taken away and the motion of the vessel were calm and tranquil, the true and precise motion of that pen would have been an arc of a perfect circle. Now if I had had that same pen continually in my hand, and had moved it only a little sometimes this way or that, what alterations should I have brought into the main extent of this line?

Simplicio: Less than that which would be given to a straight line a thousand yards long which deviated from absolute straightness here and there by a flea's eye.

Sagredo: Then if an artist had begun drawing with that pen on a sheet of paper when he left the port and had continued doing so all the way to Alexandretta, he would have been able to derive from the pen's motion a whole narrative of many figures, completely traced and sketched in thousands of directions, with landscapes, buildings, animals, and other things. Yet the actual real essential movement marked by the pen point would have been only a line; long, indeed, but very simple. But as to the artist's own actions, these would have been conducted exactly the same as if the ship had been standing still. The reason that of the pen's long motion no trace would remain except the marks drawn upon the paper is that the gross motion from Venice to Alexandretta was common to the paper, the pen, and everything else in the ship. But the small motions back and forth, to right and left, communicated by the artist's fingers to the pen but not to the paper, and belonging to the former alone, could thereby leave a trace on the paper which remained stationary to those motions.[1]

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[1] *Dialogue*, op. cit., pp. 171ff.

Or

> *Salviati:* Imagine yourself in a boat with your eyes fixed on a point of the sail yard. Do you think that because the boat is moving along briskly, you will have to move your eyes in order to keep your vision always on that point of the sail and follow its motion?
>
> *Simplicio:* I am sure that I should not need to make any change at all; not just as to my vision, but if I had aimed a musket I should never have to move it a hairsbreadth to keep it aimed, no matter how the boat moved.
>
> *Salviati:* And this comes about because the motion which the ship confers upon the sail yard, it confers also upon you and upon your eyes, so that you need not move them a bit in order to gaze at the top of the sail yard, which consequently appears motionless to you. (And the rays of vision go from the eye to the sail yard just as if a cord were tied between the two ends of the boat. Now a hundred cords are tied at different fixed points, each of which keeps its place whether the ship moves or remains still.)[2]

It is clear that these situations lead to a non-operative concept of motion even within common sense.

On the other hand, common sense, and I mean 17th-century Italian-artisan common sense, also contains the idea of the *operative* character of all motion. This latter idea arises when a limited object that does not contain too many parts moves in vast and stable surroundings; for example, when a camel trots through the desert, or when a stone descends from a tower.

Now Galileo urges us to 'remember' the conditions in which we assert the non-operative character of shared motion in this case also, and to subsume the second case under the first.

Thus, the first of the two paradigms of non-operative motion mentioned above is followed by the assertion that – 'It is likewise true that the earth being moved, the motion of the stone in descending is actually a long stretch of many hundred yards, or even many thousand; and had it been able to mark its course in motionless air or upon some other surface, it would have left a very long slanting line. But that part of all this motion which is common to the rock, the

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[2] ibid., pp. 249ff. That phenomena of *seen* motion depend on *relative* motion has been asserted by Euclid in his *Optics*, Theon red. par. 49ff. An old scholion of par. 50 uses the example of a boat leaving the harbour: Heiberg, vii, 283. The example is repeated by Copernicus in Book 1, Chapter viii, of *De Revol*. It was a commonplace in mediaeval optics. Cf. Witelo, *Perspectiva*, iv par 138 (Basel, 1572, p. 180).

tower, and ourselves remains insensible and as if it did not exist. There remains observable only that part in which neither the tower nor we are participants; in a word, that with which the stone, in falling, measures the tower.'[3]

And the second paradigm precedes the exhortation to 'transfer this argument to the whirling of the earth and to the rock placed on top of the tower, whose motion you cannot discern because, in common with the rock, you possess from the earth that motion which is required for following the tower; you do not need to move your eyes. Next, if you add to the rock a downward motion which is peculiar to it and not shared by you, and which is mixed with this circular motion, the circular portion of the motion which is common to the stone and the eye continues to be imperceptible. The straight motion alone is sensible, for to follow that you must move your eyes downwards.'[4]

This is strong persuasion indeed.

Yielding to this persuasion, we now *quite automatically* start confounding the conditions of the two cases and become relativists. This is the essence of Galileo's trickery! As a result, the clash between Copernicus and 'the conditions affecting ourselves and those in the air above us'[5] dissolves into thin air, and we finally realize 'that all terrestrial events from which it is ordinarily held that the earth stands still and the sun and the fixed stars are moving would necessarily appear just the same to us if the earth moved and the other stood still'.[6]

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[3] ibid., pp. 172ff.

[4] ibid., p. 250.

[5] Ptolemy, *Syntaxis*, i, 1, p. 7.

[6] *Dialogue*, op. cit., p. 416: cf. the *Dialogues Concerning Two New Sciences*, transl. Henry Crew and Alfonso de Salvio, New York, 1958, p. 164: 'The same experiment which at first glance seemed to show one thing, when more carefully examined, assures us of the contrary.' Professor McMullin, in a critique of this way of seeing things, wants more 'logical and biographical justification' for my assertion that Galileo not only argued, but also cheated ['A Taxonomy of the Relation between History and Philosophy of Science', *Minnesota Studies*, Vol. 5, Minneapolis, 1971, p. 39], and he objects to the way in which I let Galileo introduce dynamical relativism. According to him 'what Galileo argues is that since his opponent *already* interprets observations made in such a context [movements on boats] in a "relativistic" way, how can he consistently do otherwise in the case of observations made on the earth's surface?' (ibid., p. 40). This is indeed how Galileo argues. But he argues so against an opponent who, according to him, 'feels a great repugnance towards recognizing this non-operative quality of motion among the things which share it in common' (*Dialogue*, op. cit., p. 171), who is convinced that a boat, apart from having relative motions, *has absolute positions and motions as well* (cf. Aristotle, Physics, 208b8ff), and who at any rate has developed the art of using different notions on different occasions without running into a contradiction. Now if *this* is the position to be attacked, then showing that an

Let us now look at the situation from a more abstract point of view. We start with two conceptual sub-systems of 'ordinary' thought (see the following table). One of them regards motion as an absolute process which always has effects, effects on our senses included. The description of this conceptual system given here may be somewhat idealized; but the arguments of Copernicus' opponents, which are quoted by Galileo himself and, according to him, are 'very plausible',[7] show that there was a widespread tendency to think in its terms, and that this tendency was a serious obstacle to the discussion of alternative ideas. Occasionally, one finds even more primitive ways of thinking, where concepts such as 'up' and 'down' are used absolutely. Examples are: the assertion 'that the earth is too heavy to climb up over the sun and then fall headlong back down again',[8] or the assertion that 'after a short time the mountains, sinking downward with the rotation of the terrestrial globe, would get into such a position that whereas a little earlier one would have had to climb steeply to their peaks, a few hours later one would have to stoop and descend in order to get there'.[9] Galileo, in his marginal notes, calls these 'utterly childish reasons [which] sufficed to keep imbeciles believing in the fixity of the earth'[10] and he thinks it unnecessary 'to bother about such men as those, *whose name is legion*, or to take notice of their fooleries'.[11] Yet it is clear that the absolute idea of motion was 'well-entrenched', and that the attempt to replace it was bound to encounter strong resistance.[12]

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opponent has a relative idea of motion, or frequently uses the relative idea in his everyday affairs, is not at all 'proof of inconsistency in his own "paradigm"' (McMullin, op. cit., p. 40). It just reveals one part of that paradigm without touching the other. The argument turns into the desired proof only if the absolute notion is either suppressed or spirited away, or else identified with the relativistic notion – and this is what Galileo actually does, though surreptitiously, as I have tried to show.

[7] *Dialogue*, op. cit., p. 328.

[8] ibid., p. 327.

[9] ibid., p. 330.

[10] ibid., p. 327.

[11] ibid., p. 327, italics added.

[12] The idea that there is an absolute direction in the universe has a very interesting history. It rests on the structure of the gravitational field on the surface of the earth, or of that part of the earth which the observer knows, and generalizes the experiences made there. The generalization is only rarely regarded as a separate hypothesis, it rather enters the 'grammar' of common sense and gives the terms 'up' and 'down' an absolute meaning. (This is a 'natural interpretation', in precisely the sense that was explained in the text above.) Lactantius, a Church father of the fourth century, appeals to this meaning when he asks (*Divinae Institutiones*, 111, De Falsa Sapientia): 'Is one really going to be so confused as to assume the existence of humans whose feet are above their heads? Where trees and fruit grow not upwards, but

The second conceptual system is built around the relativity of motion, and is also well-entrenched in its own domain of application. Galileo aims at replacing the first system by the second in *all* cases, terrestrial as well as celestial. Naive realism with respect to motion is to be *completely eliminated.*

Now, we have seen that this naive realism is on occasions an essential part of our observational vocabulary. On these occasions (Paradigm I), the observation language contains the idea of the efficacy of *all* motion. Or, to express it in the material mode of speech, our experience in these situations is an experience of objects

Paradigm I: Motion of compact objects in stable surroundings of great spatial extension - deer observed by the hunter.		*Paradigm II:* Motion of objects in boats, coaches and other moving systems.	
Natural interpretation: All motion is operative.		*Natural interpretation:* Only relative motion is operative.	
Falling stone *proves* ↓ Earth at rest	Motion of earth *predicts* ↓ Oblique motion of stone	Falling stone *proves* ↓ No *relative* motion between starting point and earth	Motion of earth *predicts* ↓ No relative motion between starting point and stone

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downwards?' The same use of language is presupposed by that 'mass of untutored men' who raise the question why the antipodeans are not falling off the earth (Pliny, *Natural History*, II, pp. 161–6, cf. also Ptolemy, *Syntaxis*, I, 7). The attempts of Thales, Anaximenes and Xenophanes to find support for the earth which prevents it from falling 'down' (Aristotle, *De Coelo*, 294a12ff) shows that almost all early philosophers, with the sole exception of Anaximander, shared in this way of thinking. (For the Atomists, who assume that the atoms originally fall 'down,' cf. Jammer, *Concepts of Space*, Cambridge, Mass., 1953, p. 11.) Even Galileo, who thoroughly ridicules the idea of the falling antipodes (*Dialogue*, op. cit., p. 331), occasionally speaks of the 'upper half of the moon', meaning that part of the moon 'which is invisible to us'. And let us not forget that some linguistic philosophers of today 'who are too stupid to recognize their own limitations' (Galileo, op. cit., p. 327) want to revive the absolute meaning of 'up-down' at least *locally*. Thus the power over the minds of his contemporaries of a primitive conceptual frame, assuming an anisotropic world, which Galileo had also to fight, must not be underestimated. For an examination of some aspects of British common sense at the time of Galileo, including astronomical common sense, see E.M.W. Tillyard, *The Elizabethan World Picture*, London, 1963. The agreement between popular opinion and the centrally symmetric universe is frequently asserted by Aristotle, e.g. in *De Coelo*, p. 308a23f.

which move absolutely. Taking this into consideration, it is apparent that Galileo's proposal amounts to a partial revision of our observation language or of our experience. An experience which partly *contradicts* the idea of the motion of the earth is turned into an experience that *confirms* it, at least as far as 'terrestrial things' are concerned.[13] This is what *actually happens*. But Galileo wants to persuade us that no change has taken place, that the second conceptual system is already universally *known*, even though it is not universally *used*. Salviati, his representative in the Dialogue, his opponent Simplicio and Sagredo the intelligent layman all connect Galileo's method of argumentation with Plato's theory of *anamnesis* – a clever tactical move, typically Galilean one is inclined to say. Yet we must not allow ourselves to be deceived about the revolutionary development that is actually taking place.

The resistance against the assumption that shared motion is non-operative was equated with the resistance which forgotten ideas exhibit towards the attempt to make them known. Let us accept this *interpretation* of the resistance! But let us not forget its *existence*. We must then admit that it restricts the use of the relativistic ideas, confining them to *part* of our everyday experience. *Outside* this part, i.e. in interplanetary space, they are 'forgotten' and therefore not active. But outside this part there is not complete chaos. Other concepts are used, among them whose very same absolutistic concepts which derive from the first paradigm. We not only use them, we must also admit that they are entirely adequate. No difficulties arise as long as one remains within the limits of the first paradigm. 'Experience', i.e. the totality of all facts from all domains, cannot force us to carry out the change which Galileo wants to introduce. The motive for a change must come from a different source.

It comes, first, from the desire to see 'the whole [correspond] to its parts with wonderful simplicity',[14] as Copernicus had already

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[13] *Dialogue*, op. cit., pp. 132 and 416.

[14] ibid., p. 341. Galileo quotes here from Copernicus' address to Pope Paul III in *De Revolutionibus*; cf. also Chapter 10 and the *Narratio Prima* (quoted from E. Rosen, *Three Copernican Treatises*, New York, 1959, p. 165): 'For all these phenomena appear to be linked most nobly together, as by a golden chain; and each of the planets, by its position, and order, and every inequality of its motion, bears witness that the earth moves and that we who dwell upon the globe of the earth, instead of accepting its changes of position, believe that the planets wander in all sorts of motions of their own.' Note that empirical reasons are absent from the argument and have to be, for Copernicus himself admits (*Commentariolus*, op. cit., p. 57) that the Ptolemaic theory is 'consistent with the numerical data'.

expressed himself. It comes from the 'typically metaphysical urge' for unity of understanding and conceptual presentation. And the motive for a change is connected, secondly, with the intention to make room from the motion of the earth, which Galileo accepts and is not prepared to give up. The idea of the motion of the earth is closer to the first paradigm than to the second, or at least it was at the time of Galileo. This gave strength to the Aristotelian arguments, and made them plausible. To eliminate the plausibility, it was necessary to subsume the first paradigm under the second, and to extend the relative notions to all phenomena. The idea of *anamnesis* functions here as a psychological crutch, as a lever which smooths the process of subsumption by concealing its existence. As a result we are now ready to apply the relative notions not only to boats, coaches, birds, but to the 'solid and well-established earth' as a whole. And we have the impression that this readiness was in us all the time, although it took some effort to make it conscious. This impression is most certainly erroneous: it is the result of Galileo's propagandistic machinations. We would do better to describe the situation in a different way, as a change of our conceptual system. Or, because we are dealing with concepts which belong to natural interpretations, and which are therefore connected with sensations in a very direct way, we should describe it as a *change of experience* that allows us to accommodate the Copernican doctrine. It is this change which underlies the transition from the Aristotelian point of view to the epistemology of modern science.

For experience now ceases to be the unchangeable fundament which it is both in common sense and in the Aristotelian philosophy. The attempt to support Copernicus makes experience 'fluid' in the very same manner in which it makes the heavens fluid, 'so that each star roves around in it by itself'.[15] An empiricist who starts from experience, and builds on it without ever looking back, now loses the very ground on which he stands. Neither the earth, 'the solid, well-established earth', nor the facts on which he usually relies can be trusted any longer. It is clear that a philosophy that uses such a fluid and changing experience needs new methodological principles which do not insist on an asymmetric judgement of theories by experience. *Classical physics* intuitively adopts such principles; at least its great and independent thinkers, such as Newton, Faraday, Boltzmann proceed in this way. But its *official doctrine* still clings to the idea of a stable and unchanging basis. The clash between this

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[15] *Dialogue*, op. cit., p. 120.

doctrine and the actual procedure is concealed by a tendentious presentation of the *results* of research that hides their revolutionary origin and suggests that they arose from a stable and unchanging source. These methods of concealment start with Galileo's attempt to introduce new ideas under the cover of anamnesis, and they culminate in Newton.[16] They must be exposed if we want to arrive at a better account of the progressive elements in science.

My discussion of the anti-Copernican argument is not yet complete. So far, I have tried to discover what assumption will make a stone *that moves alongside a moving tower* appear to fall 'straight down', instead of being seen to move in an arc. The assumption, which I shall call the *relativity principle*, that our senses notice only relative motion and are insensitive to a motion which objects have in common was seen to do the trick. What remains to be explained is *why the stone stays with the tower* and is not left behind. In order to save the Copernican view, one must explain not only why a motion that preserves the relation among visible objects *remains unnoticed*, but also, why a common motion of various objects does not affect their relation. That is, one must explain why such a motion is not a causal agent. Turning the question around in the manner explained in the text to footnote 10, page 63 of the last chapter, it is now apparent that the anti-Copernican argument described there rests on *two* natural interpretations: viz, the *epistemological assumption* that absolute motion is always *noticed*, and the *dynamical principle* that objects (such as the falling stone) which are not interfered with assume their natural motion. For Aristotelians the natural motion of an object not interfered with is *rest*, i.e. constancy of qualities and of position.[17] This corresponds to our own experience where things have to be pushed around to move. The discovery of seeds, bacteria, viruses would have been impossible without a firm belief in the qualitative part of the law – and it confirmed it in a most impressive way. Using this law scientists inferred that a stone dropped from a tower situated on a moving earth would be left behind. Thus the relativity principle must be combined with a new law of inertia in such a fashion that the motion of the earth can still be asserted. One sees at once that the following law, the *principle of circular inertia* as I shall call it, provides the required solution: an object that moves with a given angular velocity on a frictionless sphere around the centre of the earth will continue moving with the same angular velocity for ever.

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[16] 'Classical Empiricism', op. cit.

[17] This is the *general* account of motion. In the *cosmological* account we have circular motion above and up-and-down motions on earth.

Combining the appearance of the falling stone with the relativity principle, the principle of circular inertia and some simple assumptions concerning the composition of velocities,[18] we obtain an argument which no longer endangers Copernicus' view, but can be used to give it partial support.

The relativity principle was defended in two ways. The first was by showing how it helps Copernicus: this defence is *ad hoc* but not objectionable, because necessary for revealing natural interpretations. The second was by pointing to its function in common sense, and by surreptitiously generalizing that function (see above). No independent argument was given for its validity. Galileo's support for the principle of circular inertia is of exactly the same kind. He introduces the principle, again not by reference to experiment or to independent observation, but by reference to what everyone is already supposed to know.

> *Simplicio:* So you have not made a hundred tests, or even one? And yet you so freely declare it to be certain?
>
> *Salviati:* Without experiment, I am sure that the effect will happen as I tell you, because it must happen that way; and I might add that you yourself also know that it cannot happen otherwise, no matter how you may pretend not to know it.... But I am so handy at picking people's brains that I shall make you confess this in spite of yourself.[19]

Step by step, Simplicio is forced to admit that a body that moves, without friction, on a sphere concentric with the centre of the earth

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[18] These assumptions were not at all a matter of course, but conflicted with some very basic ideas of Aristotelian physics. The principle of circular inertia is related to the impetus theory, but not identical with it. The impetus theory retains the idea that it needs a force to bring about change, but it puts the force inside the changing object. Once pushed, an object continues moving in the same way in which a heated object stays warm – both contain the cause of their new state. Galileo modifies this idea in two ways. First, the circular motion is supposed to go on forever while an object kept moving by impetus will gradually slow down, just as a heated object, its analogue, gradually becomes colder. The argument for this modification is given in the text below; it is purely rhetorical. Secondly, the eternal circular motions must proceed without a cause: if relative motions are not operative, then introducing a motion with the same centre and the same angular velocity as a circular motion upheld by impetus cannot eliminate forces: we are on the way from impetus to momentum (cf. A. Maier, *Die Vorläufer Galileis im 14. Jahrhundert*, Rome, 1949). All these changes are overlooked by those who assume that the transition was the simple result of a new and better dynamics and that the dynamics was already available, but had not yet been applied in a determined way.

[19] *Dialogue*, op. cit., p. 145.

will carry out a 'boundless', a 'perpetual' motion. We know, of course, especially after the analysis we have just completed of the non-operative character of shared motion, that what Simplicio accepts is based neither on experiment nor on corroborated theory. It is a daring new suggestion involving a tremendous leap of the imagination.[20] A little more analysis then shows that this suggestion is connected with experiments, such as the 'experiments' of the *Discorsi*[21] by *ad hoc* hypotheses. (The amount of friction to be eliminated follows not from independent investigations – such investigations commence only much later, in the 18th century – but from the result to be achieved, viz. the circular law of inertia.) Viewing natural phenomena in this way leads to a re-evaluation of all experience, as we have seen. We can now add that it leads to the invention of a *new kind of experience* that is not only more sophisticated *but also far more speculative than* the experience of Aristotle or of

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[20] For a Copernican the only leap involved was the identification of the earth as a celestial object. According to Aristotle celestial objects move in circles and 'a body that moves in a circle has neither heaviness nor lightness for it cannot change its distance from the centre, neither in a natural nor in a forced way'. *De Coelo*, 269b34f.

[21] Incidentally, many of the 'experiences' or 'experiments' used in the arguments about the motion of the earth are entirely fictitious. Thus Galileo, in his *Trattato della Sfera* (*Opere*, Vol. II, pp. 21 1ff), which 'follows the opinion of Aristotle and of Ptolemy' (p. 223), uses this argument against a rotation of the earth: '... objects which one lets fall from high places to the ground such as a stone from the top of a tower would not fall towards the foot of that tower; for during the time which the stone coming rectilinearly towards the ground, spends in the air, the earth, escaping it, and moving towards the east would receive it in a part far removed from the foot of the tower *in exactly the same manner in which a stone that is dropped from the mast of a rapidly moving ship will not fall towards its foot, but more towards the stern*' (p. 224). The italicized reference to the behaviour of stones on ships is again used in the *Dialogue* (p. 126), when the Ptolemaic arguments are discussed, but it is no longer accepted as correct. 'It seems to be an appropriate time,' says Salviati (ibid., p. 180), 'to take notice of a certain generosity on the part of the Copernicans towards their adversaries when, with perhaps too much liberality, they concede as true and correct a number of experiments which their opponents have never made. Such for example is that of the body falling from the mast of a ship while it is in motion....' Earlier, p. 154, it is implied rather than observed, that the stone will fall to the foot of the mast, even if the ship should be in motion while a possible experiment is discussed on p. 186. Bruno (*La Cena de le Ceneri*, *Opere Italiane*, I, ed. Giovanni Gentile, Bari, 1907, p. 83) takes it for granted that the stone will arrive at the foot of the mast. It should be noted that the problem did not readily lend itself to an experimental solution. Experiments were made, but their results were far from conclusive. Cf. A. Armitage, 'The Deviation of Falling Bodies', *Annals of Science*, 5, 1941-7, pp. 342ff, and A. Koyré, *Metaphysics and Measurement*, Cambridge, 1968, pp. 89ff. The tower argument can be found in Aristotle, *De Coelo*, 296b22, and Ptolemy, *Syntaxis*, i, 8. Copernicus discusses it in the same chapter of *De Revol*, but tries to defuse it in the next chapter. Its role in the Middle Ages is described in M. Clagett, *The Science of Mechanics in the Middle Ages*, Madison, 1959, Chapter 10.

common sense. Speaking paradoxically, but not incorrectly, one may say that *Galileo invents an experience that has metaphysical ingredients.* It is by means of such an experience that the transition from a geostatic cosmology to the point of view of Copernicus and Kepler is achieved.[22]

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[22] Alan Chalmers, in an interesting and well-argued paper ('The Galileo That Feyerabend Missed: An Improved Case Against Method' in J.A. Schuster and R.R. Yeo (eds), *The Politics and Rhetoric of Scientific Method*, Dordrecht, 1986, pp. 1ff), distinguishes 'between Galileo's contributions to a new science, on the one hand, and the question of the social conditions in which that science is developed and practised, on the other', admits that 'propaganda' (though much less than I suggest) may have been part of his attempt to change the latter, but emphasizes that it does not affect the former. 'The main source for Galileo's contribution to science itself', says Chalmers, 'is his *Two New Sciences*' This is the work I should have studied to explore Galileo's procedure. But the *Two New Sciences* do not deal with the topic I was discussing, viz. the transition to Copernicus. *Here* Galileo used procedures rather different from those of his later work. Lynn Thorndike, who shares Chalmers' evaluation of the *Dialogue*, wished that Galileo had written a systematic textbook on that subject (*A History of Magic and Experimental Science*, Vol. 6, New York, 1941, pp. 7 and 62: 'Galileo might have done better to write a systematic textbook than his provocative dialogues'). Now for such a textbook to have substance it would have to be as general as its Aristotelian rival and it would have to show how and why Aristotelian concepts needed to be replaced at the most elementary level. Aristotelian concepts, though abstract, were closely related to common sense. Hence it was necessary to replace some common notions by others (I am now speaking about what Chalmers calls 'perceptual relativity' – p. 7). Two questions arise: how big were the changes? and was propaganda (rhetoric, were 'irrational moves') needed to carry them out? My answer to the latter question is that discourse attempting to bring about major conceptual changes is a normal part of science, common sense, and cultural exchange (for the latter cf. Chapter 16 and Chapter 17, item vi, 'open exchange'), and that it differs from the discourse carried out *within* a more or less stable framework. Personally, I am quite prepared to make it part of rationality. But there exist philosophical schools that oppose it or call it incoherent (cf. Chapter 10 of *Farewell to Reason* which discusses some of Hilary Putnam's views). *Using the terminology of these schools* I speak of Galileo's 'trickery', etc. And I add that science contains ingredients that occasionally need such 'trickery' to become acceptable. The difference between the *Sciences* and the *Dialogue*, therefore, is not between science and sociology but between technical changes in a narrow field and basic changes, realistically interpreted. My answer to the first question is that perceptual relativity, though acknowledged by many scholars (and by Aristotle himself), was not a common possession (Galileo points out that even some of his fellow scientists stumbled at this point) and thus had to be argued for. This is not at all surprising, as my discussion of qualitative difficulties in Chapter 5 shows. Besides, is it really true that a traveller on a boat sees the harbour as receding as if it were removed by some strange force? I conclude that Galileo's 'trickery' was necessary for a proper understanding of the new cosmology, that it is 'trickery' only for philosophies that set narrow conditions on conceptual change and that it should be extended to areas still restricted by such conditions (in Chapter 12 I argue that the mind-body problem is one such area).