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The Church at the time of Galileo not only kept closer to reason as defined then and, in part, even now; it also considered the ethical and social consequences of Galileo's views. Its indictment of Galileo was rational and only opportunism and a lack of perspective can demand a revision.

There were many trials in the 17th century. The proceedings started either with accusations made by private parties, with an official act by a public officer, or with an inquiry based on sometimes rather vague suspicions. Depending on the location, the distribution of jurisdiction and the balance of power at a particular time, crimes might be examined by secular courts such as the courts of kings or of free cities, by Church courts, such as the spiritual courts attached to every episcopate, or by the special courts of the Inquisition. After the middle of the 12th century the episcopal courts were greatly aided by the study of Roman law. Lawyers became so influential that, even if wholly untrained in canon law and theology, they had a much better chance of high preferment than a theologian.[1] The inquisitorial process removed safeguards provided by Roman law and led to some well-publicized excesses. What has not been publicized to the same extent is that the excesses of royal or secular courts often matched those of the Inquisition. It was a harsh and cruel age.[2] By 1600 the

1. For this complaint (made by Roger Bacon) cf. H. Ch. Lea, *A History of the Inquisition of the Middle Ages*, Vol. I, p. 309. Chapters ixff explain the details of the inquisitorial procedure, the ways in which they differed from other procedures and the reasons for the difference. Cf. also G.G. Coulton, *Inquisition and Liberty*, Boston, 1959, Chapters XI–XV.

2. Charles Henry Lea, the great liberal historian, writes: 'On the whole we may conclude that the secret prisons of the Inquisition were less intolerable places of abode than the episcopal and public gaols. The general policy respecting them was more humane and enlightened than that of other jurisdictions, whether in Spain or elsewhere, although negligent supervision allowed of abuses and there were ample resources of rigor in reserve, when the obstinacy of the impenitent was to be broken down.' *History of the Inquisition in Spain*, Vol. 2, New York, 1906, p. 534. Prisoners accused before secular courts occasionally committed crimes under the jurisdiction of

Inquisition had lost much of its power and aggressiveness. This was true especially in Italy and more particularly in Venice.[3]

The courts of the Inquisition also examined and punished crimes concerning the production and the use of knowledge. This can be explained by their origin: they were supposed to exterminate *heresy*, i.e. complexes consisting of actions, assumptions and talk making people inclined towards certain beliefs. The surprised reader who asks what knowledge has to do with the law should remember the many legal, social and financial obstacles knowledge-claims face today. Galileo wanted his ideas to replace the existing cosmology, but he was forbidden to work towards that aim. Today the much more modest wish of creationists to have their views taught in schools side by side with other and competing views runs into laws setting up a separation of Church and State.[4] Increasing amounts of theoretical and engineering information are kept secret for military reasons and are thereby cut off from international exchange.[5] Commercial interests have the same restrictive tendency. Thus the discovery of superconductivity in ceramics at (relatively) high temperatures, which was the result of international collaboration, soon led to protective measures by the American government.[6] Financial arrangements can make or break a research programme and an entire profession. There are many ways to silence people apart from forbidding them to speak – and all of them are being used today. The process of knowledge production and knowledge distribution was

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the Church so that they might be handed over to the Inquisition: Henry Kamen, *Die Spanische Inquisition*, Munich, 1980, p. 17.

3. In 1356 the secular officials of Venice forbade the Inquisitor of Treviso to try his prisoners, seized his informants and tortured them on the charge of pilfering the property of the accused. Lea, *Inquisition in the Middle Ages*, Vol. ii, p. 273.

4. A comprehensive report of one of the trials that resulted from the conflict has been published in *Science*, Vol. 215, 1982, pp. 934ff. Many other trials followed.

5. It seems that the need for secrecy in nuclear matters was first raised by the scientists themselves. Cf. the report and the documents in Spencer R. Weart and Gertrude Weiss-Szilard (eds), *Leo Szilard, His Version of the Facts*, Cambridge, Mass., 1978, esp. Chapters 2ff. Cf. also the material on the Oppenheimer case. The inventor of the telescope was forced to secrecy as the military importance of the contrivance was soon realized. Cf. Chapter 8, footnote 24.

Research teams become very secretive when approaching what they think is a Big Discovery. After all, what is at stake are patents, consultancies in industry, money and, perhaps, the honour of a Nobel Prize. For a special case cf. R.M. Haze, *Superconductors*, London, 1988. The manipulation of knowledge by the courts is discussed, with many examples, by Peter W. Huber, *Galileo's Revenge*, New York, 1991.

6. *Science*, Vol. 237, 1987, pp. 476ff and 593f. An important step towards exclusiveness consisted in assigning part of the research to the military.

never the free, 'objective', and purely intellectual exchange rationalists make it out to be.

The trial of Galileo was one of many trials. It had no special features except perhaps that Galileo was treated rather mildly, despite his lies and attempts at deception.[7] But a small clique of intellectuals aided by scandal-hungry writers succeeded in blowing it up to enormous dimensions so that what was basically an altercation between an expert and an institution defending a wider view of things now looks almost like a battle between heaven and hell. This is childish and also very unfair towards the many other victims of 17th-century justice. It is especially unfair towards Giordano Bruno, who was burned but whom scientifically minded intellectuals prefer to forget. It is not a concern for humanity but rather party interests which play a major role in the Galileo hagiography. Let us therefore take a closer look at the matter.[8]

The so-called trial of Galileo consisted of two separate proceedings, or trials. The first occurred in 1616. The Copernican doctrine was examined and criticized. Galileo received an order, but he was not punished. The second trial took place in 1632/33. Here the Copernican doctrine was no longer the point at issue. Rather, what was considered was the question of whether Galileo had obeyed the order given him in the first trial, or whether he had deceived the inquisitors into believing that the order had never been issued. The proceedings of both trials were published by Antonio Favaro in Vol. 19 of the National Edition of Galilean material. The suggestion, rather popular in the 19th century, that the proceedings contained

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7. An example is Galileo's reply to the inquiries of 12 April 1633: Maurice A. Finocchiaro, *The Galileo Affair*, Berkeley and Los Angeles, 1989, p. 262, the first two lines. The reaction of an admirer is characteristic: 'This absurd pretence . . .' Geymonat, op. cit., p. 149.

8. It cannot be denied that pressure groups, personal grievances, envy, the fact that Galileo, 'being too infatuated with his own genius' was 'unsufferable' (Westfall, op. cit., pp. 52, 38) and the rules of patronage played an important role as they, or similar circumstances, do at every trial. However, the tensions between various groups of the Church on the one side and the demands for scientific autonomy on the other were real enough; after all, their modern successors (should the sciences be given the run of our educational institutions and of society as a whole or should they be treated like any other special interest group?) are still with us. Here the Church did the right thing: the sciences do *not* have the last word in humane matters, knowledge included.

The main documents pertaining to the trial were assembled and translated with comments and an introduction by Finocchiaro, op. cit. I shall use his translations. Accounts of the trials and their problems are found in G. de Santillana, *The Crime of Galileo*, Chicago, 1954, Geymonat, op. cit., Redondi, op. cit., and, most recently, in Westfall.

falsified documents and that the second trial was therefore a farce, seems no longer acceptable.[9]

The first trial was preceded by denunciations and rumours, in which greed and envy played a part, as in many other trials. The Inquisition started to examine the matter. Experts (*qualificatores*) were ordered to give an opinion about two statements which contained a more or less correct account of the Copernican doctrine.[10] Their decision[11] concerned two points: what would today be called the *scientific content* of the doctrine and its *ethical (social) implications*.

On the first point the experts declared the doctrine to be 'foolish and absurd in philosophy' or, to use modern terms, they declared it to be unscientific. This judgement was made without reference to the faith, or to Church doctrine, but was based exclusively on the scientific situation of the time. It was shared by many outstanding scientists (Tycho Brahe having been one of them) – *and it was correct*[12] when based on the facts, the theories and the standards of

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9. One of the authors of the suggestion was the Galileo scholar Emil Wohlwill. His reasons, rather impressive at the time, are given in his *Der Inquisitionsprozess des Galileo Galilei*, Berlin, 1870. According to Wohlwill two documents of the proceedings, dated 25 Feb. 1616 and 26 Feb. 1616 (Finocchiaro, op. cit., pp. 147f) are mutually contradictory. The first advises Galileo to treat Copernicus as a mathematical model; should he reject the advice, then he is forbidden to mention Copernicus in any form whatsoever. In the second document Galileo is advised as above and immediately forbidden (i.e. without waiting for his reaction) to mention Copernicus. Wohlwill thought the second document to be a forgery. This seems now refuted. Cf. de Santillana, Chapter 13. Stillman Drake (appendix to Geymonat) devised a very intriguing hypothesis to explain the discrepancy.

10. Some critics used idiosyncrasies in the formulation as proof of a lack of comprehension on the part of the experts. But there was no need for the inquisitors to stick closely to the language of the authors they examined. Their account of Copernicanism was clear enough without such textual puritanism.

11. Finocchiaro, op. cit., p. 146.

12. Note that in rendering my judgements I rely on standards subscribed to by many modern scientists and philosophers of science. Returned to the early 17th century, these champions of rationality would have judged Galileo as the Aristotelians judged him then. Michelson, for example, would have been aghast at Galileo's attempt to get knowledge out of an instrument as little understood as the telescope and Rutherford, who was never too happy about the theory of relativity, would have produced one of his characteristic rude remarks. Salvador Luria, an outstanding microbiologist who favours theories decidable by 'clear-cut experimental step[s]', would have relegated the debate to 'outfields of science' like 'sociology' and would have stayed away from it (*A Slot Machine, a Broken Test Tube*, New York, 1985, pp. 115, 119). For what Galileo suggested was no less than to regard as true a theory which had only analogies in its favour and which suffered from numerous difficulties. And he made this suggestion in public while even today it is a deadly sin for a scientist to address the public before having consulted his peers (example in A. Pickering,

the time. Compared with those facts, theories and standards the idea of the motion of the earth was as absurd as were Velikovsky's ideas when compared with the facts, theories and standards of the fifties. A modern scientist really has no choice in this matter. He cannot cling to his own very strict standards and at the same time praise Galileo for defending Copernicus. He must either agree with the first part of the judgement of the Church experts, or admit that standards, facts and laws never decide a case and that an unfounded, opaque and incoherent doctrine can be presented as a fundamental truth. Only few admirers of Galileo have an inkling of this rather complex situation.

The situation becomes even more complex when we consider that the Copernicans changed not only views, but also standards for judging views. Aristotelians, in this respect not at all unlike modern epidemiologists, molecular biologists and 'empirical' sociologists who insist either on the examination of large statistical samples or on 'clearcut experimental steps' in Luria's sense, demanded strong empirical support while the Galileans were content with far-reaching, unsupported and partially refuted theories.[13] I do not criticize them for that; on the contrary, I favour Niels Bohr's 'this is not crazy enough'. I merely want to reveal the contradiction in the actions of those who praise Galileo and condemn the Church, but become as strict as the Church was at Galileo's time when turning to the work of their contemporaries.

On the second point, the social (ethical) implications, the experts declared the Copernican doctrine to be 'formally heretical'. This means it contradicted Holy Scripture as interpreted by the Church, and it did so in full awareness of the situation, not inadvertently (that would be 'material' heresy).

'Constraints on Controversy: the Case of the Magnetic Monopole', *Social Studies of Science*, Vol. 11, 1981, pp. 63ff). All this is realized neither by 'progressive' (i.e. scientifically inclined) princes of the Church nor by scientists, so the discussion of the 'trial of Galileo' occurs in a dream world with only little relation to the real world we and Galileo inhabit. Further arguments on that point are found in Chapter 9 of *Farewell to Reason* and Chapter 19 below.

13. As indicated in Chapter 8, footnote 1, Galileo's law of inertia was in conflict with the Copernican as well as the Keplerian treatment of planetary motion. Galileo hoped for future accommodations. That was a sensible thing to do but not in agreement with some standards of his time and of today. Today a similar clash between theoreticians and empiricists occurs in the field of epidemiology. There are theoretical reasons to expect that X-rays and other forms of particulate radiation constitute a cancer-risk down to the smallest dose. Many epidemiologists demand empirical proof, however, though it is clear that events, when occurring below a certain threshold of frequency, cannot be detected in that way.

The second point rests on a series of assumptions, among them the assumption that Scripture is an important boundary condition of human existence and, therefore, of research. The assumption was shared by all great scientists, Copernicus, Kepler and Newton among them. According to Newton knowledge flows from two sources – the word of God – the Bible – and the works of God – Nature; and he postulated divine interventions in the planetary system, as we have seen.[14]

The Roman Church in addition claimed to possess the exclusive rights of exploring, interpreting and applying Holy Scripture. Lay people, according to the teaching of the Church, had neither the knowledge nor the authority to tamper with Scripture and they were forbidden to do so. This comment, whose rigidity was a result of the new Tridentine Spirit,[15] should not surprise anyone familiar with the habits of powerful institutions. The attitude of the American Medical Association towards lay practitioners is as rigid as the attitude of the Church was towards lay interpreters – and it has the blessing of the law. Experts, or ignoramuses having acquired the formal insignia of expertise, always tried and often succeeded in securing for themselves exclusive rights in special domains. Any criticism of the rigidity of the Roman Church applies also to its modern scientific and science-connected successors.

Turning now from the form and the administrative backing of the objection to its content we notice that it deals with a subject that is gaining increasing importance in our own times – the quality of human existence. Heresy, defined in a wide sense, meant a deviation from actions, attitudes and ideas that guarantee a well-rounded and sanctified life. Such a deviation might be, and occasionally was, encouraged by scientific research. Hence, it became necessary to examine the heretical implications of scientific developments.

Two ideas are contained in this attitude. First, it is assumed that the quality of life can be defined independently of science, that it may clash with demands which scientists regard as natural ingredients of their activity, and that science must be changed accordingly. Secondly, it is assumed that Holy Scripture as interpreted by the Holy Roman Church adumbrates a correct account of a well-rounded and sanctified life.

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14. Chapter 5, footnote 4. See also the literature in footnote 6 to Chapter 4. According to Galileo (letter to Grand Duchess Christina) the idea of the two sources goes back to Tertullian *adv. Marciones* (E. Evans, ed.), 1, 18.

15. For the exact wording see Denzinger–Schoenmetzer, *Enchiridion Symbolorum*, 36th edition, Freiburg, 1976, pp. 365f.

The second assumption can be rejected without denying that the Bible is vastly richer in lessons for humanity than anything that might ever come out of the sciences. Scientific results and the scientific ethos (if there is such a thing) are simply too thin a foundation for a life worth living. Many scientists agree with this judgement.[16]

They agree that the quality of life can be defined independently of science – which is the first part of the first assumption. At the time of Galileo there existed an institution – the Roman Church – watching over this quality in its own particular way. We must conclude that the second point – Copernicus being 'formally heretical' – was connected with ideas that are urgently needed today. The Church was on the right track.

But was it perhaps mistaken in rejecting scientific opinions inconsistent with its idea of a Good Life? In Chapter 3 I argued that knowledge needs a plurality of ideas, that well-established theories

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16. Thus Konrad Lorenz in his interesting if somewhat superficial book *Die Acht Todsünden der Zivilisierten Menschheit*, Munich, 1984 (first published in 1973), p. 70 writes: 'The erroneous belief that only what can be rationally grasped or even what can be proved in a scientific way constitutes the solid knowledge of mankind has disastrous consequences. It prompts the "scientifically enlightened" younger generation to discard the immense treasures of knowledge and wisdom that are contained in the traditions of every ancient culture and in the teachings of the great world religions. Whoever thinks that all this is without significance naturally succumbs to another, equally pernicious mistake, living in the conviction that science is able, as a matter of course, to create from nothing, and in a rational way, an entire culture with all its ingredients.' In a similar vein J. Needham, initiator and part-author of a great history of Chinese science and technology, speaks of 'scientific opium', meaning by it 'a blindness to the suffering of others'. *Time, the Refreshing River*, Nottingham, 1986.

'Rationalism', writes Peter Medawar (*Advice to a Young Scientist*, New York, 1979, p. 101), 'falls short of answering the many simple and childlike questions people like to ask; questions about origins and purposes such as are often contemptuously dismissed as non-questions, or pseudo-questions, although people understand them clearly enough and long to have an answer. These are intellectual pains that rationalists – like bad physicians confronted by ailments they cannot diagnose or cure – are apt to dismiss as "imagination".'

The clearest and most perceptive statement is found in Jacques Monod, *Chance and Necessity*, New York, 1972, p. 170 (text in brackets from p. 169): 'Cold and austere,' writes Monod, 'proposing no explanation but imposing an ascetic renunciation of all other spiritual fare, [the idea that objective knowledge is the only authentic source of truth] was not of a kind to allay anxiety but aggravated it instead. By a single stroke it claimed to sweep away the traditions of hundreds of thousands of years, which had become one with human nature itself. It wrote an end to the ancient animist convenant between man and nature, leaving nothing in place of that precious bond but an anxious quest in a frozen universe of solitude. With nothing to recommend it but a certain puritan arrogance, how could such an idea win acceptance? It did not; it still has not. It has however commanded recognition; but that it did only because of its prodigious power of performance.'

are never strong enough to terminate the existence of alternative approaches, and that a defence of such alternatives, being almost the only way of discovering the errors of highly respected and comprehensive points of view, is required even by a narrow philosophy such as empiricism. Now if it should turn out that it is also required on ethical grounds, then we have two reasons instead of one rather than a conflict with 'science'.

Besides, the Church, and by this I mean its most outstanding spokesmen, was much more modest than that. It did not say: what contradicts the Bible as interpreted by us must go, no matter how strong the scientific reasons in its favour. A truth supported by scientific reasoning was not pushed aside. It was used to revise the interpretation of Bible passages apparently inconsistent with it. There are many Bible passages which seem to suggest a flat earth. Yet Church doctrine accepted the spherical earth as a matter of course. On the other hand the Church was not ready to change just because somebody had produced some vague guesses. It wanted *proof* – scientific proof in scientific matters. Here it acted no differently from modern scientific institutions: universities, schools and even research institutes in various countries usually wait a long time before they incorporate new ideas into their curricula. (Professor Stanley Goldberg has described the situation in the case of the special theory of relativity.) But there was as yet no convincing proof of the Copernican doctrine. Hence Galileo was advised to teach Copernicus *as a hypothesis*; he was forbidden to teach it *as a truth*.

This distinction has survived until today. But while the Church was prepared to admit that some theories might be true and even that Copernicus' might be true, given sufficient evidence,[17] there are

17. In a widely discussed letter which Cardinal Roberto Bellarmino, master of controversial questions at the Collegio Romano, wrote on 12 April 1615 to Paolo Antonio Foscarini, a Carmelite monk from Naples who had inquired about the reality of the Copernican system, we find the following passage (Finocchiaro, op. cit., p. 68): '. . . if there were a true demonstration that the sun is at the center of the world and the earth in the third heaven, and that the sun does not circle the earth but the earth circles the sun, then one would have to proceed with great care in explaining the Scriptures that appear contrary, *and say rather that we do not understand them than that what is demonstrated is false*. But I will not believe that there is such a demonstration, until it is shown me. Nor is it the same to demonstrate that by supposing the sun to be at the center and the earth in heaven one can save the appearances, and to demonstrate that in truth the sun is at the center and the earth in heaven; for I believe the first demonstration may be available, but I have very great doubts about the second, and in case of doubt we must not abandon the Holy Scripture as interpreted by the Holy Fathers.' In his *Considerations on the Copernican Opinion*, Finocchiaro, op. cit., pp. 70ff,

now many scientists, especially in high energy physics, who view *all* theories as instruments of prediction and reject truth-talk as being metaphysical and speculative. Their reason is that the devices they use are so obviously designed for calculating purposes and that theoretical approaches so clearly depend on considerations of elegance and easy applicability that the generalization seems to make good sense. Besides, the formal properties of 'approximations' often differ from those of the basic principles, many theories are first steps towards a new point of view which at some future time may yield them as approximations and a direct inference from theory to reality is therefore rather naive.[18] All this was known to 16th- and 17th-century scientists. Only a few astronomers thought of deferents and epicycles as real roads in the sky; most regarded them as roads on paper which might aid calculation but which had no counterpart in reality. The Copernican point of view was widely interpreted in the same way – as an interesting, novel and rather efficient model. The Church requested, both for scientific and for ethical reasons, that Galileo accept this interpretation. Considering the difficulties the model faced when regarded as a description of reality, we must admit that '[l]ogic was on the side of . . . Bellarmine and not on the side of Galileo,' as the historian of science and physical chemist Pierre Duhem wrote in an interesting essay.[19]

To sum up: the judgement of the Church experts was scientifically correct and had the right social intention, viz. to protect people from the machinations of specialists. It wanted to protect people from being corrupted by a narrow ideology that might work in restricted domains but was incapable of sustaining a harmonious life. A revision of the judgement might win the Church some friends among scientists but would severely impair its function as a preserver of important human and superhuman values.[20]

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esp. pp. 85f, Galileo addresses precisely these points. He agrees that if the Copernican astronomers are 'not more than ninety percent right, they may be dismissed' but adds that 'if all that is produced by philosophers and astronomers on the opposite side is shown to be mostly false and wholly inconsequential, then the other side should not be disparaged, nor deemed paradoxical, so as to think that it could never be clearly proved': research should be permitted even if demonstrations are not yet available. This does not conflict with Bellarmino's suggestions; it did conflict and to a certain extent still does conflict with the attitude of many modern research institutions.

18. More on this point in Nancy Cartwright, *How the Laws of Physics Lie*, Oxford, 1983.

19. *To Save the Phenomena*, Chicago, 1963, p. 78.

20. After some apparent willingness to consider the matter (cf. the address of Pope John Paul II on the centenary of Einstein's birth, published as an Epilogue in Paul Cardinal Poupard (ed.), *Galileo Galilei: Towards a Resolution of 350 Years of Debate*,

Pittsburgh, 1987) Cardinal Joseph Ratzinger, who holds a position similar to that once held by Bellarmine, formulated the problem in a way that would make a revision of the judgement anachronistic and pointless. Cf. his talk in Parma of 15 March 1990, partly reported in *Il Sabato*, 31 March 1990, pp. 80ff. As witnesses the Cardinal quoted Ernst Bloch (being merely a matter of convenience the scientific choice between geocentrism and heliocentrism cannot overrule the practical and religious centricity of the earth), C.F. von Weizsäcker (Galileo leads directly to the atom bomb) and myself (the chapter heading of the present chapter). I commented on the speech in two interviews, *Il Sabato*, 12 May 1990, pp. 54ff and *La Repubblica*, 14 July 1990, p. 20.

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Galileo's inquiries formed only a small part of the so-called Copernican Revolution. Adding the remaining elements makes it still more difficult to reconcile the development with familiar principles of theory evaluation.

Galileo was not the only scientist involved in the reform of physics, astronomy and cosmology. Neither did he deal with the whole area of astronomy. For example, he never studied the motion of the planets in as much detail as did Copernicus and Kepler and he probably never read the more technical parts of Copernicus' great work. That was not unusual. Then as now knowledge was subdivided into specialities; an expert in one field rarely was also an expert in another and distant field. And then as now scientists with widely diverging philosophies could and did comment on new suggestions and developments. Tycho Brahe was an outstanding astronomer; his observations contributed to the downfall of generally accepted views. He noticed the importance of Copernicus' cosmology – yet he retained the unmoved earth, on physical as well as on theological grounds. Copernicus was a faithful Christian and a good Aristotelian; he tried to restore centred circular motion to the prominence it once had, postulated a moving earth, rearranged the planetary orbits and gave absolute values for their diameters. The astronomers surrounding Melanchthon and his educational reform accepted and praised the first part of his achievement, but (with a single exception – Rheticus) either disregarded, or criticized, or reinterpreted (Osiander!) the second. And they often tried to transfer Copernicus' mathematical models to the Ptolemaic system.[1] Maestlin, Kepler's teacher, regarded comets as solid bodies and tried calculating the orbit of one of them. His (incorrect) result made him accept the Copernican arrangement of the planetary orbits (it still

1. Details and literature in R.S. Westman, 'The Melanchthon Circle, Rheticus, and the Wittenberg Interpretation of the Copernican Theory', *Isis*, Vol. 66, 1975, pp. 165ff.

influenced Kepler). Maestlin respected Aristotle but regarded mathematical correctness and harmony as signs of physical truth. Galileo's approach had its own idiosyncrasies, it was more complex, more conjectural, partly adapted to the greater role theological considerations played in Italy, partly determined by the laws of rhetoric or patronage. Many different personalities, professions and groups guided by different beliefs and subjected to different constraints contributed to the process that is now being described, somewhat summarily, as the 'Copernican Revolution'.

As I said at the beginning this process was not a simple thing but consisted of developments in a variety of subjects, among them the following: cosmology; physics; astronomy; the calculation of astronomical tables; optics; epistemology; and theology.

I draw these distinctions not 'in order to be precise' but because they reflect actually existing subdivisions of research. Physics, for example, was a general theory of motion that described change without reference to the circumstances under which it occurred. It comprised locomotion, the growth of plants and animals as well as the transition of knowledge from a wise teacher to an ignorant pupil. Aristotle's *Physics* and the many mediaeval commentaries on it give us an idea of the problems treated and the solutions proposed. Cosmology described the structure of the universe and the special motions that are found in it. A basic law of *physics* in the sense just explained was that a motion without motor comes to a standstill – the 'natural' situation of a body is rest (this includes lack of qualitative change). The 'natural' motions of *cosmology* were those that occurred without noticeable interference; examples are the upward motion of fire and the downward motion of stones. Aristotle's *On the Heavens* and the many mediaeval commentaries on it give us an idea of the problems and the view discussed in this domain.

The books I just mentioned were for advanced studies only. Introductory texts omitted problems and alternative suggestions and concentrated on the bare bones of the ideas then held. One of the most popular introductory texts of cosmology, Sacrobosco's *de spera*, contained a sketch of the world, and described the main spheres without giving the details of their motions – the rest is silence.[2] Still,

2. Cf. Lynn Thorndyke (ed.), *The Sphere of Sacrobosco and Its Commentators*, Chicago, 1949. The elements and their motions are briefly mentioned in the first chapter together with a simple argument in favour of the unmoved earth: the earth is situated in the centre (this is shown earlier by optical arguments, including the fact that the constellations have the same size, no matter where the daily rotation puts them) and 'quicquid a medio movetur versus circumferentiam ascendit. Terra a medio movetur, ergo ascendit, quod pro impossibile relinquitur' (p. 85). Equant, deferent

it was used as a basis for rather advanced critical comments down to Galileo's own time.

Physics and cosmology claimed to make true statements. Theology which also claimed to make true statements was regarded as a boundary condition for research in these fields though the strength of this requirement and of its institutional backing varied in time and with the location. It was never a necessary boundary condition for astronomy which dealt with the motions of the stars, but without claiming truth for its models. Astronomers were interested in models that might correspond to the actual arrangement of the planets, but they were not restricted to them. Handbooks of astronomy such as Ptolemy's handbook and the various popularizations based on it contained detailed astronomical models preceded by sketchy cosmological introductions. As far as these introductions were concerned, there existed only one cosmology – Aristotle's. Some of the handbooks also contained tables. Tables were a further step away from 'reality'. They not only used 'hypotheses', i.e. models that might not reflect the structure of reality, they also used approximations. But an astronomer's approximations did not always correspond to the excellence of his models. 'Advanced' (from our standpoint) models might be combined with crude approximations and thus give worse tables than their older counterparts.[3]

The separation between physics and cosmology on the one side and astronomy on the other was not only a practical fact; it also had a firm philosophical backing. According to Aristotle[4] mathematics does not deal with real things but contains abstractions. There exists therefore an essential difference between physical subjects such as physics, cosmology, biology and psychology and mathematical subjects such as optics and astronomy. In the encyclopaedias of the early Middle Ages the separation was a matter of course.

Optical textbooks only rarely dealt with astronomical matters.[5] Astronomy used basic optical laws such as the law of linear propagation, but the more complicated parts of optical theory were

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and epicycle are mentioned in the fourth chapter together with the miraculous nature of the solar eclipse accompanying Christ's death.

3. The example of Ptolemy–Copernicus is treated by Stanley E. Babb Jr. in *Isis*, Vol. 68, September 1977, especially p. 432.

4. *Met.*, Book xiii, Chapter 2; *Physics*, Book ii, Chapter 2. For an account and defence of Aristotle's theory of mathematics cf. Chapter 8 of my *Farewell to Reason*.

5. As an example I mention John Pecham's optics (quoted from David Lindberg (ed.), *John Pecham and the Science of Optics*, Madison, 1970): astronomical matters occur here on pages 153 (moon illusion and the northward displacement of the sun and the fixed stars, explained by vapours near the horizon), 209 (scintillation of the

not well known. The same is true of epistemology. Galileo's arguments (and the arguments of Copernicus on which they are based) brought epistemology back into science (the same happened many years later, in connection with the quantum theory).

Now is it to be expected that a collection of relatively independent subjects, research strategies, arguments and opinions such as the one just mentioned will develop in a uniform way? Can we really assume that all the physicists, cosmologists, theologians and philosophers who reacted to the Copernican doctrine were guided by the same motives and reasons and that these reasons were not only accepted by them, but were also regarded as being binding for any scientist entering the scene? The ideas of an individual scientist such as Einstein may show a certain coherence[6] and this coherence may be reflected in his standards and his theorizing. Coherence is to be expected in totalitarian surroundings that guide research either by laws, by peer pressure or by financial machinations. But the astronomers at the time of Copernicus and after did not live in such surroundings; they lived at a time of dissension, wars and general upheaval, at a time when one city (Venice, for example, and the cities under its jurisdiction) would be safe for a progressive scientist while another (such as Rome, or Florence) offered considerable dangers, and when the ideas of a single individual often faced groups of scientists not in agreement with his monomania. To show this, let us look at two astronomers who participated in the development: Copernicus himself and Maestlin, Kepler's teacher.

Copernicus wanted to reform astronomy. He explained his misgivings and the ways in which he tried to overcome them. He wrote:[7]

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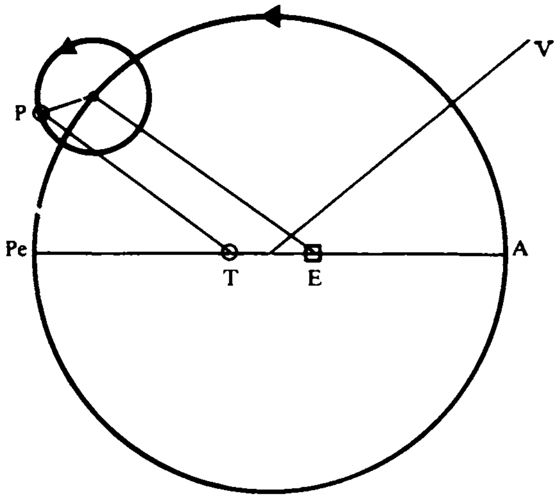
stars explained by unevennesses of their rotating surfaces which reflect the sunlight), 218 (impossibility to determine the size of the stars from their appearances), 233 (stars appear to be smaller than they actually are), 225 (they are displaced towards the north at the horizon, and the more so, the greater their distance from the meridian).

6. The case of Einstein shows that even this modest assumption goes much too far. Einstein recommended a loose opportunism as the best research strategy (cf. the quotation in the text to footnote 6 of the Introduction) and he warned that a good joke (such as the considerations leading to the special theory of relativity) should not be repeated too often: Philipp Frank, *Einstein, His Life and Times*, London, 1946, p. 261.

7. *Commentariolus*, ed. E. Rosen, *Three Copernican Treatises*, 3rd edition, New York, 1971, translation partly changed in accordance with F. Krafft, 'Copernicus Retroversus I', *Colloquia Copernicana* III and IV, Proceedings of the Joint Symposium of the IAU and the IUHPS, Torun, 1973, p. 119. In what follows I shall also use Krafft, 'Copernicus Retroversus II', loc. cit.

> The planetary theories of Ptolemaics and most other astronomers ... seemed ... to present no small difficulty. For these theories were not adequate unless certain equants were also conceived; it then appeared that a planet moved with uniform velocity neither along its own deferent nor relative to an actual centre.... Having become aware of these defects I often considered whether there could perhaps be found a more reasonable arrangement of circles from which every apparent inequality could be derived and in which everything would move uniformly about its proper centre as the rule of accomplished motion requires....



The critique of Copernicus concerns the following model that was used for calculating the longitudes of Mars, Jupiter and Saturn. The planet P moves on a small circle, the epicycle, whose centre is located on a larger circle, the deferent. The centre of the epicycle proceeds with constant angular velocity with respect to E, the equant point. The planet is observed from the earth T. E and T are on opposite sides of the centre of the deferent, having the same distance from it.

Copernicus does not question the empirical adequacy of the model. On the contrary, he admits that the planetary theories of the Ptolemaeans and others are 'consistent with the numerical data'.[8] Nor does he believe that these data are in need of correction. Instead of introducing new observations he emphasizes that

8. Rosen, op. cit., p. 59.

> we must follow in their [the ancient Greeks'] footsteps and hold fast to their observations bequeathed to us like an inheritance. And if anyone on the contrary thinks that the ancients are untrustworthy in this regard, surely the gates of this art are closed to him.[9]

Neither new observations nor the inability of Ptolemy to take care of what was known to him are the reason for Copernicus' discomfort. The difficulty he perceives lies elsewhere.

In his account Copernicus distinguishes between absolute motions and apparent motions. The second inequality of planetary motion, i.e. the fact that a planet may run ahead in its path and then reverse its direction, is 'apparent' – it must be reduced to other motions. According to Copernicus these other motions are motions on centred circles with a constant angular velocity around the centre. Ptolemy violates the condition; he uses equants. Equants explain apparent motions not by true motions but again by apparent motions where the planet 'moves with uniform velocity neither along its own deferent, nor relative to an actual centre. . . .' For Copernicus (and for many other astronomers) real motion is a circular motion around a centre with constant angular velocity.[10]

Copernicus removes excentre and equant and replaces them by epicycles.[11] In the Ptolemaic scheme each planet has now three epicycles: the old epicycle and two further epicycles for replacing the ecentric and the equant.

In order to avoid this accumulation of epicycles (which occasionally pushed the planets far out into space) Copernicus looks for a different explanation of the second inequality. He is helped by the fact that the second inequality agrees with the position of the sun.[12] It can therefore be interpreted as an apparent motion created by a real (and, of course, circular) motion on part of the earth.

The argument as reconstructed so far (after Krafft) contains two

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9. *Letter Against Werner*, in Rosen, op. cit., p. 99.

10. Erasmus Reinhold wrote on the title page of his personal copy of *de Revolutionibus: Axioma Astronomicum: Motus coelestis aequalis est et circularis vel ex aequalibus et circularibus compositus.* Quoted from Westman, 'The Melanchthon Circle', op. cit., p. 176.

11. This is true of the *Commentariolus*. In his main work he again uses excentric deferents. Only the equant is replaced by an epicycle. This 'liberation from the equant' (Erasmus Reinhold), also in lunar theory, greatly impressed some of Copernicus' admirers who paid no attention to his new cosmology and the motion of the earth. Westman, 'The Melanchthon Circle, op. cit., pp. 175, 177.

12. The mean sun in Copernicus. Kepler effects a reduction to the true sun and thereby strengthens the Copernican arrangement.

elements; a purely formal element and a reality assertion. Formally it is requested that any periodic motion be reduced to centred circular motions. The request is connected with the assumption that inequalities are apparent while circular motions alone are real. Let us call this *the first reality assumption*. But Copernicus also discovered that his procedure allowed him to incorporate every planetary path into a system, containing the 'large circle', the circle of the earth, as an absolute measure. 'All these phenomena', Copernicus writes in his main work,[13] 'are connected with one another in a most noble way, as if by a golden chain, and each planet with its position and order is a witness that the earth moves while we, who live on the terrestrial globe, failing to recognize its motion, ascribe all sorts of motions to the planets.' It is this inner connectedness of all parts of the planetary system that convinced Copernicus of the reality of the motion of the earth. I call this *the second reality assumption*.

The first reality assumption was part of the Platonic tradition; Aristotle gave it a physical basis. The second reality assumption conflicted with Aristotelian physics and cosmology. Aristotle had already criticized an earlier (Pythagorean) version of it: mathematical harmonies, which are abstractions, reflect truth only if they agree with well-confirmed physical principles. This is a reasonable request; it was used in our own century to reject Schroedinger's interpretation of wave mechanics. It is reasonable especially for those thinkers who regard mathematics as an auxiliary science that may describe but cannot constitute physical processes. It is unreasonable for a Platonist or a Pythagorean. The resulting clash between two interpretations of the nature of mathematical statements played an important role in the 'Copernican Revolution'.

Copernicus strengthened the second reality assumption by referring to traditions such as the Hermetic tradition and the idea of the exceptional role of the sun[14] and by showing how it could be reconciled with the phenomena. He made two assumptions. First, that the motion of a body is appropriate to its shape. The earth is spherical, hence its motion must be circular. Secondly, objects such as stone stay with the body (the earth) from which they were

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13. *De Revol.*, Preface to Pope Paul. Krafft assumes that Copernicus discovered this harmony in the course of his attempts to remove the equant and only later turned it into a fundamental argument in favour of a real motion of the earth.

14. The phrase 'and in the middle stands the sun' was not new. In the older astronomy the sun was indeed in the middle of the planets, with Mars, Jupiter and Saturn standing above it, and Venus, Mercury and the moon below. It also 'ruled' the planets in the sense that its motion was mirrored in the motions of all planets (the moon excepted). Cf. e.g. Macrobius, *Somnium Scipionis*.

separated – hence the falling stone stays close to the tower. According to Aristotle the natural motion of objects, i.e. the upward motion of fire and the downward motion of stones, was determined by the structure of space (central symmetry). According to Copernicus it is determined by the distribution of matter. Copernicus 'saves phenomena' such as the free fall of heavy bodies but provides neither independent arguments nor strict laws that could lead to a detailed comparison. His procedure is *ad hoc*. This does not mean that it is bad; it only means that it cannot be reconciled with the leading methodologies of today.

My second example is Michael Maestlin, Kepler's teacher. Maestlin was an expert astronomer and his judgement was generally respected. He 'only reluctantly abandoned' the Ptolemaic distribution of the spheres – but he was forced to do so by circumstances beyond his control.[15] As far as we can see, the circumstances were, first, the nova of 1572. Maestlin observed it, measured its parallax and put it beyond the sphere of the moon into the sphere of the fixed stars. The first part (beyond the moon) followed for Maestlin from the missing parallax, the second part (fixed stars) from the absence of any proper motion. According to Copernicus, whose ideas Maestlin used at this point, a planet moves more slowly the greater its distance from the sun. Observing the changes of colour and brightness Maestlin (and Tycho who saw the new star on the way to his alchemical laboratory) inferred that the region above the moon cannot be without change, as Aristotle had assumed. However, it would be rash to conclude that Maestlin (and Tycho) regarded the nova as a 'blow against the peripatetic philosophy'.[16] Many Church people, Theodore Beza among them, regarded the phenomenon as a return of the star of Bethlehem, i.e. as a supernatural event.[17] Tycho thought this comparison too modest; here, he said, is the greatest miracle since the beginning of the world, comparable at least to Joshua's stopping of the sun.[18] This means that as far as Tycho was concerned miracles refuted the idea of the *autonomy* of the laws of

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15. In what follows I am using the dissertation by R.A. Jarrell, *The Life and Scientific Work of the Tuebingen Astronomer Michael Maestlin*, Toronto, 1972, as well as R.S. Westman, 'Michael Maestlin's Adoption of the Copernican Theory', *Colloquia Copernicana* IV, Ossilineum, 1975, p. 53ff.

16. Jarrell, op. cit., p. 108.

17. Cf. the literature in P.H. Kocher, *Science and Religion in Elizabethan England*, New York, 1969, pp. 174f, footnotes 12 and 13. Cf. also Vol. vi, Chapter xxxii of Lynn Thorndike, *A History of Magic and Experimental Science*, New York, 1941.

18. *Progymnasmata*, p. 548.

nature (which was an Aristotelian idea), they did not refute *specific laws*. Maestlin, on the other hand, being perhaps more sceptical about miracles, may indeed have regarded the case as a 'blow against' Aristotle.

The next question is how serious a blow it was for him. The idea of a permanent heaven was part of cosmology and contained the special hypothesis of a fifth element. The falsehood of this hypothesis impaired neither the remaining laws of motion nor the tower argument. Both Clavius and Tycho accepted a changing heaven[19] but still used the tower argument to exclude the motion of the earth. If Maestlin's doubts reached further then this was due either to an idiosyncratic interpretation of the Aristotelian doctrines, or to personal inclinations towards a non-Aristotelian world-view. It seems that we must assume the latter.

The next decisive event on Maestlin's journey towards Copernicus was the comet of 1577. Again Maestlin, prompted by 'numerous observations', puts the comet into the superlunar region.[20] The idea that this region is free from change has now definitely been dropped.

Maestlin also tried to determine the trajectory of the comet. He found it to be moving in the path of Venus as described in Book 6, Chapter 12 of *de Revolutionibus*. Somewhat hesitatingly he now accepts the Copernican ordering of the spheres.[21] But, so he adds, he was forced to do so 'by extreme necessity'.[22]

This 'extreme necessity' arises only when geometrical considerations are given the force of cosmological arguments. Many years later Galileo cautioned against this way of reasoning: rainbows, he said, cannot be caught by triangulation. Maestlin had no such doubts. He accepted the traditional distinction between physics and astronomy and identified astronomy with mathematics: 'Copernicus wrote his entire book not as a physicist, but as an astronomer' is his comment on the margin of his copy of *de Revolutionibus*.[23] He then interpreted the results of mathematical arguments by using the second reality assumption. This means that he did not *overcome* an Aristotelian resistance against such an interpretation, he acted as if such a resistance *did not exist*. 'This argument', he wrote in his marginal notes,[24] 'is wholly in accord with reason. Such is the arrangement of

19. For Clavius cf. his commentary on Sacrobosco's sphere, 1593 edition, pp. 210f. Cf. also Westfall, op. cit., p. 44.
20. Jarrell, op. cit., p. 112.
21. Ibid., p. 117.
22. Ibid., p. 120.
23. Westman, op. cit., p. 59.
24. Ibid.

this entire, immense *machina* that it permits surer demonstrations, indeed, the entire universe revolves in such a way that nothing can be transposed without confusion of its [parts] and, hence, by means of these [surer demonstrations] all the phenomena of motion can be demonstrated most exactly, for nothing unfitting occurs in the course of their orbits.' Kepler too became a Copernican because of this harmony and because of the comet, the interesting fact being that Maestlin's calculations of the path of the comet contain serious mistakes; it did *not* move in the orbit of Venus.

Now let us compare these events and the situations in which they occur with some once popular philosophies of science. We notice at once that none of these philosophies considers all the disciplines that contributed to the debate. Astronomy is in the centre. A rational reconstruction of the developments in this area is thought to be a rational reconstruction of the Copernican Revolution itself. The role of physics (the tower argument), the fact that theology occasionally formed a strong boundary condition (cf. Tycho's reaction to his nova and to the idea of the motion of the earth) and the role of different mathematical philosophies shows that this cannot possibly be true. This fatal incompleteness is the first and most fundamental objection against all reconstructions that have been offered. They still depend on the (positivistic) prejudice that observations alone decide a case and that they can judge a theory all by themselves, without any help (or hindrance) from alternatives, metaphysical alternatives included. Moreover, they even fail in the narrow domain they have chosen for reconstruction, viz. astronomy. To show this, let us consider the following accounts:

1. *Naive empiricism*: the Middle Ages read the Bible and never looked at the sky. Then people suddenly looked upwards and found that the world was different from the opinion of the schools.

This account has disappeared from astronomy – but its analogue survives in other areas (for example, in some parts of the history of medicine). The main argument against it is that Aristotle was an arch empiricist and that Ptolemy used carefully collected data.[25]

2. *Sophisticated empiricism*: new observations forced astronomers to modify an already empirical doctrine.

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25. 'Carefully' has been contested by R.R. Newton, *The Crime of Claudius Ptolemy*, Baltimore, 1977. Newton shows that many of Ptolemy's 'data' *were manufactured* to fit his model. For his optics this has been known for a long time.

This certainly is not true for Copernicus and his followers in the 16th century. As we have seen, Copernicus thought the Ptolemaic system to be *empirically adequate* – he criticized it for *theoretical reasons*. And his 'observations' are essentially those of Ptolemy, as he says himself.

Modern comparisons of Copernican and Ptolemaic predictions 'with the facts', i.e. with 19th- and 20th-century calculations, show, furthermore, that empirical predictions were not improved and actually become worse when the competing systems are restricted to the same number of parameters.[26]

The only new observations made were those of Tycho Brahe – but they already led beyond Copernicus to Kepler. Galileo's observations belong to cosmology, not to astronomy. They lend plausibility to some of Copernicus' *analogies*. A *compelling* proof of the motion of the earth did not emerge, however, for the Galilean observations could also be accommodated by the Tychonian system.

3. *Falsificationism*: new observations refuted important assumptions of the old astronomy and led to the invention of a new one. This is not correct for Copernicus and the domain of astronomy (see above, comments on 2). The 'refutation' of the immutability of the heavens was neither compelling nor decisive for the problem of the motion of the earth. Besides, the idea of the motion of the earth was in big trouble or, if you will, 'refuted'. It could survive only if it was treated with kindness. But if *it* could be treated with kindness, then so could the older system.

We see here very clearly how misguided it is to try reducing the process 'Copernican Revolution' to a single principle, such as the principle of falsification. Falsifications played a role just as new observations played a role. But both were imbedded in a complex pattern of events which contained tendencies, attitudes, and considerations of an entirely different nature.

4. *Conventionalism*: the old astronomy became more and more complicated – so it was in the end replaced by a simpler theory. It is this assumption that led to the mocking remark of the 'epicyclical

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26. Stanley E. Babb, 'Accuracy of Planetary Theories, Particularly for Mars', *Isis*, Sep. 1977, pp. 426ff. Cf. also the earlier article of Derek de Solla Price, 'Contra Copernicus', in M. Clagett (ed.), *Critical Problems of the History of Science*, Madison, 1959, pp. 197ff; N.R. Hanson, *Isis*, No. 51, 1960, pp. 150ff as well as Owen Gingerich, 'Crisis vs Aesthetics in the Copernican Revolution', in Beer (ed.), *Vistas in Astronomy*, Vol. 17, 1974. Gingerich compares the *tables* of Stoeffler, Stadius, Maestlin, Magini and Origanus and finds all of them beset by errors of roughly the same magnitude (though not of the same distribution along the ecliptic).

degeneration'. The theory overlooks the fact that the Copernican scheme has about as many circles as the Ptolemaic one.[27]

5. *The theory of crises*: astronomy was in a crisis. The crisis led to a revolution which brought about the triumph of the Copernican system.

The answer here is the same as under 2: *empirically* there was no crisis and no crisis was resolved. A crisis did occur in cosmology, but only *after* the idea of the motion of the earth received a serious hearing. The many complaints about the inexactness of astronomical predictions that preceded Copernicus (Regiomontanus, for example) criticized the lack of precise initial conditions and accurate tables, *not* basic theory, and such a criticism would have been quite unjust, as the later examination of these theories shows.[28]

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27. The reader should consult the very instructive diagrams in de Santillana's edition of Galileo's *Dialogue*, Chicago, 1964.

28. Cf. footnote 26 above.

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The results obtained so far suggest abolishing the distinction between a context of discovery and a context of justification, norms and facts, observational terms and theoretical terms. None of these distinctions plays a role in scientific practice. Attempts to enforce them would have disastrous consequences. Popper's 'critical' rationalism fails for the same reasons.

Let us now use the material of the preceding sections to throw light on the following features of contemporary empiricism: (1) the distinctions between a context of discovery and a context of justification – norms and facts, observational terms and theoretical terms; (2) Popper's 'critical' rationalism; (3) the problem of incommensurability. The last problem will lead us back to the problem of rationality and order vs anarchism, which is the main topic of this essay.

One of the objections which may be raised against my attempt to draw methodological conclusions from historical examples is that it confounds two contexts which are essentially distinct, viz. a context of discovery, and a context of justification. *Discovery* may be irrational and need not follow any recognized method. *Justification*, on the other hand, or – to use the Holy Word of a different school – *criticism*, starts only *after* the discoveries have been made, and it proceeds in an orderly way. 'It is one thing,' writes Herbert Feigl, 'to retrace the historical origins, the psychological genesis and development, the socio-political-economic conditions for the acceptance or rejection of scientific theories; and it is quite another thing to provide a logical reconstruction of the conceptual structure and of the testing of scientific theories.'[1] These are indeed two different *things*, especially as they are done by two different *disciplines* (history of science, philosophy of science), which are quite jealous of their independence. But the question is not what distinctions a fertile mind can

1. 'The Orthodox View of Theories, in Radner–Winokur (eds), *Analyses of Theories and Methods of Physics and Psychology*, Minneapolis, 1970, p. 4.

dream up when confronted with a complex process, or how some homogeneous material may be subdivided; the question is to what extent the distinction drawn reflects a real difference and whether science can advance without a strong interaction between the separated domains. (A river may be subdivided by national boundaries but this does not make it a discontinuous entity.) Now there is, of course, a very noticeable difference between the rules of testing as 'reconstructed' by philosophers of science and the procedures which scientists use in actual research. This difference is apparent to the most superficial examination. On the other hand, a most superficial examination also shows that a determined application of the methods of criticism and proof which are said to belong to the context of justification would wipe out science as we know it – and would never have permitted it to arise.[2] Conversely, the fact that science exists proves that these methods were frequently overruled. They were overruled by procedures which belong to the context of discovery. Thus the attempt 'to retrace the historical origins, the psychological genesis and development, the socio-political-economic conditions for the acceptance or rejection of scientific theories', far from being irrelevant for the standards of test, actually leads to a criticism of these standards – *provided* the two domains, historical research and discussion of test procedures, are not kept apart by fiat.

In another paper Feigl repeats his arguments and adds some further points. He is 'astonished that ... scholars such as N.R. Hanson, Thomas Kuhn, Michael Polanyi, Paul Feyerabend, Sigmund Koch *et al.*, consider the distinction as invalid or at least misleading'.[3] And he points out that neither the psychology of invention nor any similarity, however great, between the sciences and the arts can show that it does not exist. In this he is certainly right. Even the most surprising stories about the manner in which scientists arrive at their theories cannot exclude the possibility that they proceed in an entirely different way once they have found them. *But this possibility is never realized.* Inventing theories and contemplating them in a relaxed and 'artistic' fashion, scientists often make moves that are forbidden by methodological rules. For example, they interpret the evidence so that it fits their fanciful ideas, eliminate difficulties by *ad hoc* procedures, push them aside, or simply refuse to take them seriously. The activities which according to Feigl belong to

2. Cf. the examples in Chapter 5.
3. 'Empiricism at Bay', MS, 1972, p. 2.

the context of discovery are, therefore, not just *different* from what philosophers say about justification, *they are in conflict with it.* Scientific practice does not contain two contexts moving *side by side*, it is a complicated *mixture* of procedures and we are faced by the question if this mixture should be left as it is, or if it should be replaced by a more 'orderly' arrangement. This is part one of the argument. Now we have seen that science as we know it today could not exist without a frequent overruling of the context of justification. This is part two of the argument. The conclusion is clear. Part one shows that we do not have a difference, but a mixture. Part two shows that replacing the mixture by an order that contains discovery on one side and justification on the other would have ruined science: we are dealing with a uniform practice all of whose ingredients are equally important for the growth of science. This disposes of the distinction.

A similar argument applies to the ritual distinction between methodological *prescriptions* and historical *descriptions*. Methodology, it is said, deals with what *should* be done and cannot be criticized by reference to *what is.* But we must of course make sure that our prescriptions have a *point of attack* in the historical material, and we must also make sure that their determined application leads to desirable results. We make sure by considering (historical, sociological, physical, psychological, etc.) *tendencies and laws* which tell us what is possible and what is not possible under the given circumstances and thus separate feasible prescriptions from those which are going to lead into dead ends. Again, progress can be made only if the distinction between the *ought* and the *is* is regarded as a temporary device rather than as a fundamental boundary line.

A distinction which once may have had a point but which has now definitely lost it is the distinction between *observational* terms and *theoretical* terms. It is now generally admitted that this distinction is not as sharp as it was thought to be only a few decades ago. It is also admitted, in complete agreement with Neurath's original views, that *both* theories *and* observations can be abandoned: theories may be removed because of conflicting observations, observations may be removed for theoretical reasons. Finally, we have discovered that *learning* does not go from observation to theory but always involves both elements. Experience arises *together* with theoretical assumptions not before them, and an experience without theory is just as incomprehensible as is (allegedly) a theory without experience: eliminate part of the theoretical knowledge of a sensing subject and you have a person who is completely disoriented and incapable of carrying out the simplest action. Eliminate further knowledge and his sensory world (his 'observation language') will start disintegrating, colours and other simple sensations will disappear until he is in a stage even more primitive than a small child. A small child, on the other hand, does not possess a stable perceptual world which he uses for making sense of the theories put before him. Quite the contrary – he passes through various perceptual stages which are only loosely connected with each other (earlier stages *disappear* when new stages take over – see Chapter 16) and which embody all the theoretical knowledge available at the time. Moreover, the whole process starts only because the child reacts correctly towards signals, *interprets them correctly*, because he possesses means of interpretation even before he has experienced his first clear sensation.

All these discoveries cry out for a new terminology that no longer separates what is so intimately connected in the development both of the individual and of science at large. Yet the distinction between observation and theory is still upheld. But what is its point? Nobody will deny that the sentences of science can be classified into long sentences and short sentences, or that its statements can be classified into those which are intuitively obvious and others which are not. Nobody will deny that such distinctions *can be made*. But nobody will put great weight on them, or will even mention them, *for they do not now play any decisive role in the business of science*. (This was not always so. Intuitive plausibility, for example, was once thought to be a most important guide to the truth; it disappeared from methodology the very moment intuition was replaced by experience, and by formal considerations.) Does experience play such a role? It does not, as we have seen. Yet the inference that the distinction between theory and observation has now ceased to be relevant, is either not drawn or is explicitly rejected.[4] Let us take a step forward and let us abandon this last trace of dogmatism in science!

Incommensurability, which I shall discuss next, is closely connected with the question of the rationality of science. Indeed one of the most general objections not merely to the *use of* incommensurable theories but even to the idea that *there are* such theories to be found in the history of science is the fear that they would severely restrict the efficacy of traditional, non-dialectical *argument*. Let us, therefore, look a little more closely at the critical *standards* which, according to some, constitute the content of a 'rational' argument. More especially, let us look at the standards of the Popperian school,

4. 'Neurath fails to give ... rules [which distinguish empirical statements from others] and thus unwittingly throws empiricism overboard', K.R. Popper, *The Logic of Scientific Discovery*, New York and London, 1959, p. 97.

which are still being taken seriously in the more backward regions of knowledge. This will prepare us for the final step in our discussion of the issue between law-and-order methodologies and anarchism in science.

Some readers of my arguments in the above text have pointed out that Popper's 'critical' rationalism is sufficiently liberal to accommodate the developments I have described. Now critical rationalism is either a meaningful idea or it is a collection of slogans that can be adapted to any situation.

In the first case it must be possible to produce rules, standards, restrictions which permit us to separate critical behaviour (thinking, singing, writing of plays) from other types of behaviour so that we can *discover* irrational actions and *correct* them with the help of concrete suggestions. It is not difficult to produce the standards of rationality defended by the Popperian school.

These standards are standards of *criticism*: rational discussion consists in the attempt to criticize, and not in the attempt to prove or to make probable. Every step that protects a view from criticism, that makes it safe or 'well-founded', is a step away from rationality. Every step that makes it more vulnerable is welcome. In addition, it is recommended to abandon ideas which have been found wanting and it is forbidden to retain them in the face of strong and successful criticism unless one can present suitable counter-arguments. Develop your ideas so that they can be criticized; attack them relentlessly; do not try to protect them, but exhibit their weak spots; eliminate them as soon as such weak spots have become manifest – these are some of the rules put forth by our critical rationalists.

These rules become more definite and more detailed when we turn to the philosophy of science and, especially, to the philosophy of the natural sciences.

Within the natural sciences, criticism is connected with experiment and observations. The content of a theory consists in the sum total of those basic statements which contradict it; it is the class of its potential falsifiers. Increased content means increased vulnerability, hence theories of large content are to be preferred to theories of small content. Increase of content is welcome, decrease of content is to be avoided. A theory that contradicts an accepted basic statement must be given up. *Ad hoc* hypotheses are forbidden – and so on. A science, however, that accepts the rules of a critical empiricism of this kind will develop in the following manner.

We start with a *problem*, such as the problem of the planets at the time of Plato. This problem (which I shall discuss in a somewhat idealized form) is not merely the result of *curiosity*, it is a *theoretical result*. It is due to the fact that certain *expectations* have been disappointed: on the one hand it seems to be clear that the stars must be divine, hence one expects them to behave in an orderly and lawful manner. On the other hand, one cannot find any easily discernible regularity. The planets, to all intents and purposes, move in a quite chaotic fashion. How can this fact be reconciled with the expectation and with the principles that underlie the expectation? Does it show that the expectation is mistaken? Or have we failed in our analysis of the facts? This is the problem.

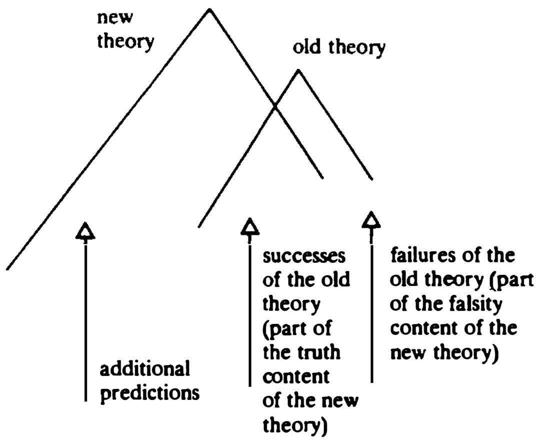
It is important to see that the elements of the problem are not simply *given*. The 'fact' of irregularity, for example, is not accessible without further ado. It cannot be discovered by just anyone who has healthy eyes and a good mind. It is only through a certain expectation that it becomes an object of our attention. Or, to be more accurate, this fact of irregularity *exists* because there is an expectation of regularity and because there are ideas which define what it means to be 'regular'. After all, the term 'irregularity' makes sense only if we have a rule. In our case the rule that defines regularity asserts circular motion with constant angular velocity. The fixed stars agree with this rule and so does the sun, if we trace its path relative to the fixed stars. The planets do not obey the rule, neither directly, with respect to the earth, nor indirectly, with respect to the fixed stars.

(In the problem we are examining now the rule is formulated explicitly and it can be discussed. This is not always the case. Recognizing a colour as red is made possible by deep-lying patterns concerning the structure of our surroundings and recognition does not occur when these patterns cease to exist.)

To sum up this part of the Popperian doctrine: research starts with a problem. The problem is the result of a conflict between an expectation and an observation which is constituted by the expectation. It is clear that this doctrine differs from the doctrine of inductivism where objective facts enter a passive mind and leave their traces there. It was prepared by Kant, Mach, Poincaré, Dingler, and by Mill (*On Liberty*).

Having formulated a problem, one tries to *solve* it. Solving a problem means inventing a theory that is relevant, falsifiable (to a degree larger than any alternative), but not yet falsified. In the case mentioned above (planets at the time of Plato), the problem is: to find circular motions of constant angular velocity for the purpose of saving the planetary phenomena. A first solution was provided by Eudoxos and then by Heracleides of Pontos.

Next comes the *criticism* of the theory that has been put forth in the



attempt to solve the problem. Successful criticism removes the theory *once and for all* and creates a new problem, viz. to explain (a) why the theory was successful so far; (b) why it failed. Trying to solve *this* problem we need a new theory that reproduces the successful consequences of the older theory, denies its mistakes and makes additional predictions not made before. These are some of the *formal conditions* which a *suitable successor of a refuted theory* must satisfy. Adopting the conditions, one proceeds by conjecture and refutation from less general theories to more general theories and expands the content of human knowledge.

More and more facts are *discovered* (or constructed with the help of expectations) and are then explained by theories. There is no guarantee that scientists will solve every problem and replace every theory that has been refuted with a successor satisfying the formal conditions. The invention of theories depends on our talents and other fortuitous circumstances such as a satisfactory sex life. But as long as these talents hold out, the enclosed scheme is a correct account of the growth of a knowledge that satisfies the rules of critical rationalism.

Now at this point, one may raise two questions.

1. Is it *desirable* to live in accordance with the rules of a critical rationalism?

2. Is it *possible* to have both a science as we know it and these rules?

As far as I am concerned, the first question is far more important than the second. True, science and related institutions play an important part in our culture, and they occupy the centre of interest for many philosophers (most philosophers are opportunists). Thus the ideas of the Popperian school were obtained by generalizing solutions for methodological and epistemological problems. Critical rationalism arose from the attempt to understand the Einsteinian revolution, and it was then extended to politics and even to the conduct of one's private life. Such a procedure may satisfy a *school philosopher*, who looks at life through the spectacles of his own technical problems and recognizes hatred, love, happiness, only to the extent to which they occur in these problems. But if we consider human interests and, above all, the question of human freedom (freedom from hunger, despair, from the tyranny of constipated systems of thought and *not* the academic 'freedom of the will'), then we are proceeding in the worst possible fashion.

For is it not possible that science as we know it today, or a 'search for the truth' in the style of traditional philosophy, will create a monster? Is it not possible that an objective approach that frowns upon personal connections between the entities examined will harm people, turn them into miserable, unfriendly, self-righteous mechanisms without charm and humour? 'Is it not possible,' asks Kierkegaard, 'that my activity as an objective [or a critico-rational] observer of nature will weaken my strength as a human being?'[5] I suspect the answer to many of these questions is affirmative and I believe that a reform of the sciences that makes them more anarchic and more subjective (in Kierkegaard's sense) is urgently needed.

But these are not the problems I want to discuss now. In the present essay I shall restrict myself to the second question and I shall ask: is it possible to have both a science as we know it and the rules of a critical rationalism as just described? And to *this* question the answer seems to be a firm and resounding NO.

To start with we have seen, though rather briefly, that the actual development of institutions, ideas, practices, and so on, often *does not start from a problem* but rather from some extraneous activity, such as playing, which, as a side effect, leads to developments which later on can be interpreted as solutions to unrealized problems.[6] Are such developments to be excluded? And, if we do exclude them, will this not considerably reduce the number of our adaptive reactions and the quality of our learning process?

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5. *Papirer*, ed. Heiberg, VII, Pt. I, sec. A, No. 182. Mill tries to show how scientific method can be understood as part of a theory of man and thus gives a positive answer to the question raised by Kierkegaard; cf. footnote 2 to Chapter 4.

6. Cf. the brief comments on the relation between idea and action in Chapter 1. For details cf. footnotes 31ff of 'Against Method', *Minnesota Studies*, Vol. 4, 1970.

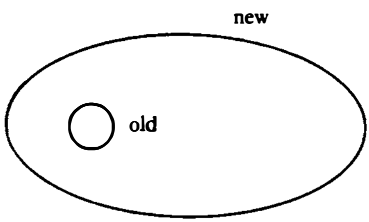
Secondly, we have seen, in Chapters 8–14, that a *strict principle of falsification*, or a 'naive falsificationism' as Lakatos calls it,[7] would wipe out science as we know it and would never have permitted it to start.

The demand for *increased content* is not satisfied either. Theories which effect the overthrow of a comprehensive and well-entrenched point of view, and take over after its demise, are initially restricted to a fairly narrow domain of facts, to a series of paradigmatic phenomena which lend them support, and they are only slowly extended to other areas. This can be seen from historical examples (footnote 12 of Chapter 8), and it is also plausible on general grounds: trying to develop a new theory, we must first take a *step back* from the evidence and reconsider the problem of observation (this was discussed in Chapter 11). Later on, of course, the theory is extended to other domains; but the mode of extension is only rarely determined by the elements that constitute the content of its predecessors. The slowly emerging conceptual apparatus of the theory *soon starts defining its own problems*, and earlier problems, facts, and observations are either forgotten or pushed aside as irrelevant. This is an entirely natural development, and quite unobjectionable. For why should an ideology be constrained by older problems which, at any rate, make sense only in the abandoned context and which look silly and unnatural now? Why should it even *consider* the 'facts' that gave rise to problems of this kind or played a role in their solutions? Why should it not rather proceed in its own way, devising its own tasks and assembling its own domain of 'facts'? A comprehensive theory, after all, is supposed to contain also an *ontology* that determines what exists and thus delimits the domain of possible facts and possible questions. The development of science agrees with these considerations. New views soon strike out in new directions and frown upon the older *problems* (what is the base upon which the earth rests? what is the specific weight of phlogiston? what is the absolute velocity of the earth?) and the older *facts* (most of the facts described in the *Malleus Maleficarum* – Chapter 8, footnote 2 – the facts of Voodoo – Chapter 4, footnote 8 – the properties of phlogiston or those of the ether) which so much exercised the minds of earlier thinkers. And where they *do* pay attention to preceding theories, they try to accommodate their factual core in the manner already described, with the help of *ad hoc* hypotheses, *ad hoc* approximations, redefinition of terms, or by simply

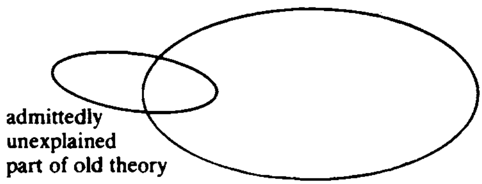
7. 'Falsification and the Methodology of Scientific Research Programmes', in Lakatos–Musgrave (eds), *Criticism and the Growth of Knowledge*, Cambridge, 1970, pp. 93ff. ('Naive falsificationism' is here also called 'dogmatic'.)

asserting, without any more detailed study of the matter, that the core 'follows from' the new basic principles.[8] They are 'grafted on to older programmes with which they [are] blatantly inconsistent'.[9]

The result of all these procedures is an interesting *epistemological illusion*: the *imagined* content of the earlier theories (which is the intersection of the remembered consequences of these theories with the newly recognized domain of problems and facts) *shrinks* and may decrease to such an extent that it becomes smaller than the *imagined* content of the new ideologies (which are the actual consequences of these ideologies *plus* all those 'facts', laws, principles which are tied to them by *ad hoc* hypotheses, *ad hoc* approximations or by the say-so of some influential physicist or philosopher of science – and which properly belong to the predecessor). Comparing the old and the new it thus *appears* that the relation of empirical contents is like this



or, perhaps, like this



while in actual fact it is much more like this

8. 'Einstein's theory is better than Newton's theory *anno 1916* ... *because* it explained everything that Newton's theory had successfully explained ...', Lakatos, op. cit., p. 214.

9. Lakatos, discussing Copernicus and Bohr, ibid., p. 143.

domain D representing the problems and facts of the old theory which are still remembered and which have been distorted so as to fit into the new framework. It is this illusion which is responsible for the persistent survival of the demand for increased content.[10]

Finally, we have by now seen quite distinctly the need for *ad hoc hypotheses*: *ad hoc* hypotheses and *ad hoc* approximations create a tentative area of contact between 'facts' and those parts of a new view which seem capable of explaining them, at some time in the future and after addition of much further material. They specify possible explananda and explanatia, and thus determine the direction of future research. They may have to be retained forever if the new framework is partly unfinished (this happened in the case of the quantum theory, which needs the classical concepts to turn it into a complete theory). Or they are incorporated into the new theory as theorems, leading to a redefinition of the basic terms of the proceeding ideology (this happened in the cases of Galileo and of the theory of relativity). The demand that the truth-content of the earlier theory as *conceived while the earlier theory reigned supreme* be included in the truth-content of the successor is violated in either case.

To sum up: wherever we look, whatever examples we consider, we see that the principles of critical rationalism (take falsifications seriously; increase content; avoid *ad hoc* hypotheses; 'be honest' – whatever *that* means; and so on) and, *a fortiori*, the principles of logical empiricism (be precise; base your theories on measurements; avoid vague and untestable ideas; and so on), though practised in special areas, give an inadequate account of the past development of science as a whole and are liable to hinder it in the future. They give an inadequate account of science because science is much more

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10. This illusion is the core of Elie Zahar's excellent paper on the development from Lorentz to Einstein. According to Zahar, Einstein superseded Lorentz with the explanation of the perihelion of Mercury (1915). But in 1915 nobody had as yet succeeded in giving a relativistic account of classical perturbation theory to the degree of approximation reached by Laplace and Poincaré, and the implications of Lorentz on the atomic level (electron theory of metals) were not accounted for either, but were gradually replaced by the quantum theory: Lorentz was 'superseded' not by one, but by at least two different and mutually incommensurable programmes. Lakatos, in his excellent reconstruction of the development of the research programme of Copernicus from the *Commentariolus* to the *De Revol.*, notes progressive changes but only because he omits the dynamical and the optical problems and concentrates on kinematics, pure and simple. Small wonder that both Zahar and Lakatos are under the impression that the content condition is still satisfied. Cf. my short note 'Zahar on Einstein', in the *British Journal for the Philosophy of Science*, March 1974 as well as R.N. Nugaev, 'Special Relativity as a Stage in the Development of Quantum Theory', *Historia Scientarium*, No. 34, 1988, pp. 57ff.

'sloppy' and 'irrational' than its methodological image. And they are liable to hinder it because the attempt to make science more 'rational' and more precise is bound to wipe it out, as we have seen. The difference between science and methodology which is such an obvious fact of history, therefore, indicates a weakness of the latter, and perhaps of the 'laws of reason' as well. For what appears as 'sloppiness', 'chaos' or 'opportunism' when compared with such laws has a most important function in the development of those very theories which we today regard as essential parts of our knowledge of nature. *These 'deviations', these 'errors', are preconditions of progress.* They permit knowledge to survive in the complex and difficult world which we inhabit, they permit *us* to remain free and happy agents. Without 'chaos', no knowledge. Without a frequent dismissal of reason, no progress. Ideas which today form the very basis of science exist only because there were such things as prejudice, conceit, passion; because these things *opposed reason*; and because they *were permitted to have their way*. We have to conclude, then, that *even within* science reason cannot and should not be allowed to be comprehensive and that it must often be overruled, or eliminated, in favour of other agencies. There is not a single rule that remains valid under all circumstances and not a single agency to which appeal can always be made.[11]

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11. Even Lakatos' ingenious methodology does not escape this indictment. Lakatos seems liberal because he forbids very little and he seems rational because he is still forbids something. But the only thing he forbids is to *describe* a 'degenerating research programme', i.e. a research programme lacking in novel predictions and cluttered with *ad hoc* adaptations, as progressive. He does not forbid its use. But this means that his standards permit a criminal to commit as many crimes as he wants provided he never lies about them. Details in my *Philosophical Papers*, Vol. 2, Chapter 10.

Appendix 1

Having listened to one of my anarchistic sermons, Professor Wigner exclaimed: 'But surely, you do not read all the manuscripts which people send you, but you throw most of them into the wastepaper basket.' I most certainly do. 'Anything goes' does not mean that I shall read every single paper that has been written – God forbid! – it means that I make my selection in a highly individual and idiosyncratic way, partly because I can't be bothered to read what doesn't interest me – and my interests change from week to week and day to day – partly because I am convinced that humanity and even Science will profit from everyone doing his own thing: a physicist might prefer a sloppy and partly incomprehensible paper full of mistakes to a crystal-clear exposition because it is a natural extension of his own, still rather disorganized, research and he might achieve success as well as clarity long before his rival who has vowed never to read a single woolly line (one of the assets of the Copenhagen School was its ability to avoid premature precision). On other occasions he might look for the most perfect proof of a principle he is about to use in order not to be sidetracked in the debate of what he considers to be his main results. There are of course so-called 'thinkers' who subdivide their mail in exactly the same way, come rain, come sunshine, and who also imitate each other's principles of choice – but we shall hardly admire them for their uniformity, and we shall certainly not think their behaviour 'rational'. Science needs people who are adaptable and inventive, not rigid imitators of 'established' behavioural patterns.

In the case of institutions and organizations such as the National Science Foundation the situation is exactly the same. The physiognomy of an organization and its efficiency depends on its members and it improves with their mental and emotional agility. Even Procter and Gamble realized that a bunch of yes-men is inferior in competitive potential to a group of people with unusual opinions and business has found ways of incorporating the most amazing nonconformists into their machinery. Special problems arise with foundations that distribute money and want to do this in a just and reasonable way. Justice seems to demand that the allocation of funds be carried out on the basis of standards which do not change from one applicant to the next and which reflect the intellectual situation in the fields to be supported. The demand can be satisfied in an *ad hoc* manner without appeal to *universal* 'standards of rationality': any free association of people must respect the illusions of its members and must give them institutional support. The illusion of *rationality* becomes especially strong when a scientific institution opposes political demands. In this case one class of standards is set against another such class – and this is quite legitimate: each organization, each party, each religious group has a right to defend its particular form of life and all the standards it contains. *But scientists go much further*. Like the defenders of The One True Religion before them they insinuate that their standards are essential for arriving at the Truth, or for getting Results and they deny such authority to the demands of the politician. They oppose all political interference, and they fall over each other trying to remind the listener, or the reader, of the disastrous outcome of the Lysenko affair.

Now we have seen that the belief in a unique set of standards that has always led to success and will always lead to success is nothing but a chimera. The *theoretical* authority of science is much smaller than it is supposed to be. Its *social* authority, on the other hand, has by now become so overpowering *that political interference is necessary to restore a balanced development*. And to judge the *effects* of such interference one must study more than one unanalysed case. One must remember those cases where science, left to itself, committed grievous blunders and one must not forget the instances when political interference did *improve* the situation.[1] Such a balanced presentation of the evidence may even convince us that the time is overdue for adding the separation of state and science to the by now quite customary separation of state and church. Science is only *one* of the many instruments people invented to cope with their surroundings. It is not the only one, it is not infallible and it has become too powerful, too pushy, and too dangerous to be left on its own. Next, a word about the *practical aim* rationalists want to realize with the help of their methodology.

Rationalists are concerned about intellectual pollution. I share this concern. Illiterate and incompetent books flood the market, empty verbiage full of strange and esoteric terms claims to express profound

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1. An example was discussed in the text to footnotes 9–12 of Chapter 4.

insights, 'experts' without brains, character, and without even a modicum of intellectual, stylistic, emotional temperament tell us about our 'condition' and the means for improving it, and they do not only preach to us who might be able to look through them, they are let loose on our children and permitted to drag them down into their own intellectual squalor.[2] 'Teachers' using grades and the fear of failure mould the brains of the young until they have lost every ounce of imagination they might once have possessed. This is a disastrous situation, and one not easily mended. But I do not see how a rationalistic methodology can help. As far as I am concerned the first and the most pressing problem is to get education out of the hands of the 'professional educators'. The constraints of grades, competition, regular examination must be removed and *we must also separate the process of learning from the preparation for a particular trade.* I grant that business, religions, special professions such as science or prostitution, have a right to demand that their participants and/or practitioners conform to standards they regard as important, and that they should be able to ascertain their competence. I also admit that this implies the need for special types of education that prepare a man or a woman for the corresponding 'examinations'. The standards taught need not be 'rational' or 'reasonable' in any sense, though they will be usually presented as such; it suffices that they are *accepted* by the groups one wants to join, be it now Science, or Big Business, or The One True Religion. After all, in a democracy 'reason' has just as much right to be heard and to be expressed as 'unreason' especially in view of the fact that one man's 'reason' is the other man's insanity. But one thing must be avoided at all costs: the special standards which define special subjects and special professions must not be allowed to permeate *general* education and they must not be made the defining property of a 'well-educated person'. General education should prepare citizens to *choose between* the standards, or to find their way in a society that contains groups committed to various standards, *but it must under no condition bend their minds so that they conform to the standards of one particular group.* The standards will be *considered*, they will be *discussed*, children will be encouraged to get proficiency in the more important subjects, *but only as one gets proficiency in a game*, that is, without serious commitment and without robbing the mind of its ability to play other games as well. Having been prepared in this way a young person may decide to devote the rest of his life to a particular profession and he may start taking it seriously forthwith. This

2. Even the law now seems to support these tendences, as is shown in Peter Huber's *Galileo's Revenge*, New York, 1991.

'commitment' should be the result of a conscious decision, on the basis of a fairly complete knowledge of alternatives, *and not a foregone conclusion.*

All this means, of course, that we must stop the scientists from taking over education and from teaching as 'fact' and as 'the one true method' whatever the myth of the day happens to be. Agreement with science, decision to work in accordance with the canons of science should be the result of examination and choice, and *not* of a particular way of bringing up children.

It seems to me that such a change in education and, as a result, in perspective will remove a great deal of the intellectual pollution rationalists deplore. The change of perspective makes it clear that there are many ways of ordering the world that surrounds us, that the hated constraints of one set of standards may be broken by freely accepting standards of a different kind, and that there is no need to reject *all* order and to allow oneself to be reduced to a whining stream of consciousness. A society that is based on a set of well-defined and restrictive rules, so that being human becomes synonymous with obeying these rules, *forces the dissenter into a no-man's-land of no rules at all and thus robs him of his reason and his humanity.* It is the paradox of modern irrationalism that its proponents silently identify rationalism with order and articulate speech and thus see themselves forced to promote stammering and absurdity – many forms of 'mysticism' and 'existentialism' are impossible without a firm but unrealized commitment to some principles of the despised ideology (just remember the 'theory' that poetry is nothing but emotions colourfully expressed). Remove the principles, admit the possibility of many different forms of life, and such phenomena will disappear like a bad dream.

My diagnosis and my suggestions coincide with those of Lakatos – up to a point. Lakatos has identified overly-rigid rationality principles as the source of some versions of irrationalism and he has urged us to adopt new and more liberal standards. I have identified overly-rigid rationality principles as well as a general respect for 'reason' as the source of some forms of mysticism and irrationalism, and I also urge the adoption of more liberal standards. But while Lakatos' great 'respect for great science'[3] makes him look for the standards within the confines of modern science 'of the last two centuries',[4] I recommend to put science in its place as an interesting but by no means exclusive form of knowledge that has many advantages but

3. 'History', p. 113.
4. ibid., p. 111.

also many drawbacks: 'Although science taken as a whole is a nuisance, one can still learn from it.'[5] Also I don't believe that charlatans can be banned just by tightening up rules.

Charlatans have existed at all times and in the most tightly-knit professions. Some of the examples which Lakatos mentions[6] seem to indicate that the problem is created by too much control and not by too little.[7] This is especially true of the new 'revolutionaries' and their 'reform' of the universities. Their fault is that they are Puritans and *not* that they are libertines.[8] Besides, who would expect that cowards will improve the intellectual climate more readily than will libertines? (Einstein saw this problem and he therefore advised people not to connect their research with their profession: research has to be free from the pressures which professions are likely to impose.[9]) We must also remember that those rare cases where liberal methodologies *do* encourage empty verbiage and loose thinking ('loose' from one point of view, though perhaps not from another) may be inevitable in the sense that the guilty liberalism is *also* a precondition of a free and humane life.

Finally, let me repeat that for me the chauvinism of science is a much greater problem than the problem of intellectual pollution. It may even be one of its major causes. Scientists are not content with running their own playpens in accordance with what they regard as the rules of scientific method, they want to universalize these rules, they want them to become part of society at large and they use every means at their disposal – argument, propaganda, pressure tactics, intimidation, lobbying – to achieve their aims. The Chinese Communists recognized the dangers inherent in this chauvinism and they proceeded to remove it. In the process they restored important parts of the intellectual and emotional heritage of the Chinese people and they also improved the practice of medicine[10]. It would be of advantage if other governments followed suit.

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5. Gottfried Benn, letter to Gert Micha Simon of 11 October 1949, quoted from Gottfried Benn, *Lyrik und Prosa, Briefe und Dokumente*, Wiesbaden, 1962, p. 235.
6. 'Falsification', p. 176, footnote 1.
7. Cf. also his remarks on 'false consciousness' in 'History', pp. 94, 108ff.
8. For an older example, cf. the *Born–Einstein Letters*, New York, 1971, p. 150.
9. ibid., pp. 105ff.
10. Cf. text to footnotes 9–12 of Chapter 4.