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In addition to natural interpretations, Galileo also changes sensations *that seem to endanger Copernicus. He admits that there are such sensations, he praises Copernicus for having disregarded them, he claims to have removed them with the help of the* telescope. *However, he offers no* theoretical *reasons why the telescope should be expected to give a true picture of the sky.*

I repeat and summarize. An argument is proposed that refutes Copernicus by observation. The argument is inverted in order to discover the natural interpretations which are responsible for the contradiction. The offensive interpretations are replaced by others, propaganda and appeal to distant, and highly theoretical, parts of common sense are used to defuse old habits and to enthrone new ones. The new natural interpretations, which are also formulated explicitly, as auxiliary hypotheses, are established partly by the support they give to Copernicus and partly by plausibility considerations and *ad hoc* hypotheses. An entirely new 'experience' arises in this way. There is as yet no independent evidence, but this is no drawback; it takes time to assemble facts that favour a new cosmology. For what is needed is a new dynamics that explains both celestial and terrestrial motions, a theory of solid objects, aerodynamics, and all these sciences are still hidden in the future.[1] *But their task is now well-defined*, for Galileo's assumptions, his *ad hoc* hypotheses included, are sufficiently clear and simple to prescribe the direction of future research.

Let it be noted, incidentally, that Galileo's procedure drastically reduces the content of dynamics. Aristotelian dynamics was a general

1. Galileo's circular law is not the right dynamics. It fits neither the epicycles which still occur in Copernicus, nor Kepler's ellipses. In fact, it is refuted by both. Still, Galileo regards it as an essential ingredient of the Copernican point of view and tries to remove bodies, such as comets, whose motion quite obviously is not circular, from interplanetary space. In his *Assayer* 'Galileo talked about comets [and interpreted them as illusions, similar to rainbows] in order to protect the Copernican system from possible falsifications.' P. Redondi, *Galileo Heretic*, Princeton, 1987, pp. 145, 31.

theory of change, comprising locomotion, qualitative change, generation and corruption, and it could also be applied to mental processes. Galileo's dynamics and its successors deal with *locomotion* only, and here again just with the locomotion of *matter*. Other kinds of motion are pushed aside with the promissory note (due to Democritos) that locomotion will eventually be capable of explaining all motion. Thus a comprehensive empirical theory is replaced by a narrow theory plus a metaphysics of motion,[2] just as an 'empirical' experience is replaced by an experience that contains speculative elements. *Counterinduction*, however, is now seen to play an important role both *vis-à-vis* theories and *vis-à-vis* facts. It clearly aids the advancement of science. This concludes the considerations begun in Chapter 6. I now turn to another part of Galileo's propaganda campaign, dealing not with natural interpretations but with the *sensory core* of our observational statements.

Replying to an interlocutor who expressed his astonishment at the

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2. The so-called scientific revolution led to astounding discoveries and considerably extended our knowledge of physics, physiology, and astronomy. This was achieved by pushing aside and regarding as irrelevant, *and often as non-existent*, those facts which had supported the older philosophy. Thus the evidence for witchcraft, demonic possession, the existence of the devil, etc., was disregarded *together* with the 'superstitions' it once confirmed. The result was that 'towards the close of the Middle Ages science was forced away from human psychology, so that even the great endeavour of Erasmus and his friend Vives, as the best representatives of humanism, did not suffice to bring about a reapproachment, and psychopathology had to trail centuries behind the developmental trend of general medicine and surgery. As a matter of fact the divorcement of medical science from psychopathology was so definite that the latter was always totally relegated to the domain of theology and ecclesiastic and civil law – two fields which naturally became further and further removed from medicine....' G. Zilboorg, MD, *The Medical Man and the Witch*, Baltimore, 1935, pp. 3ff and 70ff. Astronomy advanced, but the knowledge of the human mind slipped back into an earlier and more primitive stage. Another example is astrology. 'In the early stages of the human mind,' writes A. Comte (*Cours de Philosophie Positive*, Vol. III, pp. 273-80, ed. Littré, Paris, 1836), 'these connecting links between astronomy and biology were studied from a very different point of view, *but at least* they were studied and not left out of sight, as is the common tendency in our own time, under the restricting influence of a nascent and incomplete positivism. Beneath the chimerical belief of the old philosophy in the physiological influence of the stars, there lay a strong, though confused recognition of the truth that the facts of life were in some way dependent on the solar system. Like all primitive inspirations of man's intelligence this feeling needed rectification by positive science, but not destruction; though unhappily in science, as in politics, it is often hard to reorganize without some brief period of overthrow.' A third area is mathematics. Aristotle had developed a highly sophisticated theory of the continuum that overcame the difficulties raised by Zeno and anticipated quantum theoretical ideas on motion (see footnote 15 and text of Chapter 5). Most physicists returned to the idea of a continuum consisting of indivisible elements – if they considered such recondite matters, that is.

small number of Copernicans, Salviati, who 'act[s] the part of Copernicus',[3] gives the following explanation: 'You wonder that there are so few followers of the Pythagorean opinion [that the earth moves] while I am astonished that there have been any up to this day who have embraced and followed it. Nor can I ever sufficiently admire the outstanding acumen of those who have taken hold of this opinion and accepted it as true: they have, through sheer force of intellect, done such violence to their own senses as to prefer what reason told them over that which sensible experience plainly showed them to be the contrary. For the arguments against the whirling [the rotation] of the earth we have already examined [the dynamical arguments discussed above] are very plausible, as we have seen; and the fact that the Ptolemaics and the Aristotelians and all their disciples took them to be conclusive is indeed a strong argument of their effectiveness. But the experiences which overtly contradict the annual movement [the movement of the earth around the sun] are indeed so much greater in their apparent force that, I repeat, there is no limit to my astonishment when I reflect that Aristarchus and Copernicus were able to make reason so conquer sense that, in defiance of the latter, the former became mistress of their belief.'[4]

A little later Galileo notes that 'they [the Copernicans] were confident of what their reason told them!'[5] And he concludes his brief account of the origins of Copernicanism by saying that 'with reason as his guide he [Copernicus] resolutely continued to affirm what sensible experience seemed to contradict'. 'I cannot get over my amazement', Galileo repeats, 'that he was constantly willing to persist in saying that Venus might go around the sun and might be more than six times as far from us at one time as at another, and still look always equal, when it should have appeared forty times larger.'[6]

The 'experiences which overtly contradict the annual movement', and which 'are much greater in their apparent force' than even the dynamical arguments above, consist in the fact that 'Mars, when it is close to us . . . would have to look sixty times as large as when it is most distant. Yet no such difference is to be seen. Rather, when it is in opposition to the sun and close to us it shows itself only four or five

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3. *Dialogue*, op. cit., pp. 131 and 256.

4. ibid., p. 328. At other times Galileo speaks much more belligerently and dogmatically, and apparently without any awareness of the difficulties mentioned here. Cf. his preparatory notes for the letter to Grand Duchess Christina, *Opere*, V, pp. 367ff.

5. ibid., p. 335.

6. ibid., p. 339.

times as large as when, at conjunction, it becomes hidden behind the rays of the sun.'[7]

'Another and greater difficulty is made for us by Venus which, if it circulates around the sun, as Copernicus says, would now be beyond it and now on this side of it, receding from and approaching towards us by as much as the diameter of the circle it describes. Then, when it is beneath the sun and very close to us, its disc ought to appear to us a little less than forty times as large as when it is beyond the sun and near conjunction. Yet the difference is almost imperceptible.'

In an earlier essay, *The Assayer*, Galileo expressed himself still more bluntly. Replying to an adversary who had raised the issue of Copernicanism he remarks that '*neither Tycho, nor other astronomers nor even Copernicus could clearly refute* [*Ptolemy*] inasmuch as a most important argument taken from the movement of Mars and Venus stood always in their way'. (This 'argument' is mentioned again in the *Dialogue*, and has just been quoted.) He concludes that 'the two systems' (the Copernican and the Ptolemaic) are 'surely false'.[8]

We see that Galileo's view of the origin of Copernicanism differs markedly from the more familiar historical accounts. He neither points to *new facts* which offer inductive *support* to the idea of the moving earth, nor does he mention any observations that would *refute* the geocentric point of view but be accounted for by Copernicanism. On the contrary, he emphasizes that not only Ptolemy, but Copernicus as well, is refuted by the facts,[9] and he praises Aristarchus and Copernicus for not having given up in the face of

7. ibid., p. 334.

8. *The Assayer*, quoted from *The Controversy on the Comets of 1918*, op. cit., p. 185.

9. This refers to the period before the end of the 16th century; cf. Derek J. de S. Price, 'Contra-Copernicus: A Critical Re-Estimation of the Mathematical Planetary Theory of Ptolemy, Copernicus and Kepler', in M. Clagett (ed.), *Critical Problems in the History of Science*, Madison, 1959, pp. 197-218. Price deals only with the *kinematic* and the *optical* difficulties of the new views. (A consideration of the dynamical difficulties would further strengthen his case.) He points out that 'under the best conditions a geostatic or heliostatic system using eccentric circles (or their equivalents) with central epicycles can account for all angular motions of the planets to an accuracy better than 6' ... excepting only the special theory needed to account for ... Mercury and excepting also the planet Mars which shows deviations up to 30' from a theory. [This is] certainly better than the accuracy of 10' which Copernicus himself stated as a satisfactory goal for his own theory' which was difficult to test, especially in view of the fact that refraction (almost 1° on the horizon) was not taken into account at the time of Copernicus, and that the observational basis of the predictions was less than satisfactory.

Carl Schumacher (*Untersuchungen über die ptolemäische Theorie der unteren Planeten*, Münster, 1917) has found that the predictions concerning Mercury and Venus made by Ptolemy differ at most by an amount of 30' from those of Copernicus. The deviations found between modern predictions and those of Ptolemy (and Copernicus), such tremendous difficulties. He praises them for having proceeded *counterinductively.*

This, however, is not yet the whole story.

For while it might be conceded that Copernicus acted simply on faith, it may also be said that Galileo found himself in an entirely different position. Galileo, after all, invented a new dynamics. And he invented the telescope. The new dynamics, one might want to point out, removes the inconsistency between the motion of the earth and the 'conditions affecting ourselves and those in the air above us'.[10] And the telescope removes the 'even more glaring' clash between the changes in the apparent brightness of Mars and Venus as predicted on the basis of the Copernican scheme and as seen with the naked eye. This, incidentally, is also Galileo's own view. He admits that 'were it not for the existence of a superior and better sense than natural and common sense to join forces with reason' he would have been 'much more recalcitrant towards the Copernican system'.[11] The 'superior and better sense' is, of course, the *telescope*, and one is inclined to remark that the apparently counterinductive procedure was as a matter of fact induction (or conjecture plus refutation plus new conjecture), *but one based on a better experience*, containing not only better natural interpretations but also a better sensory core than was available to Galileo's Aristotelian predecessors.[12] This matter must now be examined in some detail.

The telescope is a 'superior and better sense' that gives new and more reliable evidence for judging astronomical matters. How is this hypothesis examined, and what arguments are presented in its favour?

In the *Sidereus Nuncius*,[13] the publication which contains his first

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which in the case of Mercury may be as large as 7°, are due mainly to wrong constants and initial conditions, including an incorrect value of the constant of precession. For the versatility of the Ptolemaic scheme cf. N.R. Ilanson, *Isis*, No. 51, 1960, pp. 150–8.

10. Ptolemy, *Syntaxis*, i, 7.

11. *Dialogue*, op. cit., p. 328.

12. For this view cf. Ludovico Geymonat, *Galileo Galilei*, transl. Stillman Drake, New York, 1965 (first Italian edition 1957), p. 184. For the story of Galileo's invention and use of the telescope cf. R.S. Westfall, 'Science and Patronage', *Isis*, Vol. 76 1985, pp. 11ff. According to Westfall, Galileo 'saw the telescope more as an instrument of patronage than as an instrument of astronomy' (p. 26) and had to be pushed into some astronomical applications by his pupil (and staunch Copernican) Castelli. Galileo's telescopes were better than others in circulation at the time and were much in demand. But he first satisfied the demands of potential patrons. Kepler, who complained about the quality of telescopes (cf. next chapter, footnote 21 and text) and who would have loved to possess a better instrument, had to wait.

13. *The Sidereal Messenger of Galileo Galilel*, transl. E. St Carlos, London, 1880, reissued by Dawsons of Pall Mall, 1960, p. 10.

telescopic observations, and which was also the first important contribution to his fame, Galileo writes that he 'succeeded (in building the telescope) through a deep study of the theory of refraction'. This suggests that he had *theoretical reasons* for preferring the results of telescopic observations to observations with the naked eye. But the particular reason he gives – his insight into the theory of refraction – is not *correct* and is not *sufficient* either.

The reason is not correct, for there exist serious doubts as to Galileo's knowledge of those parts of contemporary physical optics which were relevant for the understanding of telescopic phenomena. In a letter to Giuliano de Medici of 1 October 1610,[14] more than half a year after publication of the *Sidereus Nuncius*, he asks for a copy of Kepler's *Optics* of 1604,[15] pointing out that he had not yet been able to obtain it in Italy. Jean Tarde, who in 1614 asked Galileo about the construction of telescopes of pre-assigned magnification, reports in his diary that Galileo regarded the matter as a difficult one that he had found Kepler's *Optics* of 1611[16] so obscure 'that perhaps its own author had not understood it'.[17] In a letter to Liceti, written two years before his death, Galileo remarks that as far as he was concerned the nature of light was still in darkness.[18] Even if we consider such utterances with the care that is needed in the case of a whimsical author like Galileo, we must yet admit that his knowledge of optics was inferior by far to that of Kepler.[19] This is also the

14. Galileo, *Opere*, Vol. X, p. 441.

15. *Ad Vitellionem Paralipomena quibus Astronomiae Pars Optica Traditur*, Frankfurt, 1604, to be quoted from *Johannes Kepler, Gesammelte Werke*, Vol. II, Munich, 1939, ed. Franz Hammer. This particular work will be referred to as the 'optics of 1604'. It was the only useful optics that existed at the time. The reason for Galileo's curiosity was most likely the many references to this work in Kepler's reply to the *Sidereus Nuncius*. For the history of this reply as well as a translation cf. *Kepler's Conversation with Galileo's Sidereal Messenger*, transl. E. Rosen, New York, 1965. The many references to earlier work contained in the *Conversation* were interpreted by some of Galileo's enemies as a sign that 'his mask had been torn from his face' (G. Fugger to Kepler, 28 May 1610, Galileo, *Opere*, Vol. X, p. 361) and that he (Kepler) 'had well plucked him', Maestlin to Kepler, 7 August (Galileo, *Opere*, Vol. X, p. 428). Galileo must have received Kepler's *Conversation* before 7 May (*Opere*, X, p. 349) and he acknowledges receipt of the printed *Conversation* in a letter to Kepler of 19 August (*Opere*, X, p. 421).

16. *Dioptrice*, Augsburg, 1611, *Werke*, Vol. IV, Munich, 1941. This work was written after Galileo's discoveries. Kepler's reference to them in the preface has been translated by E. St Carlos, op. cit., pp. 37, 79ff. The problem referred to by Tarde is treated in Kepler's *Dioptrice*.

17. Geymonat, op. cit., p. 37.

18. Letter to Liceti of 23 June 1640. *Opere*, VIII, p. 208.

19. Kepler, the most knowledgeable and most lovable of Galileo's contemporaries, gives a clear account of the reasons why, despite his superior knowledge of

conclusion of Professor E. Hoppe, who sums up the situation as follows:

> Galileo's assertion that having heard of the Dutch telescope he reconstructed the apparatus by mathematical calculation must of course be understood with a grain of salt; for in his writings we do not find any calculations and the report, by letter, which he gives of his first effort says that no better lenses had been available; six days later we find him on the way to Venice with a better piece to hand it as a gift to the Doge Leonardi Donati. This does not look like calculation; it rather looks like trial and error. The calculation may well have been of a different kind, and here it succeeded, for on 25 August 1609 his salary was increased by a factor of three.[20]

Trial and error – this means that 'in the case of the telescope it was *experience* and not mathematics that led Galileo to a serene faith in the reliability of his device'.[21] This second hypothesis on the origin of the telescope is *also* supported by Galileo's testimony, in which he writes that he had tested the telescope 'a hundred thousand times on a hundred thousand stars and other objects'.[22] Such tests produced great and surprising successes. The contemporary literature – letters, books, gossip columns – testifies to the extraordinary impression which the telescope made as a means of improving *terrestrial vision.*

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optical matters, he 'refrained from attempting to construct the device'. 'You, however,' he addresses Galileo, 'deserve my praise. Putting aside all misgivings you turned directly to visual experimentation' (*Conversation*, op. cit., p. 18). It remains to add that Galileo, due to his lack of knowledge in optics, had no 'misgivings' to overcome: 'Galileo . . . was totally ignorant of the science of optics, and it is not too bold to assume that this was a most happy accident both for him and for humanity at large', Ronchi, *Scientific Change*, ed. Crombie, London, 1963, p. 550.

20. *Die Geschichte der Optik*, Leipzig, 1926, p. 32. Hoppe's judgement concerning the invention of the telescope is shared by Wolf, Zinner and others. Huyghens points out that superhuman intelligence would have been needed to invent the telescope on the basis of the available physics and geometry. After all, says he, we still do not understand the workings of the telescope. ('Dioptrica', *Hugenii Opuscula Postuma*, Ludg. Bat., 1903, 163, paraphrased after A.G. Kästner, *Geschichte der Mathematik*, Vol. IV, Göttingen, 1800, p. 60.)

21. Geymonat, op. cit., p. 39.

22. Letter to Carioso, 24 May 1616, *Opere*, X, p. 357: letter to P. Dini, 12 May 1611, *Opere*, IX, p. 106: 'Nor can it be doubted that I, over a period of two years now, have tested my instrument (or rather dozens of my instruments) on hundreds and thousands of objects near and far, large and small, bright and dark; hence I do not see how it can enter the mind of anyone that I have simple-mindedly remained deceived in my observations.' The hundreds and thousands of experiments remind one of Hooke, and are most likely equally spurious. Cf. footnote 9 of Chapter 9.

Julius Caesar Lagalla, Professor of Philosophy in Rome, describes a meeting of 16 April 1611, at which Galileo demonstrated his device: 'We were on top of the Janiculum, near the city gate named after the Holy Ghost, where once is said to have stood the villa of the poet Martial, now the property of the Most Reverend Malvasia. By means of this instrument, we saw the palace of the most illustrious Duke Altemps on the Tuscan Hills so distinctly that we readily counted its each and every window, even the smallest; and the distance is sixteen Italian miles. From the same place we read the letters on the gallery, which Sixtus erected in the Lateran for the benedictions, so clearly, that we distinguished even the periods carved between the letters, at a distance of at least two miles.'[23]

Other reports confirm this and similar events. Galileo himself points to the 'number and importance of the benefits which the instrument may be expected to confer, when used by land or sea'.[24]

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23. Legalla, *De phaenomenis in orbe lunae novi telescopii usa a D. Galileo Galilei nunc iterum suscitatis physica disputatio* (Venice, 1612), p. 8; quoted from E. Rosen, *The Naming of the Telescope*, New York, 1947, pl. 54. The regular reports (*Avvisi*) of the Duchy of Urbino on events and gossip in Rome contain the following notice of the event: 'Galileo Galilei the mathematician, arrived here from Florence before Easter. Formerly a Professor at Padua, he is at present retained by the Grand Duke of Tuscany at a salary of 1,000 scudi. He has observed the motion of the stars with the *occiali*, which he invented or rather improved. Against the opinion of all ancient philosophers, he declares that there are four more stars or planets, which are satellites of Jupiter and which he calls the Medicean bodies, as well as two companions of Saturn. He has here discussed this opinion of his with Father Clavius, the Jesuit. Thursday evening, at Monsignor Malavasia's estate outside the St Pancratius gate, a high and open place, a banquet was given for him by Frederick Cesi, the marquis of Monticelli and nephew of Cardinal Cesi, who was accompanied by his kinsman, Paul Monaldesco. In the gathering there were Galileo; a Fleming named Terrentius; Persio, of Cardinal Cesi's retinue, [La] Galla, Professor at the University here; the Greek, who is Cardinal Gonzaga's mathematician; Piffari, Professor at Siena, and as many as eight others. Some of them went out expressly to perform this observation, and even though they stayed until one o'clock in the morning, they still did not reach an agreement in their views' (quoted from Rosen, op. cit., p. 31).

24. *Sidereal Messenger*, op. cit., p. ii. According to Berellus (*De Vero Telescopii Inventore*, Hague, 1655, p. 4), Prince Moritz immediately realized the military value of the telescope and ordered that its invention – which Berellus attributes to Zacharias Jansen – be kept a secret. Thus the telescope seems to have commenced as a secret weapon and was turned to astronomical use only later. There are many anticipations of the telescope to be found in the literature, but they mostly belong to the domain of natural magic and are used accordingly. An example is Agrippa von Nettesheim, who, in his book on occult philosophy (written 1509, Book II, chapter 23), writes 'et ego novi ex illis miranda conficere, et specula in quibus quis videre poterit quaecunque voluerit a longissima distantia'. 'So may the toy of one age come to be the precious treasure of another', Henry Morley, *The Life of Cornelius Agrippa von Nettesheim*, Vol. II, p. 166.

The *terrestrial success* of the telescope was, therefore, assured. Its application to the *stars*, however, was an entirely different matter.

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Nor does the initial experience *with the telescope provide such reasons. The first telescopic observations of the sky are indistinct, indeterminate, contradictory and in conflict with what everyone can see with his unaided eyes. And the only theory that could have helped to separate telescopic illusions from veridical phenomena was refuted by simple tests.*

To start with, there is the problem of telescopic vision. This problem *is* different for celestial and terrestrial objects; and it was also *thought to be* different in the two cases.[1]

It was thought to be different because of the contemporary idea that celestial objects and terrestrial objects are formed from different materials and obey different laws. This idea entails that the result of an interaction of light (which connects both domains and has special properties) with terrestrial objects cannot, without further discussion, be extended to the sky. To this physical idea one added, entirely in accordance with the Aristotelian theory of knowledge (and also with present views about the matter), the idea that the senses are *acquainted* with the close appearance of terrestrial objects and are, therefore, able to perceive them distinctly, even if the telescopic image should be vastly distorted, or disfigured by coloured fringes. The stars are not known from close by.[2] Hence we cannot in their case use our *memory* for separating the contributions of the telescope and those which come from the object

1. This is hardly ever realized by those who argue (with Kästner, op. cit., p. 133) that 'one does not see how a telescope can be good and useful on the earth and yet deceive in the sky'. Kästner's comment is directed against Horky. See below, text to footnotes 9–16 of the present chapter.

2. That the senses are acquainted with our everyday surroundings, but are liable to give misleading reports about objects outside this domain, is proved at once by the *appearance of the moon*. On the earth large but distant objects in familiar surroundings, such as mountains, are seen as being large, and far away. The appearance of the moon, however, gives us an entirely false idea of its distance and its size.

itself.[3] Moreover, all the familiar cues (such as background, overlap, knowledge of nearby size, etc.), which constitute and aid our vision on the surface of the earth, are absent when we are dealing with the sky, so that new and surprising phenomena are bound to occur.[4] Only a new theory of vision, containing both hypotheses concerning the behaviour of light within the telescope and hypotheses concerning the reaction of the eye under exceptional circumstances, could have bridged the gulf between the heavens and the earth that was, and still is, such an obvious fact of physics and of astronomical observation.[5] We shall soon have occasion to comment on the theories that were available at the time and we shall see that they were unfit for the task and were refuted by plain and obvious facts. For the moment, I want to stay with the observations themselves and I want to comment on the contradictions and difficulties which arise when one tries to take the celestial results of the telescope at their face value, as indicating stable, objective properties of the things seen.

Some of these difficulties already announce themselves in a report of the contemporary *Avvisi*[6] which ends with the remark that 'even though they (the participants in the gathering described) went out expressly to perform this observation (of "four more stars or planets, which are satellites of Jupiter as well as of two companions of Saturn"[7]), and even though they stayed until one in the morning, they still did not reach an agreement in their views.'

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3. It is not too difficult to separate the letters of a familiar alphabet from a background of unfamiliar lines, even if they should happen to have been written with an almost illegible hand. No such separation is possible with letters which belong to an unfamiliar alphabet. The parts of such letters do not hang together to form distinct patterns which stand out from the background of general (optical) noise (in the manner described by K. Koffka, *Psychol. Bull.*, 19, 1922, pp. 551ff, partly reprinted in M.D. Vernon (ed.), *Experiments in Visual Perception*, London, 1966; cf. also the article by Gottschaldt in the same volume).

4. For the importance of cues such as diaphragms, crossed wires, background, etc., in the localization and shape of the telescope image and the strange situations arising when no cues are present cf. Chapter IV of Ronchi, *Optics*, op. cit., especially pp. 151, 174, 189, 191, etc. Cf. also R.L. Gregory, *Eye and Brain*, New York, 1966, *passim* and p. 99 (on the autokinetic phenomenon). F.P. Kilpatrick (ed.), *Explorations in Transactional Psychology*, New York, 1961, contains ample material on what happens in the absence of familiar cues.

5. It is for this reason that the 'deep study of the theory of refraction' which Galileo pretended to have carried out (text to footnote 13 of Chapter 8) would have been quite *insufficient* for establishing the usefulness of the telescope; cf. also footnote 16 of the present chapter.

6. Details in Chapter 8, footnote 23.

7. This is how the ring of Saturn was seen at the time. Cf. also R.L. Gregory, *The Intelligent Eye*, p. 119.

Another meeting that became notorious all over Europe makes the situation even clearer. About a year earlier, on 24 and 25 April 1610, Galileo had taken his telescope to the house of his opponent, Magini, in Bologna to demonstrate it to twenty-four professors of all faculties. Horky, Kepler's overly-excited pupil, wrote on this occasion;[8] 'I never slept on the 24th or 25th April, day or night, but I tested the instrument of Galileo's in a thousand ways,[9] both on things here below and on those above. *Below it works wonderfully*; in the heavens it deceives one, as some fixed stars [Spica Virginis, for example, is mentioned, as well as a terrestrial flame] are seen double.[10] I have as witnesses most excellent men and noble doctors and all have admitted the instrument to deceive.... This silenced Galileo and on the 26th he sadly left quite early in the morning... not even thanking Magini for his splendid meal....' Magini wrote to Kepler on 26 May: 'He has achieved nothing, for more than twenty learned men were present; yet nobody has seen the new planets distinctly (nemo perfecte vidit); he will hardly be able to keep them.'[11] A few months later (in a letter signed by Ruffini) he repeats: 'Only some with sharp vision were convinced to some extent.'[12] After these and other negative reports had reached Kepler from all sides, like a paper avalanche, he asked Galileo for witnesses:[13] 'I do not want to hide it from you that quite a few Italians have sent letters to Prague asserting that they could not see those stars [the moons of Jupiter] with your own telescope. I ask myself how it can be that so many deny the phenomenon, including those who use a telescope. Now, if I consider what occasionally happens to me, then I do not at all regard it as impossible that a single person may see what thousands are unable to see....[14] Yet I regret that the

8. Galileo, *Opere*, Vol. X, p. 342 (my italics, referring to the difference commented upon above, between celestial and terrestrial observations).

9. The 'hundreds' and 'thousands' of observations, trials, etc., which we find here again are hardly more than a rhetorical flourish (corresponding to our 'I have told you a thousand times'). They cannot be used to infer a life of incessant observation.

10. Here again we have a case where external clues are missing. Cf. Ronchi, *Optics*, op. cit., as regards the appearance of flames, small lights, etc.

11. Letter of 26 May, *Opere*, III.

12. ibid., p. 196.

13. Letter of 9 August 1610, quoted from Caspar–Dyck, *Johannes Kepler in Seinen Briefen*, Vol. 1, Munich, 1930, p. 349.

14. Kepler, who suffered from Polyopia ('instead of a single small object at a great distance, two or three are seen by those who suffer from this defect. Hence, instead of a single moon ten or more present themselves to me', *Conversation*, op. cit., footnote 94; cf. also the remainder of the footnote for further quotations), and who was familiar with Platter's anatomical investigations (cf. S.L. Polyak, *The Retina*, Chicago, 1942, pp. 134ff for details and literature), was well aware of the need for a *physiological criticism of astronomical observations*.

confirmation by others should take so long in turning up.... Therefore, I beseech you, Galileo, give me witnesses as soon as possible....' Galileo, in his reply of 19 August, refers to himself, to the Duke of Toscana, and Giuliano de Medici 'as well as many others in Pisa, Florence, Bologna, Venice and Padua, who, however, remain silent and hesitate. Most of them are entirely unable to distinguish Jupiter, or Mars, or even the Moon as a planet....'[15] – not a very reassuring state of affairs, to say the least.

Today we understand a little better why the direct appeal to telescopic vision was bound to lead to disappointment, especially in the initial stages. The main reason, one already foreseen by Aristotle, was that the senses applied under abnormal conditions are liable to give an abnormal response. Some of the older historians had an inkling of the situation, but they speak *negatively*, they try to explain the *absence* of satisfactory observational reports, the *poverty* of what is seen in the telescope.[16] They are unaware of the possibility that the observers might have been disturbed by *strong positive illusions* also. The extent of such illusions was not realized until quite recently, mainly as the result of the work of Ronchi and his school.[17] Here sizeable variations are reported in the placement of the telescopic image and, correspondingly, in the observed *magnification*. Some observers put the image right inside the telescope making it change

15. Caspar–Dyck, op. cit., p. 352.

16. Thus Emil Wohlwill, *Galileo und sein Kampf für die Kopernikanische Lehre*, Vol. 1, Hamburg, 1909, p. 288, writes: 'No doubt the unpleasant results were due to the lack of training in telescopic observation, and the restricted field of vision of the Galilean telescope as well as to the absence of any possibility for changing the distance of the glasses in order to make them fit the peculiarities of the eyes of the learned men....' A similar judgement, though more dramatically expressed, is found in Arthur Koestler's *Sleepwalkers*, p. 369.

17. Cf. Ronchi, *Optics*, op. cit.: *Histoire de la Lumière*, Paris, 1956; *Storia del Cannochiale*, Vatican City, 1964; *Critica dei Fondamenti dell'Acustica e del'Ottica*, Rome, 1964; cf. also E. Cantore's summary in *Archives d'histoire des sciences*, December 1966, pp. 333ff. I would like to acknowledge at this place that Professor Ronchi's investigations have greatly influenced my thinking on scientific method. For a brief historical account of Galileo's work cf. Ronchi's article in A.C. Crombie (ed.), *Scientific Change*, London, 1963, pp. 542–61. How little this field is explored becomes clear from S. Tolansky's book *Optical Illusions*, London, 1964. Tolansky is a physicist who in his microscopic research (on crystals and metals) was distracted by one optical illusion after another. He writes: 'This turned our interest to the analysis of other situations, with the ultimate unexpected discovery that optical illusions can, and do, play a very real part in affecting many daily scientific observations. This warned me to be on the lookout and as a result I met more illusions than I had bargained for.' The 'illusions of direct vision', whose role in scientific research is slowly being rediscovered were well known to mediaeval writers on optics, who treated them in special chapters of their textbooks. Moreover, they treated lens-images as *psychological* phenomena, as

its lateral position with the lateral position of the eye, exactly as would be the case with an after image, or a reflex inside the telescope – an excellent proof that one must be dealing with an 'illusion'.[18] Others place the image in a manner that leads to no magnification at all, although a linear magnification of over thirty may have been promised.[19] Even a doubling of images can be explained as the result of a lack of proper focusing.[20] Adding the many imperfections of the contemporary telescopes to these psychological difficulties,[21] one can well understand the scarcity of satisfactory reports and one is rather astonished at the speed with which the reality of the new phenomena was accepted, and, as was the custom, publicly acknowledged.[22] This development becomes even more puzzling

results of a misapprehension, for an image 'is merely the appearance of an object outside its place' as we read in John Pecham (cf. David Lindberg, 'The "Perspectiva Communis" of John Pecham', *Archives Internationales d'histoire des sciences*, 1965, p. 51, as well as the last paragraph of Proposition ii/19 of Pecham's *Perspectiva Communis*, which is to be found in *John Pecham and the Science of Optics*, D. Lindberg (ed.), Madison, 1970, p. 171).

18. Ronchi, *Optics*, op. cit., p. 189. This may explain the frequently uttered desire to look *inside* the telescope. No such problems arise in the case of *terrestrial* objects whose images are regularly placed 'in the plane of the object' (ibid., p. 182).

19. For the magnification of Galileo's telescope cf. *The Sidereal Messenger*, op. cit., p. 11, cf. also A. Sonnefeld, 'Die Optischen Daten der Himmelsfernrohre von Galileo Galilie', *Jenaer Rundschau*, Vol. 7, 1962, pp. 207ff. The old rule 'that the size, position and arrangement according to which a thing is seen depends on the size of the angle through which it is seen' (R. Grosseteste, *De Iride*, quoted from Crombie, *Robert Grosseteste*, Oxford, 1953, p. 120), which goes back to Euclid, is *almost always wrong*. I still remember my disappointment when, having built a reflector with an alleged linear magnification of about 150, I found that the moon was only about five times enlarged, and situated quite close to the ocular (1937).

20. The image remains sharp and unchanged over a considerable interval – the lack of focusing may show itself in a doubling, however.

21. The first usable telescope which Kepler received from Elector Ernst of Köln (who in turn had received it from Galileo), and on which he based his *Narratio de observatis a se quartuor Jovis satellibus*, Frankfurt, 1611, showed the stars as *squares* and intensely *coloured* (*Ges. Werke*, IV, p. 461). Ernst von Köln himself was unable to see anything with the telescope and he asked Clavius to send him a better instrument (*Archivio della Pontifica Universita Gregoriana*, 530, f 182r). Francesco Fontana, who from 1643 onwards observed the phases of Venus, notes an unevenness of the boundary (and infers mountains), cf. R. Wolf, *Geschichte der Astronomie*, Munich, 1877, p. 398. For the idiosyncrasies of contemporary telescopes and descriptive literature cf. Ernst Zinner, *Deutsche und Niederländische Astronomische Instrumente des 11 bis 18. Jahrhunderts*, Munich, 1956, pp. 216-21. Refer also to the author catalogue in the second part of the book.

22. Father Clavius (letter of 17 December 1610, *Opere*, X, p. 485), the astronomer of the powerful Jesuit Collegium Romanum, praises Galileo as the first to have observed the moons of Jupiter and he recognizes their reality. Magini, Grienberger, and others soon followed suit. It is clear that, in doing so, they did not proceed

when we consider that many reports of even the best observers were either plainly *false*, and capable of being shown as such at the time, or else *self-contradictory*.

Thus Galileo reports unevenness, 'vast protuberances, deep chasms, and sinuosities'[23] at the inner boundary of the lighted part of the moon while the outer boundary 'appear[s] not uneven, rugged, and irregular, but perfectly round and circular, as sharply defined as if marked out with a pair of compasses, and without the indentations of any protuberances and cavities'.[24] The moon, then, seemed to be

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according to the methods prescribed by their own philosophy, or else they were very lax in the investigation of the matter. Professor McMullin (op. cit., footnote 32) makes much of this quick acceptance of Galileo's telescopic observations: 'The regular periods observed for the satellites and for the phases of Venus strongly indicated that they were not artefacts of physiology or optics. There was surely no need for "auxiliary sciences"...' 'There was no need for auxiliary sciences,' writes McMullin, while using himself the unexamined auxiliary hypothesis that astronomical events are distinguished from physiological events by their regularity and their intersubjectivity. But this hypothesis is *false*, as is shown by the moon illusion, the phenomenon of fata morgana, the rainbow, haloes, by the many microscopic illusions which are so vividly described by Tolansky, by the phenomena of witchcraft which survive in our textbooks of psychology and psychiatry, though under a different name, and by numerous other phenomena. The hypothesis was also *known to be false* by Pecham, Witelo, and other mediaeval scholars who had studied the regular and intersubjective 'illusions' created by lenses, mirrors, and other optical contrivances. In antiquity the falsehood of McMullin's hypothesis was *commonplace*. Galileo explicitly discusses and repudiates it in his book on comets. Thus a new theory of vision was needed, not just to *accept* the Galilean observations, but also to provide *arguments* for their astronomical reality. Of course, Clavius may not have been aware of this need. This is hardly surprising. After all, some of his sophisticated 20th-century successors, such as Professor McMullin, are not aware of it either. In addition we must point out that the 'regular periods' of the moons of Jupiter were not as well known as McMullin insinuates. For his whole life Galileo tried to determine these periods in order to find better ways of determining longitude at sea. He did not succeed. Later on the same problem returned in a different form when the attempt to determine the velocity of light with more than one moon led to conflicting results. This was found by Cassini shortly after Roemer's discovery – cf. I.B. Cohen, 'Roemer and the first determination of the velocity of light (1676)', *Isis*, Vol. 31 (1940), pp. 347ff. For the attitude of Clavius and the scientists of the Collegium Romanum cf. the very interesting book *Galileo in China* by Pasquale M. d'Elia, S.J., Cambridge, Mass., 1960. The early observations of the astronomers of the Collegium are contained in their own 'Nuncius Sidereus', *Opere*, III/1, pp. 291–8.

23. *The Sidereal Messenger*, op. cit., p. 8.

24. op. cit., p. 24. – cf. the drawing on page 97 which is taken from Galileo's publication. Kepler in his *Optics* of 1604 writes (on the basis of observations with the unaided eye): 'It seemed as though something was missing in the circularity of the outmost periphery' (*Werke*, Vol. II, p. 219). He returns to this assertion in his *Conversation* (op. cit., pp. 28ff), criticizing Galileo's telescopic results by what he himself had seen with the unaided eye: 'You ask why the moon's outermost circle does not also appear irregular. I do not know how carefully you have thought about this subject or whether your query, as is more likely, is based on popular impression. For in

full of mountains at the inside but perfectly smooth at the periphery, and this despite the fact that the periphery *changed* as the result of the slight libration of the lunar body.[25] The moon and some of the planets, such as for example Jupiter, were enlarged while the apparent diameter of the fixed stars decreased: the former were brought nearer whereas the latter were pushed away. 'The stars,' writes Galileo, 'fixed as well as erratic, when seen with the telescope, by no means appear to be increased in magnitude in the same proportion as other objects, and the Moon itself, gain increase of size; but in the case of the stars such increase appears much less, so that you may consider that a telescope, which (for the sake of illustration) is powerful enough to magnify other objects a hundred times, will scarcely render the stars magnified four or five times.'[26]

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my book [the *Optics* of 1604] I state that there was surely some imperfection in that outermost circle during full moon. Study the matter, and once again tell us, how it looks to you....' Here the results of naked eye observation are quoted against Galileo's telescopic reports – and with perfectly good reason, as we shall see below. The reader who remembers Kepler's polyopia (cf. footnote 14 to this chapter) may wonder how he could trust his senses to such an extent. The reply is contained in the following quotation (*Werke*, II, pp. 194ff): 'When eclipses of the moon begin, I, who suffer from this defect, become aware of the eclipse before all the other observers. Long before the eclipse starts, I even detect the direction from which the shadow is approaching, while the others, who have very acute vision, are still in doubt.... The afore-mentioned waviness of the moon [cf. the previous quotation] stops for me when the moon approaches the shadow, and the strongest part of the sun's rays is cut off....' Galileo has two explanations for the contradictory appearance of the moon. The one involves a lunar atmosphere (*Messenger*, op. cit., pp. 26ff). The other explanation (ibid., pp. 25ff), which involves the tangential appearance of series of mountains lying behind each other, is not really very plausible as the distribution of mountains near the visible side of the lunar globe does not show the arrangement that would be needed (this is now even better established by the publication of the Russian moon photograph of 7 October 1959; cf. Zdenek Kopal, *An Introduction to the Study of the Moon*, North Holland, 1966, p. 242).

25. The librations were noticed by Galileo. C.G. Righini, 'New Light on Galileo's Lunar Observations', in M.L Righini-Bonelli and R. Shea (eds), *Reason, Experience and Mysticism in the Scientific Revolution*, New York, 1975, pp. 59ff. Thus it was not sloppiness of observations but the phenomena themselves that misguided Galileo.

In two letters to the journal *Science* (2 May and 10 October 1980) T.H. Whitaker accused me of giving a misleading account of Galileo's observational skill – I called him a poor observer when his lunar observations were in fact rather impressive. The accusation is refuted by the text to footnotes 29 and 30 and by footnote 46 of the present chapter. Whitaker obviously thought my quotations from Wolf (text to footnote 28) reflected my own opinion. He also points out that the copperplates of Galileo's observations are much better, from a modern point of view, than the woodcuts which accompanied the *Nuncius*. This is true but does not invalidate my description of the debate which was based on the published account.

26. *Messenger*, op. cit., p. 38; cf. also the more detailed account in *Dialogue*, op. cit., pp. 336ff. 'The telescope, as it were, removes the heavens from us,' writes A. Chwalina

The strangest features of the early history of the telescope emerge, however, when we take a closer look at Galileo's *pictures of the moon.*

It needs only a brief look at Galileo's drawings, and a photograph of similar phases, to convince the reader that 'none of the features recorded ... can be safely identified with any known markings of the lunar landscape'.[27] Looking at such evidence it is very easy to think that 'Galileo was not a great astronomical observer; or else that the excitement of so many telescopic discoveries made by him at that time had temporarily blurred his skill or critical sense'.[28]

Now this assertion may well be true (though I rather doubt it in view of the quite extraordinary observational skill which Galileo exhibits on other occasions).[29] But it is poor in content and,

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in his edition of *Kleomedes, Die Kreisbewegung der Gestirne* (Leipzig, 1927, p. 90), commenting on the decrease of the apparent diameter of *all* stars with the sole exception of the sun and the moon. Later on, the different magnification of planets (or comets) and fixed stars was used as a means of distinguishing them. 'From experience, I know', writes Herschel in the paper reporting his first observation of Uranus (*Phil Trans.*, 71, 1781, pp. 493ff – the planet is here identified as a *comet*), 'that the diameters of the fixed stars are not proportionally magnified with higher powers, as the planets are; therefore, I now put on the powers of 460 and 932, and found the diameter of the comet increased in proportion to the power, as it ought to be....' It is noteworthy that the rule did not invariably apply to the telescopes in use at Galileo's time. Thus, commenting on a comet of November 1618, Horatio Grassi ('On the Three Comets of 1618', in *The Controversy of the Comets of 1618*, op. cit., p. 17) points out 'that when the comet was observed through a telescope it suffered scarcely any enlargement', and he infers, perfectly in accordance with Herschel's 'experience', that 'it will have to be said that it is more remote from us than the moon....' In his *Astronomical Balance* (ibid., p. 80) he repeats that, according to the common experience of 'illustrious astronomers' from 'many parts of Europe' the comet observed with a very extended telescope received scarcely any increment.... Galileo (ibid., p. 177) accepts this as a fact, criticizing only the conclusions which Grassi wants to draw from it. All these phenomena refute Galileo's assertion (Assayer, op. cit., p. 204) that the telescope 'works always in the same way'. They also undermine his theory of irradiation (cf. footnote 56 to this chapter).

27. Kopal, op. cit., p. 207.

28. R. Wolf (*Geschichte der Astronomie*, p. 396) remarks on the poor quality of Galileo's drawings of the moon ('... seine Abbildung des Mondes kann man ... kaum ... eine Karte nennen'), while Zinner (*Geschichte der Sternkunde*, Berlin, 1931, p. 473) calls Galileo's observations of the moon and Venus 'typical for the observations of a beginner' His picture of the moon, according to Zinner, 'has no similarity with the moon' (ibid., p. 472). Zinner also mentions the much better quality of the almost simultaneous observations made by the Jesuits (ibid., p. 473), and he finally asks whether Galileo's observations of the moon and Venus were not the result of a fertile brain, rather than of a careful eye ('sollte dabei der Wunsch der Vater der Beobachtung gewesen sein?') – a pertinent question, especially in view of the phenomena briefly described in footnote 34 to this chapter.

29. The discovery and identification of the moons of Jupiter were no mean achievements, especially as a useful stable support for the telescope had not yet been developed.

I submit, not very interesting. No new suggestions emerge for additional research, and the possibility of a *test* is rather remote.[30] There are, however, other hypotheses which do lead to new suggestions and which show us how complex the situation was at the time of Galileo. Let us consider the following two.

Hypothesis I. Galileo recorded faithfully what he saw and in this way left us evidence of the shortcomings of the first telescopes as well as of the peculiarities of contemporary telescopic vision. Interpreted in this way Galileo's drawings are reports of exactly the same kind as are the reports emerging from the experiments of Stratton, Ehrismann, and Kohler[31] – except that the characteristics of the physical apparatus and the unfamiliarity of the objects seen must be taken into account too.[32] We must also remember the many conflicting views which were held about the surface of the moon, even at Galileo's time,[33] and which may have influenced what observers saw.[34] What would be needed in order to shed more light on the matter is an empirical collection of all the early telescopic results, preferably in parallel columns, including whatever pictorial representations

30. The reason, among other things, is the great variation of telescopic vision from one observer to the next, cf. Ronchi, *Optics*, op. cit., Chapter IV.

31. For a survey and some introductory literature cf. Gregory, op. cit., Chapter 11. For a more detailed discussion and literature cf. K.W. Smith and W.M. Smith, *Perception and Motion*, Philadelphia, 1962, reprinted in part in M.D. Vernon, op. cit. The reader should also consult Ames' article 'Aniseikonic Glasses', *Explorations in Transactional Psychology*, which deals with the change of *normal* vision caused by only slightly abnormal optical conditions. A comprehensive account is given by I. Rock, *The Nature of Perceptual Adaptation*, New York, 1966.

32. Many of the old instruments, and excellent descriptions of them, are still available. Cf. Zinner, *Deutsche und Niederlandische astronomische Instrumente*.

33. For interesting information the reader should consult the relevant passages of Kepler's *Conversation* as well as of his *Somnium* (the latter is now available in a new translation by E. Rosen, who has added a considerable amount of background material: *Kepler's Somnium*, ed. Rosen, Madison, 1967). The standard work for the beliefs of the time is still Plutarch's *Face on the Moon* (it will be quoted from H. Cherniss' translation of *Moralia XII*, London, 1967).

34. 'One describes the moon after objects one thinks one can perceive on its surface' (Kästner, op. cit., Vol. IV, p. 167, commenting on Fontana's observational reports of 1646). 'Maestlin even saw rain on the moon' (Kepler, *Conversation*, op. cit., pp. 29f, presenting Maestlin's own observational report); cf. also da Vinci, notebooks, quoted from J.P. Richter, *The Notebooks of Leonardo da Vinci*, Vol. II, New York, 1970, p. 167: 'If you keep the details of the spots of the moon under observation you will often find great variation in them, and this I myself have proved by drawing them. And this is caused by the clouds that rise from the waters in the moon. . . .' For the instability of the images of unknown objects and their dependence on belief (or 'knowledge') cf. Ronchi, *Optics*, op. cit., Chapter IV.

have survived.[35] Subtracting instrumental peculiarities, such a collection adds fascinating material to a yet-to-be-written history of perception (and of science).[36] This is the content of Hypothesis I.

Hypothesis II is more specific than Hypothesis I, and develops it in a certain direction. I have been considering it, with varying degrees of enthusiasm, for the last two or three years and my interest in it has been revived by a letter from Professor Stephen Toulmin, to whom I am grateful for his clear and simple presentation of the view. It seems to me, however, that the hypothesis is confronted by many difficulties and must, perhaps, be given up.

Hypothesis II, just like Hypothesis I, approaches telescopic reports from the point of view of the theory of perception; but it adds that the practice of telescopic observation and acquaintance with the new telescopic reports changed not only what was seen through the telescope, *but also what was seen with the naked eye.* It is obviously of importance for our evaluation of the contemporary attitude towards Galileo's reports.

That the appearance of the stars, and of the moon, may at some time have been much more indefinite than it is today was originally suggested to me by the existence of various theories about the moon which are incompatible with what everyone can plainly see with his own eyes. Anaximander's theory of partial stoppage (which aimed to explain the phases of the moon), Xenophanes' belief in the existence of different suns and different moons for different zones of the earth, Heraclitus' assumption that eclipses and phases are caused by the turning of the basins, which for him represented the sun and the moon[37] – all these views run counter to the existence of a stable and plainly visible surface, a 'face' such as we 'know' the moon to possess. The same is true of the theory of Berossos which occurs as late as Lucretius[38] and, even later, in Alhazen.

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35. Chapter 15 of Kopal, op. cit., contains an interesting collection of exactly this kind. Wider scope has W. Schulz, *Die Anschauung vom Monde und seinen Gestalten in Mythos und Kunst der Völker*, Berlin, 1912.

36. One must, of course, also investigate the dependence of what is seen on the current methods of pictorial representation. Outside astronomy this was done by E. Gombrich, *Art and Illusion*, London, 1960, and L. Choulant, *A History and Bibliography of Anatomical Illustration*, New York, 1945 (translated, with additions, by Singer and others), who deals with anatomy. Astronomy has the advantage that *one* side of the puzzle, viz. the stars, is fairly simple in structure (much simpler than the uterus, for example) and relatively well known; cf. also Chapter 16 below.

37. For these theories and further literature cf. J.L.D. Dreyer, *A History of Astronomy from Thales to Kepler*, New York, 1953.

38. For Berossos, cf. Toulmin's article in *Isis*, No. 38, 1967, p. 65. Lucretius writes (*On the Nature of Things*, transl. Leonard, New York, 1957, p. 216): 'Again, she

Now such disregard for phenomena which for us are quite obvious may be due either to a certain indifference towards the existing evidence, which was, however, as clear and as detailed as it is today, *or else to a difference in the evidence itself.* It is not easy to choose between these alternatives. Having been influenced by Wittgenstein, Hanson, and others, I was for some time inclined towards the second version, but it now seems to me that it is ruled out both by physiology (psychology)[39] and by historical information. We need only remember how Copernicus disregarded the difficulties arising from the variations in the brightness of Mars and Venus, which were well known at the time.[40] And as regards the face of the moon, we see that Aristotle refers to it quite clearly when observing that 'the stars do not *roll.* For rolling involves rotation: but the "face", as it is called, of the moon is always seen.'[41] We may infer, then, that the occasional disregard for the stability of the face was due not to a lack of clear impressions, but to some widely held views about the unreliability of the senses. The inference is supported by Plutarch's discussion of the matter which plainly deals not with what is seen (except as evidence for or against certain views) but with certain *explanations* of phenomena otherwise *assumed to be well known*:[42] 'To begin with,' he says, 'it is absurd to call the figure seen in the moon an affection of vision ... a condition which we call bedazzlement (glare). Anyone who asserts this does not observe that this phenomenon should rather have occurred in relation to the sun, since the sun lights upon us keen and violent, and moreover does not explain why dull and weak eyes discern no distinction of shape in the moon but her orb for them has an even and full light whereas those of keen and robust

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may revolve upon herself / like to a ball's sphere – if perchance to be – / one half of her dyed o'er with glowing light / and by the revolution of that sphere / she may beget for us her varying shapes / until she turns that fiery part of her / full to the sight and open eyes of men. . . .'

39. Cf. text to footnotes 50ff of my 'Reply to Criticism', op. cit., p. 246.

40. In antiquity the differences in the magnitudes of Venus and Mars were regarded as being 'obvious to our eyes', Simplicius, *De Coelo*, II, 12, Heiberg, p. 504. Polemarchus here considers the difficulties of Eudoxos' theory of homocentric spheres, viz. that Venus and Mars 'appear in the midst of the retrograde movement many times brighter, so that [Venus] on moonless nights causes bodies to throw shadows' (objection of Autolycus) and he may well be appealing to the possibility of a deception of the senses (which was frequently discussed by ancient schools). Aristotle, who must have been familiar with all these facts, does not mention them anywhere in *De Coelo* or in the *Metaphysics*, though he gives an account of Eudoxos' system and of the improvements of Polemarchus and Kalippus. Cf. footnote 7 of Chapter 8.

41. *De Coelo*, 290a25ff.

42. op. cit., p. 37, cf. also S. Sambursky, *The Physical World of the Greeks*, New York, 1962, pp. 244ff.

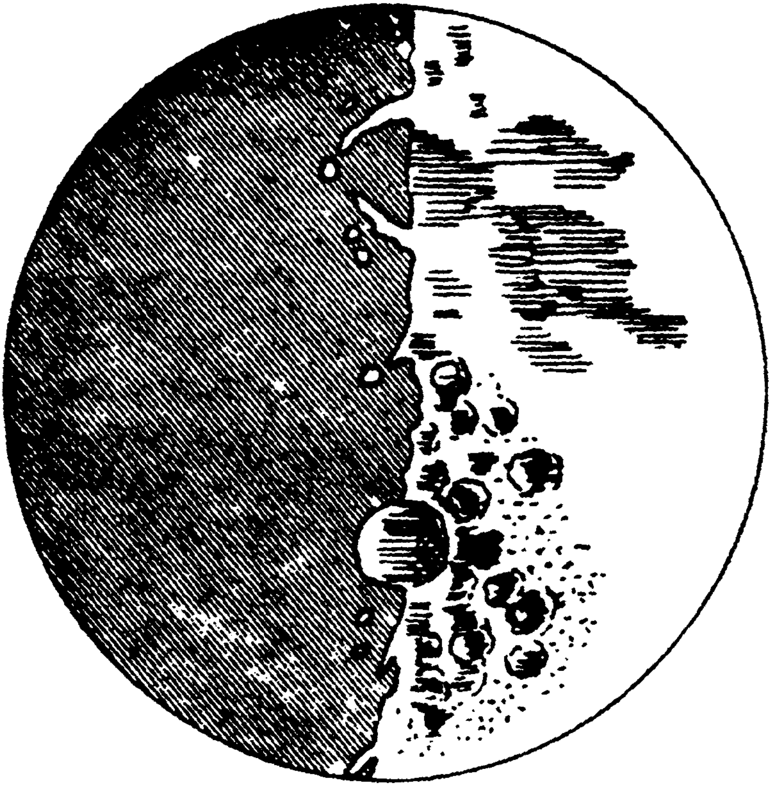

FIGURE 1. The shape of a lunar mountain and a walled plain, from Galileo *Sidereus Nuncius*, Venice, 1610 (cf. p. 111).

vision make out more precisely and distinctly the pattern of facial features and more clearly perceive the variations.' 'The unevenness also entirely refutes the hypothesis,' Plutarch continues,[43] 'for the shadow that one sees is not continuous and confused, but is not badly depictured by the words of Agesianax: "She gleams with fire encircled, but within / Bluer than lapis show a maiden's eye / And dainty brow, a visage manifest." In truth, the dark patches submerge beneath the bright ones which they encompass ... and they are thoroughly entwined with each other so as to make the delineation of the figure resemble a painting.' Later on the stability of the face is used as an argument against theories which regard the moon as being made of fire, or air, for 'air is tenuous and without configuration, and so it naturally slips and does not stay in place'.[44] The *appearance* of the moon, then, seemed to be a well-known and distinct phenomenon. What was in question was the *relevance* of the phenomenon for astronomical theory.

We can safely assume that the same was true at the time of Galileo.[45]

But then we must admit that Galileo's observations could be checked with the naked eye and could in this way be exposed as illusory.

Thus the circular monster below the centre of the disk of the moon[46] is well above the threshold of naked eye observation (its

43. ibid., cf. however, footnote 17 to this chapter, Pliny's remark (*Hist. Nat.*, II, 43, 46) that the moon is 'now spotted and then suddenly shining clear', as well as da Vinci's report, referred to in footnote 34 to this chapter.

44. ibid., p. 50.

45. A strong argument *in favour* of this contention is Kepler's description of the moon in his *Optics* of 1604: he comments on the broken character of the boundary between light and shadow (*Werke*, II, p. 218) and describes the dark part of the moon during an eclipse as looking like torn flesh or broken wood (ibid., p. 219). He returns to these passages in the *Conversation* (op. cit., p. 27), where he tells Galileo that 'these very acute observations of yours do not lack the support of even my own testimony. For [in my] *Optics* you have the half moon divided by a wavy line. From this fact I deduced peaks and depressions in the body of the moon. [Later on] I describe the moon during an eclipse as looking like torn flesh or broken wood, with bright streaks penetrating into the region of the shadow.' Remember also that Kepler criticizes Galileo's telescopic reports on the basis of his own naked eye observations; cf. footnote 24 of this chapter.

46. 'There is one other point which I must on no account forget, which I have noticed and rather wondered at it. It is this: The middle of the Moon, as it seems, is occupied by a certain cavity larger than all the rest, and in shape perfectly round. I have looked at this depression near both the first and the third quarters, and I have represented it as well as I can in the second illustration already given. It produces the same appearance as to effects of light and shade as a tract like Bohemia would produce on the Earth, if it were shut in on all sides by very lofty mountains arranged on the circumference of a perfect circle; for the tract in the moon is walled in with peaks of such enormous height that the furthest side adjacent to the dark portion of the moon is

diameter is larger than 3½ minutes of arc), while a single glance convinces us that the face of the moon is not anywhere disfigured by a blemish of this kind. It would be interesting to see what contemporary observers had to say on the matter[47] or, if they were artists, what they had to draw on the matter.

I summarize what has emerged so far.

Galileo was only slightly acquainted with contemporary optical *theory*.[48] His telescope gave surprising results on the earth, and these results were duly praised. Trouble was to be expected in the sky, as we know now. Trouble promptly arose: the telescope produced spurious and contradictory phenomena and some of its results could be refuted by a simple look with the unaided eye. Only a new *theory* of telescopic vision could bring order into the chaos (which may have been still larger, due to the different phenomena seen at the time even with the naked eye) and could separate appearance from reality. Such a theory was developed by Kepler, first in 1604 and then again in 1611.[49]

According to Kepler, the place of the image of a punctiform object is found by first tracing the path of the rays emerging from the object according to the laws of (reflection and) refraction until they reach the eye, and by then using the principle (still taught today) that 'the image will be seen in the point determined by the backward

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seen bathed in sunlight before the boundary between light and shade reaches half way across the circular space . . .' (*Messenger*, op. cit., pp. 21ff). This description, I think, definitely refutes Kopal's conjecture of observational laxity. It is interesting to note the difference between the woodcuts in the *Nuncius* (p. 131, Figure I) and Galileo's original drawing. The woodcut corresponds quite closely to the description while the original drawing with its impressionistic features ('Kaum eine Karte,' says Wolf) is vague enough to escape the accusation of gross observational error.

47. 'I cannot help wondering about the meaning of that large circular cavity in what I usually call the left corner of the mouth,' writes Kepler (*Conversation*, op. cit., p. 28), and then proceeds to make conjectures as to its origin (conscious efforts by intelligent beings included).

48. Contemporary academic optics went beyond simple geometrical constructions (which Galileo may have known) and included an account of *what is seen* when looking at a mirror, or through a lens, or a combination of lenses. Excepting irradiation Galileo nowhere considers the properties of telescopic *vision*. Aristotelians writing after Galileo's telescopic observations did. Cf. Redondi, op. cit., pp. 169ff.

49. I have here disregarded the work of della Porta (*De Refractione*) and of Maurolycus who both anticipated Kepler in certain respects (and are duly mentioned by him). Maurolycus makes the important step [*Photismi de Lumine*, transl. Henry Crew, New York, 1940, p. 45 (on mirrors) and p. 74 (on lenses)] of considering only the cusp of the caustic; but a connection with what is seen on *direct* vision is still not established. For the difficulties which were removed by Kepler's simple and ingenious hypothesis cf. Ronchi, *Histoire de la Lumière*, Chapter III.

intersection of the rays of vision from both eyes'[50] or, in the case of monocular vision, from the two sides of the pupil.[51] This rule, which proceeds from the assumption that 'the image is the work of the act of vision', is partly empirical and partly geometrical. It bases the position of the image on a 'metrical triangle'[52] or a 'telemetric triangle'[53] as Ronchi calls it,[54] that is constructed out of the rays which finally arrive at the eye and is used by the eye *and the mind* to place the image at the proper distance. Whatever the optical system, whatever the total path of the rays from the object to the observer, the rule says that the mind of the observer utilizes its *very last part only* and bases its visual judgement, the perception, on it.

The rule considerably simplified the science of optics. However, it needs only a second to show that it is false: take a magnifying glass, determine its focus, and look at an object close to it. The telemetric triangle now reaches beyond the object to infinity. A slight change of distance brings the Keplerian image from infinity to close by and back to infinity. No such phenomenon is ever observed. We see the image, slightly enlarged, in a distance that is most of the time identical with the actual distance between the object and the lens. The visual distance of the image remains constant, however much we may vary the distance between lens and object and even when the image becomes distorted and, finally, diffuse.[55]

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50. *Werke*, II, p. 72. The *Optics* of 1604 has been partly translated into German by F. Plehn, *J. Keplers Grundlagen der geometrischen Optik*, Leipzig, 1922. The relevant passages occur in section 2 of Chapter 3, pp. 38–48.

51. ibid., p. 67.

52. 'Cum imago sit visus opus', ibid., p. 64. 'In visione tenet sensus communis oculorum suorum distantiam ex assuefactione, angulos vero ad illam distantiam notat ex sensu contortionis oculorum', ibid., p. 66.

53. 'Triangulum distantiae mensorium', ibid., p. 67.

54. *Optics*, op. cit., p. 44. One should also consult the second chapter of this book for the history of pre-Keplerian optics.

55. ibid., pp. 182, 202. This phenomenon was known to everyone who had used a magnifying glass only once, Kepler included. Which shows that disregard of familiar phenomena does not entail that the phenomena were seen differently (cf. text to footnote 44 to this chapter). Isaac Barrow's account of the difficulty of Kepler's rule was mentioned above (text to footnote 18 to Chapter 5). According to Berkeley (op. cit., p. 141) 'this phenomenon ... entirely subverts the opinion of those who will have us judge of distances by lines and angles. ...' Berkeley replaces this opinion by his own theory according to which the mind judges distances from the clarity or confusion of the primary impressions. Kepler's idea of the telemetric triangle was adopted at once by almost all thinkers in the field. It was given a fundamental position by Descartes according to whom 'Distantiam ... discimus, per mutuam quandam conspirationem oculorum' (*Dioptrice*, quoted from *Renati Descartes Specima Philosophiae*, Amsterdam, 1657, p. 87). 'But,' says Barrow, 'neither this nor any other difficulty shall ... make me renounce that which I know to be manifestly agreeable to reason.' It is this attitude

This, then, was the actual situation in 1610 when Galileo published his telescopic findings. How did Galileo react to it? The answer has already been given: he raised the telescope to the state of a 'superior and better sense'.[56] What were his reasons for doing

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which was responsible for the slow advance of a scientific theory of *eye glasses* and of visual optics in general. 'The reason for this peculiar phenomenon,' writes Moritz von Rohr (*Das Brillenglas als optisches Instrument*, Berlin, 1934, p. 1), 'is to be sought in the close connection between the eye glass and the eye and it is impossible to give an acceptable theory of eye glasses without understanding what happens in the process of vision itself....' The telemetric triangle omits precisely this process, or rather gives a simplistic and false account of it. The state of optics at the beginning of the 20th century is well described in A. Gullstrand's 'Appendices to Part I' of Helmholtz's *Treatise on Physiological Optics*, transl. Southall, New York, 1962, pp. 261ff. We read here how a return to the psycho-physiological process of vision enabled physicists to arrive at a more reasonable account even of the physics of optical imagery: 'The reason why the laws of actual optical imagery have been, so to speak, summoned to life by the requirements of physiological optics is due partly to the fact that by means of trigonometrical calculations, tedious to be sure, but easy to perform, it has been possible for the optical engineer to get closer to the realities of his problem. Thus, thanks to the labours of such men as Abbe and his school, technical optics has attained its present splendid development; whereas, with the scientific means available, a comprehensive grasp of the intricate relations in the case of the imagery in the eye has been actually impossible.'

56. 'O Nicholas Copernicus, what a pleasure it would have been for you to see this part of your system confirmed by so clear an experiment!' writes Galileo, implying that the new telescopic phenomena are additional support for Copernicus (*Dialogue*, op. cit., p. 339). The difference in the appearance of planets and fixed stars (cf. footnote 26 to this chapter) he explains by the hypothesis that 'the very instrument of seeing [the eye] introduces a hindrance of its own' (ibid., p. 335), and that the telescope removes this hindrance, viz. *irradiation*, permitting the eye to see the stars and the planets as they really are. (Mario Giuducci, a follower of Galileo, ascribed irradiation to refraction by moisture on the surface of the eye, *Discourse on the Comets of 1618*, op. cit., p. 47.) This explanation, plausible as it may seem (especially in view of Galileo's attempt to show how irradiation can be removed by means other than the telescope) is not as straightforward as one might wish. Gullstrand (op. cit., p. 426) says that 'owing to the properties of the wave surface of the bundle of rays refracted in the eye ... it is a mathematical impossibility for any cross section to cut the caustic surface in a smooth curve in the form of a circle concentric with the pupil'. Other authors point to 'inhomogeneities in the various humours, and above all in the crystalline lens' (Ronchi, *Optics*, op. cit., p. 104). Kepler gives this account (*Conversation*, op. cit., pp. 33ff): 'Point sources of light transmit their cones to the crystalline lens. There refraction takes place, and behind the lens the cones again contract to a point. But this point does not reach as far as the retina. Therefore, the light is dispersed once more, and spreads over a small area of retina, whereas it should impinge on a point. Hence the telescope, by introducing another refraction, makes this point coincide with the retina....' Polyak, in his classical work *The Retina*, attributes irradiation partly to 'defects of the dioptrical media and to the imperfect accommodation' but 'chiefly' to the 'peculiar structural constitution of the retina itself' (p. 176), adding that it may be a function of the brain also (p. 429). None of these hypotheses covers *all* the facts known about irradiation. Gullstrand, Ronchi, and Polyak (if we omit his reference to the brain

so? This question brings me back to the problems raised by the evidence (against Copernicus) that was reported and discussed in Chapter 8.

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which can be made to explain anything we want) cannot explain the disappearance of irradiation in the telescope. Kepler, Gullstrand and Ronchi also fail to give an account of the fact, emphasized by Ronchi, that large objects show no irradiation at their edges ('Anyone undertaking to account for the phenomenon of irradiation must admit that when he looks at an electric bulb from afar so that it seems like a point, he sees it surrounded by an immense crown of rays whereas from nearby he sees nothing at all around it,' *Optics*, op. cit., p. 105). We know now that large objects are made definite by the lateral inhibitory interaction of retinal elements (which is further increased by brain function), cf. Ratliff, *Mach Bands*, p. 146, but the variation of the phenomenon with the diameter of the object and under the conditions of telescopic vision remains unexplored. Galileo's hypothesis received support mainly from its agreement with the Copernican point of view and was, therefore, largely *ad hoc*.

10

On the other hand, there are some telescopic phenomena which are plainly Copernican. Galileo introduces these phenomena as independent evidence for Copernicus while the situation is rather that one refuted view – Copernicanism – has a certain similarity to phenomena emerging from another refuted view – the idea that telescopic phenomena are faithful images of the sky.

According to the Copernican theory, Mars and Venus approach and recede from the earth by a factor of 1:6 or 1:8, respectively. (These are approximate numbers.) Their change of brightness should be 1:40 and 1:60, respectively (these are Galileo's values). Yet Mars changes very little and the variation in the brightness of Venus 'is almost imperceptible'.[1] These experiences 'overtly contradict the annual movement [of the earth]'.[2] The telescope, on the other hand, produces new and strange *phenomena*, some of them exposable as illusory by observation with the naked eye, some contradictory, some having even the appearance of being illusory, while the only *theory* that could have brought order into this chaos, Kepler's theory of vision, is refuted by evidence of the plainest kind possible. But – and with this I come to what I think is a central feature of Galileo's procedure – *there are telescopic phenomena*, namely the telescopic variation of the brightness of the planets, *which agree more closely with Copernicus than do the results of naked-eye observation*. Seen through the telescope, Mars does indeed change as it should according to the Copernican view. Compared with the total performance of the telescope this change is still quite puzzling. It is just as puzzling as is the Copernican theory when compared with the pre-telescopic evidence. But the change is in harmony with the predictions of Copernicus. *It is this harmony* rather than any deep understanding of

1. The actual variations of Mars and Venus are four magnitudes and one magnitude respectively.
2. *Dialogue*, op. cit., p. 328.

cosmology and of optics *which for Galileo proves Copernicus and the veracity of the telescope* in terrestrial *as well as* celestial matters. And it is this harmony on which he builds an entirely new view of the universe. 'Galileo,' writes Ludovico Geymonat,[3] referring to this aspect of the situation, 'was not the first to turn the telescope upon the heavens, but ... he was the first to grasp the enormous interest of the things thus seen. And he understood at once that these things fitted in perfectly with the Copernican theory whereas they contradicted the old astronomy. Galileo had believed for years in the truth of Copernicanism, but he had never been able to demonstrate it despite his exceedingly optimistic statements to friends and colleagues [he had not even been able to remove the refuting instances, as we have seen, and as he says himself]. Should direct proof [should even mere *agreement* with the evidence] be at last sought here? The more this conviction took root in his mind, the clearer to him became the importance of the new instrument. In Galileo's own mind faith in the reliability of the telescope and recognition of its importance were not *two separate acts*, rather, they were *two aspects of the same process*.' Can the absence of independent evidence be expressed more clearly? 'The *Nuncius*', writes Franz Hammer in the most concise account I have read of the matter,[4] 'contains two unknowns, the one being solved with the help of the other.' This is entirely correct, except that the 'unknowns' were not so much unknown as known to be false, as Galileo on occasions says himself. It is this rather peculiar situation, this harmony between two interesting but refuted ideas, which Galileo exploits in order to prevent the elimination of either.

Exactly the same procedure is used to preserve his new dynamics. We have seen that this science, too, was endangered by observable events. To eliminate the danger Galileo introduces friction and other disturbances with the help of *ad hoc* hypotheses, treating them as tendencies *defined* by the obvious discrepancy between fact and theory rather than as physical events *explained* by a theory of friction for which new and independent evidence might some day become

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3. op. cit., pp. 38ff (my italics).

4. *Johannes Kepler*, *Gesammelte Werke*, op. cit., Vol. IV, p. 447. Kepler (*Conversation*, op. cit., p. 14) speaks of 'mutually self-supporting evidence'. Remember, however, that what is 'mutually self-supporting' are two refuted hypotheses and *not* two hypotheses which have *independent support* in the domain of basic statements. In a letter to Herwarth of 26 March 1598, Kepler speaks of the 'many reasons' he wants to adduce for the motion of the earth, adding that 'each of these reasons, taken for itself, would find only scant belief (Caspar–Dyck, *Johannes Kepler in seinen Briefen*, Vol. 1, Munich, 1930, p. 68).

available (such a theory arose only much later, in the 18th century). Yet the agreement between the new dynamics and the idea of the motion of the earth, which Galileo increases with the help of his method of *anamnesis*, makes both seem more reasonable.

The reader will realize that a more detailed study of historical phenomena such as these creates considerable difficulties for the view that the transition from the pre-Copernican cosmology to that of the 17th century consisted in the replacement of refuted theories by more general conjectures which explained the refuting instances, made new predictions, and were corroborated by observations carried out to test these new predictions. And he will perhaps see the merits of a different view which asserts that, while the pre-Copernican astronomy *was in trouble* (was confronted by a series of refuting instances and implausibilities), the Copernican theory *was in even greater trouble* (was confronted by even more drastic refuting instances and implausibilities); but that being in harmony *with still further inadequate theories* it gained strength, and was retained, the refutations being made ineffective by *ad hoc* hypotheses and clever techniques of persuasion. This would seem to be a much more adequate description of the developments at the time of Galileo than is offered by almost all alternative accounts.

I shall now interrupt the historical narrative to show that the description is not only *factually adequate*, but that it is also *perfectly reasonable*, and that any attempt to enforce some of the more familiar methodologies of the 20th century would have had disastrous consequences.

11

Such 'irrational' methods of support are needed because of the 'uneven development' (Marx, Lenin) of different parts of science. Copernicanism and other essential ingredients of modern science survived only because reason was frequently overruled in their past.

A prevalent tendency in philosophical discussions is to approach problems of knowledge *sub specie aeternitatis*, as it were. Statements are compared with each other without regard to their history and without considering that they might belong to different historical strata. For example, one asks: given background knowledge, initial conditions, basic principles, accepted observations – what conclusions can we draw about a newly suggested hypothesis? The answers vary considerably. Some say that it is possible to determine degrees of confirmation and that the hypothesis can be evaluated with their help. Others reject any logic of confirmation and judge hypotheses by their content, and by the falsifications that have actually occurred. But almost everyone takes it for granted that precise observations, clear principles and well-confirmed theories *are already decisive*; that they can and must be used *here and now* to either eliminate the suggested hypothesis, or to make it acceptable, or perhaps even to prove it.

Such a procedure makes sense only if we can assume that the elements of our knowledge – the theories, the observations, the principles of our arguments – are *timeless entities* which share the same degree of perfection, are all equally accessible, and are related to each other in a way that is independent of the events that produced them. This is, of course, an extremely common assumption. It is taken for granted by most logicians; it underlies the familiar distinction between a context of discovery and a context of justification; and it is often expressed by saying that science deals with propositions and not with statements or sentences. However, the procedure overlooks that science is a complex and heterogeneous *historical process* which contains vague and incoherent anticipations of future ideologies side by side with highly sophisticated theoretical systems and ancient and petrified forms of thought. Some of its elements are available in the form of neatly written statements while others are submerged and become known only by contrast, by comparison with new and unusual views. (This is the way in which the inverted tower argument helped Galileo to discover the natural interpretations hostile to Copernicus. And this is also the way in which Einstein discovered certain deep-lying assumptions of classical mechanics, such as the assumption of the existence of infinitely fast signals. For general considerations, cf. the last paragraph of Chapter 5.) Many of the conflicts and contradictions which occur in science are due to this heterogeneity of the material, to this 'unevenness' of the historical development, as a Marxist would say, and they have no immediate theoretical significance.[1] They have much in common with the problems which arise when a power

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1. According to Marx, 'secondary' parts of the social process, such as demand, artistic production or legal relations, may get ahead of material production and drag it along: cf. *The Poverty of Philosophy* but especially the *Introduction to the Critique of Political Economy*, Chicago, 1918, p. 309: 'The unequal relation between the development of material production and art, for instance. In general, the conception of progress is not to be taken in the sense of the usual abstraction. In the case of art, etc., it is not so important and difficult to understand this disproportion as in that of practical social relations, e.g. the relation between education in the U.S. and Europe. The really difficult point, however, that is to be discussed here is that of the unequal development of relations of production as legal relations.' Trotsky describes the same situation: 'The gist of the matter lies in this, that the different aspects of the historical progress – economics, politics, the state, the growth of the working class – do not develop simultaneously along parallel lines' ('The School of Revolutionary Strategy', speech delivered at the general party membership meeting of the Moscow Organization of July 1921, published in *The First Five Years of the Communist International*, Vol. II, New York, 1953, p. 5). See also Lenin, *Left-Wing Communism – an Infantile Disorder* (op. cit., p. 59), concerning the fact that multiple causes of an event may be out of phase and have an effect only when they occur together. In a different form, the thesis of 'uneven development' deals with the fact that capitalism has reached different stages in different countries, and even in different parts of the same country. This second type of uneven development may lead to inverse relations between the accompanying ideologies, so that efficiency in production and radical political ideas develop in inverse proportions. 'In civilized Europe, with its highly developed machine industry, its rich, multiform culture and its constitutions, a point of history has been reached when the commanding bourgeoisie, fearing the growth and increasing strength of the proletariat, comes out in support of everything backward, moribund, and medieval. . . . But all young Asia grows a mighty democratic movement, spreading and gaining in strength' (Lenin, 'Backward Europe and Advanced Asia', *Collected Works*, Vol. 19, op. cit., pp. 99ff). For this very interesting situation, which deserves to be exploited for the philosophy of science, cf. A.C. Meyer, *Leninism*, Cambridge, 1957, Chapter 12 and L. Althusser, *For Marx*, London and New York, 1970, Chapters 3 and 6. The philosophical background is splendidly explained in Mao Tse-tung's essay *On Contradiction* (*Selected Readings*, Peking, 1970, p. 70, especially section IV).

station is needed right next to a Gothic cathedral. Occasionally, such features are taken into account; for example, when it is asserted that physical laws (statements) and biological laws (statements) belong to different conceptual domains and cannot be directly compared. But in most cases, and especially in the case observation vs theory, our methodologies project the various elements of science and the different historical strata they occupy on to one and the same plane, and proceed at once to render comparative judgements. This is like arranging a fight between an infant and a grown man, and announcing triumphantly, what is obvious anyway, that the man is going to win (the history of science is full of inane criticisms of this kind and so is the history of psychoanalysis and of Marxism). In our examination of new hypotheses we must obviously take the historical situation into account. Let us see how this is going to affect our judgement!

The geocentric hypothesis and Aristotle's theory of knowledge and perception are well adapted to each other. Perception supports the theory of locomotion that entails the unmoved earth and it is in turn a special case of a comprehensive view of motion that includes locomotion, increase and decrease, qualitative alteration, generation and corruption. This comprehensive view defines motion as the transition of a form from an agent to a patient which terminates when the patient possesses exactly the same form that characterized the agent at the beginning of the interaction. Perception, accordingly, is a process in which the form of the object perceived enters the percipient as precisely the same form that characterized the object so that the percipient, in a sense, assumes the properties of the object.

A theory of perception of this kind (which one might regard as a sophisticated version of naive realism) does not permit any major discrepancy between observations and the things observed. 'That there should be things in the world which are inaccessible to man not only now, and for the time being, but in principle, and because of his natural endowment, and which would therefore never be seen by him – this was quite inconceivable for later antiquity as well as for the Middle Ages.'[2] Nor does the theory encourage the use of

2. F. Blumenberg, *Galileo Galilei, Sidereus Nuncius, Nachricht von neuen Sternen*, Vol. 1, Frankfurt, 1965, p. 13. Aristotle himself was more open-minded: 'The evidence (concerning celestial phenomena) is furnished but scantily by sensations, whereas respecting perishable plants and animals we have abundant information, living as we do in their midst . . .', *De Part. Anim.*, 644b26ff. In what follows, a highly idealized account is given of later Aristotelianism. Unless otherwise stated, the word 'Aristotle' refers to this idealization. For the difficulties in forming a coherent picture of Aristotle *himself* cf. Düring, *Aristoteles*, Heidelberg, 1966. For some differences between Aristotle and his mediaeval followers cf. Wolfgang Wieland, *Die Aristotelische Physik*, Göttingen, 1970.

instruments, for they interfere with the processes in the medium. These processes carry a true picture only as long as they are left undisturbed. Disturbances create forms which are no longer identical with the shape of the objects perceived – they create *illusions*. Such illusions can be readily demonstrated by examining the images produced by curved mirrors,[3] or by crude lenses (and remember that the lenses used by Galileo were far from the level of perfection achieved today): they are distorted, the lens-images have coloured fringes, they may appear at a place different from the place of the object and so on. Astronomy, physics, psychology, epistemology– all these disciplines collaborate with the Aristotelian philosophy to create a system that is coherent, rational and in agreement with the results of observation as can be seen from an examination of Aristotelian philosophy in the form in which it was developed by some mediaeval philosophers. Such an analysis shows the inherent power of the Aristotelian system.

The role of observation in Aristotle is quite interesting. Aristotle is an empiricist. His injunctions against an overly-theoretical approach are as militant as those of the 'scientific' empiricists of the 17th and 18th centuries. But while the latter take both the truth and the content of empiricism for granted, Aristotle explains the nature of experience and why it is important. Experience is what a normal observer (an observer whose senses are in good order and who is not drunk or sleepy, etc.) perceives under normal circumstances (broad daylight; no interference with the medium) and describes in an idiom that fits the facts and can be understood by all. Experience is *important for knowledge* because, given normal circumstances, the perceptions of the observer contain identically the same forms that reside in the object. Nor are these explanations *ad hoc*. They are a direct consequence of Aristotle's general theory of motion, taken in conjunction with the physiological idea that sensations obey the same physical laws as does the rest of the universe. And they are confirmed by the evidence that confirms either of these two views (the existence of distorted lens-images being part of the evidence). We understand today a little better why a theory of motion and perception which is now regarded as false could be so successful (evolutionary explanation of the adaptation of organisms; movement in media).

3. Already a plain mirror gives rise to an interesting illusion. To notice it, first look at yourself in a plain mirror. You will see your face at its 'normal' size. Then let some steam condense on the surface of the mirror and draw the outline of your face in the steam. The outline will look about half the size of your face.

The fact remains that no decisive empirical argument could be raised against it (though it was not free from difficulties).

This harmony between human perception and the Aristotelian cosmology is regarded as illusory by the supporters of the motion of the earth. In the view of the Copernicans there exist large-scale processes which involve vast cosmic masses and yet *leave no trace* in our experience. The existent observations therefore count no longer as tests of the new basic laws that are being proposed. They are not directly attached to these laws, and they may be entirely disconnected. *Today, after* the success of modern science led to the belief that the relation between man and the universe is not as simple as is assumed by naive realism, we can say that this was a correct guess, that the observer is indeed separated from the laws of the world by the special physical conditions of his observation platform, the moving earth (gravitational effects; law of inertia; Coriolis forces; influence of the atmosphere upon optical observations; aberration; stellar parallax; and so on...), by the idiosyncrasies of his basic instrument of observation, the human eye (irradiation; after-images; mutual inhibition of adjacent retinal elements; and so on...) as well as by older views which have invaded the observation language and made it speak the language of naive realism (natural interpretations). Observations may contain a contribution from the thing observed, but this contribution merges with other effects (some of which we have just mentioned), and it may be completely obliterated by them. Just consider the image of a fixed star as viewed through a telescope. This image is displaced by the effects of refraction, aberration and, possibly, of gravitation. It contains the spectrum of the star not as it is now, but as it was some time ago (in the case of extra-galactic supernovae the difference may be millions of years), and distorted by Doppler effect, intervening galactic matter, etc. Moreover, the extension and the internal structure of the image is entirely determined by the telescope and the eyes of the observer: it is the telescope that decides how large the diffraction disks are going to be, and it is the human eye that decides how much of the structure of these disks is going to be seen. It needs considerable skill *and much theory* to isolate the contribution of the original cause, the star, and to use it for a test, but this means that non-Aristotelian cosmologies can be tested only after we have *separated* observations and laws with the help of auxiliary sciences describing the complex processes that occur between the eye and the object, and the even more complex processes between the cornea and the brain. We must *subdivide* what we perceive to find a core that mirrors the stimulus and nothing else. In the case of Copernicus we need a new *meteorology* (in the good old

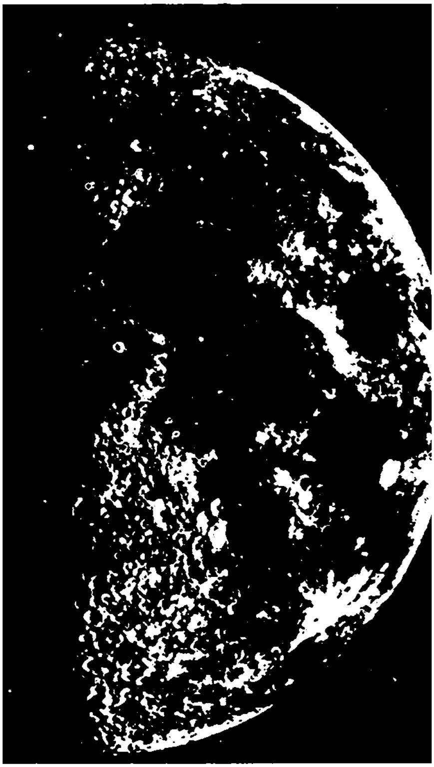

FIGURE 2. Moon, age seven days (first quarter).

sense of the word, as dealing with things below the moon), a new science of *physiological optics* that deals with the subjective (mind) and the objective (light, medium, lenses, structure of the eye) aspects of vision as well as a new *dynamics* stating the manner in which the motion of the earth might influence the physical processes at its surface. Observations become relevant only *after* the processes described by these new subjects have been inserted between the world and the eye. The language in which we express our observations may have to be revised as well so that the new cosmology receives a fair chance and is not endangered by an unnoticed collaboration of sensations and older ideas. In sum: *what is needed for a test of Copernicus is an entirely new world-view containing a new view of man and of his capacities of knowing.*[4]

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4. Bacon realized that scientific change involves a reformation not only of a few ideas, but of an entire world-view and, perhaps, of the very nature of humans. 'For the senses are weak and erring', he writes in *Novum Organum*, Aphorism 50. 'For man's sense is falsely asserted to be the standard of things; on the contrary, all the perceptions, both of the senses and of the mind bear reference to man and not to the universe, and the human mind resembles those uneven mirrors which impart their own properties to different objects from which rays are emitted and distort and disfigure them' (Aphorism 41). Bacon repeatedly comments on the 'dullness, incompetency and errors of the senses' (50) and permits them only to 'judge the experiment' while it is the experiment that functions as a judge 'of nature and the thing itself (50). Thus when Bacon speaks of the 'unprejudiced senses' he does not mean sense-data, or immediate impressions, but reactions of a sense organ *that has been rebuilt* in order to mirror nature in the right way. Research demands *that the entire human being be rebuilt.* This idea of a physical and mental reform of humanity has religious features. A 'demolishing branch' (115), an 'expiatory process', a 'purification of the mind' (69) must precede the accumulation of knowledge. 'Our only hope of salvation is to begin the whole labour of the mind again' (Preface) but only 'after having cleansed, polished, and levelled its surface' (115). Preconceived notions (36), opinions (42ff), even the most common words (59, 121) 'must be abjured and renounced with firm and solemn resolution so that the access to the kingdom of man, which is founded on the sciences, may resemble that to the kingdom of heaven, where no admission is conceded except to children' (68).

A reform of man is necessary for a correct science – but it is not sufficient. Science, according to Bacon, not only orders events, it is also supposed to give physical reasons. Thus Ptolemy and Copernicus give us 'the number, situation, motion, and periods of the stars, as a beautiful outside of the heavens, whilst the flesh and the entrails are wanting; that is, a well fabricated system, or the physical reasons and foundations for a just theory, that should not only solve phenomena, as almost any ingenious theory may do, but show the substance, motions and influences of the heavenly bodies as they really are.' *Advancement of Learning*, Chapter 4, quoted from Wiley Books, New York, 1944, p. 85. Cf. also the *Novum Organum*, op. cit., p. 371: 'For let no one hope to determine the question whether the earth or heaven revolve in the diurnal motion, unless he have first comprehended the nature of spontaneous motion': the new man needs a new physics in order to give substance to his astronomy. Galileo did not succeed in providing such a physics.

It is obvious that such a new world-view will take a long time appearing, and that we may never succeed to formulate it in its entirety. It is extremely unlikely that the idea of the motion of the earth will at once be followed by the arrival, in full formal splendour, of all the sciences that are now said to constitute the body of 'classical physics'. Or, to be a little more realistic, such a sequence of events is not only extremely unlikely, *it is impossible in principle*, given the nature of humans and the complexities of the world they inhabit. Today Copernicus, tomorrow Helmholtz – this is but a Utopian dream. Yet it is only *after* these sciences have arrived that a test can be said to make sense.

This need to *wait*, and to *ignore* large masses of critical observations and measurements, is hardly ever discussed in our methodologies. Disregarding the possibility that a new physics or a new astronomy might have to be judged by a new theory of knowledge and might require entirely new tests, empirically inclined scientists at once confront it with the *status quo* and announce triumphantly that 'it is not in agreement with facts and received principles'. They are of course right, and even trivially so, but not in the sense intended by them. For at an early stage of development the contradiction only indicates that the old and the new are *different* and *out of phase*. It does not show which view is the *better* one. A judgement of *this* kind presupposes that the competitors confront each other on equal terms. How shall we proceed in order to bring about such a fair comparison?

The first step is clear: we must *retain* the new cosmology until it has been supplemented by the necessary auxiliary sciences. We must retain it in the face of plain and unambiguous refuting facts. We may, of course, try to explain our action by saying that the critical observations are either not relevant or that they are illusory, but we cannot support such an explanation by a single objective reason. Whatever explanation we give is nothing but a *verbal gesture*, a gentle invitation to participate in the development of the new philosophy. Nor can we reasonably remove the received *theory* of perception which says that the observations are relevant, gives reasons for this assertion, and is confirmed by independent evidence. Thus the new view is arbitrarily separated from data that supported its predecessor

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Science-loving philosophers, including those who call themselves 'critical', are quick to criticize thinkers who do not share their pet ideas. Bacon was often criticized for not at once falling for Copernicus. He was criticized for this unspeakable crime by philosophers whose own 'rationalism' would never have allowed Copernicus to live. An example is K.R. Popper, *The Open Society and Its Enemies*, Vol. 2, p. 16.

and is made more 'metaphysical': a new period in the history of science commences with a *backward movement* that returns us to an earlier stage where theories were more vague and had smaller empirical content. This backward movement is not just an accident; it has a definite function; it is essential if we want to overtake the *status quo*, for it gives us the time and the freedom that are needed for developing the main view in detail, and for finding the necessary auxiliary sciences.[5]

This backward movement is indeed essential – but how can we persuade people to follow our lead? How can we lure them away from a well-defined, sophisticated and empirically successful system and make them transfer their allegiance to an unfinished and absurd hypothesis? To a hypothesis, moreover, that is contradicted by one observation after another if we only take the trouble to compare it with what is plainly shown to be the case by our senses? How can we convince them that the success of the *status quo* is only apparent and is bound to be shown as such in 500 years or more, when there is not a single argument on our side (and remember that the illustrations I used two paragraphs earlier derive their force from the successes of classical physics and were not available to the Copernicans).[6] It is clear that allegiance to the new ideas will have to be brought about by means other than arguments. It will have to be brought about by *irrational means* such as propaganda, emotion, *ad hoc* hypotheses, and appeal to prejudices of all kinds. We need these 'irrational means' in order to uphold what is nothing but a blind faith until we have found the auxiliary sciences, the facts, the arguments that turn the faith into sound 'knowledge'.

It is in this context that the rise of a new secular class with a new outlook and considerable contempt for the science of the schools, its methods, its results, even for its language, becomes so important. The barbaric Latin spoken by the scholars, the intellectual squalor of academic science, its other-worldliness which is soon interpreted as uselessness, its connection with the Church – all these elements are

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5. An example of a backward movement of this kind is Galileo's return to the kinematics of the *Commentariolus* and his disregard for the machinery of epicycles as developed in the *De Revol*. For an admirable *rational* account of this step cf. Imre Lakatos and Eli Zahar, 'Why Did Copernicus' Research Programme Supersede Ptolemy's?', in *Imre Lakatos, Philosophical Papers* Vol. I, Cambridge 1978.

6. They were available to the sceptics, especially to Aenesidemus, who points out, following Philo, that no object appears as it is but is modified by being combined with air, light, humidity, heat, etc.; cf. *Diogenes Laertius*, IX, 84. However, it seems that the sceptical view had only little influence on the development of modern astronomy, and understandably so: one does not start a movement by being reasonable.

now lumped together with the Aristotelian cosmology and the contempt one feels for them is transferred to every single Aristotelian argument.[7] This guilt-by-association does not make the arguments less *rational*, or less conclusive, *but it reduces their influence* on the minds of those who are willing to follow Copernicus. For Copernicus now stands for progress in other areas as well, he is a symbol for the ideals of a new class that looks back to the classical times of Plato and Cicero and forward to a free and pluralistic society. The association of astronomical ideas and historical and class tendencies does not produce new arguments either. But it engenders a firm commitment to the heliocentric view – and this is all that is needed at this stage, as we have seen. We have also seen how masterfully Galileo exploits the situation and how he amplifies it by tricks, jokes, and *non-sequiturs* of his own.[8]

We are here dealing with a situation that must be analysed and understood if we want to adopt a more reasonable attitude towards the issue between 'reason' and 'irrationality'. Reason grants that the ideas which we introduce in order to expand and to improve our

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7. For these social pressures cf. Olschki's magnificent *Geschichte der neusprachlichen wissenschaftlichen Literatur*. For the role of Puritanism cf. R.F. Jones, op. cit., Chapters V and VI.

8. In a remarkable book, *Galileo Heretic*, Princeton, 1987 (first published, in Italian, in 1982), Pietro Redondi has described the groups both inside the Church (and including the Pope himself) and outside of it who looked favourably upon new scientific developments, the views on perception, continuity, matter and motion that had been explained by Galileo in his *Assayer* among them. Being in direct conflict with the traditional account of the Eucharist, the most important sacrament, these views were considerably more dangerous than Copernicanism and could be tolerated only as long as the groups and the Pope himself had the upper hand in the complex political developments of the time (Thirty Years' War; French and Spanish politics; the French alliance with the Pope). The political reversal of the Pope's fortunes, the accusations of leniency towards heretics that were raised against him on political grounds cast a shadow on his attitude towards scientific matters as well (here, too, he seemed to support heresy) and made protective measures necessary. Redondi tries to show (a) that the physics of the time was connected with theological doctrines such as the doctrine of the Eucharist and that a history of science that neglects the connection becomes incomprehensible and (b) that the attitude towards scientific problems caused by the connection and thus the attitude towards innovation changed with the political climate. The second part of (b) may well be true but there is only weak evidence to support the rest: what Galileo says about atomism in the *Assayer* is much too brief and indefinite to conflict with transsubstantiation (it is an aside almost, not an elaborate statement) and with the exception of a rather problematic document no such conflict was perceived. (Cf. R.S. Westfall, *Essays on the Trial of Galileo*, Vatican Observatory Publications, 1989, pp. 84ff.) What is valuable in Redondi's account is that he widens the domain of possible influences and thus undermines the (anachronistic) belief that then as now scientific rationality was restricted to the internal problem situation of a scientific discipline.

knowledge may *arise* in a very disorderly way and that the *origin* of a particular point of view may depend on class prejudice, passion, personal idiosyncrasies, questions of style, and even on error, pure and simple. But it also demands that in *judging* such ideas we follow certain well-defined rules: our *evaluation* of ideas must not be invaded by irrational elements. Now, what our historical examples seem to show is this: there are situations when our most liberal judgements and our most liberal rules would have eliminated a point of view which we regard today as essential for science, and would not have permitted it to prevail – and such situations occur quite frequently. The ideas survived and they *now* are said to be in agreement with reason. They survived because prejudice, passion, conceit, errors, sheer pigheadedness, in short because all the elements that characterize the context of discovery, *opposed* the dictates of reason *and because these irrational elements were permitted to have their way*. To express it differently: *Copernicanism and other 'rational' views exist today only because reason was overruled at some time in their past.* (The opposite is also true: witchcraft and other 'irrational' views have *ceased* to be influential only because reason was overruled at some time in *their* past.)[9]

Now, assuming that Copernicanism is a Good Thing, we must also admit that its survival is a Good Thing. And, considering the conditions of its survival, we must further admit that it was a Good Thing that reason was overruled in the 16th, 17th and even the 18th centuries. Moreover, the cosmologists of the 16th and 17th centuries did not have the knowledge we have today, they did not know that Copernicanism was capable of giving rise to a scientific system that is acceptable from the point of view of 'scientific method'. They did not know which of the many views that existed at their time would lead to future reason when defended in an 'irrational' way. Being without such guidance they had to make a guess and in making this guess they could only follow their inclinations, as we have seen. Hence it is advisable to let one's inclinations go against reason *in any circumstances*, for it makes life less constrained and science may profit from it.

9. These considerations refute J. Dorling, who, in *British Journal for the Philosophy of Science*, Vol. 23, 1972. 189f, presents my 'irrationalism' as a presupposition of my research, not as a result. He continues: '...one would have thought that the philosopher of science would be most interested in picking out and analysing in detail those scientific arguments which did seem to be rationally reconstructible.' One would have thought that the philosopher of science would be most interested in picking out and analysing in detail those moves which are necessary for the *advancement* of science. Such moves, I have tried to show, often resist rational reconstruction.

It is clear that this argument, that advises us not to let reason overrule our inclinations and occasionally to suspend reason altogether, does not depend on the historical material which I have presented. If my account of Galileo is historically correct, then the argument stands as formulated. If it turns out to be a fairy-tale, then this fairy-tale tells us that a conflict between reason and the preconditions of progress is *possible*, it indicates how it might arise, and it forces us to conclude that our chances to progress *may* be obstructed by our desire to be rational. And note that progress is here defined as a rationalistic lover of science would define it, i.e. as entailing that Copernicus is better than Aristotle and Einstein better than Newton. Of course, there is no need to accept this definition, which is certainly quite narrow. I use it only to show that an idea of reason accepted by the majority of rationalists may prevent progress as defined by the very same majority. I now resume the discussion of some details of the transition from Aristotle to Copernicus.

The first step on the way to a new cosmology, I have said, is a step *back*: apparently relevant evidence is pushed aside, new data are brought in by *ad hoc* connections, the empirical content of science is drastically reduced. Now the cosmology that happens to be at the centre of attention and whose adoption causes us to carry out the changes just described differs from other views in one respect only: it has features which at the time in question seem attractive to some people. But there is hardly any idea that is totally without merit and that might not also become the starting point of concentrated effort. No invention is ever made in isolation, and no idea is, therefore, completely without (abstract or empirical) support. Now if partial support and partial plausibility suffice to start a new trend – and I have suggested that they do – if starting a new trend means taking a step back from the evidence, if any idea can become plausible and can receive partial support, then the step back is in fact a step forward, and away from the tyranny of tightly-knit, highly corroborated, and gracelessly presented theoretical systems. 'Another different error', writes Bacon on precisely this point,[10] 'is the ... peremptory reduction of knowledge into arts and methods, from which time the sciences are seldom improved; for as young men rarely grow in stature after their shape and limbs are fully formed, so knowledge, whilst it lies in aphorisms and observations, remains in a growing

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10. *Advancement of Learning* (1605 edition), New York, 1944, p. 21. Cf. also the *Novum Organum*, Aphorisms 79, 86, as well as J.W.N. Watkins' splendid little book *Hobbes' System of Ideas*, London, 1965, p. 169.

state; but when once fashioned into methods, though it may be further polished, illustrated and fitted for use, is no longer increased in bulk and substance.'

The similarity with the arts which has often been asserted arises at exactly this point. Once it has been realized that a close empirical fit is no virtue and that it must be relaxed in times of change, then style, elegance of expression, simplicity of presentation, tension of plot and narrative, and seductiveness of content become important features of our knowledge. They give life to what is said and help us to overcome the resistance of the observational material.[11] They *create* and maintain interest in a theory that has been partly removed from the observational plane and would be inferior to its rivals when judged by the customary standards. It is in this context that much of Galileo's work should be seen. This work has often been likened to *propaganda*[12] – and propaganda it certainly is. But propaganda of this kind is not a marginal affair that surrounds allegedly more substantial means of defence, and that should perhaps be avoided by the 'professionally honest scientist'. In the circumstances we are considering now, *propaganda is of the essence*. It is of the essence because interest must be created at a time when the usual methodological prescriptions have no point of attack; and because this interest must be maintained, perhaps for centuries, until new reasons arrive. It is also clear that such reasons, i.e. the appropriate auxiliary sciences, need not at once turn up in full formal splendour. They may at first be quite inarticulate, and may even conflict with the existing evidence. Agreement, or partial agreement, with the cosmology is all that is needed in the beginning. The agreement shows that they are at least *relevant* and that they may some day produce full-fledged positive evidence. Thus the idea that the telescope shows the world as it really is leads to many difficulties. But the support it lends to, and receives from, Copernicus is a hint that we might be moving in the right direction.

We have here an extremely interesting relation between a general view and the particular hypotheses which constitute its evidence. It is often assumed that general views do not mean much unless the relevant evidence can be fully specified. Carnap, for example, asserts that 'there is no independent interpretation for [the language in terms of which a certain theory or world-view is formulated]. The system *T* [the axioms of the theory and the rules of derivation] is itself

11. 'What restitutes to scientific phenomenon its life, is art' (*The Diary of Anaïs Nin*, Vol I, p. 277).

12. Cf. A. Koyré, *Etudes Galiléennes*, Vol. III, Paris, 1939, pp. 53ff.

an uninterpreted postulate system. [Its] terms obtain only an indirect and incomplete interpretation by the fact that some of them are connected by correspondence rules with observational terms.'[13] 'There is no independent interpretation,' says Carnap and yet an idea such as the idea of the motion of the earth, which was inconsistent with the contemporary evidence, which was upheld by declaring this evidence to be irrelevant and which was therefore cut from the most important facts of contemporary astronomy, managed to become a nucleus, a crystallization point for the aggregation of other inadequate views which gradually increased in articulation and finally fused into a new cosmology including new kinds of evidence. There is no better account of this process than the description which John Stuart Mill has left us of the vicissitudes of his education. Referring to the explanations which his father gave him on logical matters he writes: 'The explanations did not make the matter at all clear to me at the time; but they were not therefore useless; they remained as a nucleus for my observations and reflections to crystallize upon; the import of his general remarks being interpreted to me, by the particular instances which came under my notice *afterwards*.[14] In exactly the same manner the Copernican view, though devoid of cognitive content from the point of view of a strict empiricism or else refuted, was needed in the construction of the supplementary sciences even before it became testable with their help and even before it, in turn, provided them with supporting evidence of the most forceful kind.

There is a further element in this tapestry of moves, influences, beliefs which is rather interesting and which received attention only recently – the role of patronage. Today most researchers gain a reputation, a salary and a pension by being associated with a university and/or a research laboratory. This involves certain conditions such as an ability to work in teams, a willingness to subordinate one's ideas to those of a team leader, a harmony between one's ways of doing science and those of the rest of the profession, a certain style, a way of presenting the evidence – and so on. Not everyone fits conditions such as these; able people remain unemployed because they fail to satisfy some of them. Conversely the reputation of a university or a research laboratory rises with the

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13. 'The Methodological Character of Theoretical Concepts', *Minnesota Studies in the Philosophy of Science*, Vol. I, Minneapolis, p. 47.

14. *Autobiography*, quoted from *Essential Works of John Stuart Mill*, ed. Lerner, New York, 1965, p. 21.

reputation of its members. In Galileo's time patronage played a similar role. There were certain ways of gaining a patron and of keeping him. The patron in turn rose in estimation only if he succeeded to attract and to keep individuals of outstanding achievement. According to Westfall,[15] the Church pennitted the publication of Galileo's *Dialogue* in the full knowledge of the controversial matters contained in it '[n]ot least because a Pope [Urban VIII] who gloried in his reputation as a Maecenas, was unwilling to place it in jeopardy by saying no to the light of his times', and Galileo fell because he violated his side of the rules of patronage.[16]

Considering all these elements, the 'Rise of the Copernican World-View' becomes a complicated matter indeed. Accepted methodological rules are put aside because of social requirements (patrons need to be persuaded by means more effective than argument), instruments are used to redefine experience instead of being tested by it, local results are extrapolated into space despite reasons to the contrary, analogies abound – and yet all this turns out, in retrospect, to have been the correct way of circumventing the restrictions implied by the human condition. This is the material that should be used to get better insight into the complex process of knowledge acquisition and improvement.

To sum up the content of the last five chapters:

When the 'Pythagorean idea' of the motion of the earth was revived by Copernicus it met with difficulties which exceeded the difficulties encountered by contemporary Ptolemaic astronomy. Strictly speaking, one had to regard it as refuted. Galileo, who was convinced of the truth of the Copernican view and who did not share the quite common, though by no means universal, belief in a stable experience, looked for new kinds of fact which might support Copernicus and still be acceptable to all. Such facts he obtained in two different ways. First, by the invention of his *telescope*, which changed the *sensory core* of everyday experience and replaced it by puzzling and unexplained phenomena; and by his *principle of relativity and his dynamics*, which changed its *conceptual components*. Neither the telescopic phenomena

15. op. cit., p. 73.

16. Further details on these matters in Chapter 8, footnote 12 of the present essay, Westfall, op. cit., and M. Biagioli, *Galileo Courtier*. M. Finocchiaro, *Galileo and the Art of Reasoning*, Dordrecht, 1980, has commented on Galileo's use of rhetoric, while M. Pera and W.R. Shea (eds), *Persuading Science – The Art of Scientific Rhetoric*, 1991, and especially Marcello Pera, *Science and Rhetoric*, forthcoming, comment on scientific rhetoric in general.

nor the new ideas of motion were acceptable to common sense (or to the Aristotelians). Besides, the associated theories could be easily shown to be false. Yet these false theories, these unacceptable phenomena, were transformed by Galileo and converted into strong support of Copernicus. The whole rich reservoir of the everyday experience and of the intuition of his readers is utilized in the argument, but the facts which they are invited to recall are arranged in a new way, approximations are made, known effects are omitted, different conceptual lines are drawn, so that a *new kind of experience* arises, *manufactured* almost out of thin air. This new experience is then *solidified* by insinuating that the reader has been familiar with it all the time. It is solidified and soon accepted as gospel truth, despite the fact that its conceptual components are vastly more speculative than are the conceptual components of common sense. Following positivistic usage we may therefore say that Galileo's science rests on an *illustrated metaphysics*. The distortion permits Galileo to advance, but it prevents almost everyone else from making his effort the basis of a critical philosophy (for a long time emphasis was put either on his mathematics, or on his alleged experiments, or on his frequent appeal to the 'truth', and his propagandistic moves were altogether neglected). I suggest that what Galileo did was to let refuted theories support each other, that he built in this way a new world-view which was only loosely (if at all!) connected with the preceding cosmology (everyday experience included), that he established fake connections with the perceptual elements of this cosmology which are only now being replaced by genuine theories (physiological optics, theory of continua), and that whenever possible he replaced old facts by a new type of experience which he simply *invented* for the purpose of supporting Copernicus. Remember, incidentally, that Galileo's procedure drastically reduces the content of dynamics: Aristotelian dynamics was a general theory of change comprising locomotion, qualitative change, generation and corruption. Galileo's dynamics and its successors deal with locomotion only, other kinds of motion being pushed aside with the promissory note (due to Democritos) that locomotion will eventually be capable of comprehending *all* motion. Thus, a comprehensive empirical theory of motion is replaced by a much narrower theory plus a metaphysics of motion, just as an 'empirical' experience is replaced by an experience that contains speculative elements. This, I suggest, was the actual procedure followed by Galileo. Proceeding in this way he exhibited a style, a sense of humour, an elasticity and elegance, and an awareness of the valuable weaknesses of human thinking, which has never been equalled in the history of science. Here is an almost inexhaustible source of material for methodological speculation and, much more importantly, for the recovery of those features of knowledge which not only inform, but which also delight us.[17]

17. A few years ago Martin Gardner, the pitbull of scientism, published an article with the title 'Anti-Science, the Strange Case of Paul Feyerabend' *Critical Inquiry*, Winter 1982/83. The valiant fighter seems to have overlooked these and other passages. I am not against science. I praise its foremost practitioners and (next chapter) suggest that their procedures be adopted by philosophers. What I object to is narrow-minded philosophical interference and a narrow-minded extension of the latest scientific fashions to all areas of human endeavour – in short what I object to is a rationalistic interpretation and defence of science.