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METHODOLOGICAL ASPECTS OF GALILEO'S THOUGHT

The scientific thought of a past classic like Galileo can be examined from many points of view. Here I would like to discuss the interpretation of Galileo's *Dialogue* from the scientific and from the methodological points of view, as an illustration of a general problem that one encounters in the reading of past scientific works. The problem is one that involves a tension between scientific relevance and historical accuracy. I shall argue that in order to overcome it one must learn to identify the content of a past classic work from the point of view of scientific method, as distinct from the point of view of substantive scientific theory. In other words, classic scientific texts can be useful to scientists by providing models of how to proceed in appropriately similar circumstances, though they can be misleading when read merely as anticipations of present-day knowledge.

Scientists from Newton to Einstein have found Galileo's *Dialogue on the two Chief World Systems* to be rich in scientific content. It is well known, for example, that in the *Principia* Newton attributed to Galileo a knowledge of the law of inertia, the law of force, the principle of superposition, the law of squares, and the parabolic path of projectiles[1]; what is not well known is that it was not Galileo's *Two New Sciences* but his *Dialogue* that Newton had read[2]. It is also well known that, in his Foreword to Drake's translation of the *Dialogue,* Einstein summarizes its scientific content as being a result about the nonexistence of an abstract center of the universe, with analogies to his own work. According to Einstein, „Galileo

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[1] **I. Newton,** ***Mathematical Principles of Natural Philosophy,*** **Berkeley: University of California Press 1934, pp. 21—22.**

[2] **Cf. I. B. Cohen,** ***Newtons's Attribution of the First Two Laws of Motion to Galileo,*** **[in:]** ***Symposium Intemazionale di Storia, Metodologia, Logica e Filosofia della Scienza,*** **ed. by Academie Internationale d'Histoire des Sciences, Florence: Gruppo Italiano di Storia delle Scienze 1967, pp. XXVI and XXVIII.**

opposes the introduction of this ‘nothing’ (center of the universe) that is yet supposed to act on material bodies; he considers this quite unsatisfactory... [because] although it accounts for the spherical shape of the earth it does not explain the spherical shape of the other heavenly bodies”[1]. Einstein then suggests

> **...that a close analogy exists between Galileo’s rejection of the hypothesis of a center of the universe for the explanation of the fall of heavy bodies, and the rejection of the hypothesis of an intertial system for the explanation of the inertial behavior of matter. (The latter is the basis of the theory of general relativity.) Common to both hypothesis is the introduction of a conceptual object with the following properties:**
>
> **(1) . It is not assumed to be real, like ponderable matter (or a „field”).**
>
> **(2) . It determines the behavior of real objects, but it is in no way affected by them.**
>
> **The introduction of such conceptual elements, though not exactly inadmissable from a purely logical point of view, is repugnant to the scientific instinct[2].**

Typical of scientists’ attitude is perhaps the judgment expressed by Arthur Schuster in the 1916 Presidential Address to the British Association for the Advancement of Science: „Modern science began not at the date of this or that discovery, but on the day that Galileo decided to publish his *Dialogues* (1632)[3]”. The exact reason for this scientific popularity is not clear. In part, it may be that scientists feel very much at home in a work characterized by fluid and plastic thought, with which they can identify in some way. Such scientific intuitions, besides having value in themselves, fit very well with the latest biographical evidence, which suggests that the book constitutes Galileo’s mature synthesis of physics and astronomy[4].

If scientists’ attitudes are so universally positive, the same cannot be said of historians of science. For one thing, many of them tend to pay more attention to the *Two New Sciences,* though in so doing they often engage in a self-defeating exercise insofar as they begin by choosing *it* for its seemingly more scientific character and then end up concluding that its scientific content is rather meager after all[5]. Other historians are misled by the fact that the *Dialogue* has several other aspects (rhetorical, philosophical, cultural) into thinking that its scientific content is not noteworthy, as if it were impossible for a work to have all these dimensions simultaneously[6].

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[1] **A. Einstein, *Foreword,* (in:] G. Galilei, *Dialogue Concerning the Two Chief World Systems,* tr. S. Drake, Berkeley: University of California Press 1967, pp. XI—XIII.**

[2] **Ibid., p. XIII.**

[3] **Quoted in S. M. Uzdilek, *Galileo Galilei, The Founder of Experimental Philosophy and...,* [in:] *Symposium Internazionale di Storia, Metodologia, Logica e Filosofia delle Ścierna,* p. 230.**

[4] **Cf. S. Drake, *Galileo at Work: His Scientific Biography,* Chicago: University of Chicago Press 1978.**

[5] **Cf. W. L. Wisan, *The New Science of Motion: A Study of Galileo's 'De motu’,* Archive for History of Exact Sciences, 13, 1974, p. 298.**

[6] **Cf. A. Koyre, *Galileo Studies,* tr. J. Mepham, Hassocks, Sussex: Harvester Press 1978, p. 158.**

Most historians of science regard the book as a chapter in the history of the struggle for Copemicanism; two attitudes are prevalent here. Some look for a demonstration of the truth of the Copernican system; when they fail to find a scientifically valid one, the scientific worth of the *Dialogue* depreciates in their eyes[9]. In criticism of such historians, we could say, with Richard P. Feynman:

In learning any subject of a technical nature where mathematics plays a role, one is confronted with the task of understanding and storing away in the memory a huge body of facts and ideas, held together by certain relationships which can be „proved" or „shown" to exist between them. It is easy to confuse the proof itself with the relationship it establishes. Clearly, the important thing to learn and to remember is the relationship, not the proof. In any particular circumstance we can either say „it can be shown that" such and such is true, or we can show it. In almost all cases, the particular proof that is used is concocted, first of all, in such form that it can be written quickly and easily on the chalkboard or on paper, and so that it will be as smooth-looking as possible. Consequently, the proof may look deceptively simple, when in fact, the author might have worked for hours trying different ways of calculating the same thing until he has found the neatest way, so as to be able to show that it can be shown in the shortest amount of time! The thing to be remembered, when seeing a proof, is not the proof itself, but rather that it *can be shown* that such and such is true. Of course, if the proof involves some mathematical procedures or „tricks" that one has not seen before, attention should be given not to the trick exactly, but to the mathematical idea involved.

It is certain that in all the demonstrations that are made in a course such as this, not one has been remembered from the time when the author studied freshman physics. Quite the contrary: he merely remembers that such and such is true, and to explain how it can be shown he invents a demonstration at the moment it is needed. Anyone who has really learned a subject should be able to follow a similar procedure, but it is no use remembering the proofs[1].

The second „Copernican" approach to Galileo's *Dialogue* defines its content to be essentially *propaganda* for Copemicanism. The truth behind this interpretation is that the book had considerable practical impact, that, in fact, it is full of rhetorically significant passages, and that practical considerations lurk everywhere in its conception and composition. However, the only responsible way of defining its scientific content in terms of the rhetoric of the earth's motion is in the context of the *science of rhetoric;* for after all the study of the art of persuasion and of achieving practical effects by verbal means is the subject of

[1] **R. P. Feynman, R. B. Leighton, and M. Sands, *The Feynman Lectures on Physics,* Reading, Mass.: Addison—Wesley 1963, vol. 1, p. 14—1.**

a discipline which is at least as old as Aristotle's rhetoric[11]. However, here we are concerned with its scientific content, scientific in the sense of natural science.

The divergence between scientists' and historians' perception of the scientific content of the *Dialogue* acquires its clearest expression in connection with the interpretations emanating from the medievalist historians. Naturally enough, when these scholars read the *Dialogue* the main thing they perceive is similarities and differences between what's contained therein and the views of various medieval thinkers[12]. Though this kind of exercise informs us about the medieval content, if any, of Galileo's *Dialogue*, it is not clear that it tells us anything about the nature of its scientific content. The less tenable form of the medievalist interpretation would speak of a medievalist origin of, or influence upon, Galileo's book; I believe that this version of the medievalist thesis is presently being rendered of historical interest only (if I may be allowed the pun) through the efforts of Stillman Drake and his followers[13]. Let us then formulate the thesis in the more tenable form according to which the *Dialogue* is claimed to contain views which *de facto* happen to be similar to those found in various medieval texts. If true, this might be very interesting from a number of points of view, which it is not our job to elaborate. For our task here is to ask of what relevance would the alleged medieval aspects of the *Dialogue* be to understanding its scientific content. Here no one, not even the medievalists themselves, would accept the relevant equation (scientific=medieval); so there is no question of confusion, as there is for the identification of science with demonstrative proof, presupposed in one of the other interpretations. But if we avoid the relevant equation, then the relevance can only be defended by the influence thesis; but if we accept the latter, then at least for the present case of Galileo, we draw fire from the guns of Drake and his followers, a fire which is very difficult to resist and which may therefore very well consume us. In the light of the continued

[11] In recent times this view has originated from A. Koestler's, *The Sleepwalkers,* New York: Grosset & Dunlap 1959, has been propagandized by P. K. Feyerabend's *Against Method,* Atlantic Highlands, NJ: Humanities Press 1975, and traces of it can be found in D. Shapere, *Galileo: A Philosophical Study,* Chicago: University of Chicago Press 1974, p. 105, in W. R. Shea, *Galileo's Intellectual Revolution,* New York: Science History Publications 1972, p. 117, and in G. de Santillana, *Die Crime of Galileo,* Chicago: University of Chicago Press 1955, pp. 174—182.

[12] The classic locus of such an approach is the works of Pierre Duhem. The best recent elaboration, partly grounded on new textual evidence, is the work of W. A. Wallace, such as *Mechanics from Bradwardine to Galileo,* Journal of the History of Ideas 32,1971, pp. 16—28, *Galileo and Reasoning Ex Suppositione,* [in:] Boston Studies in the Philosophy of Science, vol. 32, ed. by R. S. Cohen et. al., Dordrecht: Reidel 1976, pp. 79—104; and *Galileo and the Doctores Parisienses,* [in:] *New Perspectives on Galileo,* pp. 87—138.

[13] See, for example, S. Drake's *Galileo at Work,* and his *Galileo Studies,* Ann Arbor: University of Michigan Press 1970.

resistance by scientific intuition[14] to the infusion of medieval elements into modern science, we may set aside the work of these historians in our attempt to examine the scientific content of Galileo's *Dialogue*.

Another common approach to the analysis of the scientific content of this book and of Galileo's work in general, is to examine it from the point of view of Newtonian science. This is of course more satisfactory but only slightly so. The practice was more justified before the advent of post classical physics, but nowadays it is no longer proper to equate science with Newton's work. There is no question that the detection of what might be called the Newtonian content of Galileo's book is a highly relevant exercise; of the approaches so far considered it is perhaps the most relevant. Nevertheless the Newtonian approach breaks down completely when the Newtonian content blinds one to the more-modern scientific content[15], and it can be shown still to be inadequate when there is no conflict[16].

A conflict exists, then, between scientists' and historians' perceptions of the scientific content of the *Dialogue*. It won't do to point out that historians can often show that what scientists perceive as being in the *Dialogue* is not there in some sense, and that by and large scientists find in their work whatever they want to find. For, even when inaccurate, scientists' interpretations are suggestive of the way the scientific mind works and hence relevant to the analysis of the book's scientific content. Moreover, though historians tend to be more

[14] See, for example, J. Seeger, *Men of Physics: Galileo Galilei, His Life and Works,* New York: Pergamon Press 1966, pp. 36—37; and idem, *On the Role of Galileo in Physics,* Physis 5, 1963, pp. 5—38.

[15] The passage on semicircular fall in the *Dialogue,* pp. 162—167 is a good example of how concern with classical physics can blind one to its content from the point of view of modem physics. One can very easily read it the kind of force-free geometrical physics that is characteristic of kinematical general relativity, whereas it is commonly taken to indicate that Galileo's view of „inertia" did not coincide with Newton's.

[16] For example, at the beginning of the First Day Galileo suggests that the solar system may have originated by letting the planets fall with uniform acceleration from their place of creation toward the sun until they reached their respective orbits, at which time and place the motion they had acquired must have been changed from straight and accelerated to circular and uniform. In this case there is no conflict between classical and modem physics, and the suggestion is in some sense wrong. However, in trying to understand what Galileo has in mind in this passage, Newton-minded scholars uncritically use Kepler's third law in reconstructing Galileo's reasoning. For example, in *Galileo, Newton, and the divine order of the solar system,* [in:] *Galileo, Man of Science* I. B. Cohen considers no fewer than five different reconstructions each of which, however, presupposes Kepler's third law (pp. 212—218). I believe Cohen's justification speaks for itself: „In this presentation I have, of course, introduced Kepler's third or harmonic law, which seems not to have been known to Galileo. This anachronistic procedure is wholly justifiable, however, since the third law is a quite accurate representation of the relation among planetary data as known to Galileo and other Copemicans" (p. 213)! The same approach is taken by S. Nakayama, [in:] *Galileo and Newton's Problem of World-Formation,* Japanese Studies in the History of Science 1, 1962, pp. 76—82. By contrast, in *Galileo's 'Platonic Cosmogony' and Kepler's 'Prodromus',* Journal for the History of

accurate, what they sometimes perceive in the *Dialogue* is not there either[17], and, more importantly, one can often show, as I have done above, that what historians of science perceive in the *Dialogue* is often not science, but something else. The real problem is, then, that historians' interpretations *tend* to be accurate but irrelevant, whereas scientists' interpretations *tend* to be relevant but inaccurate. Is it possible to combine relevance to science and accuracy to the text? Unfortunately, it may not be possible because on the one hand the *historical attitude as such* is bound to introduce irrelevancies, whereas the *scientific attitude as such* is bound to introduce inaccuracies. Why is this so? I shall first give an abstract general argument and then a concrete one grounded on.

An historian of science is usually someone who is acquainted with the development of science, and hence with scientific ideas and facts of various time periods. However great his acquaintance with contemporary science and the latest textbooks and journals is, this is for him merely the science of the present period. The minute he holds the latter with some special reverence or gives it special status, he thereby abandons the historical attitude, which requires of him a period-free neutrality or objectivity. Of course, his specialized area of competence is likely to be a particular period, but the historical sensibility does not allow him to magnify this accident of his biography into an historical or historiographical fact. What happens, then, when an historian opens a book like Galileo's *Dialogue* and we ask him to tell us about its scientific content? When the question is asked in the more or less loaded terminology of „scientific" content, perhaps he will refuse to answer it, and confess that he doesn't know what is being asked in asking for the specifically *scientific* content of the book. We can make the question less loaded and ask about the intellectual content of the book. The intellectual elements that he is going to come up with will be ones involving similarities and differences with other such elements found in previous and in subsequent periods, thinkers, texts. The relevance of such comparisons and contrasts, the relevance, that is, to science or at least to the interests and concerns of (living) scientists, this relevance is exactly what is hard to see.

Let us look at the scientists' situation now. A natural scientist is one who is making more or less original contributions to the understanding of nature. Science is for him what he himself (and his peers) do. When he opens the *Dialogue* the intellectual elements that are likely to attract his attention are

Astronomy 4, 1973, pp. 174—191, S. Drake, with more adequacy, interprets Galileo's cosmogony as actually involving a *groping toward* Kepler's law (pp. 185—187); Drake's article is primarily and analysis of previously unavailable documents. However, there is no documentary excuse for the Newtonian readings of this passage in the *Dialogue* since my reconstruction below shows that by merely considering this passage it is possible to give an interpretation which may be said to correspond to the tip of the iceberg uncovered by Drake.

[17] See my *Galileo and the Art of Reasoning,* Boston and Dordrecht: Reidel 1980, Chapter 9 and 10.

86 those that have similarity to his own scientific involvements. Because of the growth of scientific knowledge he will necessarily be blind to problems and ideas

that may heve been central to the book’s author but which do not relate to present day scientific research; this relation has to be one identifiable by his scientific intuition, not one demonstrable by historiographical, philosophical, or logical means. The growth of scientific knowledge makes it also unlikely, in principle, that the elements detected by the scientist are really in the book; for this would be like saying that Galileo was concerned with problems whose content and substance, as opposed to method and general character, were similar to those of the contemporary scientist; since the latter’s problems are usually definable only by reference, at least implicit, to past scientific achievements, it follows that the actual similarity of content and substance is excluded almost in principle. Of course, this becomes less true as one gets closer to Galileo’s own period; so that, for example, Newton could accurately claim more similarity of content. However, the real question is whether the extent of this similarity is significant. The fact is that, almost as a matter of definition, the more creative a scientist is, the more his achievements surpass his predecessors, (in content, we must remember, not necessarily in approach). It follows that he is less likely to understand them, that is to say understand them in the historical sense of accuracy. Of course, in another sense, the scientific sense, it may be said that he is the only one who really understands his predecessors, since he is the one who superseded them by using their results and building upon them. However, this superior scientific understanding is merely another way of saying that the scientific *relevance* of what he finds in his predecessors is guaranteed, a point I have already conceded; but what I am discussing here is its historical accuracy, which remains necessarily problematic.

A beautiful example of this tension between scientific relevance and historical accuracy is provided by available accounts of the tidal theory of the Fourth Day. As is well known, in the last „Day” of the *Dialogue,* Galileo justifies the earth’s motion by arguing that only its combined daily axial rotation and annual orbital revolution can explain the existence of tides. In spite of the essential incorrectness of this tidal theory, scientific readers of Galileo’s book have perceived a number of other scientific elements in it. For example, Ernst Mach saw Galileo as grappling with the relativity of motion and with the „fixed” stars as a fixed reference frame[18]. Others have seen him anticipating secondary tidal phenomena which can indeed be shown to be consequences of the earth’s motions[19]. When the question of the textual basis of such

[18] ***The Science of Mechanics,* tr. J. J. McCormack, La Salle, IL: Open Court, 1960, pp. 263—264.**

[19] **H. L. Burstyn, *Galileo's Attempt to Prove that the Earth Moves,* Isis, 53,1962, pp. 161—185; and V. Nobile, *Suit' argomento galileiano della quarta giornata dei 'Dialoghi'e sue aitinenze col problema fondamentale della geodesia,* Atti dell’ Accademia Nazionale dei Lincei, Classe di Scienze Fisiche, 16, 1954, p. 432.**

interpretations is seriously discussed, their proponents usually end up admitting that such elements of a tidal theory are not really in the text[20]. On the other hand, a recent account which stays closer to the text ends up concluding that this part of Galileo's book has no scientific content[21].

Another example of this tendential conflict is the beginning of the book, where Galileo discusses the nature of, and interrelationships among, natural motions, perpetual motions, circular motions, and rectilinear motions[22]. The more scientifically relevant analyses of this passage say that Galileo is here expressing some ideas about inertial motion, either the principle that inertial motion is along circles, which *may* be viewed as a *correct approximation* to Einstein's principle that inertial motion is along geodesics[23], or the principle that rotations around the center of gravity are inertial, which is an approximation to conservation of angular momentum[24], or that Galileo's concept of inertia has some similarities and some differences with the law of inertia of classical physics, but that it is arbitrary to regard any one, or any few ones, of the several features of classical inertia as constituting its essence[25]; however, any talk of „inertia"in these accounts is textually groundless. On the other hand, the more historically accurate analyses argue that in this passage Galileo is elaborating the idea that heavenly motions could perpetuate themselves merely by being circular, not because he himself accepts this idea, but because it might predispose his opponents to favor Copernicanism, and so he is acting as a propagandist rather than as a physicist[26].

I believe it is possible to devise an interpretation of this part of Galileo's book which is both historically accurate and scientific relevant, and this involves interpreting it as a series of discussions about the concept of acceleration and its importance[27]. However, though that may show that it is in principle possible, it does not suggest any general principle which would help overcoming the tension. In order to formulate and illustrate such a facilitating principle, one must move beyond the kind of scientific relevance considered so far, or to be more exact, one must distinguish two types of relevance, substantive and methodological. The former pertains to the level of scientific

[20] H. L. Burstyn, *Galileo and the Earth-Moon System,* Isis, 54, 1963, p. 401.

[21] W. R. Shea, *Galileo's Intellectual Revolution,* New York: Science History Publications 1972, pp. 178—184.

[22] Galilei, *Dialogue,* pp. 9—30.

[23] Cf. J. Agassi, *Towards an Historiography of Science,* The Hague: Mouton, 1963, p. 7—8.

[24] Cf. S. Drake, *A Further Reappraisal of Impetus Theory,* Studies in History and Philosophy of Science, 7, 1976, pp. 332—333; and idem, *Galileo at Work,* pp. 185—186.

[25] Cf. Shapere, *Galileo,* pp. 121—125.

[26] Cf. S. Drake, *Galileo Studies,* Ann Arbor: University of Michigan Press, 1970, pp. 240—278; and idem, *Galileo and the Law of Inertia,* American Journal of Physics, 32, 1964, pp. 601—608.

[27] See my *Galileo and the Art of Reasoning,* pp. 79—92.

 concepts, problems, and ideas; the latter pertains to the level of proper procedure and the nature of scientific knowledge. The point is that scientists have been, and properly are, interested in deriving methodological lessons from past examples, so that they may use in their own present problems those procedures and guiding ideas which helped past scientists solving *their* (specifically) different problems. In fact, as I shall now show, even when a scientist refers to the substantive scientific content, he usually does so for the purpose of making a methodological point. I shall take the earlier examples of Newton and Einstein.

In the *Principia,* Newton’s reference to Galileo occurs in the scholium that follows the statement of the three laws of motion and six corollaries[28]; the scholium begins by saying, „Hitherto I have laid down such principles as have been received by mathematicians, and are confirmed by abundance of experiments. By the first two Laws and the first two Corollaries, *Galileo* discovered...”[29] The section referring to Galileo ends as follows: „...On the same Laws and Corollaries depend those things which have been demonstrated concerning the times of the vibration of pendulums, and are confirmed by the daily experiments of pendulum clocks[30]”. Then Newton goes on to say, „By the same, together with Law III, Sir Christofer Wren, Dr. Wallis, and Mr. Huygens, the greatest geometers of our times, did severally determine...”[31]. The first two laws are the law of inertia and the law of force; the first corollary is the principle of the composition of velocities, the second is the principle of the composition and resolution of forces. I suggest that the function of Newton’s reference to Galileo is to serve as a methodological argument in support of the first two laws and corollaries. The structure of Newton’s argument is the following:

In his work on the law of squares, the parabolic trajectory of projectiles, and the period of vibration of the pendulum, Galileo used and accepted the first two laws and corollaries. Therefore, in dealing with the problems discussed in this book, we should accept the first two laws and corollaries.

The argument, of course, presupposes two things: that Galileo is an appropriate scientific model, and that the situation he was in is appropriately similar to Newton’s. That is, the argument is simultaneously an argument from authority and from analogy. The truth of Newton’s premise here is the sort of thing that historians have disputed. At any rate, there is no question that historical investigations can be of great service to a scientist for the establishment of such propositions on which such arguments can be grounded. The plausibility of the *inference* of course depends on the two presuppositions. In Newton’s time the

[28] **Newton, *Mathematical Principles of Natural Philosophy,* pp. 21—22.**
[2’] **Ibid., p. 21.**
[30] **Ibid., p. 22.**
[31] **Ibid.**

appropriateness of using Galileo as a scientific model may have been a matter of some controversy, nowadays it no longer is; whereas the appropriateness of the analogy appealed to by Newton neither was nor is disputable, since the two situations involve obiously similar problems about the motion of bodies. However, for other such arguments that a scientist may want to give, the analogy between the two situations may be subject to question.

Turning now to Einstein's Foreword to Drake's translation of the *Dialogue,* Einstein's argument quoted above may be reconstructed as follows:

(El) In the *Dialogue,* Galileo rejected the hypothesis of a center of the universe for the explanation of the fall of heavy bodies.

(E2) The hypothesis of an inertial system for the explanation of the inertial behavior of matter is analogous to the one being rejected in Galileo's *Dialogue* (because both hypotheses introduce a conceptual object which (1) does not have the same kind of reality that matter or fields do, and (2) affects, without being affected by, the behavior of real objects).

(E3) Therefore, this hypothesis about an inertial system should be rejected (insofar as it is as unscientific as the one rejected by Galileo).

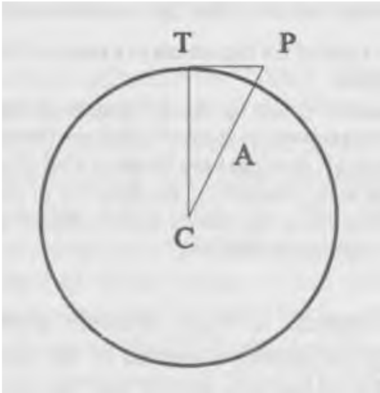
This argument is very explicit in what Einstein says, in fact he even gives a subargument to justify the appropriateness of the analogy (E2). He takes for granted the propriety of Galileo as a model, and his historical claim (El) seems to contain no obvious textual inaccuracies.

Such methodological arguments are common in scientific research, and they have an obvious „relevance". In a general examination of the methodological content of a book like the *Dialogue* one cannot, however, provide specific instances of such methodological arguments; but one can provide the groundwork for them. That is, one can describe or reconstruct the various scientific discussions and simultaneously formulate in general (though judiciously qualified) terms a methodological claim which each scientific situation illustrates. The scientist can then use such a series of methodological reconstructions by determining which one (or which ones) of them applies (or apply) to the situation or problem he himself faces. In such a determination he can be assisted by deciding whether his own case is an instance of the general claim formulated in the methodologist's reconstruction.

Here only one example will have to suffice of this kind of groundwork for a methodological utilization of a text like Galileo's book[32]. In the Second Day, among other topics, Galileo criticizes the centrifugal force objection to the earth's rotation[33]. The objection was that the earth could not rotate because if it did centrifugal force would scatter all terrestial objects into the heavens. Galileo replies that terrestrial rotation would not have this consequence because the centripetal tendency of a terestrial body due to weight would be greater than the

For a full analysis, see my *Galileo and the Art of Reasoning,* Chapter 5. —
Galilei, *Dialogue,* pp. 188—210.

centrifugal one due to the rotation. In support of this inequality he first gives a geometrical argument, which he then qualifies with methodological considerations. The argument is a mathematical proof of a theorem to the effect that given a circle and an arbitrarily large ratio, one can always find a point (P) outside the circle but close enough to it such that the tangent from that point (PT) bears that ratio to the portion (PA) of the secant outside the circle[34].



The methodological discussion[35] may be reconstructed as follows: Mathematical truths are about abstract entities in the sense that they are statements about the necessary consequences of certain definitions and axioms; for example, the proposition that a sphere touches a plane in only one point is about abstract spheres and planes in that it is a necessary consequence of the definition of a sphere and the principle that a straight line is the shortest distance between two points. Mathematical truths are also about physical reality, though only conditionally; that is, a mathematical proposition is physically true if the abstract entities it is about happen to exist as material entities in physical reality; for example, the proposition that a sphere touches a plane in only one point is true of physical reality in the sense that *if* there happen to be material spheres and planes, *then* they touch in only one point. Third, mathematical truths are *applicable* to physical reality because and insofar as material entities approach or approximate abstract ones; for when material entities do not approximate one type of abstract entity, they approximate another; for example, if and to the extent that material spheres and planes touch in more than one point, they instantiate abstract sphere and planes that are imperfect, and of these it is equally true in mathematics that they touch in more than one point. Finally, the real problem is to use the proper type of abstract

[34] Ibid., pp. 197—203. [35] Ibid., pp. 203—210.

entity in terms of which to interpret physical entities and processes, for though one is sure that the latter must correspond to *some* type of abstract entity or situation which is treatable mathematically, one cannot be sure of which one; for example, in the above mentioned answer to the centrifugal force objection, the crucial question is whether at all times the downward tendency can be equated to PA and the outward tendency to PT.

To summarize, we began by considering Galileo's *Dialogue* of 1632 as an example of a scientific classic of the sort that historically oriented science teachers might want to use. Having asked what the book's scientific content is, we argued that it has a richer scientific content than most historians of science are inclined to believe, that their judgments contrast somewhat with scientists', that this contrast is partly the result of a tension that exists between scientific relevance and historical accuracy, and that there are general reasons for thinking that this problem is insuperable. We then illustrated this tension in terms of Galileo's account of the tides in the Fourth Day, and by reference to the discussion of natural motion at the beginning of the book. In an attempt to combine scientific relevance and historical accuracy, we referred to a possible but not especially instructive example of such combination. We concluded that the most important scientific content such a book possesses is its methodological content, that it to say, the lessons, ideas, and suggestions that scientists can get from it about the nature and proper methods for the acqusition of knowledge, illustrating such lessons with examples from Newton, Einstein, and our own reconstruction.

[9] Cf. E. McMullin, *Introduction: Galileo, Man of Science,* [in:] *Galileo, Man of .science,* ed. by E. McMullin, New York: Basic Books 1968, p. 31; and idem, *The Conception of Science in Galileo's Work,* [in:] *New Perspectives on Galileo,* ed. by R. E. Butts and J. C. Pitt, Boston and Dordrecht: Reidel 1978, pp. 247—251.