Impetus Mechanics as a Physical Argument for Copernicanism

The Argument

One of the earliest arguments for Copernicanism was a widely accepted fact: that on a horizontal plane a body subject to no external resistance can be set in motion by the smallest of all possible forces. This fact was contrary to Aristotelian physics; but it was a physical argument (by abduction) for the possibility of the Copernican world system. For it would be explained if that system was true or at least possible.

Galileo argued: only nonviolent motions can be caused by the smallest of all possible forces; hence resistance-free horizontal motions are nonviolent; this confirms Copernicanism insofar as it designates the rotations of celestial spheres (being resistance-free horizontal motions) as nonviolent.

Galileo’s argument was compatible with (and supportive of) the specific Copernican version of impetus mechanics; but it was also compatible with a (somewhat qualified) principle of inertia. Thus it promoted decisively the transition from impetus mechanics to classical inertial mechanics.

I. Introduction

1. The Physical Justification of Geokineticism by Classical Mechanics

The ultimate success of Copernicanism depended on the success of a physical theory still unknown to Copernicus, but factually demanded by the Copernican world system. It was not only in retrospect that the need for a physical justification of this system, not given by Copernicus himself, was recognized: this insight was not merely a consequence of the post-Newtonian knowledge of how satisfactorily the full-blown Classical Mechanics explained the geokinetic view of the solar system. Even before Newton, there was reason to say: “Let no one hope to decide the question whether it is the Earth or Heaven that really revolves in the diurnal motion, until he has first comprehended the nature of spontaneous rotation” (Bacon, [1620] 1860, 123).
It seems that the physical justification of Copernicanism consisted in nothing less than the complete mechanical explanation of the Earth’s motion(s). Thus there is good reason to say that the so-called Copernican Revolution was brought to a close only by Newtonian Mechanics, which provided an adequate explanation for the motions of the Earth as resulting from its inertia and gravitation.1 In fact, it is obvious that the concepts of inertia and gravitation have an exact equivalent neither in Copernicus’s theory nor in the physical theories of his contemporaries.

We must distinguish, however, between a sufficient mechanical explanation and a single physical argument: If we do not find a sufficient explanation of the Earth’s motion in pre-Newtonian physics, this does not mean that there were no physical arguments for it. The mechanical explanation of the Copernican planetary system (insofar as it justified Copernicanism) was itself a physical argument for it, since every physical justification is a physical argument. On the other hand, not every physical argument for Copernicanism was a mechanical explanation of the Copernican system.

As is well known, in his pro-Copernican writings Galileo defended Copernicanism partly by explaining the Earth’s rotation on the basis of certain mechanical principles, and partly by explaining certain phenomena (the existence of Jupiter’s moons, tides, sunspots, etc.) using Copernicanism itself (or certain implications of it) as an explanans. The important point is that not only his explanation of, but also his explanations by the Copernican system were physical arguments for it (although not all of them were tenable).

The success of Copernicanism depended, ultimately, on successful explanations of the first type; but it seems to me that these explanations were prepared by explanations of the second type. Galileo's contribution to the mechanical explanation of the Copernican system concerned mainly the problem of the Earth’s persistent

1 Cf. Kuhn 1976, 122: “Copernicus in the sixteenth century provided only a new mathematical description of the way the planets move; he was not successful in explaining why the planets moved as he said they did. Initially, his mathematical astronomy made no physical sense, and it therefore posed new sorts of problems for his successors. Those problems were only resolved by Newton, whose dynamics supplied the missing keystone to Copernicus’s mathematical system [ . . . ].” So far it is true that the Copernican Revolution, as it is to be found in the De Revolutionibus of 1543, was “incomplete” (Kuhn 1976, 229) and has been completed only by Newton. There is, on the other hand, good reason to say that the label “Copernican Revolution” is not really appropriate for the revolution in astronomy that culminated in the work of Newton. Almost none of Copernicus’s revolutionary ideas survived in the form that he himself had given them; instead, they were transformed by Kepler, Galileo, Newton and others: “[ . . . ] the system of the world that was fully elaborated by Copernicus in his De Revolutionibus in 1543 had no fundamental impact on astronomy until after 1609, when Kepler published his own radical reconstruction of Copernican astronomy. From that time on we can begin to discern a revolution in astronomy, culminating in the work of Newton. But this revolution was not merely the Copernican Revolution delayed by half a century. Rather, the new astronomy was in a real sense not Copernican at all (though it is often still called ‘the Copernican Revolution’). Kepler’s reconstruction essentially rejected almost all of Copernicus’s postulates and methods; what remained was primarily the central idea that the Sun is immobile, while the Earth moves in an annual circumsolar orbit and has a daily rotation. But this concept was not original with Copernicus, as he was well aware; it came from his ancient predecessor, Aristarchus of Samos” (Cohen 1985, 38–39); cf. ibid., chapters 7–10. What is of interest in the present paper is not, however, the label “Copernican Revolution,” but rather the question of why the “central idea” of Copernicus survived. Although it came from Aristarchus, it is this very idea that I designate in the present paper by the word “Copernicanism.”
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diurnal rotation. This explanation presupposed the refutation of pre-Copernican cosmologies, which denied the very possibility of the Earth’s motions. In terms of Aristotle’s theory of motion, the Earth’s rotation would have to be classified as “forced” (or “unnatural”) motion, and as such could not be perpetual. The Aristotelian classification was incompatible with Galileo’s explanation. In order to show that the Earth’s rotation was physically possible, Galileo had to introduce arguments of the second type: he had to show that, in contrast to Copernicanism, the Aristotelian theory of motion was unable to explain certain phenomena. Did Galileo have such arguments? I think he had: as we shall see, he was able to refer to mechanical phenomena that could not be explained by the Aristotelian dichotomy of “natural” and “forced” motions, but only by a theory of motion on which Copernicus himself had based his view that the Earth’s rotation was possible.

In order to show that the suitability of the Copernican system as an *explanans* for certain mechanical phenomena was itself an essential reason for the system’s success, we must first look at some of its physical implications. We must take into account the physical views that Copernicus himself, explicitly or implicitly, connected with his astronomical theory. Or, more precisely: What was Copernicus’s own physical justification for Copernicanism?

2. Is there a Physical Argument for Geokineticism in Copernicus’s “De Revolutionibus”?

One might expect that the opening chapters of Book 1 of *De Revolutionibus* would introduce something like physical arguments to support the Earth’s motion, since it deals with the physical problems involved in the geokinetic view; the eighth chapter of this book closes with the words: “You see, then, that all these arguments make it more likely that the Earth is able to move (*probabilior sed mobilitas terrae*) than that it is at rest. This is especially true of the daily rotation, as particularly appropriate to the Earth” (Rosen’s translation [Copernicus 1978, 17.38–40] modified; Copernicus 1984, 16.29–30). To what arguments does Copernicus refer? Four arguments are to be found in chapter 8.

The *first* of these arguments (chap. 8, 16.4–20) is intended to prove the inconsistency of the Aristotelian concept of simple, rectilinear, natural motion. Copernicus uses it in order to classify the Earth’s motion as a “natural motion,” although it is rotatory. As we shall see, this argument involves a physical theory that, although un-Aristotelian, is not very original: It implicitly operates with impetus dynamics; its explanation of the acceleration of freely falling bodies presupposes that accelerated motions are not simple and therefore (in the Aristotelian sense) not natural motions. As we shall see, it is conclusive if and only if one accepts that the concept of natural motion, taken in the strict Aristotelian sense, is incompatible with the concept of

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2 Chapter, page, and line references here and in the following text relate to the Latin text of the critical edition (Copernicus [1543] 1984).
acceleration as presupposed by impetus dynamics in at least one of its predominant forms. So the argument is an *argumentum ad hominem*: it is a "physical" argument only in the sense that it operates with certain views concerning the classification of physical phenomena. But if we understand by "physical argument" a reference to facts that are held to support the Earth's motion, Copernicus's argument is certainly not a physical one.

The second argument (chap. 8, 16.21–22) asserts that "immobility is deemed nobler and more divine than change and instability, which are therefore better suited to the Earth than to the universe" (Copernicus 1978, 17.28–30). This argument is, like the following one, merely rhetorical, not physical.

The third argument (chap. 8, 16.22–24) maintains that it is absurd to attribute motion to the outermost sphere of the universe — i.e., to the place in which bodies are located — instead of attributing this motion to the bodies located in it.³

The fourth argument (chap. 8, 16.24–28), finally, draws attention to the astronomical observation that the distance between the planets and the Earth varies. This argument refers to a physical fact, but need support only the view that the circular celestial orbits are not concentric. It does not necessarily imply geokineticism.

Copernicus does not offer physical arguments in support of the Earth's motion, at least not if we understand "physical arguments" to involve a reference to facts supposed to support the Earth's mobility. Below I shall use the term "physical argument" mainly in this sense. What Copernicus does do in the eighth chapter, and elsewhere in the First Book, is to appeal to such current opinions concerning motion as are able to back up his geokinetic system. In the opening chapters of *De Revolutionibus* he tries to give in rough outline a reinterpretation of current concepts of traditional physics in order to accommodate them to the cosmological views underlying the mathematical astronomy of the subsequent books. He justifies his system, not by showing that it can successfully explain physical facts, but rather by attempting to make current and more or less traditional ways of explaining motion compatible with the Earth's motion. I do not know whether Copernicus ever realized that his system required physical arguments (in the sense of this term used here). He does seem to have realized, however, that it needed a foundation or at least an interpretation that made plausible the idea that the Earth's motion is physically possible and not completely incompatible with reasonable views about nature.⁴

The following investigations examine in detail the extent to which Copernicus, explicitly or implicitly, uses impetus dynamics to justify a new classification of motion (part II of this paper). We shall see (parts III and IV) that the Copernican classification of motion made it possible to use impetus dynamics as an *explanans* for a new

⁴ Although a number of recent interpreters, e.g., Kuhn 1976, 144–45, and Westman 1980, 105–47, see the real or primary justification for Copernicus's system as drawn from the technical domain of mathematical astronomy or as lying in its mathematical harmony and elegance, they do not deny that an attempt at a natural philosophical foundation of the Earth's mobility is offered in Book 1 of *De Revolutionibus*. An important essay concerning Copernicus's natural philosophical justification of his system, especially his beliefs about the substance and motion of celestial orbs, is Jardine's 1982 article.
type of mechanical phenomena. The impetus theory, as applied to the Earth’s rotation, constituted a far more formidable challenge to traditional natural philosophy than Copernicus himself appears to have realized.

II. Copernicus’s Reinterpretation of Traditional Physics

1. Copernicus’s Concept of Natural Motion

Copernicus proceeds from a theory of motion that still makes use of the Aristotelian distinction between natural and forced motions (chap. 8, 14.18–23). He identifies (as did Aristotle) “natural” with “simple,” i.e., not compound, motions; and he attributes simple motions to “simple bodies,” insofar as simple motions suit the “nature” of simple bodies (chap. 7, 13.32–14.4; chap. 8, 16.1–2). All other motions are called “forced”; they do not result from the nature of a body, but are caused by external forces (chap. 8, 4.19–20).

Copernicus’s distinction between natural and forced motions agrees with the original Aristotelian distinction in name only. Copernicus does not define simple bodies as the five Aristotelian elemental bodies (earth, water, air, fire, and aether), which differ from each other in certain qualities and are determined only by these qualities. In chapter 4, Copernicus defines the simplicity of a body only by its figure. In his view, there is only one sort of simple body: the “simplest” one is spherical (chap. 4, 9.21–24). A spherical body is simple because of its “form,” “wherein neither beginning nor end can be found, nor can the one be distinguished from the other, while the sphere itself traverses the same points to return upon itself” (Copernicus 1978, 10.31–33).

Copernicus supposes that the spherical form or shape entails the simplicity of a spherical body; but he adds that the perfect simplicity of the spherical form is realized only by the rotatory movement of the spherical body. So the real simplicity of the body is determined not only by its form, but also by its motion, which – actualiter – integrates the parts of the body into a simple wholeness (chap. 4, 9.21–24).

Thus Copernicus’s concept of natural motion is analytically included in his concept of a simple body: The motion natural to a sphere is its uniform and persistent rotation (motus aequalis ac circularis perpetuus, chap. 4, 9.19–20); for “by this very act the sphere expresses (exprimere) its form as the simplest body” (Copernicus 1978, 10.30–31).5 The idea that the rotation of a simple body “expresses” its form does not mean that the form of a simple body causes its rotation; it rather means that the (potential) “mobility” that a spherical body owes to its form is actualized only in its

5 As Jardine (1982, 180) points out, Copernicus’s claim that the motion natural to a sphere is rotation is precisely the claim made by Aristotle in De Caelo 2.3, 286a11–13; Copernicus’s remarks concerning circular motion in De Revolutionibus 1, chap. 4, 9.21–24 are closely based on De Caelo 2.6, 288a24–28.
simple (rotatory) motion. As Copernicus writes, the rotatory motion of a spherical body is "appropriate by nature to its form" (ibid., 16.10–11)\(^6\) (\textit{illi formae a natura congruere}, chap. 8, 15.8–9). Rotation is "simple" insofar as it is uniform, persistent, and circular. "The statement that the motion of a simple body is simple holds true in particular for circular motion, as long as the simple body abides in its natural place and with its whole. For when it is in place, it has none but circular motion, which remains wholly within itself like a body at rest" (ibid., 17.2–5) (chap. 8, 16.1–4). Here the Aristotelian term "natural place" does not designate the place to which a naturally moved body strives as its natural aim. For Copernicus the place in which the rotating body abides is natural apparently because of its congruence with the spherical shape of the body.

The simple rotation of a spherical body is the only kind of motion that Copernicus \textit{expressis verbis} calls "natural." This restriction of the Aristotelian concept of natural motion is linked with the fact that Copernicus (unlike Aristotle) does not regard the rectilinear upward and downward motions of the Aristotelian elemental bodies as simple motions. "Surely Aristotle's division of simple motion into three types, away from the middle, toward the middle, and around the middle, will be construed merely as a logical exercise (\textit{rationis solummodo actus}). In like manner we distinguish line, point, and surface, even though one cannot exist without another, and none of them without body" (ibid., 17.24–27) (chap. 8, 16.17–20).

For Copernicus, the rectilinear motions "away from the middle" and "toward the middle" are not simple, because the rotation of the Earth entails that those motions are in fact composed of rectilinear and circular motions (chap. 8, 15.31–39). Copernicus explains the circular component of the downward motion of heavy bodies by saying that these bodies, as parts of the terrestrial globe, "being predominantly earthy, undoubtedly retain the same nature as the whole of which they are parts" (ibid., 16.40–41) (chap. 8, 15.33–34). At first sight this explanation seems to be a vague anticipation of the principle of inertia; but in fact it is merely a modification of Copernicus's concept of "natural" circular motion. Similarly, Copernicus explains the circular component of the upward motion of light bodies, especially of fire, by saying that these bodies contain "earthy matter" (\textit{terrena materia}), "for also fire here on the earth feeds mainly on earthy matter, and flame is defined as nothing but blazing smoke" (ibid., 16.43–44) (chap. 8, 15.35–36). Copernicus calls fire "earthy fire," and emphasizes that "we behold no other" kind of fire (\textit{neque enim alium videmus}) (chap. 8, 16.12) (ibid., 17.16). The (earthy) fire is, in his view, not a simple elemental body to which a certain natural place (in the Aristotelian sense: as the aim of its natural upward motion) can be attributed. In his view, the upward motion of fire is "forced": "things [ . . . ], being fiery, are driven forcibly upward" (ibid., 16.42–43) (chap. 8, 15.34). Their upward motion results from "the property of fire to expand what it enters" (ibid., 16.44–45) (chap. 8, 15.36). The upward motion of fiery things

\(^6\) Neither Copernicus nor Aristotle implies that all spheres rotate by virtue of their sphericity, or that sphericity is sufficient to explain rotation. Cf. Jardine 1982, 180. See also below, chap. 4 of part II of this paper.
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is a "motion of expansion" (ibid., 16.47) (motus extensivus, chap. 8, 15.38), and it is an effect of "fire power" (ignea vis, chap. 8, 15.34). Copernicus probably alludes to the recent invention of firearms by writing (chap. 8, 15.37–39) that fire "expands what it enters [. . .] with such great force that it cannot be prevented in any way by any device (nullis machinis, chap. 8, 15.37) from bursting through restraints and completing its work (rupto carcere suum expleat opus, chap. 8, 15.37–38). But the motion of expansion is directed from the center to the circumference. Therefore, if any part of the Earth is set afire, it is carried from the middle upwards" (ibid., 16.45–17.2). By comparing fire with a jailbreaker (Edward Rosen's English translation of this passage is not very precise), Copernicus is referring to the violence of its motion. The earthly fire, "after it has been lifted up high, slackens all at once, thereby revealing (confiteri, chap. 8, 16.13) the reason to be the violence applied to the earthy matter" (ibid., 17.16–18). A more literal translation would express that the slackening fire makes, as it were, a full confession of its violent act.

John Donne (1611, lines 205–6) pertinently rendered what Copernicus had in mind by using these metaphors:

And new Philosophy calls all in doubt,
The Element of fire is quite put out.

Copernicus intends to get rid of the five Aristotelian elements, since his cosmology has no room for absolutely light bodies, which by their nature tend to move upwards to their natural place "below" the celestial spheres. He seems also to regard the air as an "earthly" rather than an absolutely light element; he attributes to it "cognition" (chap. 8, 15.21) with the Earth, looking upon it as the atmosphere of the terrestrial globe. "Atmosphere"7 is a post-Copernican term, coined as a general concept after Kepler (1596, chap. XVI) and other Copernicans had assumed that, by analogy to the terrestrial air, the Moon and Sun have atmospheres. The modern idea, however, that the "light" elements of celestial bodies are in fact integral parts of them can be traced back to Copernicus.

2. Copernicus's Concept of Gravity

This idea actually means that all light bodies are in fact light only in a relative sense. The relativity of lightness results from Copernicus's new interpretation of the concept of gravitas: "gravity is nothing but a certain natural desire (appetentia naturalis), which the divine providence of the Creator of all things has implanted (indita) in parts, to gather as a unity and a whole by combining in the form of a globe" (Copernicus 1978, 18.6–8) (chap. 9, 17.3–6). "This impulse (affectio) is present, we may suppose, also in the Sun, the Moon, and the other brilliant planets, so that through its operation (efficacia) they remain in that spherical shape which they

7 The modern term “atmosphaera” was probably introduced by W. Snell in his Latin edition of Stevin 1608, Pars II, Lib. 3: Cosmographia atmosphaerae. See Krafft’s 1976 article, p. 1214.
display” (ibid., 18.8-10) (chap. 9, 17.6-8). Alexander von Humboldt (1847, 347-48) read into this passage the idea of universal gravitation. This interpretation is untenable, insofar as no idea of the mutual attraction of masses is to be found in the text. But Humboldt’s interpretation can be improved by a slight alteration. For it is true that in this passage Copernicus has grasped the idea of a universal relative gravitation. When he writes that “the parts” of the Earth and of the other celestial bodies spontaneously tend to combine into spheres, and that this spontaneous tendency is nothing other than gravity – in the first chapter (7.8-9) Copernicus compares this tendency with the tendency of the parts of a waterdrop to form a sphere – he obviously has in mind both the (relatively) heavy, and the (relatively) light parts of all celestial bodies. This means that bodies, insofar as they are parts of a certain celestial body, can be regarded as having gravity in relation to that celestial body.8 The idea of universal relative gravitation is manifested in Copernicus’s neglect of the fourth and fifth elements, as well as in his regarding terrestrial air as a kind of ocean (mare), and the wind in the air as a kind of ocean current (fluctus) (chap. 8, 15.30: *Quid enim est aliud ventus in aere quam fluctus in mari?*).9 All celestial bodies have their center of gravity in themselves.

The view that gravity is a “natural tendency” of the parts of the terrestrial globe does not immediately entail that the rectilinear motions of these parts towards the center of the Earth are “natural motions.” At any rate, Copernicus refuses to accept this consequence. He emphasizes: “Rectilinear motion occurs only to things that are not in proper condition and are not in complete accord with their nature, when they are separated from their whole and forsake its unity” (Copernicus 1978, 17.9-11) (chap. 8, 16.6-8). Moreover, he argues that the fall of heavy bodies, like the rectilinear upward motion of fire, is not a “simple,” “constant,” and “uniform” (simpex, uniformis, et aequalis, 16.9) motion, “even when deprived of circular motion” (Copernicus 1978, 17.18). He knows (chap. 8, 13.32-14.4) that Aristotle had no scruples about regarding the motion of falling bodies as simple motion, despite their acceleration. For Copernicus, however, this acceleration proves that free fall is a compound motion; to be more precise, it is compound “even when deprived of circular motion,” or even when we leave the circular motion of the terrestrial globe out of consideration. His view of accelerated free fall as a compound motion is obviously borrowed from impetus mechanics. This borrowing is already

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8 Copernicus’s idea that all parts of a body tend towards its center is similar to the Stoic concept of body. Furthermore, the Stoics, like Copernicus, believed that the so-called “light” or “not-heavy” elements naturally tend to move towards the Earth’s center. In contrast to Copernicus, however, the Stoics did not identify the centripetal tendency of light elements with their gravity or heaviness. Nor did they deviate from Aristotle’s view that the Earth’s center is the center of the universe. According to the Stoics, there is only one center in the universe towards which all its parts naturally tend to move. In other words, the Stoics developed an idea of universal centripetality, not of universal (relative) gravitation. Cf. Wolff (forthcoming).

9 E. Rosen (in his notes to the text of *On the Revolutions*, Copernicus 1978, 352) draws attention to a similar view in Pliny [1942] 1969, 44, 114: “The wind is understood to be nothing but a wave of air.” This view obviously has a Stoic background.
recognizable in his use of the phrase “impetus ponderis” (chap. 8, 16.10).\(^{10}\) Rectilinear motions are not simple motions, he says, “for they cannot be governed (nequeunt temperari) by their lightness (levitas) or by the impetus of their weight” (Copernicus 1978, 17.14) (chap. 8, 16.8–10). In other words: because of their impetus ponderis or their lightness (i.e., the violent force of fire), respectively, they cannot keep their movement in a temperate mode.

3. The Influence of Impetus Mechanics on Copernicus

For Copernicus, the concept of impetus is important in other respects as well, and this reveals the influence of impetus mechanics on his scientific thought.\(^{11}\) To begin with, the concept of impetus helps him distinguish forced motions from natural ones. Copernicus describes violently (secundum violentiam) moved bodies as things into which a force (vis) or an impetus is introduced (infertur) (chap. 8, 14.19). The traditional impetus theory of motion, too, had explained forced motions by an impetus, i.e., by a force that is impressed on movable bodies and which, as a result of its conflict with the nature of the moved bodies, is exhausted in the course of their motion. In a similar way, Copernicus writes that things to which a force or an impetus is applied cannot long endure the unnatural impact (chap. 8, 14.19–20). As for the acceleration of falling bodies, the traditional impetus theory, at least since Buridan, had tried to explain it too by an impetus.\(^{12}\) According to this explanation, the gravity of a heavy body is not the actual cause of its acceleration. Its gravity is the natural tendency of a heavy body to remain in or to strive for its natural place. If the heavy body is not in its natural place and if there is no obstacle to downward motion, gravity causes uniform motion at a minimal speed. But the uniform motion lasts only for a minimal time interval, because this motion imparts to the moved body an impetus, which adds a motion of a certain minimal speed for the following minimal time interval; this accelerated motion in turn produces an impetus, etc., so that in the course of motion a continual accumulation of impetus, and therefore a continual acceleration, takes place. Similarly, Copernicus writes that whatever falls, moves slowly at first, but increases its speed “because of its fall” (cadendo, chap. 8, 16.11). There seems to be a difference between Copernicus’s view and the traditional

\(^{10}\) Already for Buridan, simple motion was no longer the motion of a naturally downward (or upward) moving body. The introduction of impetus as an explanans for acceleration had forced him to assume that (at least) no sublunar rectilinear motion is simple. Cf. Funkenstei{\textes}n, 1971, 329–48.

\(^{11}\) Kuhn 1976, 120, seems to have been aware of the fact that “impetus dynamics, in one of a number of forms very like Buridan’s,” is implied by some of Copernicus’s arguments. But he did not say in which ones.

\(^{12}\) Cf. Franklin 1976, 47–50; and Wolff 1980, 218–28. Buridan’s explanation of the acceleration of falling bodies is in some respects similar to that given by Abu’l-Barakat (died 1168). But it has, I think, nothing in common with Hipparchus’s theory of the acceleration of falling bodies, which, unlike most historians of science (e.g. Franklin 1976, 9–10 and 105), I do not believe to be a version of the impetus theory. See Wolff (forthcoming).
impetus theory insofar as he regards free fall, as well as the impetus produced by free fall, as contrary to nature. For traditional impetus mechanics, the accelerating impetus of falling bodies was analogue of impetus as the cause of violent motions of projectiles. As regards the accelerating impetus, it was commonly not assumed to be exhausted, because it was assumed not to be in conflict with the natural tendency of heavy bodies. Copernicus, on the other hand, makes a point of the assumption that even the impetus ponderis (like lightness – levitas – i.e., the “violence of fire,” and like rectilinear motion itself) is always produced as a result of a disturbance of nature. He compares the rectilinear motions of the parts of celestial bodies with the sickness of an animal: “Since circular motion belongs to wholes, but parts have rectilinear motion in addition, we can say that ‘circular’ subsists with ‘rectilinear’ as ‘being alive’ with ‘being sick’ ” (Copernicus 1978, 17.21–23) (chap. 8, 16.15–17). In consequence of this view, Copernicus regards the impetus ponderis as being exhausted: “Circular motion, however, always rolls along uniformly, since it has an unfailing cause (causa indeficiens). But rectilinear motion has a cause that quickly stops functioning (desinere festinans). For when rectilinear motion bring bodies to their own place, they cease to be heavy or light (cessant esse gravia vel levia), and their motion ends” (ibid., 17.18–21) (chap. 8, 16.13–15). The phrase “cessant esse gravia vel levia” seems to mean nothing else than that the impetus ponderis and the lightness, respectively, slacken off; for gravity, as the “natural desire” of the parts of celestial bodies to integrate into a globe, could scarcely be regarded as slackening off.

In Copernicus’s view, gravity, as an original and natural tendency of bodies, does not actually seem to be a motor force, but rather a formative cause. If gravity would naturally move bodies towards the centers of celestial bodies, there would be – according to the traditional impetus theory – a conflict between the natural downward movement and the circular movement of heavy bodies as parts of a rotating globe. Copernicus apparently tries to avoid just this conflict. But as a consequence of this avoidance he leaves two questions open.

First, he implicitly postulates a difference between gravitas (i.e., gravity as a formative cause of celestial bodies and as an invariable and universal property of all its parts) and pondus (i.e., weight as a variable motor force). But he does not try to answer the question of the relation between these two. We shall see, however, that the so-called buoyancy theory of falling or rising bodies, given later by Benedetti and Galileo (cf. Drake and Drabkin 1969, 36–41), as well as their distinction between gravity and weight as absolute and specific weight, was prepared indirectly not only by the so-called Archimedean Principle, but also by Copernicus’s distinction between gravity and weight and by his interpretation of (relative) lightness as an effect of the expansion of bodies.

Second, Copernicus does not try to answer the question of what causes the rotation of celestial bodies, if not gravity itself as a nonmoving principle. Indeed, even if gravity is a mere formative cause, it could be regarded as the tendency of the parts of the rotating globe to perform a circular motion. But is it, then, the cause of their motion, or is it merely the cause of the curvature of the path of their motion?
4. The Cause of the Earth's Rotation –
The Interpretations of Birkenmajer, Koyré, and Rosen

The text of *De Revolutionibus* gives us no clue about Copernicus's opinion regarding the second question. Aleksander Birkenmajer writes: "Es hält schwer, daran zu zweifeln, dass diese beiden Ursachen (It can hardly be doubted that these two causes) (viz. gravity and the efficient cause of the rotation of celestial bodies) trotz der zwischen ihnen vorhandenen Unterschiede im Grunde genommen identisch sein müssen (despite the differences existing between them, must be basically identical)."13 But this is mere guesswork. The conjecture that gravity and the efficient cause of rotation are actually identical could at best be defended by the hint that Copernicus, when he says that gravity is a kind of impressed force (*appetentia indita*), regards gravity itself as an *impetus*. Pierre Duhem (1909, 870) has drawn attention to the fact that the identification of gravity with a force that is the efficient cause of the Earth's rotation can be traced back to Oresme's *Livre du Ciel et du Monde*. "We can say," Oresme writes, "that the force causing the lower region of the world to move in a circle is its nature or form; and this same force – similar in nature to that which draws iron to the magnet – moves the earth to its proper place when it gets outside"(Oresme 1968, 528, 203–6). However, Oresme finally rejected the idea of the Earth's rotation; he never distinguished (as Copernicus does) the center of the Earth from the center of the universe nor did he question the Aristotelian theory of natural rectilinear motions.

According to Alexandre Koyré and Edward Rosen,14 Copernicus does not identify the cause of the rotation of celestial bodies with the gravity of their parts. Koyré and Rosen believe that for Copernicus this cause is merely the geometrical form of the spherical body. Rosen refers to his translation of the beginning of chapter 4 (9.21–24), a text that we have already interpreted: "The motion appropriate to a sphere (*mobilitas sphaeræ*) is a rotation in a circle. By this very act the sphere expresses its form as the simplest body, wherein neither beginning nor end can be found, nor can the one be distinguished from the other, while the sphere itself traverses the same points to return upon itself" (Copernicus 1978, 10.30–33). Rosen comments on the first sentence of this passage as follows: "This simple proposition states Copernicus's entire conception of celestial mechanics. For him the universe is an all-embracing sphere, containing lesser spheres; and the sphere, as a geometrical form, is endowed with the property of circular motion. This is the sum total of his answer to the question, why do the heavens go round? Satisfied with this explanation of the cause of motion, he envisages the task of the astronomer as the endeavor to trace the pattern of motion, to solve the problem how do the heavens go round?" Koyré asserts: "Copernicus seems to believe that a spherical shape, the most perfect geometrical form, and the one that all natural bodies endeavor to assume because of

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13 See Birkenmajer's notes to the text of *De Revolutionibus* in Copernicus 1959, 131.
this very perfection, is not only more suitable for motion (as is universally admitted), but is sufficient cause thereof, and naturally engenders the most perfect and most natural of motions, namely circular motion” (Koyré 1973, 58).

But the Koyré-Rosen interpretation is no less conjectural than that of Birkenmajer – and it encounters some difficulties. The most serious problem can be demonstrated by the following consideration: If Copernicus believed that the rotation of celestial bodies is caused only by their spherical form, it would follow that all celestial spheres have a rotatory movement. Therefore, the innermost and the outermost spheres of the universe, the Sun as well as the sphere of the fixed stars, would have to rotate around their center. But this conclusion contradicts Copernicus’s conviction that these spheres, at least the sphere of the fixed stars, are at rest.

Another objection to the Koyré-Rosen interpretation is that, if it were correct, Copernicus’s dynamics would entail that the whole terrestrial atmosphere of air, having a spherical form, must have a rotatory motion around the center of the Earth, and that this motion is caused not by the motion of the Earth – which is surrounded by that atmosphere – but rather by its own geometrical form. In chapter 8 of the First Book, however, Copernicus says that the air or (more precisely) “a not small part of the air” participates in the movement of the Earth for one of the following reasons: “The reason may be either that the nearby air, mingling with earthy or watery matter, conforms to the same nature as the Earth, or that the air’s motion, acquired from the Earth by proximity (contiguitas) shares without resistance in its unceasing rotation” (Copernicus 1978, 16.25–28) (chap. 8, 15.21–24). And Copernicus adds that “we can maintain that part of the air [viz. ‘the uppermost belt of air’] is unaffected by the Earth’s motion on account of its great distance from the Earth” (ibid., 16.32–34) (chap. 8, 15.27–29). Copernicus explains the fact that the rotation of the Earth cannot be perceived by feeling a motion of air from east to west, not by reason of the geometrical form of the atmosphere, but rather on the basis of traditional causal ideas: he obviously assumes that the atmosphere is moved from west to east (not by its geometrical form but) by the Earth, and participates in its rotation. Copernicus himself compares his explanation with the Aristotelian idea that the uppermost regions of air and fire participate in the motion of the celestial spheres (Aristotle Meteorologica 1, 3). John Philoponus and medieval impetus mechanics had explained this transmission of motion by an impressed force. Oresme, then, had written – and thus may have prepared Copernicus’s explanation – that the rotation of the Earth caused by an impetus would be just as easily thinkable as the rotation of the sphere of fire assumed by Aristotle: “Besides, I ask Aristotle what force it is that moves fire in the diurnal movement of its sphere, for we cannot say that the heavens pull it thus or seize it violently, not only because this motion is perpetual but also because the concave surface of the heavens is so highly polished – as noted in chapter 11 – that it passes over the sphere of fire without rubbing, pulling, or pushing – as stated in chapter 18. So, we must say that fire is moved circularly by its own nature and form or by some intelligence or celestial influence. Exactly the same could be
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said by one who maintains that the Earth has diurnal rotation and that the sphere of fire remains at rest" (Oresme 1968, 528–30).

Copernicus’s idea of the causal dependence of the rotation of the terrestrial atmosphere shows that Rosen’s conjecture is untenable.15 At the same time, it illustrates how far the principle of inertia was from Copernicus’s mind. Like Aristotle and the traditional impetus theory, he does not consider the possibility of a motion not caused by something. Like Aristotle and the impetus theory, he even seems to postulate that a moving cause is acting instantaneously. For he postulates the existence of an “unfailing cause” (causa indeficiens, chap. 8, 16.14) as the cause of the “simple, constant, and uniform” circular motion.

5. Impetus and the Rotation of the Earth

If it is true that Copernicus postulates the presence of a moving cause, and if we cannot follow Koyré and Rosen in interpreting this cause as the geometrical form of the body moved, we must offer an alternative interpretation of the passage (chap. 4, 9.21–24) used by Rosen as evidence. It seems to me that we have to compare this passage with chapter 8, where Copernicus – in similar words – says (15.8–9) that the mobility (mobilitas) of the sphere is by nature congruent (a natura congruere) with its form (forma). He seems to assume that the possibility of a (simple, constant, and uniform) motion follows from the geometrical form of a sphere, so that, if this possibility is being actualized by a certain cause, “by this very act” (ipso actu, chap. 4, 9.22) the geometrical form of the sphere “as the simplest body” is being “expressed”: the actualization of the rotatory motion (caused by a certain entity) actually integrates the parts of the sphere, which are potentially integrated by the spherical form of the body, to a simple wholeness.16

A similar idea can be found in the writings of Nicholas of Cusa. Although it cannot be shown that Copernicus was acquainted with them, the parallels are interesting.17 In the first book of his 1463 Dialogus de ludo globi, Cusanus wrote: “The spherical figure, then, is most suitable for eternal motion. If it acquires motion naturally, it will never come to rest. Therefore, if it moves about itself in such a way that it is the center of its own motion, it moves forever. And this is the natural motion with which the outermost sphere moves, a motion without violence or weariness, a motion shared by

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15 Further arguments against the view shared by Koyré and Rosen are to be found in Jardine’s article (1982, 180).

16 Jardine (1982, 180) writes that in the passage in question “it is not asserted that a sphere expresses its form by rotation, but rather that the mobilitas of a sphere thus expresses its form.” This is, I think, incorrect. Copernicus clearly asserts that a sphere expresses its form “by this very act,” i.e., by its circular motion. Mobilitas enim sphaerae, est in circulum uolui, ipso actu formam suam expressimtis, in simpilissimo corpore, ubi non est reperire principium nec finem, nec uno ab altero seeernere, dum per eadem in seipsam mevetur.

17 The similarity between the dynamics of Nicholas of Cusa and of Copernicus has frequently been noted. See Duhem [1907] 1955, 186 and 210; Koyré 1973, 58 and 113.
all bodies that have a natural motion." Rosen, who cites this passage in his commentary (Copernicus 1978, 348), remarks: "According to Cusa, God had set the spheres in motion once and for all at the time of the creation, providing them with the initial impulse which keeps them running forever. Thus he dispensed with Aristotle's unmoved Prime Mover, which operated as the unceasing original cause of all motion [. . . ]. Copernicus, on the other hand, by attributing eternal circular motion to the sphere as a geometrical form, had no need for an initial impetus [. . . ]" (ibid., 348).

Rosen's contrast between Copernicus and Cusanus in fact does not exist. It is true that Copernicus, unlike Nicholas of Cusa, has no need for an initial impetus that causes the rotation of the firmament; this is not, however, because he identifies this cause with the geometrical form of the firmament, but rather because he does not attribute any motion to the outermost sphere. On the other hand, it is true that Cusanus does need an initial impetus that keeps the spheres running forever; but in comparison with Copernicus he seems not to have an essentially different view of the geometrical form as a condition of natural motion. According to Cusanus, too, rotation is a natural motion if the rotating body has a perfectly spherical form. His dialogue of 1463 states, with respect to a ball: "Attention must be paid to the fact that the movement of the ball decreases and ceases while the ball itself keeps whole and undamaged; for its motion is not by nature appropriate to the ball, but it is moved accidentally and by force. It comes to rest when the impetus, which has been impressed in it, ceases (impetu deficiente). However, if the ball were a perfect sphere, as we have supposed just now, the rolling motion would never cease, because it would be natural for the sphere and never forced" (Kues [1463] 1967, 242). For Nicholas of Cusa, the geometrical form of the sphere is not its moving cause, but the condition of its having its rotation "by nature" (Kues [1463] 1967, 242). In this way Cusanus geometrized the concept of natural motion; and it is just this geometrization that we come across again in Copernicus.

It is worthwhile stressing that the geometrization of the concept of natural motion was obviously compatible with the assumption that the natural motion of a sphere requires a moving force or an impetus. It thus seems that nothing can be said against interpreting Copernicus's "causa indeficiens" as an impetus that moves the celestial bodies around their center. According to traditional impetus mechanics, too, an impetus can be an unfailing cause as long as it does not come into conflict with the nature and the natural tendencies of the body moved (or with any external resistance) (Wolff 1987, 84–85). In Copernicus's construction of the rotatory motions of celestial bodies no such conflict can arise, because rotation does not alter the relative positions of the parts of the rotating sphere (and because there is no resistance to the motion of a rotating celestial body). Therefore, if gravity is nothing but the natural tendency of these parts to keep their relative positions within a celestial body, it cannot come into conflict with the impetus that only moves the celestial body around its center.

18 Cf. the Latin text of this dialogue in Kues [1463] 1967, 240.
A conflict can arise in one respect only. Traditional impetus mechanics had assumed that motions caused by an impetus are always rectilinear, except when they are rotations of coherent solid bodies, for instance millstones, grindstones, or the firmament. The Earth is obviously not such a body, as we know from its parts on its surface, which are connected with the terrestrial globe only by their gravity. So it seems to be conceivable that these parts of the Earth would not keep their relative positions were the Earth moved around its center. The impetus that causes the rotation of the terrestrial globe seems, then, to be at the same time the cause of the rectilinear motions of those parts of it that are connected to it only by gravity.

It is significant that Copernicus himself deals with just this problem. At the beginning of chapter 8 (14.24–29) he discusses a physical argument against the Earth’s rotation allegedly advanced by Ptolemy, but in fact not traceable back to him.\(^{19}\) According to this argument, as presented by Copernicus, “things which undergo an abrupt rotation seem utterly unsuited to gather [bodies to themselves] and seem more likely, if they have been produced by combination, to fly apart unless they are held together by some bond (nisı cohaerentia aliqua firmitate contineantur). The Earth would long ago have burst asunder [...] and dropped out of the skies” (Copernicus 1978, 15.13–16) (chap. 8, 14.8–10). Copernicus counters the objection by pointing out that an impetus (14.28) drives the parts of a rotating body farther away from the center the faster their circular motion is. And the farther away from the center the parts are, the faster their motion will be. Thus, the objection cuts the ground from under the feet of those who assume that not the Earth, but rather the sphere of the fixed stars, moves around its center in the period of twenty-four hours.

This tu-quoque argument used by Copernicus is not very sound; for the objection had referred to the special nature of the terrestrial globe, the parts of which are not held together aliqua firmitate. But Copernicus uses still another argument (chap. 8, 14.17–19), that a motion “in accordance with nature produces effects contrary to those resulting from violence” (Copernicus 1978, 15.27–28). Therefore, if we presuppose that the rotation of the Earth is a natural motion, we are not allowed to expect that “the Earth and everything earthy will be disrupted by a rotation created through nature’s handiwork (facta per efficaciam naturae), which is quite different from what art or human intelligence accomplish” (ibid., 15.31–33) (chap. 8, 14.21–23).

Copernicus seems to admit indirectly that impetus, being the cause of rotatory motion, is also able to effect a rectilinear motion in the parts of the rotating body; but he obviously feels compelled to restrict the possibility of such an effect to the area of

\(^{19}\) Cf. Koyré 1973, 112: “Copernicus is apparently referring to the Almagest, Book 1, chap. 6. However, Ptolemy does not speak there of the destructive action of centrifugal force, but only of the disruptive force of motions as such. Had Copernicus found some precise mention of centrifugal force? It is possible, but it is also likely that he himself put this interpretation on Ptolemy’s text, either because he believed that this disclosed the true meaning of the great astronomer, or because he felt that he ought to make his objection in a better, i.e., stronger, way.” The idea of centrifugal force is, I think, incompatible with Ptolemy’s basically Stoic presuppositions. See Wolff forthcoming, esp. n. 31.
artificial mechanisms.\textsuperscript{20} For this restriction, however, he offers no real physical argument.

6. \textit{Copernicus's Modifications of Traditional Impetus Theory}

Considering Copernicus's caution in his treatment of the physical problems inherent in his geokinetic view, and considering that he finally left many of these problems unresolved, we ought to notice the dependence of his views on traditional ideas, in particular on the modes of distinguishing and explaining used by the impetus theory. It is, as we have seen, by no means true that, as Alexandre Koyré (1973, 113) contended, "the theory of \textit{impetus} is not found in the writings of Copernicus"; and it is not true that \textit{De Revolutionibus} refers to the impetus theory only by a single "gelegentliche Erwähnung" (occasional mention) in chapter 8 of Book 1 (\textit{quibus enim vis vel impetus infertur}), as Anneliese Maier has contended.\textsuperscript{21} In addition to several characteristic features that impetus mechanics and Aristotelian physics have in common anyway, Copernicus seems to have borrowed the following specific doctrines from the impetus theory:

1. The view that forced motion is caused by an \textit{impetus}, which is exhausted in the course of motion;
2. The interpretation of accelerated free fall as a compound motion, and the explanation of the acceleration of free fall by an \textit{impetus ponderis};
3. The view that all motions require an instantaneously moving cause, which in the case of natural motions, however, is not a final cause – as Aristotle had assumed.

In addition, Copernicus takes over various doctrines already developed by earlier supporters of the impetus theory, modifying them in certain respects. So he seems to adopt:
4. The geometrization of the concept of natural motion as found in Nicholas of Cusa;
5. The abolishment of final causes (the Aristotelian "unmoved mover," "intelligences") of natural circular motions;

\textsuperscript{20} Cf. Zilsel 1940, 116: "Obviously Copernicus thinks centrifugal force appears only in 'artificial' and not in 'natural' rotation. The modern answer to Ptolemy's objection, the argument that the effects of centrifugal force may be neglected compared with gravity, would not have been entirely out of the way. Copernicus himself uses the analogous argument against the objection that the revolution of the Earth around the Sun must bring about parallactic shiftings of the fixed stars. There he argues quite correctly that these cannot be observed (with the insufficient instruments of his period, as we have to add) because of the great distance of the fixed stars (\textit{De Revolutionibus} I, chap. 10 [...]). Certainly positions of stars could already be measured in antiquity, whereas in the time of Copernicus no way was available of measuring centrifugal forces and comparing them quantitatively with gravitation."

\textsuperscript{21} Anneliese Maier (1968,293). Maier conceded, however, that the adherents of the impetus theory, at least those of the fifteenth and early sixteenth century, "lassen sich meist nur aus gelegentlichen Bemerkungen erschliessen" (in most cases are discernable from (their) occasional remarks only) (ibid., 292).
6. The idea that *impetus* is an unfailing cause as long as it does not come into conflict with the nature or a natural tendency of the moved body or with any external resistance to the motion (cf. Buridan's doctrine of the "permanence" of *impetus*). All these ideas are of strategic importance in Copernicus's anti-Aristotelian argumentation. Widely held by his contemporaries, they could be exploited as pieces of evidence in favor of his reasoning. But in some respects Copernicus went beyond and modified the convictions of traditional impetus mechanics. These modifications were directly necessitated by Copernicus's geokineticism:

7. The relativization of lightness and heaviness – the interpretation of the apparently spontaneous upward motion of light bodies as a forced "motion of expansion" and the reinterpretation of gravity (the idea of a universal relative gravitation);
8. The reduction of the concept of the natural ("simple") motion of "simple" bodies to the circular motion of spherical bodies (rotation);
9. The view that terrestrial and celestial bodies have the same kind of natural motion;
10. The idea that the natural motion of the Earth is persistent;
11. The extension of the concept of forced motion to all rectilinear motions.

With regard to these modifications, it turns out that Copernicus gave a new interpretation to precisely those basic concepts that the traditional impetus theory of motion had taken over without alteration from Aristotelian cosmology. The important thing is that Copernicus's reinterpretation of these concepts concerns exclusively their *extensional meanings*, without changing their original content.

III. The Function of Copernicanism in the Transformation of Impetus Mechanics into Classical Inertial Mechanics

1. *The Mechanical Significance of Cosmology: Benedetti*

The rationality of Copernicus's argumentation consisted precisely in the fact that he did not simply break with traditional physical convictions, but rather confined himself to a merely extensional semantic change of the concepts contained in them. He proceeded extremely economically, limiting that change to alterations that seemed unavoidable in order to defend geokineticism. This economy, however, had disadvantages as well as advantages. On the one hand, the Copernican view remained impugnable; for Copernicus, by not using physical arguments to defend the conceptual change, left the actual physical problems undecided. These problems could be used by his opponents to champion the traditional world view.

On the other hand, Copernicus's argumentation was able to inspire natural philosophers and mechanicians to experiment with his reinterpretation of their own traditional concepts. Even if they were entirely unsympathetic to the new view of the
world, they had to examine whether Copernicus's new classification of natural motions entailed a reasonable theory of nonsimple motions. This fact may be illustrated, for instance, by reference to the Portuguese mathematician Pedro Nunes (1502–1578), who – although a declared anti-Copernican – was deeply impressed by Copernicus's modification of the traditional theory of motion. In his *Rules and Instruments for the Art of Navigation* (1566, chap. 11, 105–6), Nunes wrote: "It is a question for philosophers to discuss whether Copernicus, by means of the arguments used by Ptolemy to show that the Earth has no circular motion at all, reasons soundly when he says that not only the Earth but also earthy things and all heavy things – wherever they may be located – are carried in a natural motion from west to east, while they undergo an additional rectilinear motion when they depart in any way whatever from their natural places, and that circular motion subsists with rectilinear motion not otherwise than being alive with being sick. For nothing will be said to move either away from the middle or toward the middle which does not also move around the same middle. Copernicus revised these principles in order to be able to explain why, if the Earth travels in a circle, heavy bodies hurled upward nevertheless return vertically to those places lying below them."  

A century ago, Emil Wohlwill (1884, 72–79) drew attention to the fact that Copernicanism stimulated the discussion of questions that in a narrower sense were mechanical ones. Indeed, it seems worthwhile to investigate how the problem of analyzing the different types of rotation prompted the discussion of Copernicus's interpretation of rotatory movements. As we have seen, Copernicus maintained that there is a fundamental difference between natural and artificial rotation; but he left open the question as to the true physical reason for this difference. Such a question was of both theoretical and practical importance, because another question was immediately connected with it, namely, what are the physical impediments to persistent artificially produced rotations, and to what extent can they be technically diminished. The connection between these problems can be illustrated by the following example.

Giovanni Battista Benedetti, a convinced Copernican, received an official letter from the Master of the Hospice of the Duke of Savoy, Joannes Paul Capra of Novara, requesting an answer to some mechanical questions. Benedetti was commissioned to solve the following problem: "Suppose a millstone rested on a virtually mathematical point and was set in circular motion; could that circular motion continue without end, it being assumed that the millstone is perfectly round and smooth?" (Benedetti [1585] 1969, 228). We should note that the very question demands a comparison of the rotation of a millstone with that of the terrestrial globe, which indeed rests on a mathematical point. The answer given by Benedetti and published in his *Diversarum Speculationum Mathematicarum et Physicarum Liber* of 1585 (ibid., 228–30) is obviously based on presuppositions that we have met in the eighth chapter of *De Revolutionibus*, in which Copernicus classified artificial rotations as forced motions.

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22 E. Rosen cites this passage in his notes, see Copernicus 1978, 354.
Benedetti tries to give a more sophisticated analysis of artificial rotation in order to explain why that motion is, in fact, not permanent, and why it would not be permanent even if the axis of the rotating body were a mathematical point. His explanation is based on the idea that it is characteristic of forced motions to produce a specific conflict between the *impetus*, as the efficient cause of the forced motion, and certain other conditions of that motion. Benedetti’s view is that in the case of artificial rotations the conflict is between the moving cause of rotation, *impetus*, and the cohesiveness by which the component parts of the rotating body are compelled to move circularly around the axis of the body. As a result of this conflict, he says, the *impetus* becomes weaker, and therefore the rotation cannot be permanent. Benedetti’s explanation that forced motions are nonpermanent because the *impetus* is exhausted as a result of a particular conflict follows traditional (pre-Copernican) lines. His novel idea is that artificial rotation is contrary, not to the *nature* of the rotating circular body itself, but to the *nature of its parts*. The conflict produced in artificial rotations stems from the fact that the *impetus* that set the circular body in motion would impart rectilinear motions to its parts if these parts did not impede one another from following rectilinear paths along (or in parallel to) the tangent of the circular body, i.e., if they were not in each other’s way because of their cohesiveness. Two forces act on each part: the *impetus*, which acts in a straight line and compels the whole body to move around its axis; and the force by which one part violently impedes another from moving in a straight line. This conflict is not between an *impetus* and the *nature* or *natural tendencies* of the body moved, but rather a conflict between an *impetus* and a violent force. The novel idea is not that the rotation of a millstone is a forced motion, but that it is *composed* of two motions, neither of which is natural. Benedetti as it were decomposes the rotating body into its parts and its rotation into their circular translations. Benedetti’s analytical approach seems to be a means to distinguish the causes of artificial rotation from those of natural rotation. The important thing is that it agrees with the Copernican view regarding the essential difference between artificial and natural rotations. As we have seen, Copernicus had interpreted the spherical form of the terrestrial globe (and thus the circular form of the motion of its parts) as a result of the gravity of its parts; and he had assumed that if the speed of its rotation were increased by an *impetus*, the parts would proceed centrifugally. Compare with this what Benedetti writes about a rotating top: “Imagine that while the body, i.e., the top, is spinning around very rapidly it is cut or divided into many parts. You will observe not that those parts immediately fall toward the center of the universe, but that they move in a straight line, and, so to speak, horizontally. No one, so far as I know, has previously made this observation on the subject of the top” (Benedetti [1585] 1969, 229).

Benedetti does not explicitly refer to Copernicus in this context, but it seems evident that he transfers Copernicus’s analytical interpretation of natural rotation to artificial rotation. For Benedetti, the essential difference between artificial and natural rotation is, I believe, that a component part of an artificially rotating body is
being compelled by an external cause, namely, by the other parts, to deviate from motion in a straight line, while in the case of the natural rotation of the Earth its parts deviate from a straight line by nature as a result of their own centripetal tendency. Note that for Benedetti, artificial rotations are unnatural; not, however, because of the moving impetus, but rather because of the conflict between the force exerted by one part of the rotating body on the other by impeding it from proceeding in a straight line. “When these parts are in motion, they have by nature a tendency (impetus) to move along a straight path. Hence, since all these parts are joined and any one of them is continuous with another, they suffer constraint in moving circularly and they remain joined together in such motion only under compulsion. For the more they move, the more they grow in them the natural tendency to move in a straight line, and therefore the more contrary to their nature is their circular motion. And so they come to rest naturally: for, since it is natural to them when they are in motion to move in a straight line, it follows that the more they rotate under compulsion, the more does one part resist the next one and – so to speak – hold back the one in front of it” (ibid., 230). Benedetti here uses an ambiguous concept of nature. His basic presupposition is: “Every heavy body, when moved either naturally or by force, tends by its nature to move in a straight line” (ibid., 230). Accordingly, Benedetti attributes to the “nature” of a single heavy body a certain spontaneous motion, i.e., motion towards the center of the Earth; but, in a second sense, it is the “nature” of this body to perform its (spontaneous or forced) motion in a straight line. In this second sense, however, it is the “nature” of bodies to move in a straight line not only when they are moved by “nature” in the first sense, but also when their movement is not appropriate to that nature. Benedetti applies to “nature” in the second sense Copernicus’s term “naturalis appetitus.” This term designates the tendency of a heavy body to move in a straight line, no matter whether this movement is caused by an impetus or by gravity.

According to the assumption implicit in Benedetti’s theory, the essential difference between artificial and natural rotation is that in the former case external resistances (the parts of the rotating body themselves) come in conflict with the “nature” (in the second sense) of those parts, i.e., their natural tendency to move in a straight line because of their impressed impetus, while in the latter case such a conflict does not take place. Benedetti does not explicitly analyze the rotation of the Earth. But from his analysis of artificial rotation, and taking into account his acceptance of the Copernican world system, we may perhaps conclude that, in his view, the rotation of the Earth does not produce a conflict of the “nature” (in the second sense) of the parts of the terrestrial globe with external resistances. The impetus impressed on them, and their gravity, are both “natural tendencies” to move in a straight line, although in different directions. Insofar as they both belong to the “nature” (in the second sense) of heavy bodies, they cannot come into conflict with each other, but rather result in a circular motion composed of two rectilinear motions. In this sense, the rotation of the Earth could be interpreted as a “natural motion.”
2. Benedetti and the Principle of Inertia: A Critique of Various Interpretations

Benedetti’s advance beyond Copernicus seems to be refining his analytical view of rotation by applying it to artificial rotations. From a retrospective point of view, this was a step towards the principle of inertia. If, however, we do fail to consider the possible (if not probable) connection between Benedetti’s analysis and Copernicanism, we venture an inappropriate judgment upon his achievement. It is frequently said that Benedetti laid the ground for the principle of inertia by breaking with the scholastic idea of circular impetus, i.e., a special kind of impetus that effects only circular motions (Dijksterhuis 1969, 270). This assertion seems to be misguided, for various reasons. First, it cannot be denied that even according to Benedetti, an impetus that moves a body around its axis effects a circular motion. Second, the doctrine that a circular motion that is not rotatory can result from an impetus has apparently never existed in the history of impetus theory. John Buridan and Henry of Hessia, for instance, who occasionally refer to an impetus motionis circularis, obviously did not have in mind translatory motions of heavy bodies on a circular path, but only the rotations of millstones, grindstones, or the celestial spheres around their axes. As for the rest, the assumption that every impetus acts in a straight line has little to do with the principle of inertia, so long as it is unquestioned that impetus is an instantaneously acting, transferable, and exhaustible efficient cause of motion. In this respect, too, Benedetti has kept to traditional convictions.

Anneliese Maier, who likewise interpreted Benedetti’s analysis as “eine erste Spur des Inertialprinzips” (the earliest trace of the principle of inertia) (Maier 1968, 306) made the opposite mistake. She read into Benedetti’s considerations on the impetus that sets a wheel in rotatory motion the assumption that the impetus does not act in a straight line, but “in kreisförmiger Richtung” (a circular direction) (ibid., 307). In her view, Benedetti considers the “circular” action of the impetus to be the reason why rotation does not persist; because the impetus acts circularly, it comes in conflict with the naturalis appetitus of the component parts of a wheel to have a rectilinear motion. Maier has interpreted this tendency (i.e., the appetitus naturalis insitus corpori per lineam rectam progrediendi) as “das Trägheitsprinzip” (principle of inertia) (ibid., 307). For Maier, the conflict between the circular impetus and the rectilinear appetitus naturalis, allegedly maintained by Benedetti, indirectly reveals the incompatibility of the impetus theory with the principle of inertia, and shows that this principle cannot be regarded as a mere “Weiterentwicklung” (further development) of the impetus theory; the principle of inertia “stammt aus ganz anderen Sphären,” (stems from quite different spheres) and occurs in Benedetti’s analysis “in einer Form, die mit der Impetus hypothese nicht in Einklang zu bringen ist, und sie tatsächlich durchkreuzt” (in a form which cannot be reconciled with the impetus
hypothesis and actually contradicts it) (ibid., 306–7). In truth, however, Benedetti’s concept of *appetitus naturalis* does not imply any idea concerning inertia, insofar as it does not refer to the permanence, but only to the direction or the spatial form of motion, no matter whether this motion is (in the traditional sense) natural (i.e., caused by gravity) or forced (i.e., caused by an impressed *impetus*).

Benedetti’s step towards the inertia principle of Classical Mechanics consists, I think, simply in the analytic view of rotation itself. For this view made it possible to regard circular motion around an axis as a composition of rectilinear motions. Stillman Drake (1976, 323) rightly holds Benedetti’s contribution to the development of mathematical physics to consist in having considered motions on curved lines to be composed of rectilinear motions. Drake refers to Benedetti’s analysis of the motion of a projectile released from a spinning sling. Benedetti wrote: “When the projectile is released from the sling it takes its path, with nature as its guide, from the point where it has quit the sling along a line tensioned to the circle which it last made” (Benedetti [1585] 1969, 189). Here, too, Benedetti refers to the “nature” of heavy bodies to proceed straight on if moved by an *impetus*. Then he adds: “Now it is true that the impressed *impetus* gradually and continuosly decreases” (ibid., 189). He says nothing about the cause for that decrease; but we have already seen that, in his view, the *impetus* decreases because of external resistances. In this context, the resistance of the medium of the projectile’s motion may well be the cause. “Hence the downward tendency of the body, caused by its heaviness, enters at once and, mingling itself with the impressed force, does not permit the line to remain straight for long, but causes it quickly to become curved. For the body is moved by two powers, one impressed by force, the other by nature” (ibid., 189). Benedetti accordingly explained the curved path of the projectile by the combination of two rectilinear tendencies. Drake is perfectly right when he comments: “The importance of Benedetti’s solution rests, however, not in the rectilinear character of the path of the stone under impressed impetus, but in the recognition of composition rather than conflict between that impetus and the natural downward tendency of the heavy projectile” (Drake 1976, 323–24). But Drake adds: “To me it is the more surprising that having recognized the composition of tendencies in a projectile, Benedetti should have insisted on their conflict in a rotating body” (ibid., 325). Drake does not take into consideration that whenever Benedetti refers to such a conflict, he has in mind only artificial rotations, namely those of millstones, grindstones, and tops. Moreover, Drake does not see that Benedetti’s analysis of rotations is compatible with considering natural circular motions around an axis (for instance, those of the parts of the terrestrial body) to be compound motions. Therefore it seems that Drake is wrong in maintaining that, according to Benedetti, an *impetus* cannot bring about persistent rotations of celestial bodies. Such a view appears strange, considering Benedetti’s unreserved admiration for the “beautiful system of Aristarchus of Samos that has been so divinely expounded by Nicholas Copernicus” (Benedetti [1585] 1969, 221). Furthermore,
I think that Drake is wrong to interpret Benedetti as having argued that the motion of a body on a surface concentric with the Earth is slowed by a conflict of the straight *impetus* "with the circular path" (Drake 1976, 336). Of course it is true that Benedetti "gave up any chance to conceive of inertia or a conservation of motion as such" (ibid., 325). But that does not mean that Benedetti denied the possibility of permanent motion altogether. On the contrary, Benedetti contributed a great deal to the idea of conservation of motion, although he never gave up of the essential presuppositions of impetus mechanics. From the perspective of Classical Mechanics, his mistake consisted in an incorrect physical explanation of the nonpermanence of artificial rotations, an explanation which implicitly entails an incorrect distinction between artificial and natural rotations.

3. Galileo, Impetus Mechanics, and the Origin of his Principle of Inertia: Drake’s Interpretation

This was the mistake that Galileo corrected. As we shall see, however, it would be unfounded to say that Galileo, by doing this, gave up the essential presuppositions of the impetus theory accepted by Copernicus and Benedetti. Galileo’s advances towards the classical inertial principle obviously were steps on the same ground trod upon by Benedetti. The break with traditional impetus mechanics, which Anneliese Maier held to be recognizable already in Benedetti, had not yet taken place even in Galileo’s considerations. Nevertheless, we can observe in them the decisive breakthrough towards the classical concept of inertia.

It is a widely held opinion that Galileo discovered a general law of inertia – not, however, of “rectilinear inertia,” but only of “circular inertia” – and that this principle of circular inertia was incompatible with the Newtonian law of inertia. It seems to me, however, that Stillman Drake (1968, 282–92) and José Coffa (1968, 261–81) have refuted this opinion, although I do not agree with them that there is a principle of rectilinear inertia in Galileo’s writings. It is true that Galileo took into consideration only one kind of real permanent motion: the rotation of celestial bodies around their own axes. But Newton had no knowledge of other real permanent motions either. Rotation, if free of external resistances, was considered to be a true inertial phenomenon by Newton, too, as shown by the explanatory note in his *Principia* to his First Law of Motion. On the other hand Galileo, like Benedetti, apparently assumed that an *impetus* imparted to a single heavy body acts in a straight direction, but that its gravity prevents the rectilinear motion of that body from ever being realized. So Galileo, in his *Dialogue Concerning the Two Chief World Systems*, (1970, 193) claimed that a rotating wheel or the Earth will tend to hurl an object on tremendous velocities of the heavenly bodies as an objection to the view that the Earth is at rest at the center.

25 The idea of *impetus* as a rectilinear force is expressed in the following words: “Up to this point [Salviati explains to Simplicio] you knew all by yourself that the circular motion of the projector impresses an impetus upon the projectile to move, when they separate, along the straight line tangent to the circle of motion at the point of separation, and that continuing with this motion, it travels ever farther from the thrower.”
its surface in a straight line tangential to its surface, a line which this object "would continue to follow with uniform motion if its weight did not bend it downward" (ibid., 193, 199). Drake and Coffa have taken the fact that Galileo bothers to argue in detail that a rotating Earth will not have dissipative effects as evidence that he could not have subscribed to the law of "circular inertia." According to Galileo, the dissipative effects of rotating bodies depend on their speed and radius. Apparently he thought of circular motion as the resultant of the effects of two rectilinear agencies: *impetus* and gravity. But Galileo tried to avoid making statements about the essence of *impetus* as a rectilinear force, so that modern readers find it hard to recognize any difference between Galileo's use of the concept of *impetus* within certain contexts, on the one hand, and the use of certain dynamical concepts (e.g., the concept of *vis inertiae*) within Classical Mechanics, on the other. There are, perhaps, only two differences between Galileo's and Newton's ideas of the conservation of motion (and it may be that these two differences are connected with one another): (1) Galileo made no attempt to explain the *translatory* motions of celestial bodies on curved lines by rectilinear forces; (2) Galileo did not explicitly give up the idea that the *impetus* is a (potentially) permanent *vis impressa*, that is, a transferable force which, after having been transferred to a movable body, continues to act instantaneously as long and insofar as it has not been exhausted for certain reasons. With regard to this second difference I disagree with Drake and Coffa.

Considering the modernity of Galileo's theory of motion, anachronistic misinterpretations of his theory can easily occur. From a post-Newtonian point of view, the insight into the possibility of the conservation of motion presupposes the insight into the possibility of force-free motions. But Galileo's mechanics does not develop an idea of force-free motions. Hence we are not allowed to presume that Galileo's attempts to prove the possibility of permanent rotations were guided by the desire to exclude motor forces from physics. This presumption would be based on a fallacy that presupposes what is still to be proved: that the rise of classical inertial mechanics became possible only after, and as a result of, the definitive break with the dynamic principles of impetus mechanics. True, impetus mechanics and inertial principles are

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26 Galileo classified not only the rotations of the Earth and other celestial bodies, but also the rectilinear downward motion of heavy bodies towards the center of the Earth, as "natural." There is no real contradiction between (1) Galileo's Copernican view that the "natural" motion of the Earth (*as a whole celestial body*) is its daily rotation (Galileo 1970, 142), and (2) his view that the only "natural" motion of heavy bodies (*as single parts of the terrestrial globe*) is their rectilinear motion towards the center of the Earth. For "natural" is a relation, not a monadic predicate; and (1) and (2) are compatible, provided *horizontal motions* of heavy bodies — i.e., motions of objects on a surface concentric with the Earth — are not forced, i.e., not contrary to the nature of heavy bodies. As we shall see, the young Galileo argued that just this condition is fulfilled. There is thus no reason either to assume with Winifred Wisan that for Galileo the circular motion of heavy bodies around the center of the Earth is their "natural" motion, or to agree with Alan Chalmers and Richard Nicholas, who criticize Wisan's assumption and "are reluctant to attribute to Galileo the Copernican view that the rotation of the Earth is natural." See Wisan 1974, 263; and Chalmers and Nicholas 1983, 333–34 and 339.

27 I suggest that Drake, Coffa, Chalmers, and Nicholas are mistaken when they attribute to Galileo the idea of "inertial motion" in the *Newtonian* sense, i.e., "in the sense that it persists indefinitely without a cause" (Chalmers and Nicholas 1983, 333; italics mine). Galileo "neither raises nor correctly answers the general question how would a body move if no forces acted on it?" (Wisan 1974, 262).
incompatible from a Newtonian point of view. But we should not infer that pre-
Newtonian physics must have explained the possibility of the conservation of motion
without applying dynamic principles resting on a modified impetus theory of motion.

In his article, “A Further Reappraisal of Impetus Theory: Buridan, Benedetti, and
Galileo,” Drake (1976, 326–32) interprets Galileo’s contribution to the development
of the principle of inertia as consisting in a theory of force-free motions. He admits
that “the terminology” of this description of Galileo’s theory is anachronistic, but not
the interpretation itself (ibid., 328). “It is,” Drake writes, “hardly a fair description
of Galileo’s serious physics to say that it remained remote from inertial consider-
ations in the modern sense. Nor is it reasonable to portray it as the direct descen-
dant of medieval impetus theory, in which the whole point was to provide a force capable
of driving the body on in continued motion, whereas Galileo’s earliest analysis
consisted in eliminating the need for any such force” (ibid., 330).

According to Drake, the roots of the idea of force-free motion are to be found
already in Galileo’s early writings, above all in the *De Motu Antiquiora*, probably
written during his stay at the University of Pisa (1589–92). Drake interprets as
force-free those motions clarified by Galileo in this early work as “neither natural nor
forced,” and which, in a marginal note there, he called “neutral motions.” Referring
to this class of motions, Drake writes: “Certainly Galileo’s ideas [ . . . ] did not derive
from Buridan via Benedetti, who rejected the perpetual rotations from which
Galileo first derived his class of motions neither forced nor natural” (ibid., 330). If
my previous interpretation of Benedetti is right, it is probably not true that Benedetti
rejected perpetual rotations in general. Therefore it seems fair to be skeptical about
Drake’s assumption. Below I shall examine the way in which Galileo, in *De Motu*
and his other early writings, came closer to the inertial principle than Benedetti had.

In these writings Galileo seems in no way to have questioned that the presence of
forces is required for the continuation of motions. There is no attempt to criticize the
dynamic principles of impetus mechanics. On the contrary, Galileo uses such prin-
ciples in order to explain motions, for instance the motion of projectiles or the
acceleration of free fall, which he explains by the slackening of a “residual force” by
which the falling body was originally removed from its natural place. Like Coper-
nicus, Benedetti, and Giordano Bruno, Galileo consciously uses the principles of
impetus mechanics as a tool for his anti-Aristotelian argumentation, while sixteenth-

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28 With this interpretation Drake has taken up a proposal of Coffa (cf. his article “Galileo’s Concept of
Inertia” (1968), who performed a conceptual analysis of Galileo’s terrestrial physics in order to refute the
common notion that it involved, or was based on, a concept of “circular inertia.” While Coffa’s analysis
dealt mainly with Galileo’s last books, the *Dialogue* and *Two New Sciences*, Drake extended his interpret-
ation to Galileo’s earliest investigations.

29 A radically new chronology of Galileo’s undated writings has recently been suggested by Carugo and
Crombie 1983, Fasc. 2, 58–68. The reasons they offer for this new ordering, however, are weak, and there
is strong evidence against their proposal. See Wisan 1984, Fasc. 2, 85–88.

30 In *De Motu*, Galileo calls Hipparchus as a witness to the truth of his explanation of the acceleration of
free fall. In my view it is more than questionable whether this reference (which has its basis only in a very
short passage of Simplicius’s commentary on Aristotle’s *De Caelo*) is well-founded. Cf. Wolff, forthcoming.
But it is conspicuous that Galileo’s explanation agrees perfectly with Copernicus’s view, according to
which all rectilinear motions are affected by violent forces.
century Aristotelianism had in the meantime adapted itself to the views of impetus mechanics (Maier 1968, 304–5). There is no indication that Galileo’s early theory of motion entails that there are particular motions, the perpetuation of which does not require the presence of forces. And with respect to the class of motions “neither natural nor forced,” Galileo, as we shall see, considers it appropriate to ask what their causes or forces are.

4. Galileo’s Early Writings and Copernicanism

We should not fail to notice the main motives underlying Galileo’s early theory of motion. Did Galileo write *De Motu* in order to examine arguments for geokineticism? Whatever the answer, it is conspicuous that in his anti-Aristotelian argument Galileo aims partly at the same points as Copernicus did in his reinterpretation of the traditional theory of motion. Emil Wohlwill assumed that *De Motu* has a Copernican background (Wohlwill 1884, 15:72–82). This view is nowadays scarcely accepted, but it is, I believe, based on good reasons. Wohlwill concluded, especially from Galileo’s discussion of the problem of rotation in *De Motu* and *De Motu Dialogus* (likewise about 1590), that the young Galileo was inspired by Copernicus. Wohlwill interpreted Galileo’s famous first letter to Kepler, dated August 4, 1597, as referring to his years at Pisa (where he had written *De Motu* and *De Motu Dialogus*) when he said that he had been an adherent of the Copernican theory “already for many years,” and that by means of this theory he had been able to discover the causes of several physical effects that perhaps could not be satisfactorily explained by the geostatic theory (Galileo 1968, X:68). Wohlwill considered in particular *De Motu Dialogus* to be “ein unzweideutiges Zeugnis für Galileis Beschäftigung mit der Copernicanischen Lehre in der Pisaner Periode” (incontestable evidence of Galileo’s occupation with the Copernican doctrine during his Pisa period) (Wohlwill 1884, 82).

In a passage of this dialogue the discourse turns to the fiction of a marble sphere rotating around its axis, which rests in the center of the universe; with respect to this marble sphere Galileo says (using one of the figures in the dialogue as his spokesman): “[... ] if a beginning of motion were imparted to the sphere by an external

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31 Although nobody will deny that Galileo was acquainted with Copernicus’s *De Revolutionibus* when he wrote *De Motu*; cf. Clavelin 1974, 130. Clavelin points to the fact that in *De Motu* Galileo borrowed a certain argument from Book 3 of Copernicus’s *De Revolutionibus*.

32 Emil Strauss pondered what effects Galileo was referring to; from the fact that Galileo “seine astronomischen Entdeckungen noch nicht gemacht hatte” (had not yet made his astronomical discoveries) and from the presupposition that “die Auffindung des Beharrungsgesetzes, soweit man von einer solchen sprechen darf, und die Untersuchungen über den Fall nicht unmittelbar mit der Frage der Weltsysteme zusammenhangen,” (Galileo’s discovery of the law of inertia – insofar as one may call it thus – and his investigations into the free fall are not directly connected with the problem of the system of the world) Strauss concluded that Galileo must have had in mind the tidal theory he developed at length in the *Dialogus* nearly half a century later (see Galileo [1891] 1982 xvii [Strauss’s “Einleitung”]). Scholars (cf. Drake 1970, 200–13) seem to have generally accepted this conclusion despite the fact that its premises are not really well-founded. Wohlwill’s opposite opinion seems to be forgotten today. His important study of 1883/84 is nowadays scarcely known.
mover, the sphere would perhaps move not with forced but with natural motion. For there would be no resistance at the axis, and the parts of the sphere would neither approach the center of the universe nor recede from it" (Galileo 1968, 1:373–74; trans. Drake and Drabkin 1969, 338). According to Wohlwill, in this passage Galileo has in mind the analogy with the rotation of the Earth, and Wohlwill remarks: “The fact that Galileo is veiling here the true core of his thoughts is in accord with the caution he practiced for another twenty years regarding his relation to the Copernican doctrine. The context of his statements makes it credible that he at that time, like other researchers even later, confined himself to endorsing the daily motion of the Earth” (Wohlwill 1884, 1582).

According to Drake, on the other hand, “it is evident” from just that passage of De Motu Dialogus on which Wohlwill commented “that Galileo was not a Copernican when he wrote this.”33 It is, however, by no means clear where that evidence lies. Drake seems to have in mind something that Wohlwill had already observed, namely, the manner in which Galileo’s spokesman in the Dialogus speaks about a marble sphere rather than about the Earth, or about the center of the Earth, or about the “perhaps” (Drake 1969, 338) natural motion of the marble sphere around its axis rather than about its natural motion. We should pay attention to the reason for just that “perhaps,” given by Galileo’s spokesman; for it seems to indicate that Galileo had a clear idea of the Copernican connotations of the concept of a “natural” rotation: “Now I said ‘perhaps.’ For, if the motion were not forced, it would endure forever; but such eternity of motion seems quite out of keeping with the nature of the Earth itself, to which rest seems more congenial than motion” (ibid., 330).

From Galileo’s remark that [to the Earth] “rest seems more congenial than motion,” one might infer that he was an adherent of the Ptolemaic system when he wrote this. But is this inference valid? Galileo might have let his spokesman speak ironically. We know from Galileo’s letter to Kepler that, “for many years” he did not dare to publish his thoughts concerning the Copernican doctrine because, in view of the predominant anti-Copernicanism of his contemporaries, he feared exposing himself to ridicule. If Galileo intended to publish the Dialogus when he wrote it, he undoubtedly had a motive to conceal his real opinion by using, as he did, an (obviously and deliberately weak) argument in support of the Ptolemaic world system.

Now, whether it be irony or caution or reserve that Galileo displays in the Dialogus, his un-Copernican terminology is similar to that of Benedetti, who used the Aristotelian term “center of the universe” with an explicit reference to the center of the Earth, although he explicitly accepted the Copernican system. Furthermore, Galileo’s 1597 letter to Kepler gives reason for raising the following questions. Since Galileo wrote there that, “for many years,” he was an adherent of the Copernican doctrine because it had enabled him to explain “many” otherwise unexplainable phenomena, one may ask what phenomena he meant? From the notebooks of Paolo

33 See Drake’s footnote to Drabkin’s translation of the Dialogus in Drake and Drabkin 1969, 338.
Sarpi one may infer that Galileo in 1595 disposed of a mechanical explanation of the tides which presupposed the Earth’s rotation (Drake 1970, chap. 10). Emil Strauss conjectured that in his letter to Kepler Galileo had only the tides in mind when he mentioned the “many” phenomena explained by Copernicanism. Strauss argued that Galileo’s investigations concerning free fall and motions on inclined and horizontal planes had nothing to do with the truth or even the physical possibility of the Copernican world (Strauss [1891] 1982 xvii). Strauss’s view is nowadays widely accepted, although it does not make the words in Galileo’s letter to Kepler comprehensible. However, Strauss’s argument seems to me quite unfounded, as I will show below.

Of course, it may be conceded that the early writings of Galileo are in any case not “Copernican” writings in the same sense as, for instance, the Dialogus of 1632 is: they are not propaganda for geokineticism. But we have good reason to examine to what extent they deal with Copernicanism indirectly, by discussing some of its physical problems. Strictly speaking, in the present paper it is not really of interest whether Galileo in De Motu tried to solve Copernican problems. In our context the real question is to what extent Galileo de facto solved certain problems that sooner or later – in any case before he wrote his famous letters to Kepler and Jacopo Mazzoni (May 30, 1597) – he himself identified as Copernican problems.

We have already seen that two of the most important problems discussed in De Motu are: (1) how the relationship between relative and absolute gravity is to be explained physically; and (2) how the supposed difference between natural and artificial rotations can be explained. In De Motu Galileo gives answers to both questions, similar to Benedetti’s answers. For this and for other reasons, it is widely held that Benedetti’s Diversarum Speculationum Liber directly influenced the young Galileo (e.g., Wallace 1984, 230; but see already Wohlwill 1883, 14:385; Vailati 1898, 359—83; Bordiga 1925—26). Some of the similarities may result from the common Copernican view.

Galileo, like Benedetti, tried to solve the first problem by applying the hydrostatic “Principle of Archimedes” to all (light and heavy) elemental bodies: he attributed to all these bodies an essential heaviness or “proper gravity” (propria gravitas [Galileo 1968, I:251; Engl. trans. Galileo 1960, 13]) while explaining the quantitative differences in their weights as the result of their specific weights. Thus the “Principle of Archimedes” acquires a cosmological significance that it did not have in antiquity, because it now helped solve a physical problem of Copernicanism.

As for the second problem, the passage quoted (ibid.) from De Motu Dialogus (Galileo 1968, I:373—74; see above, text with notes 32 and 33) shows that the young Galileo took into account the possibility of a “natural” (and therefore perpetual) rotation of a celestial body such as a marble sphere at the center of the universe, and thus similar to the Earth or (if we take a Copernican view) the Sun. The view that the rotation of a celestial body around its axis is a “natural motion” is also to be found in Galileo’s later writings, for instance in his Dialogus of 1632 (Galileo 1968, 142). In the text of De Motu, however, this view is not expressed with the same distinctness.
At first sight, Galileo seems here to take a quite different position when he classifies rotations as “neither natural nor forced.” Yet we must consider the context of the argument in which Galileo makes use of that classification.

5. The Problem of Perpetual Rotation in Galileo’s “De Motu Antiquiora”

We encounter that classification in chapter 16 of *De Motu*, entitled “On the Question Whether Circular Motion is Natural or Forced.” In discussing this question, Galileo argues in a manner that could best be called dialectic. He begins by defining the “natural” and “forced” motions of heavy bodies as motions toward or away from their natural places. The classification of rotation as “neither natural nor forced” immediately (i.e., logically) follows from these definitions, as we shall see. But we should not think that these definitions are Galileo’s last word on natural and forced motions. Galileo subsequently shows that these definitions entail certain difficulties, even a paradox. Galileo’s argument in the first part of *De Motu* (chapters 1-14), which relates only to the concept of gravity and to rectilinear upward and downward motions, has a similar form. Here, too, Galileo first proceeds (at the beginning of chapter 1) from the traditional concept of the “natural motion” of heavy and light bodies, according to which natural motions are either downward or upward; thereafter, Galileo’s arguments show that natural upward motions are not compatible with the “proper gravity” (*propria gravitas*) of light bodies. Thus Galileo finally gives up that concept of natural motion that he had apparently accepted at first. According to the new concept of natural motion, the apparently spontaneous upward motion of light bodies is to be explained by the “Principle of Archimedes,” and results from their extrusion: Galileo follows Benedetti’s buoyancy theory of rising and falling bodies, and Copernicus’s idea of a universal relative gravitation.34

At the beginning of chapter 16, Galileo defined the natural motions of heavy bodies as motions toward their natural places, and forced motions as motions away from their natural places (Galileo 1968, 1:304; Engl. trans. Galileo 1960, 72). This definition immediately seems to imply that circular motions of heavy bodies, and therefore rotations, cannot simply be subsumed under the class of either natural or forced motions: they seem to be neither simply natural nor simply forced. But it would be surprising if that were already the answer to the title-question of chapter 16.

Galileo went on to discuss four different cases of circular motions, in each case the rotation of a sphere around its axis. The sphere consists either of homogeneous or of heterogeneous parts, and its center is either the center of the universe or another center. With one exception (the case of the sphere of heterogeneous parts, its center not at the center of the universe), all rotations are classified as “neither natural nor

34 In his *Memoranda on Motion* (written about 1590) Galileo writes: “Upward motion is not natural,” “downward motion is far more natural than upward motion” (1968, vol. 1, 413–14; English translation in Drake and Drabkin 1969, 384).
forced motions." For, if we take into consideration not the motion of the sphere as a whole, but only the motion of its parts, they and their center of gravity respectively are not moved towards the center of the universe as their natural place. Here we observe in Galileo's argument the same analytical approach that we have already observed in Benedetti. But unlike Benedetti, Galileo considers the circular motions not only of the parts of the sphere, but also of their center of gravity. The center of gravity of the parts of a rotating sphere, whose center is not at the center of the universe, is either moved or not. If not moved, it moves neither towards the center of the universe, nor away from it; so the motion of a homogeneous sphere is "neither natural nor forced." If the center of gravity is moved, it sometimes does and sometimes does not move towards the center of the universe. In this case, the rotating sphere is heterogeneous, and its motion is not "neither natural nor forced," but rather sometimes natural and sometimes forced" (Galileo 1968, 1:307; Engl. trans. Galileo 1960, 75).

Galileo seems to want to argue for his modification of Benedetti's analytical approach by showing that the location of the center of gravity is relevant to the classification of rotations, and that his new analysis leads to a result different from Benedetti's. For it is obvious that, according to Galileo's new analysis, an essential difference between a rotation around the center of the universe and other rotations does not seem to be necessary. Therefore the Copernican idea of an essential difference between "natural" and "artificial" rotations does not seem to have a secure basis. We should notice, in this context, that the definition of natural and forced motions used by Galileo in this argument accords with Benedetti's assumptions concerning rectilinear natural or forced motions.

However, like Benedetti, Galileo is interested not merely in a conceptual classification of rotations, but also in discussing the physical problem of whether perpetual rotations are possible. From Galileo's mere classification of rotations, nothing can be concluded regarding that problem. Galileo demonstrates the insufficiency of this classification (and therefore indirectly of his definition of natural and forced motions) by showing that it entails a contradiction. If the rotation of a sphere is "neither natural nor forced," it will move perpetually as well as not perpetually: "For if its motion is not contrary to nature, it seems that it should move perpetually; but if its motion is not according to nature, it seems that it should finally come to rest" (Galileo 1968, 1:305; Engl. trans. Galileo 1960, 73).

Here we must note that the supposed contradiction occurs and Galileo's argument is convincing only because he has implicitly revised his definition of natural and forced motion. By presupposing that natural motions are perpetual and forced motions are not perpetual, he adopts a Copernican view, but at the same time he contradicts his definition of natural and forced motion, which defines these motions by the concept of natural place; and the natural place of a heavy body is supposed to be the end of its natural motion. Therefore it is obvious that Galileo here abandons his previous position and makes the answer to the question whether rotations are
natural motions or not, conditional on the solution to the physical problem whether rotations are perpetual motions or not.

Within chapter 16 there are only vague suggestions of an answer to that physical problem. But these suggestions show clearly enough that Galileo has a solution of this problem in mind which deviates from Benedetti’s result. In order to reconstruct Galileo’s solution, we have to look at what he says about the possibility of a perpetual motion of a sphere with regard to each of four cases. Let us consider these cases separately:

(a) The heterogeneous sphere, having its center outside the center of the universe and a rotation that is a “sometimes natural and sometimes forced” motion, is not perpetually moved. For “while it moves from the highest to the lowest position, its motion will surely be natural. But with the impetus it has received, it cannot be lifted forcibly to the same extent as it has fallen naturally. The reason for this will be explained in its proper place, but because of this fact the difficulty in the ascent is greater than the downward tendency in the descent”(Galileo 1968, I:307; Engl. trans. Galileo 1960, 75–76).

(b) Such a difficulty does not exist for a homogeneous sphere outside the center of the universe. “For the center of gravity of the sphere neither approaches nor recedes from the center of the universe, and the weight of the parts of the sphere that are moving up is equal to the weight of the parts that are moving down, so that the sphere is always in equilibrium”(Galileo 1968, I:306; Engl. trans. Galileo 1960, 75). But Galileo adds: “Yet by accident such a rotation turns out to be a case of forced motion, because, of course, there is resistance at the supports of the axis. [ ... ] But the finer and the smoother the ends of the axis are, the less resistance will they encounter: so that, if we imagine them to be indivisible, no resistance will then arise from them. It also happens that such motion is retarded by the condition of the surface of the sphere, if it is rough and uneven” (ibid., 93). Here we have mutatis mutandis Galileo’s answer to Benedetti’s problem of whether the rotation of a millstone, its axis being reduced to a virtually mathematical point, is perpetual or not. Galileo does not explicitly say that it is substantially not “a case of forced motion.”

(c) Nor is the rotation of a heterogeneous sphere around its center, this being the center of the universe, a forced motion. True, the center of gravity, which is in this case different from the center of the universe, will be kept outside it only by force. “But rotation of the sphere about the center of the universe would not be by force, for in such a rotation the center of gravity would describe a circle about the center of the universe, neither approaching it nor receding from it. This being the case, it will rotate neither naturally nor by force”(Galileo 1968, I:305; Engl. trans. Galileo 1960, 73). Again Galileo does not explicitly say whether we have here a case of perpetual motion. Apparently he only wants to argue that if a sphere rotates around its
(stereometric) center, this being at the same time the center of the universe, the mere deviation of the center of gravity of that sphere from the center of the universe does not have any significance for the question of whether the sphere will rotate perpetually or not.

(d) Therefore the decisive question is whether a homogeneous sphere that has its (stereometric) center at the center of the universe is able to rotate perpetually "after receiving a start of motion from an external mover" (Galileo 1968, 1:305; Engl. trans. Galileo 1960, 73). Again we do not get a direct answer. But Galileo seems to have an answer to the question. For he declares that "this [viz. chapter 16] is not the place to answer it. For we must first consider by what agency (a quo) nonnatural motion takes place" (Galileo 1968, 1:306; Engl. trans. Galileo 1960, 74–75).

If we want to reconstruct Galileo's answer, we have to understand to what question Galileo alludes. I. E. Drabkin held that Galileo is referring here to the question "by what agency projectiles are moved." This question is the title-question of chapter 17, and Drabkin may have believed that the remark, "first we must consider by what agency etc.," in chapter 16 refers to the ensuing chapter. But in chapter 17 there seems to be nothing that immediately entails an argument for or against the possibility of perpetual rotation. Therefore we should bear in mind that Galileo's treatise De Motu must be regarded as a text in the planning stage (Fredette 1972, 335–36). Drake presumed that a marginal note in chapter 14 was added by Galileo after he had written chapter 16 (Drake 1976, 327). Therefore it is not out of the question that Galileo discussed the problem "by what agency nonnatural motion takes place" earlier than in chapter 16.

6. Perpetual Rotation and "Neutral Motion"

Emil Wohlwill assumed that Galileo's discussion of this problem has its place in the immediate context of just that marginal note that Drake presumed to have been written after chapter 16 (Wohlwill 1884, 15:78–79). It is conspicuous that, within the immediate context of this marginal note, Galileo also called the resistance-free motion of bodies supported on a horizontal surface concentric with the Earth "neither natural nor forced" (Galileo 1968, 1:299; Engl. trans. Galileo 1960, 66). There he said, that such a motion can be caused "by the smallest of all possible forces": "A body subject to no external resistance on a plane sloping, no matter how little, below the horizon will move down in natural motion, without the application of any external force. [. . .] And the same body on a plane sloping upward, no matter how little, above the horizon, does not move up [the plane] except by force. And so the conclusion remains that [A] on the horizontal plane itself the motion of the body is neither natural nor forced. But if its motion is not forced motion, then [B] it can be made to move by the smallest of all possible forces" (ibid.).

35 Cf. Drabkin's footnote 7 to On Motion, in Galileo 1960, 75.
We must consider whether this passage, referring to the “smallest of all possible forces” as an agency of motions that are “neither natural nor forced,” is an answer to the question, “by what agency nonnatural motion takes place.”

First we must direct our attention to some details: In the quoted passage, Galileo’s classification of a motion as “neither natural nor forced” does not refer to the rotation of a sphere, but to the motion of a heavy body supported on a horizontal surface concentric with the terrestrial globe. But here this classification is not, as in chapter 16, presented as an inference from a mere definition, but as a conclusion from certain dynamic premises. Therefore the conclusion itself has a dynamic content. It means that the agency of a nonnatural motion of a heavy body (of any size) can be a force however small, because neither a natural tendency (gravity) nor a violent force contrary to the nature of the heavy body resists it. Galileo complements this argument by a certain mathematical proof, to which he adds, in the already mentioned marginal note, that motions neither natural nor forced should be called “neutral motions” (Galileo 1960, 67). In that note he also says that “mixed motions” (i.e. motions natural as well as forced) do not exist. “For since the forced motion of heavy bodies is away from the center, and their natural motion toward the center, the motion which is partly upward and partly downward cannot be compounded from these two; unless perhaps we should say that such a mixed motion is that which takes place on the circumference of a circle around the center of the universe. But such motion will be better described as ‘neutral’ than as ‘mixed.’ For ‘mixed’ partakes of both, ‘neutral’ of neither” (Galileo 1960, 67). The De Motu Dialogus (Galileo 1968, I:373; Engl. trans. Drake and Drabkin 1969, 373) shows that by “mixed motions” Galileo occasionally means rotations. Therefore the marginal note apparently expresses the idea that rotations are not mixed motions. It indicates that Galileo has reduced the nonviolent character of rotations to the nonviolent character of the motion of heavy bodies that, being supported on a horizontal surface concentric with the Earth, move neither upward nor downward. In other words, the note seems to document Galileo’s idea that the dynamic analysis of rotations depends on the dynamic analysis of the horizontal translatory motion of heavy bodies.

If this interpretation is correct, we may assume that Galileo’s dynamic analysis of “neutral motions” in chapter 14 has to do with the rotation problem in chapter 16. But now the question arises what this analysis contributes to the solution of the physical problem of whether a rotation that is “neither natural nor forced” can be a perpetual motion.

If my interpretation is correct, Galileo’s answer to the question “by what agency nonnatural motions take place” is: Whatever the size or weight of the moved body is, it can be moved by the smallest of all possible forces if that motion is neither natural nor forced, i.e., if it is a horizontal motion. This answer seems to me equivalent with

36 Regarding these “neutral motions” Drake has said with good reasons that they “certainly represent the first clue from which Galileo proceeded over a period of years to his restricted inertial principle for terrestrial physics” (1976, 327–28).
the idea that heavy bodies have a disposition in favor of neutral motions, although these motions are not natural. In his later writings (for the first time in *Le Meccaniche*, written not later than 1600) Galileo calls this disposition of a heavy body its "indifference between motion and rest." It is commonly held that this concept of indifference belongs to "the essential core of the inertial concept" (Drake 1970, 251). A body that is "indifferent" to motion and rest, or that has a disposition in favor of neutral motion, will not begin that motion without being moved by an external force, however small; but on the other hand, once set into neutral motion by some external mover, neither will it come to rest spontaneously. In other words, because neutral motions are not violent motions and not contrary to the nature of heavy bodies, they continue as long as there is no external resistance against them.

It must be admitted that in his early writings Galileo does not explicitly draw these consequences from his assumptions that neutral motions are not forced motions and that the least force is sufficient to produce them. Furthermore, he does not explicitly state the possibility of the perpetual rotation of a sphere with its center at the center of the universe. This conclusion would immediately follow from the assumption that all heavy bodies have a disposition in favor of neutral motions. But only in his later writings does he draw such a conclusion: there he deduces the perpetual continuance of motions from the indifference of heavy bodies between motion and rest wherever he discusses the possibility of perpetual motions (Wohlwill 1884, 15:79). It seems to me appropriate to say that Galileo here, by drawing such a conclusion, uses a physical argument for Copernicanism in the sense that he explains geokineticism by the disposition of heavy bodies in favor of neutral motions in this context (i.e. their indifference between motion and rest appears as an explanans of the Earth's mobility).

The physical argument used by Galileo in his later writings to demonstrate the indifference of heavy bodies towards motion and rest is his reference to the supposed fact that these bodies, when supported on horizontal surfaces, can be moved horizontally by the smallest of all possible forces, if there is no external resistance. It is precisely to this supposed fact that Galileo refers in chapter 14 of *De Motu*. But in this chapter the fact is not explicitly presented as an argument for the thesis that neutral motions are perpetual; furthermore, it does not appear as part of an explanans of the perpetual rotation of the Earth. In other words, it does not appear there as the premise of an inference. On the contrary, as we have already seen, Galileo deduces it as part of the conclusio of an inference. As we have seen, his argument proceeds from a consideration of the natural motions of heavy bodies on inclined planes to the limiting case of motions on horizontal planes, and concludes that (A) these motions are not forced, and (B) they can therefore be produced by the smallest of all possible forces (see above, first section of this chapter).

Cf. Galileo 1960, 171. "Hence it is perfectly clear that on an exactly balanced surface the ball would remain indifferent and questioning between motion and rest, so that any the least force would be sufficient to move it, just as on the other hand any little resistance, such as that merely of the air that surrounds it, would be capable of holding it still."

A similar view is to be found already in Wohlwill 1884, 15:79.
7. Galileo's Apparent Withdrawal of Impetus Mechanics

There are two possible interpretations of Galileo's aim in this argument. Either he wants to prove his conclusion true; or, being already convinced of its truth for some other reasons, he wants to show from what premises the conclusion can be deduced.

In order to decide which interpretation is to be preferred, we must first note that the two parts of the conclusion

(A) “horizontal motions are not forced”

and

(B) “they can be produced by the smallest of all possible forces”

are incompatible from the point of view of certain pre-Copernican theories. According to Aristotelian physics, every motion produced by an external force, no matter how small, is a forced motion. If we consider that Aristotelian physics presupposed a strict dichotomy between natural and forced motions, Galileo's conclusion is not really cogent. It would be cogent only if it were already presupposed that horizontal motions are not forced. Therefore, if we were to regard Galileo's argument as a deductive proof of the conclusio, his reasoning would contain a vicious circle. It would not ground the thesis that horizontal motions are “not forced,” and are therefore “neutral motions” in the sense of being nonnatural. It would actually be based on a merely arbitrary decision against the Aristotelian view.

It seems to me that this vicious circle is why Drake favors the opinion that Galileo's concepts of “neutral motion” and “indifference to motion or to rest,” and, as a result, his restricted inertial principle for terrestrial physics, were possible only because he did not apply or (partially) rejected impetus mechanics as a general means of explaining motions. Drake seems to assume that impetus mechanics, like the Aristotelian theory of motion, generally denied that (sublunar) rotations around the center of the universe, if caused by an external efficient cause and therefore not natural motions in the Aristotelian sense, are nevertheless not forced motions and, consequently, can be persistent. Drake writes: “It is important to note that the origin of his concept [i.e. Galileo's concept of a ‘neutral’ motion] was in no way related to impetus theory, with which Galileo was perfectly satisfied at the time” (Drake 1970, 250). And: “In my opinion the essential core of the inertial concept lies in the idea, explicitly stated above, of a body's indifference to motion or to rest and its continuance in the state it is once given. This idea is, to the best of my knowledge, original with Galileo. It is not derived from, or even compatible with, impetus theory, which assumed a natural tendency of every body to come to rest” (ibid., 251). The supposed incompatibility of Galileo's ideas with the impetus theory implies that they could not be derived from it. Hence they seem to be based on a decision against impetus theory (and all the more against the Aristotelian theory of motion), and this decision apparently could not be justified by any argument deducible from impetus theory or the Aristotelian theory of motion. It is true that the ideas of “neutral motion” and “indifference to motion and rest” could not simply be derived either from the Aristotelian theory of motion or from the common impetus theory, but they
could be derived from a modified impetus theory. This modification was not the work of Galileo alone. The idea that horizontal motion is not contrary to the nature of heavy bodies is implied in Copernicus's cosmology, which assumes that the rotation of the Earth, as a whole of heavy bodies, is appropriate to its nature as a whole, and which—if my interpretation is right—at least does not exclude the possibility that this rotation is caused by an *impetus*. As we have seen, Oresme had already taught that a rotation (of the Earth or of fire) around the center of the universe is not contrary to the nature of these elements, even if that motion is caused by an external motive cause. It is noteworthy that in his commentary on Aristotle’s physics, which was known to the young Galileo, John Philoponus, who is said to be the initiator of impetus theory, taught that the rotation of the fire sphere, which is moved around the center of the universe by an impressed force imparted to the fire sphere by the moved celestial spheres, is neither natural nor forced, but “supernatural” (*hyperphýes*) (Vitelli 1887, 378.26). The Aristotelian strict dichotomy between natural and forced motions was not generally accepted by the adherents of impetus mechanics. It must be admitted that impetus theorists generally held that impressed forces are “fatigable,” so that motions which are their instantaneous effects cannot be perpetual. However, according to the impetus theory (not in Philoponus, but at least since Buridan) fatigability does not mean fatigue, and impetus theory by no means “assumed a natural tendency of every body to come to rest,” as Drake writes. Rather, an *impetus*, although a fatigable impressed force, does not fatigue if there is no internal or external resistance that exhausts the impressed force. For Buridan, therefore, the rotation of the celestial sphere is “permanent,” because it has no external resistance and is not contrary to its nature, although it is caused by an impressed force. Nicholas of Cusa synthesized this idea of permanent rotation with the idea that rotations of spherical bodies are generally not forced motions but, although caused by an *impetus*, are appropriate to the nature of those bodies. It was just this synthesis that Copernicus utilized in order to prove the possibility of the Earth’s rotation.

Hence it is obvious that Galileo’s conclusion (*A*) (horizontal motion “is not forced”) does not express a completely original idea; rather it follows analytically...
from the Copernican thesis that the Earth's rotation is not a forced motion. On the other hand, we must consider that the Copernican thesis, and therefore \( A \), was at that time highly controversial. And if it were only the result of an inference that is in fact a vicious circle, it could not be very convincing.

8. Copernicanism and the Continuous Transition from Impetus Mechanics to Classical Inertia! Mechanics

We are not compelled to interpret Galileo’s inference as a demonstration of a theorem on the basis of well-established premises. Galileo’s inference is perhaps only an attempt to show what premises are presupposed if one accepts what the conclusion (which is held to be proved on other grounds) maintains to be true. This second interpretation fits well with the fact that in chapter 14 there are other demonstrations, meant to prove the truth not of conclusion \( A \), but only of conclusion \( B \) (that horizontal motions can be produced by the smallest of all possible forces). We need not discuss the details of these demonstrations here. But it is noteworthy that Galileo holds that conclusion \( B \) states a physical fact that can be demonstrated empirically. Galileo knows that this supposed fact is an idealized phenomenon, because “our demonstrations [... ] must be understood of bodies free from all external resistance. But since it is perhaps possible to find such bodies in the realm of matter, one who performs an experiment on the subject should not be surprised if the experiment fails, that is, if a large sphere, even though it is on a horizontal plane, cannot be moved with a minimal force. For [... ] a plane cannot actually be parallel to the horizon, since the surface of the Earth is spherical, and a plane cannot be parallel to such a surface. Hence, since in the aforesaid experiment the plane touches the sphere in only one point, if we move away from that point, we shall have to be moving up. There is good reason, therefore, why it will not be possible to move the sphere from that point with an arbitrarily minimal force” (Galileo 1968, I:301; Engl. trans. Galileo 1960, 68).

The discovery that, if there is no external resistance, the smallest force is sufficient to move a heavy body of any size and of any weight horizontally was not made by Galileo alone, and it seems to have been uncontroversial among mechanicians of the time, although it contradicts certain traditional dynamic convictions. Aristotle had taught that the weights of heavy bodies moved were \textit{ceteris paribus} proportional to the motive forces (Aristotle 1936, \textit{Physics} VII: 5). As opposed to this doctrine, Cardano, in his \textit{Opus Novum de Proportionibus} of 1570, maintained: “Every spherical body being in contact with a plane [horizontal] surface only in one point is laterally moved by any force which is able to cut the medium for, because the body is moved neither upward nor downward, but, so to speak, circularly around the center of the universe,
its weight does not act” (prop. 40).

41 Benedetti made a similar statement in his Speculationum Liber ([1585] 1969): “it will be clear that a force no matter how small (so to speak)” drawing or impelling the center of gravity of a spherical body supported on a horizontal surface will move that body in any horizontal direction (Benedetti [1585] 1969, 185). Like Cardano and Galileo, Benedetti emphasized that horizontal surfaces are spherical and concentric with the Earth.

Therefore it is obvious that Galileo’s conclusion \((B)\) is actually not a novel idea.\(^42\)

What is new in Galileo’s argument however, is that he regards conclusion \((B)\) as a direct consequence of conclusion \((A)\). Thus, by assuming that \((A)\) (“horizontal motions are not forced”) entails \((B)\) (“they can be produced by the smallest of all possible forces”), Galileo offers an explanation of \((B)\) that is different from Cardano’s explanation. Neither Cardano nor Benedetti seems to have attempted to explain the easy mobility of spherical heavy bodies supported on a surface concentric with the Earth by the assumption that horizontal motions are not forced. This is the step taken by the young Galileo: He seems to have noticed that \((B)\) can be regarded as a logical consequence of \((A)\); for \((B)\) is indeed a logical consequence of \((A)\) if we regard \((B)\) as conclusion of an enthymeme that has as its second (tacit) premise the proposition \((C)\), that (provided there is no obstacle) motions not forced take place without application of any (external) force. \((C)\) undoubtedly would have been accepted by the adherents of Aristotle as well as by those of Copernicus (and impetus theory). Assuming \((A)\) that horizontal motions, although not natural, are nevertheless not forced (and therefore “neutral motions”), Galileo uses a theory as explanans which is in full accordance with Copernicus’s geokineticism\(^43\) and which, on the other hand, contradicts the traditional Aristotelian theory of natural and forced motions. Galileo seems to have noticed that only the Copernican view makes the physical phenomenon \((B)\), denied by Aristotle’s physics, explainable, that motive forces and weights of horizontally moved bodies are \(ceteris paribus\) not really proportional. Since the Copernican view was at the time highly controversial, it is unlikely that Galileo should not have recognized that his explanation included a decision in favor of Copernicanism.

IV. Conclusion: Galileo’s Physical Argument for Copernicanism as an Abductive Inference

Indeed, Galileo’s explanation was \textit{de facto} a physical argument for Copernicanism. If my interpretation is correct, this explanation can be reconstructed as an abductive argument for geokineticism having the following logical form:

\(^{41}\) Cf. Cardano [1662] 1967, 480: Omne corpus sphaericum tangens planum in puncto movetur ad latus per quamcumque vim, quae medium dividere potest. [ . . . ] Nam cum non ascendat, nec descendat, sed quasi in circulo ad centrum mundi moveatur, pondus non affert.

\(^{42}\) It is an idea that may have resulted from the application of certain principles of statics to the analysis of artificial rotations. I owe this hint to Gideon Freudenthal and Jürgen Renn.

\(^{43}\) See note 25.
The surprising phenomenon \( P \) is observed; 
\( P \) would be explained as a matter of course if hypothesis \( H \) were true; hence, there is reason to think that \( H \) is true.\(^4^4\)

If we interpret \( P \) as \( (B) \) and \( H \) as \( (A) \), we see that the explanation of \( (B) \) by \( (A) \) is an argument for \( (A) \).

According to Charles Sanders Peirce, abductive inferences play a decisive role in the generation of new theories. This seems to be true at least in the case of Galileo: his abductive physical argument for Copernicanism opened the way to a new science of motion: Classical Mechanics.

In his later writings, Galileo made explicit use of his physical argument.\(^4^5\) Unlike his explanation of the tides, it was an argument for geokineticism not in the sense of an argument for the reality, but only in the sense of an argument for the possibility of the Earth’s rotation.\(^4^6\) Galileo’s argument shows that his decision against the Aristotelian and in favor of the Copernican view was not a merely arbitrary break with traditional theories of motion. Although his argument prepared the essential core of the inertial principle (which in its unrestricted Newtonian formulation contradicts the dynamic principles of impetus theory), there is no contradiction between Galileo’s argument and impetus theory. On the contrary, insofar as Copernicus himself had based his astronomy on the principles of impetus theory, Galileo’s argument was based on them too. Copernicanism made possible the continuous transition from the conceptual framework of the theory of impetus to that of classical inertial mechanics.

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\(^4^4\) Abduction takes this form according to Peirce and Hanson. See Hartshorne 1960, V: 189; and Hanson 1958, 86. The word “surprising” in the scheme of the first premise should not be understood in the sense of “unexpected by person \( X \),” but rather in the sense of “hitherto not satisfactorily explained in a scientific discipline and in need of scientific explanation.”


\(^4^6\) When Strauss ([1891] 1982, xvii) wrote that Galileo regarded his explanation of the tides as his sole decisive argument for Copernicanism, he may have had in mind that, unlike Galileo’s other arguments, it argued for the reality and not merely for the possibility of the Earth’s rotation.


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