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Re-examining Galileo's Theory of Tides

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Introduction

Some fifteen years ago, Stillman Drake pointed out in his article on *History of Science and Tide Theories*[1] that the state of historical research at that time concerning Galileo's theory of tides was highly unsatisfactory. More specifically, what deserved re-examining was the Italian scientist's claim to having furnished a physical proof of the Copernican double motion of the Earth (the annual revolution and the diurnal rotation) by means of a model that explained the ebb and flow of the oceans as a consequence of the acceleration and retardation imparted to sea water by the composition of two speeds: that due to the annual motion of the terrestrial globe and that due to its diurnal rotation.

Today, Drakes argument still holds true. On the one hand, owing to a lack of mathematical knowledge, many professional historians have tended to oversimplify Galileo's theory, so that what has usually been presented as Galileo's position is but 'a parody or caricature of it'.[2] On the other hand, the very few working scientists who have paid some desultory attention to the history of their chosen disciplines have drawn overly facile conclusions regarding Galileo's conception of tide motions, probably without direct consultation of all the original sources. As a result, Galileo's theory of tides has frequently been brushed aside as being an unfortunate episode, unable as it was to explain the flux and reflux of the sea and patently wrong in its claiming to demonstrate the double motion of the Earth, namely, the diurnal rotation about its polar axis and the annual revolution around the Sun. Yet, there is no getting away from the fact that precisely such a 'bizarre' theory was uppermost in Galileo's mind throughout his long scientific career, for it was, in his view, the very ground upon which the Copernican world system was eventually to be founded and physically proved.

In this study, I have therefore set myself the twin task of re-proposing the Galilean tide model in all its complexity by bringing to light its completely forgotten vision of the flux and reflux of the sea as a wave-like phenomenon and reassessing its claim to being a physical proof of the double motion which Copernicus had assigned to the Earth in his *De Revolutionibus*.

During the course of my investigations I was on one occasion faced with an *impasse*. I had long been at a loss as to how to explain how Galileo could possibly have reached at least one of his two most insightful and surprising conclusions regarding the dependence of the reciprocation time – in modern terms, frequency of oscillation – of different sea basins on their different depths and lengths. He clearly formulates two laws: a) that frequency increases with depth (width remaining constant), and b) that frequency decreases with width (depth remaining constant). Amazing as it may seem, he gives us no clue to the specific research strategy which must have brought him to such discoveries, nor does he bother to justify his assertions. Why? I was baffled. The only thing which is clearly asserted, though not directly referring to the laws of oscillation, is that he had been trying in many ways to simulate tide-related phenomena by means of mechanical models. I therefore decided to repeat – re-invent, if you like – his experiments using a very simple parallelepipedal glass tank and, sure enough, found a possible answer

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[1] S. Drake, "History of Science and Tide Theories", *Physis*, XXI, 1979, pp. 61–69.
[2] *Ibid.*, p. 63.

to my question. As we shall see, Galileo might possibly have observed much more in his experiments than his physics and mathematics were able to cope with. For, he must have found himself face to face with the formidable difficulties of wave motions and undulatory phenomena.

At the very heart of this research programme lay his quest for a physical proof of Copernican astronomy. It was the idea of devising a periodic tide-generating cause whose law could be deduced from the kinematic principle of composition of uniform or quasi-uniform motions. The Earth-Moon system orbiting the Sun could be studied as a two-body system giving rise to periodic regularities in the rate of change of motion of the surface of the Earth that, on a first approximation, were comparable with the main periods of tides commonly accepted in Galileo's time. The 'crucible' wherein this periodic cause was to be transformed into tidal ebb and flow was the ocean basin, the geometry of which became, in his theory, the key to the understanding of the striking variety that wave-like tide phenomena manifest, beyond their basic periodicities. This is the very key that has been lost by science historians, a loss that has so far precluded a more balanced assessment of the many aspects of Galileo's tide theory.

Galileo did not wholly succeed in cracking the code of the 'chemistry' of tidal waves. This would have required analytical methods much more powerful than the elementary theory of proportions, which was all the daily tools of his mathematical laboratory amounted to. He was nonetheless able to come up with a satisfactorily working 'principle of superimposition of waves'.

According to this principle, different periodic patterns of behaviour could be predicted with some degree of numerical accuracy, and the absolute wave could be thought of as being the result of a wave induced by the 'external' cause due to the motions of the Earth-Moon system and a wave due to the 'internal' response of the basin. This response was regulated by the geometric characteristics of each particular sea basin. The global picture that emerges from my study indicates that the two laws of motion within artificial vessels and sea basins must have been regarded by the Pisan scientist as being only a partial success. They represent in all probability just the outcroppings of a more ambitious research programme.

This ampler vision aimed to cast light on the multifaceted nature of the relationship between, on the one hand, the motions of sea water on the surface of the moving Earth and of the superficial air strata of the atmosphere girdling a rotating planet and, on the other, the Earth's Copernican motions. In spite of Galileo's long labouring at his intuition of the existence of such a complex relationship, his overall programme has hitherto been overlooked and his incomplete achievements dismissed virtually unanimously by science historians as being simply irrelevant and/or patently wrong. In order to illumine the background against which Galileo's reasoning on the problem of tides evolved, I have also repeated a few extremely interesting experiments with rotating vessels (both closed and open) to test their ability to drag the air contained inside. One of these experiments is clearly described by Galileo himself, while the others are proposed by Galileo's most able opponent, the Jesuit, Orazio Grassi, Professor of Mathematics at the Collegio Romano. The broader intellectual arena of this debate is the dispute over the theory of the origin of comets, but the outcome of the battle of interpretations these experiments gave rise to bears on Galileo's theory of trade winds, which in turn – as we shall see – is strictly connected with his tide model. Indeed, this series of experiments suggests that Galileo might have conceived of his tide theory as being part of a broader system, according to which the physical interaction between fluid currents and/or waves within a moving vessel and the motion of the 'vessel' was to be explained.

In Galileo's view, the terrestrial physics of fluids within spinning buckets was to become a mechanical model of a cosmological effect: the interaction between the Earth rotating about its polar axis (while orbiting the Sun) and the sea and wind motions on its surface. On the other hand, Grassi's conception of the same phenomena stuck to the Aristotelian creed of the crystalline spheres. Yet, the outcome of *my* experiments is strictly in accordance with the observations reported by *both* Grassi *and* Galileo. As I shall argue, this shows that Galileo's interpretation of the 'dragging' effect occurring near the wall of a rotating vessel as being due only to a local phenomenon caused by friction was supported by the evidence furnished by his experiment, but it also makes clear that Grassi's objections, based on more complicated, though not conceptually equivalent, versions of the Galilean experiment, were not naïve at all. On the contrary, they were certainly established on the sound basis of a genuinely experimental attitude that clearly emerges from behind the scenes of Grassi's preoccupation of saving the 'old' cosmology of the solid celestial orbs.

All in all, Galileo's claim to having furnished a physical proof of the Copernican astronomy based on a causal link between tides and the motions of the Earth (and of the Moon, insofar as the tide monthly period is concerned) has turned out to be a fascinating intellectual problem to our understanding of motion in the universe. In grappling with this challenge, I have come to believe that it extended far beyond the scope of his physics and astronomy – particularly of his mathematical language – and that his genius expressed itself through a powerful intuition, with which his technical resources simply could not keep pace. On this ground, I feel justified, in making his challenge 'explicit', in having gone so far as to re-formulate it in the more advanced language of today's rational mechanics. After all, whereas historical facts belong to the past, historians' questions are necessarily posed in the present. Although mine might be regarded as an 'a-historical' treatment, I believe it is a proper way to answer the following historiographical question: did Galileo succeed in attributing to tide phenomena the status of physical proof of the Copernican motions of the Earth?

To understand why Earth's motions and tides are inextricably intertwined, we have therefore to shift our point of view and look at the equations of motion that describe the dynamic behaviour of bodies subjected to universal gravitation. If we accept the Newtonian description of motion in the universe, the key to the dilemma of the Earth's motions' influence on the flux and reflux of the sea lies precisely in the 'simple' nature of the Newtonian law of attraction between bodies. As we shall see, it is this 'simplicity' that *cancels out* the term that may legitimately be defined as the *quasi-Galilean* contribution to the equations of tidal motion. This contribution may be regarded as being only 'quasi' Galilean insofar as it depends solely on the Earth's orbiting the Sun (and not on a combination of the annual revolution with diurnal rotation), and therefore does not entirely coincide with Galileo's model of the action exerted by the double motion of the Earth on sea water.

The reason why, in a Newtonian universe, tides do not depend on the Earth's annual motion is that, although it is necessary to take this motion properly into account in order to write down correctly the tide equations, the quasi-Galilean term that describes this motion is made to disappear by another term (whose absolute value is equal but has the opposite algebraic sign). This second term stems from the mathematical form of the law of universal gravitation by a purely physical coincidence, a coincidence due precisely to this mathematical 'simplicity'. We shall prove that by slightly varying the form of the law of universal gravitation this coincidence no longer occurs, so that the quasi-Galilean term subsists and the set of equations shows how tides, in such a hypothetical universe, are effectively influenced by the Earth's motion.

Nevertheless, to discover both how the simplicity of Newton's law of universal gravitation actually works in the machinery of tides and why it disappears when we admit a different law to be true is no mean task. And, in this sense, we can appreciate the significance of the challenge that Galileo issued, a significance that is far removed from the naïvety that has continually been attributed to his tide theory.

1. Galileo's tide-generating acceleration

1.1 The woad-grindstone model

The document in which, for the first time, Galileo expounds at length his ideas about the tides is the *Discourse on the Tides* that was written in Rome as a letter to Cardinal Orsini early in 1616.[1] Although it does not contain the subsequent developments regarding the monthly and annual tidal periods which are to appear in the Fourth Day of the *Dialogue Concerning the Two Chief World Systems*, published sixteen years later in 1632, the *Discourse* explains in full the model of the diurnal regularity of tide motions, and many parts of it are reproduced almost verbatim in the *Dialogue*.

First of all, in Galileo's view, the flux and reflux of the sea is not "a process of expansion and contraction of seawater" but "a process of true local motion in the sea, a displacement,[2] so to speak, now toward one end and now toward the opposite end of a sea basin".[3] It is worth noting that, from the very beginning of the *Discourse*, the definition of the tidal motions is couched in terms that clearly tend to characterise the phenomena as undulatory and kinematic, rather than in terms of a static expansion, or bulge, as they

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[1] Wherever English translations are available, I have generally used them. Otherwise, Galileo's works are quoted and translated from G. Galilei, *Le opere di Galileo Galilei*, Edizione Nazionale edited by A. Favaro, Firenze, Barbera, 1890–1909, 20 volumes, to which I refer in the abridged form of the title, *Le opere di Galileo Galilei*, followed by the Roman numeral of the volume and the page numbers. The *Discourse* has recently been translated into English by M. Finocchiaro in M. Finocchiaro [ed.], *The Galileo Affair*, Berkeley, University of California Press, 1989, pp. 119–133. See the original *in Le opere di Galileo Galilei*, V, pp. 377–395.

[2] In the original Galileo says that the flux and reflux "[. . .] è nei mari un vero moto locale e, per così dire, progressivo" where 'progressivo' is an adjective referred to 'moto' which clearly conveys the idea of a progressive movement, whereas the noun 'displacement' could possibly suggest a rather static process.

[3] G. Galilei, *Discourse*, *op. cit.*, p. 120.

had traditionally been conceived of until Galileo.[4] This is the seminal notion on which the Galilean hallmark is stamped; from it all his subsequent insights stem, and through it he eventually arrives at his unequivocal, though qualitative, understanding of the tides as a wave-like phenomenon. Having defined what a tidal motion is, the *Discourse* goes on to look for the means by which the sea waters on the Earth's surface can be set in motion in such a manner that flux and reflux necessarily result. In other words, Galileo is seeking to find the 'primary cause', whose regular action may be connected with the regularity of tidal oscillations. There are two ways to set the sea waters in motion: in Galileo's words,

> Motion can also be given to water when the containing vessel is somehow moved. This can occur in two ways, one of which would be to raise and to lower alternately the two ends of the vessel; [...]. However, such a phenomenon of *oscillation* cannot take place in our case. In fact, even if the Earth had some *reciprocal oscillation*, this would not provide the waters with means for flowing back and forth; for it flows in an *oscillating vessel* insofar as the *oscillation* lowers now one and now the other end of the vessel, that is, insofar as this lower end approaches the common centre of heavy objects [...]. The other way of transmitting motion to water through the motion of the containing vessel is by moving the vessel forward, without tilting it in any way, but merely moving it with motion *alternately accelerated and retardated*.[5]

This passage is very important. In the first place, it is an explicit statement in characteristic and unequivocal language of Galileo's central preoccupation with the notion of *oscillation*- of the ocean's ceaseless surging to and fro over the seabed – as the key to a proper understanding of tidal ebb and flow. And he regards *oscillation* not as the result

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[4] Although the phenomenon of the tides had attracted the attention of many Mediaeval and Renaissance scholars, who had held different opinions on the subject and had produced various attempts at explaining the mechanism of the sea's ebbing and flowing, still in Galileo's time the prevailing theory was that tides are caused by the Moon's influence over the sea waters, and no matter what the physical mechanism actually invoked was, to the vast majority of philosophers the observational evidence suggested that tide phenomena must clearly be thought of as the effect of enormous water masses slowly moving under some sort of 'influence', in other words, as a great surging upwards of sea water. There is no comprehensive study on the Renaissance tide theories. The most recent contribution is P. Ventrice, *La Discussione sulle maree, tra astronomia, meccanica e filosofia nella cultura veneto-padovana del cinquecento,* Venezia, Isituto Veneto di Scienze Lettere e Arti, Memorie della Classe di Scienze Fisiche, Matematiche e Naturali, Vol.XXXIV, 1989, the scope of which is confined to the Venice-Padua area. See also P. Duhem, *Le système du Monde. Histoire des Doctrines cosmolgiques de Platon à Copernic,* 10 volumes, Paris, Hermann, 1913–1959, particularly tome 9, chapter 15, *La théorie des marées,* which borders on the Renaissance. Finally, R.Almagià, "La dottrina della marea nell' antichità classica e nel Medio Evo. Contributo alla storia della Geografia scientifica", *Atti della R. Accademia dei Lincei, Serie Quinta, Memorie della Classe di Scienze Fisiche, Matematiche e Naturali,* Vol. 5, 1904, pp. 377–513, which has a brief section given over to the end of the Middle Ages.

[5] G. Galilei, *Discourse*, *op. cit*., p. 120–121. The italics are mine.

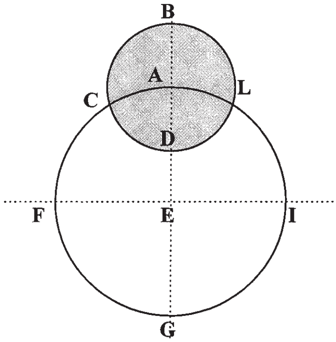

Figure 1.1

of alternately lowering the extremes of the earthly vessel[6] but specifically as the result of a dynamic process, 'with motion *alternately accelerated and retardated*'. A far cry from the classical dogma that invokes the Moon's supposed ability or occult quality, as Galileo would have called it, to attract the sea waters and cause them to pile up beneath it! What is more, it conjures up a disarming picture of the great man tinkering in his laboratory with vessels and buckets of water. And we learn from the same *Discourse*, a few pages after the passage quoted above, that Galileo had indeed been carrying out such experiments by means of "artificial vessels, [...] in which one can observe in detail these amazing combinations of motions".[7] We shall see in a following section how these experiments could possibly have revealed to him the much more complex nature of the tide phenomena, a nature whose mathematical and physical properties defied the explanatory potential of the geometrical methods at his disposal.

Galileo observed that moving a vessel makes the liquid inside it move too, but how could one recognise the same simple model in the great sea basins lying on the Earth's surface? Well, let us have a look at Galileo's answer (sketched in Fig. 1.1) and at the possible material source of such an apparently 'abstract' excogitation (depicted in Fig. 1.2).

In Fig. 1.1 the greater circle represents the orbit of the Earth around the Sun – which for Galileo was circular since, as is well known, he never accepted Kepler's elliptical orbits – while the smaller one is the rotating Earth itself. Before going on to elucidate in greater detail the workings of this gigantic tide-generating machine, it is worth stopping to consider what might have been the probable origin of the contrivance.[8] At first sight

[6] The expression "the oscillation lowers now one and now the other end of the vessel, that is, insofar as this lower end approaches the common centre of heavy objects" explicitly evokes the image of the balance suspended at its centre, or fulcrum. This image is evidently a static one.

[7] G. Galilei, *Discourse, op. cit.*, p. 127.

[8] On the origin of Galileo's model of the double motion of the Earth there are two opposite points of view. They hinge on the interpretation of three notes in which Paolo Sarpi, the famous Servite friar who met Galileo probably in 1592 and subsequently participated in Galileo's investigations, describes the selfsame analogy of the flux and reflux of the seas with the motions of water in a moving vessel. See P. Sarpi, *Scritti Filosofici e Teologici*, Bari, Laterza, 1951, p. 115. A critical edition with commentary has recently been published by L. Cozzi and L. Sosio, see P. Sarpi, *Pensieri Naturali, Metafisici e Matematici*, Milano-Napoli, Ricciardi Editore, 1996, pp. 423–426.

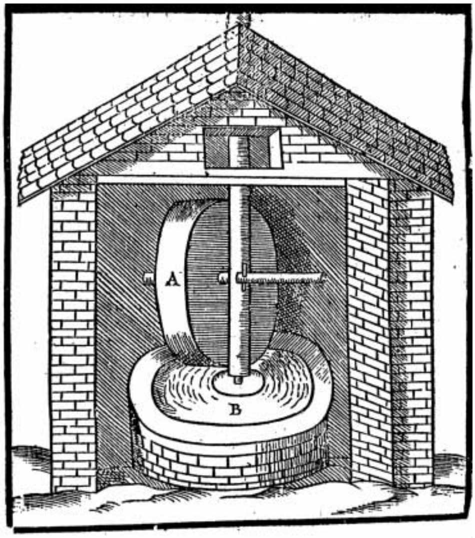

Figure 1.2

the idea of the double circle seems to be a replica of the epicycle-deferent model, a well-known mechanism of planetary motions widely used both in Ptolemaic and Copernican astronomy, but there is no textual evidence of such a comparison with a purely astronomical kinematic device. It is more likely that it originated in a shrewd empirical observation that Galileo made as regards a possible analogy with the somehow double circular movement of a 'woad-grindstone' [macina da guado][9] – which is depicted in Fig. 2[10] – and of

According to L. Sosio, Galileo and Sarpi discussed together the question of tidal ebb and flow, but the first hint to the formulation of the idea of the accelerating vessel came from Sarpi (see *ibid*., pp. CLVIff.) A different view is held by Stillman Drake, who maintains that Galileo inspired Sarpi's notes. See S. Drake, *Galileo's Studies*, Ann Arbor, The University of Michigan Press, 1970, pp. 200ff. – in which Sarpi's notes are translated into English – and S. Drake, *Galileo at Work. His Scientific Biography*, New York, Dover Editions, 1995 (1st ed. Chicago, The University of Chicago Press, 1978), pp. 36–37, where the author confirms his opinion. As the chronology of Sarpi's fragments cannot be established, the question seems bound to remain open.

[9] 'Woad' (*Isatis Tinctoria*) is a plant formerly much grown for the blue colouring matter yielded by its leaves and roots, which were crushed after having been dried.

[10] Fortunately, we have a very precise idea of what a woad-grindstone might have looked like in Galileo's time. Figure 1.2 depicts exactly the machine to which Galileo refers. The wooden engraving is from a small and rare tract published in 1629 by the Italian inventor and architect Giovanni Branca and recently re-published in a critical edition by L. Firpo (see G. Branca, *Le Macchine*, Torino, U.T.E.T., 1976, from which the image has been taken). Giovanni Branca is better known as a precursor of the steam-engine, as he anticipated the application of steam as a power source in the most famous of his *macchine*, a device in which a boiler powers a sort of turbine, which in turn operates a mortar to obtain gun-powder (see *Ibid*., pp. 56–57). The best essay on G. Branca is now L. Firpo's introduction to the critical edition of *Le Macchine*, see *Ibid*., pp. VII–XXVII. See also L. Thorndike, *A History of Magic and Experimental Science*, VII, New York-London, 1958, pp. 617–618. A working model of the steam-engine appears to have been

which we have a direct textual proof in a fragment relating to the *Dialogue Concerning the Two Chief World Systems*. The passage I am referring to reads as follows:

> *The* wheel of the woad-grindstone *seems* to have two motions about . . . by imagining it as being a sphere, think as to whether they can occur about its *centre*. . . .[11]

The wheel A (the grindstone) turns about its horizontal axis, while at the same time this horizontal axis rotates about the vertical one and runs over the 'bed' B, in which the material to be crushed is placed. The analogy with the scheme of Fig. 1.1 is not exact because in the epicyclic scheme we have two rotations about parallel axes[12] – the Earth's diurnal rotation and the annual around the Sun, which, in the model of Fig. 1.1, are normal to the plane of the drawing – whereas in the woad-grindstone the two axes of rotation are perpendicular to each other. Nonetheless, it is clear that the woad-grindstone represents a mechanical system in which the total motion of the wheel turns out to be a 'sum' of two independent rotational motions and 'imagining it [the wheel] as being a sphere' Galileo's shrewd eye could have transformed his seemingly simple observation into the more abstract and profound idea of the composition of the two rotational motions of the terrestrial globe.

Let us now go back to the kinematic 'chemistry' of Galileo's astronomical woad-grindstone (Fig. 1.1). The direction of the annual revolution is from A to F, the same as the diurnal rotation, which is from B to C. Although each of these two motions is 'equable and uniform', nevertheless, from the combination of the two, "there results a very unequal motion for the parts of the Earth's surface, so that each of these parts moves with different speeds at different times of the day".[13]

In other words, by composing the tangential speed of a point P on the Earth's surface – an arbitrary point belonging to the Earth's surface – resulting from the daily rotation with the speed tangential to the selfsame surface due to the annual motion, one obtains a total speed, variable from point to point on the Earth's surface, which is maximum in B, where the two components are added, and minimum in D, where they are subtracted.

> So far, then, we see that any body of water (be it a sea, a lake, or a pond) has a continuous but nonuniform motion, since it is retardated during some hours of the day and much accelerated during others; we also have the principle and the cause why the water contained in it, being fluid and not firmly attached to the container, flows and moves now in this and now in the opposite direction. *And*

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built by Branca in Milan, but all traces of it have been lost (see the entry in *Dizionario Biografico degli Italiani*, XIII, Roma, Istituto dell' Enciclopedia Italiana, 1971, pp. 758–759).

[11] Galileo's comment is published amongst many other fragments in an Appendix to the *Dialogue Concerning the Two Chief World Systems in Le opere di Galileo Galilei*, VII, p. 542. The italics represent words that are virtually illegible, given the deterioration of the original manuscript.

[12] In this model there is no inclination of the Earth's diurnal rotation axis with respect to the ecliptic.

[13] G. Galilei, *Discourse, op. cit.*, p. 123.

we can invoke this as primary cause of the effect, without which it would not occur at all.[14]

Here is the mechanism by means of which the double Copernican motion transforms the solid Earth into a celestial vessel, which – in conjunction with its superficial waters and every free particle on its surface, though without being rigidly joined to them – is gently and regularly 'accelerated and retardated' by what Galileo defines as the 'primary cause' of the ebb and flow of the waters of the sea. Thus, the definition of the Galilean "primary cause" may be expressed as follows: variation in 'absolute motion' of sea beds stemming from the addition of the speed due to annual revolution to that due to diurnal rotation. In order to contrast this 'primary cause' with the subsequent findings reported by Newton in the *Principia*, and which were to lead to his formulation of the concept of 'tide-generating force', I propose, for the sake of simplicity in the following discussions in this study, to refer to the Galilean 'primary cause' as 'tide-generating acceleration'.[15]

The two main kinematic characteristics of tide-generating acceleration are, on the one hand, its diurnal periodicity (in the sense that the maximum absolute motion occurs at midnight, whereas the minimum occurs at noon, so that one maximum and one minimum occur once a day) and, on the other hand, its continuity of action, which in turn implies that Galileo had some intuition of the vector nature of speed, or, at least, of the necessity of correctly composing homogeneous 'speed parts' – in this case, speed parts along the same direction. In other words, Galileo overtly recognises that what matters in his model is the horizontal component of the tide-generating acceleration, namely, that part of it that is tangential to the Earth's surface. In consequence, there must be a well-defined distribution on the Earth's surface of tide-generating acceleration that varies continuously between two extremes and is zero at two points, C and L, in the diagram of Fig. 1.1.[16] Well, of course, this cannot be acceleration as we conceive of it today, given that, in the first place, it lacks the vector nature of the concept of acceleration and, in the second place, at maximum or at minimum speed acceleration must be zero (e.g. in

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[14] G. Galilei, *Discourse, op. cit.*, p. 124. It has to be noted that the English translation mysteriously leaves out the last entire sentence of Galileo's text in which the expression 'primary cause' is introduced. I have supplied a translation of the missing part – which is given in italics – so as to complete Galileo's passage, which has great significance. The original is "[...] questa potremo noi domandare causa primaria dell' effetto, senza la quale esso del tutto non sarebbe". See *Le opere di Galileo Galilei*, V, p. 383.

[15] I am aware of the risk of fallaciously introducing modern concepts with modern terminology. Although it is well beyond the scope of my study to embark on an analysis of the meaning of 'speed' and 'acceleration' in Galileo's mechanics, I think that using modern terms in a well defined and limited context can sometimes help to clarify the debate between scholars. In this instance, 'acceleration' is quite appropriate with meaning of variation of absolute speed – i.e. speed referred to some frame of reference in which the Earth's orbital speed can be measured – even though 'absolute motion' is the expression that Galileo uses (see G. Galilei, *Discourse, op. cit.*, p. 123–124).

[16] "Each of these parts [of the Earth's surface] moves with different speeds at different times of the day", and again "[each part of the Earth's surface] within a period of twenty-four hours moves sometimes very fast, sometimes slowly, and twice at intermediate speeds" (G. Galilei, *Discourse, op. cit.*, p. 123–124).

an orthogonal Cartesian graphical representation the tangent to the curve of the speed, which is known also as the derivative, is parallel to one axis), whereas the Galilean tide-generating acceleration is maximum or minimum, respectively, but never zero.

Still, Galileo looks on his 'primary cause' as real acceleration, partly because of his being unable to handle the vector nature of speed in circular motion in an appropriate manner – this requires vector operations so as to take into account variations in speeds caused by changes in direction – and partly because of his being unable to discern the phase lag even between scalar acceleration and speed. To sum up, his first, and major, error is the assumption that it is legitimate to substitute the correct vector sum $\vec{V}_P = \vec{V} + \vec{\omega} \times \vec{R}$[17] for the scalar sum $V_p = Vt + \omega \cdot R$, where $\vec{V}_p$ is the absolute speed of a point P attached to the Earth's surface (in Galileo's model the Earth is circle BCDL, Fig.1), ω is the modulus of the Earth's diurnal angular velocity $\vec{\omega}$, R is Earth's radius and Vt is the tangential component to the Earth's surface of speed $\vec{V}$, which is the absolute velocity of centre A of the Earth in its orbit around the Sun.[18] And his second, though less important, error is the ingenuous assumption that maximum acceleration or deceleration occurs where maximum or minimum absolute speed V_p occurs too. In other words, *tide-generating acceleration* is given for Galileo by what we would define in modern terminology as the derivative of the scalar function $V_p = Vt + \omega \cdot R$; and works perfectly as such in his mind, i.e. as true, modern acceleration, because he conceives of it as being the necessary result of the variation of absolute speed V_p that occurs on the Earth's surface.

We could summarise Galileo's predicament as follows: first, he recognises quite correctly that the woad-grindstone model depicted in Fig. 1.1 entails a variation in absolute speed on the Earth's surface; second, he proposes this variation as the 'primary cause' of tidal ebb and flow, namely, as tide-generating acceleration; third, he understands the periodic and continuous action of the 'primary cause' but he is at a loss as to how to carry out the mathematical calculations involved in the kinematics of circular motion. This very technical handicap appears to be what prevents him from seeing what we can easily see by supplying the mathematics that he lacked. As we shall demonstrate in Sect. 4, which is devoted to the 'tide-motion-of-the-Earth problem', it is not difficult to see that, while the derivative of the scalar equation $V_p = Vt + \omega \cdot R$ gives the Galilean tide-generating acceleration, the derivative of the vector equation $\vec{V}_p = \vec{V} + \vec{\omega} \times \vec{R}$ – which is the operation one should perform to calculate the absolute acceleration of a point P attached to the Earth's surface – surprisingly gives tide-generating acceleration equal to zero, though it does yield a term that will be defined as quasi-Galilean tide-generating acceleration. Yet, the solution to the vexed question of the relation between

[17] Wherever it is required to use vector quantities I have indicated them as it is usual with a vector line over the letters, so that for example $\vec{V}_P$ means vector speed of P. It goes without saying that Galileo did not use any vector symbolism nor did he ever write mathematical formulas like those given above – it is well known that Galileo's mathematical language amounted to the simple symbolism of the theory of proportions. Nevertheless, using clear modern terminology helps to clarify our ideas and especially to evaluate better the content of ancient and less precise concepts by contrasting them with ours.

[18] In the context of this mode V has to be regarded as the Earth's centre speed, the Earth being regarded as a point in its orbit around the Sun.

the Earth's double motion and terrestrial phenomena like the tides lies concealed half in the very nature of $\vec{V}_p$ and half in the nature of gravity. Moreover, we shall also see that it is gravity that annihilates the true contribution of the Earth's motion to tidal ebb and flow. And although this contribution does not stem from the fully Galilean, but only from the quasi-Galilean, tide-generating acceleration, its role is vital in determining the tide equations of motions.

1.2 Composition of speeds and relativity

We have so far been speaking of $\vec{V}_p$ without stressing the fact that Galileo adopts the idea of composition of speeds without calling into question the legitimacy of his operation. Doubt is purported to have been cast for the first time by a group of French physicists after the publication of the *Dialogue* in 1632 and this was communicated by Jean-Jacques Bouchard, a correspondent of Galileo's, in a letter to him from Rome dated 5th September 1633.[19] Nevertheless, the reservations expressed regarded only what they erroneously presumed to be Galileo's claims that different parts of the Earth 'which always move in the same way relative to themselves *and to the water*' could impart 'a diversity of motions' to the sea waters.[20] The phrase 'moving in the same way relative to themselves and the waters' could be seen as the 'manifesto' of the entire historiographical quarrel regarding the 'fallacy' in Galileo's argument. About three centuries later these few words were once again to puzzle historians of science and scholars.[21] However, the French physicists had clearly admitted that 'different parts of the Earth move with greater speed when they descend along the line of direction of the annual motion than when they move in the opposite direction'.[22] This important aspect of their criticism – which, as far as I am aware, has hitherto not been noticed – is worth considering carefully because it casts light on the only real difficulty, namely, Galileo's problematic use of the principle of composition of speeds.

This difficulty would appear to be twofold, but in reality it is not. On the one hand, it appears to spring simply from a banal confusion widely spread amongst Galileo's interpreters between the concepts of 'motion of the vessel' and of 'motion of the contained water', namely, their failure to recognise that the two concepts are distinct; on the other,

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[19] See *Le opere di Galileo Galilei*, XV, pp. 251–252. Galileo's answer, if there was one, is unfortunately lost. The complete passage is discussed also in W. Shea, *Galileo's Intellectual Revolution*, 2nd ed., New York, Science History Publication, 1977, (1st ed. 1972), p. 176. Nevertheless, he fails to recognise that the French physicists had accepted the composition of speeds. See also W. Shea, "Galileo's Claim to Fame: the Proof that the Earth Moves from the Evidence of the Tides", *British Journal for the History of Science*, 5, 1970, pp. 111–127. See below for a discussion of secondary literature on this important theme in connection with the Galilean 'relativity principle' and *Sect. 4. The 'Warping' of History* where more technical questions are taken into account.

[20] The italics are mine. See *Le opere di Galileo Galilei*, XV, pp. 252.

[21] It must be stressed also that taking for granted that 'different parts of the Earth always move in the same way relative to themselves and to the water' is tantamount to assuming exactly what has to be demonstrated.

[22] The translation is quoted from W. Shea, *Galileo's Intellectual Revolution, op. cit.*, p. 176.

it is related to the application to the Earth's double motion of the kinematic principle of composition of speeds, which Galileo invokes and uses without giving any justification for performing such a sophisticated operation in an intuitive manner. A discussion of the latter point may help clarify why 'motion of the vessel' and 'motion of the contained water' are two independent and distinct issues.

It is well known that one of the basic principles of classical mechanics is the *principle of composition of speeds*, i.e. the principle that states simply that an observer – defined for the sake of simplicity by a Cartesian frame of reference for measuring distance and a clock for measuring time – can determine the speed of a body by knowing the speed evaluated by another observer and the speed of this second observer with respect to himself.

It is worth noting that Galileo applies the principle in its full generality to his woad-grindstone model and gets round the difficulties of the vector nature of speeds by assuming that, simply by adding the right component – i.e. along the same direction – of the annual speed to the tangential speed due to diurnal rotation, the result must be an 'absolute motion' variable from point to point on the Earth's surface. In other words, Galileo imagines a point P on the Earth's surface and asks himself what its absolute speed $\vec{V}_p$ is. His answer – as we have seen if we set aside his misconception as regards the vector nature of speeds – is simply $\vec{V}_p = \vec{V} + \vec{\omega} \times \vec{R}$ by virtue of the principle of composition of two speeds, namely, the annual speed $\vec{V}$, evaluated by an observer that could be thought of as attached to the Sun or the fixed stars or even coinciding with an absolute space at rest, and $\vec{\omega} \times \vec{R}$, which is evaluated by an observer attached to the Earth but not rotating with it.[23]

This is the crux of the whole matter.

Galileo's reasoning and application of the composition of speeds must be dealt with as a separate issue, quite distinct from the discussion contained in the famous pages of the Second Day of the *Dialogue*, where he formulates his principle of relativity, which has since become known in classical mechanics as Galilean relativity. The principle of composing speeds has very little to do with that of Galilean relativity. For, the latter rests on the recognition that the uniform and rectilinear speed that is equally possessed by bodies does not influence the motions of these bodies relative to one another. Newton was to extend this principle, recognising that even parallel and rectilinear accelerations that are equally possessed by bodies have no influence on the motions of the bodies relative to one another. The former principle – that of composing speeds – is a distinct and equally powerful tenet of classical mechanics, which connects the evaluation of motion among

[23] It must be pointed out that the principle of composition of speeds is one of the most general principles of kinematics; it can be applied to any two observers whose motions relative to each other are arbitrary. The so-called Galilean relativity principle of today's mechanics may be seen as a consequence of the general principle of composition of speeds in the simple case in which one of the two observers moves in a straight line at uniform speed. Of the many treatises on mechanics, one to which I am particularly indebted for a discussion of these questions is A.F. D'Souza, V.K. Garg, *Advanced Dynamics*, Englewood Cliffs, Prentice-Hall, 1984. See pp. 28–37. It is noticeable that the relativity principle has attracted the attention of virtually all the historians who have studied Galileo, whereas this problematic example of application of a more general and more powerful principle has so far gone almost unheeded.

different and arbitrarily moving observers and is more general than the latter in the sense that its application is not restricted to a special class of privileged frames of reference.

Now, let us point out that, contrary to what has been argued by some scholars,[24] there is no contradiction whatsoever between the application of the principle of composition of speeds in Galileo's woad-grindstone model, which is proposed both in the *Discourse* and in the Fourth Day of the *Dialogue*, and the notion of relativity that is formulated only in the Second Day of the *Dialogue*.[25] This touches on the second aspect of the

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[24] A contradiction was pointed out recently by F. Minazzi in his book *Il Flauto di Popper*, Milano, Angeli, 1994, p. 254. The author claims that in Galileo's physics only a shock could provide the Earth with the acceleration necessary to set the seas in motion and that "[...]Galileo ha finito per sostenere un risultato che entra in palese e flagrante contrasto con lo stesso, classico, principio di relatività che aveva enunciato nella seconda giornata del dialogo." Exactly the same argument was expressed earlier by P. Rossi, *La Scienza e la Filosofia dei Moderni*, Torino, Bollati Boringhieri, 1989, p. 126, which is a revision with slight changes of a previous work first published in 1972 under another title, *Aspetti della Rivoluzione Scientifica*, Napoli, Morano, 1972. See also G. Morpurgo-Tagliabue, *I Processi di Galileo e l'Epistemologia*, Roma, Armando Armando, 1981, where, on page 110, there is the following sentence "L' errore nella questione delle maree sta proprio nell' avervi voluto vedere un' eccezione a quel principio [the relativity principle]". For M. Clavelin, *The Natural Philosophy of Galileo*, Cambridge, The Mit Press, 1974 (1st ed., *La philosophie naturelle de Galilée: essai sur les origines et la formation de la mécanique classique*, Paris, A. Colin, 1968), p. 479, Galileo "failed to see that the Fourth Day of the Dialogue was in conflict with the second...". Again, A. Rupert Hall in *From Galileo to Newton*, New York, Harper and Row, 1963, p. 77, says that "[the theory of tides] conflicted with Galileo's earlier, correct enunciation of the properties of inertial systems."

W. Shea would appear to have been the only author to recognise, though in an implicit and confused manner and at a non-technical level, that the question of the composition of speed may be separated from that of relativity, given that he does not invoke the supposed conflict between the two points, and, commenting on the 'error' that was pointed out by the French physicists (see above), he attributes to "Galileo's failure to distinguish centripetal acceleration" from linear acceleration the real flaw in his tide theory (W. Shea, *Galileo's Intellectual Revolution, op. cit.*, p. 176).

Earlier on, the 'relativity' argument was put forward by E. Aiton in his article "Galileo's Theory of Tides", *Annals of Science*, X, 1954, pp. 47–80, which, despite the title, devotes to Galileo less than a third of the total number of pages. On page 46 the author maintains that "his [Galileo's] error appears all the more regrettable when it is remembered that his reply to the arguments of the constant east winds and the westward displacement of projectiles, advanced in opposition to the hypothesis of the Earth's motion, implies the falsity of his theory of tides". In the same year another Italian scholar published an article which made use of the same argument from the relativity principle. See M.G. Galli, "Sopra un memorabile errore di Galileo", *Scientia*, XLVIII, 1954, pp. 73–79. On page 76 he says that "Galileo avrebbe avuto un mezzo semplicissimo per controllare il suo errore [his tide theory]: applicare il principio di relatività da lui scoperto". The author has recently furnished another, more extensive, contribution to the debate in M.G. Galli, "L' argomentazione di Galileo dedotta dal fenomeno delle maree", *Angelicum*, vol. 60, 1983, pp. 387–427. Nevertheless, on page 410 he confirms the same judgement on the relativity principle question.

[25] It must be stressed that the supposed contradiction springs only from the confusion between the general kinematic idea of composition of speeds – which Galileo felicitously, though without

interpretative question first raised by the French physicists mentioned in Bouchard's letter to Galileo – that is, the distinction between 'motion of the vessel' and 'motion of the contained water'.

What Galileo is doing in his woad-grindstone model is merely looking for the absolute speed or motion $\vec{V}_p$ of a point P *on* and *belonging to* the Earth's solid crust. And for him the question as to whether any water or air particle possesses $\vec{V}_p$ in equal measure and at the same time – and, above all, by virtue of what sort of mechanism – is precisely the point that for him has to be decided. Galileo is here tackling the very problem as to why and in what way the fluid bodies like sea water and air are carried along by their *natural vessels*, i.e. the sea beds and the Earth's surface – it being remembered that he somehow regards mountains and valleys as the walls of natural containers of the atmosphere that surrounds the planet. He knows that if, and only if, uniform[26] speed is for any reason equally possessed by bodies, then there must be no physical effect that tells us about the presence of that common speed. But the moving Earth itself is the natural vessel that confers motion on the fluid bodies; they are not therefore set in motion by any other means than the mechanical connection with their terrestrial vessels, having no motion of their own.

Let us recall briefly Galileo's words on this subject of paramount importance. He imagines a ship inside whose cabin below decks there are some flies, butterflies and a bowl of water with some fish in it. After having asked the reader to observe the motions of these creatures relative to one another when the ship is at rest, he invites him to observe the same situation when the ship is moving with 'uniform motion and not fluctuating this way and that'. Then:

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discussing its legitimacy, applies to his model in order to find the absolute speed, or motion, of a point on the Earth's surface – and the particular physical instance that occurs when one of two speeds is uniform and rectilinear, in which case it is as if this speed were not present at all. To discern the difference one must bear in mind that the principle of composition of speeds holds true for arbitrary moving frames of reference. What is common to all the afore-mentioned historians is their tendency to scoff at Galileo's supposedly facile self-contradiction, which is purportedly evident in his not applying his relativity principle to his theory of tides, whereas there is no such thing as a direct connection between the vessel's absolute speed $\vec{V}_p$ and the speed of a water or air particle contained therein. A discussion of the relationship between Galileo's principle of inertia and his principle of relativity is in M.A. Tonnelat *Inertie et relativité dans la physique de Galilée*, in I.B. Cohen *et al., Atti del Symposium internazionale di storia, metodologia logica e filosofia della scienza 'Galileo nella storia e nella filosofia della scienza'*, Firenze, G. Barbera, 1967, pp. 29–37.

[26] It is not within the scope of the present study to enter into the debate on Galileo's conception of the principle of inertia. Whether Galileo believed in a sort of circular inertia, as many a historian seems to think, and therefore in the uniform circular speed as a speed that can last forever without any external forces to sustain it, is open to question. Yet, the problem of the kinematic description of the motion of fluid bodies on the Earth's surface remains untouched. The principle of composition of speeds is independent of the concept of inertia – circular or rectilinear – simply because the inertia law is a dynamic law, whereas the former principle is deducible from pure kinematic concepts. Amazingly as it may seem, Galileo appears to have been somehow aware of this difference, even if he had no dynamics upon which he could found his intuition.

> You will discover not the least change in all the effects named, nor could you tell from any of them whether the ship was moving or standing still. [...] The cause of all these correspondences of effects is the fact that the ship's motion is common to all the things contained in it, and to the air also. That is why I said you should be below decks; for if this took place above in the open air, which would not follow the course of the ship, more or less noticeable differences would be seen in some of the effects noted. [...] The flies likewise and the butterflies, held back by the air, would be unable to follow the ship's motion if they were separated from it by a perceptible distance. But keeping them near it, they would follow it without effort or hindrance; for the ship, being a structure with an *indented*[27] surface, *carries with it a part of the nearby air.* [28]

It is the ship that carries along with it not only all the creatures that are contained in its cabin but also – by virtue of its irregular external shape – a portion of the nearby air. And the common motion would hardly be appreciable even in the open air, providing that the phenomena were observed very near to the ship where the experimental condition nearly approaches that below decks – Galileo evidently knows that the closest strata of air are dragged from the ship, for it is by way of being a structure with an 'indented surface'.[29] Everything is carried by the ship, the vessel which moves uniformly and without fluctuating.

Now, the Earth is the great vessel that, unlike the ship, has a 'continuous but non-uniform motion, since it is retarded during some hours of the day and much accelerated during others' and as such cannot correspond to the uniformly-moving ship of the famous experiment proposed in the Second Day of the *Dialogue*. On the other hand, there is a one-to-one correspondence between the sea waters and the atmosphere surrounding the globe (where birds fly and projectiles are fired) and the water inside the bowl and the air in the cabin of Galileo's thought experiment (where small living creatures move around undisturbed), the former elements being carried by their planetary vessels, while the latter are transported by the ship. It is in the structure of this only partial analogy that the discrepancies that have been claimed to exist between Galilean relativity and the application of the principle of composition of speeds to his tide model disappear.

Not only does this partial analogy clarify the relationship that Galileo establishes between some of his basic ideas on motion, speed and relativity, it also casts new light on another consequence that has so far been misinterpreted precisely because thought of as being in grave contradiction with the principle of relativity. I am referring to the explanation of the trade winds as an effect of the Earth's daily rotation about its polar axis and of the 'tenuous' nature of air, which would prevent it from remaining stuck to the surface of the planet.

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[27] I have corrected the English translation *unbroken*, which is wrong. See below for a discussion of this delicate point.

[28] G. Galilei, *Dialogue Concerning the Two Chief World Systems*, tr. with revised notes by S. Drake, 2nd ed., Berkeley, University of California Press, 1967, pp. 187–188. The italics are mine.

[29] The English translation renders the Italian text "[...] la nave stessa, come di fabbrica anfrattuosa", where *anfrattuosa* has meaning of 'full of creeks', as 'for the ship, being an *unbroken* structure'. This changes the meaning of the passage and loses the power of Galileo's precise image.

> As a tenuous body not firmly attached to the Earth, air does not seem bound to obey its motion, except insofar as the *roughness and unevenness of the Earth's surface carries away and takes with it the part next to it*. We may believe this is the part that does not extend much above the tallest mountains. This portion of the air is all the more inclined toward the Earth's turning, insofar as it is full of vapours, fumes and exhalations and these are all elemental substances inclined by their nature taoward the same terrestrial motions. However, wherever the causes of motion are lacking, that is, wherever the Earth's surface has large flat spaces [. . .] the cause that would make the amibent air conform completely to the Earth's turning is partially ineffective. Thus, in such places, while the Earth turns towards the east, one should constantly feel a breeze blowing from east to west, and such a wind should be more noticeable where the terrestrial turning is fastest; this is in places farthest from the poles and close to the equatorial circle.[30]

Although it comes as a bit of surprise that, even here, Galileo does not bother to furnish any further physical explanation as to why the closest air strata should be carried by the Earth[31] – apart from the old idea of the air near the surface of the planet being full of earthly vapours, fumes and exhalations associated with the traditional notion of the elemental substances' following their natural inclination – what must be noted is the strong analogy with the supposed ability of the ships indented surface to drag the closest air strata along with it. This makes clear that for Galileo there exists no contradiction between his relativity principle – as it is set forth in the example of the ship – and his need to excogitate a mechanism of cause and effect, by virtue of which the motion of the air as well as of the sea waters is perpetuated by their natural basins.

And, indeed, there is no contradiction at all. For, there is no correspondence between the uniform motion of the ship and the non-uniform motion of the Earth as regards the problem of the tides.[32] On the other hand, quite independently of the fact that, for

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[30] G. Galilei, *Discourse, op. cit.*, p. 131–132. The italics are mine. As is well known, Galileo's explanation of the trade winds has nothing to do with their real physical cause, namely, the Coriolis acceleration due to diurnal rotation which is responsible for the westward deviation of the air masses blowing towards the equator on the surface of the Earth.

[31] By the same token, it strikes us that Galileo makes use of a principle – like that of composition of speeds – that he is unable to justify, or is at least at a loss as to how to derive fully from other principles. What is more, we shall see that a third, perhaps even more baffling, example of this suspect procedure is his promulgation of the laws of oscillation of the sea basins, which are foisted on the astonished reader without any mathematical or physical proof and without any serious attempt to discuss them beyond the analogy of the simple-pendulum motions.

[32] As we have seen as regards the Galilean inertia law, here, in the same manner, whatever meaning one decides to attribute to Galileo's concept of 'uniform motion' – circular or rectilinear uniformity – at the Earth's surface, the motion of the sea beds and of the entire solid crust is by no means 'uniform', it being composed of two regular motions, namely, the annual revolution about the Sun and the planets diurnal rotation. In Simplicius' words, this very fact 'may only look like a great paradox to me, though I am no mathematician or astronomer' (G. Galilei, *Dialogue Concerning the Two Chief World Systems, op. cit.*, p. 426). Indeed, a great deal of mathematics is the mandatory prerequisite to realise how circular motions combine and even Galileo failed in his attempt to analyse this problem quantitatively.

Galileo, the air must be carried by the Earth's unevenness and roughness, his relativity principle holds true. In accordance with this principle, when 'motion is common to all the things' contained in a vessel and the vessel is moving with 'uniform motion and not fluctuating this way and that', then 'not the least change in all the effects' can be detectable.[33] In other words, whatever the means that sets and keeps the air in motion, the phenomena that occur therein, such as the flight of the birds or the motions of projectiles, are unaffected by their participation in the common diurnal rotation.[34]

What remains yet to be considered is the question as to what, in the whole gamut of Galileo's physical principles and in his conceptual framework, is ultimately responsible for maintaining the motion of the air masses as well as of the sea waters. Is it a pre-Newtonian Earth's force of gravity acting on fluid particles that have started to travel at the same instant and at the same speed as the Earth? Is it a circular inertia in the place of this gravity? Or is it some other physical phenomenon (such as friction and thrust due to the walls of the sea and air basins) that has progressively accelerated the fluid bodies to which it must be deemed to be mechanically connected? Again, does this phenomenon cease to exert its effect after the fluid particles have reached the Earth's speed, or does it continue to act, dragging or pushing as the case may be? Whatever the answers to these questions are, it is evident that they involve broader considerations of Galileo's physics

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[33] As regards the motion of bodies near the Earth's surface, as long as diurnal rotation can be regarded as being virtually non-rotational in the region near a point P on the surface of the globe – i.e. as long as $\vec{V}_p$ is describable as a function independent of the angular velocity of the Earth – the classical relativity principle is valid. Traditionally, this principle is associated with the concept of uniform and rectilinear motion, but, though it is perhaps less known, it also holds true – as Newton set forth in the sixth corollary to the laws of motions in his *Principia* – when bodies share a common acceleration. In this regard, the typical example is the falling lift – a slightly modified version of Galileo's original thought experiment – inside which any phenomena occur exactly in the same way as in the chamber of a uniformly descending lift, except that the weight of all masses due to the Earth's gravitational field has disappeared. This elementary thought experiment has been translated into reality in the era of the orbiting space stations like the American Skylab (more recently the Space Shuttle). Everyone remembers the famous clips of the Skylab astronauts throwing small objects to each other, and their uniform and rectilinear motions: these images have become in our imagination both a proof that classical relativity holds true even for 'fluctuating' systems (providing that rotation is zero or negligible) and a live demonstration of the inertia law (assuming that it does not rotate, the Skylab may be considered as a frame of reference with respect to which the inertia law is valid).

[34] Following this train of thought, one might go so far as to say that Galileo could, in consequence, have predicted tides in the atmosphere as well as in the seas. Conversely, he could well have predicted uniform 'trade currents' in the upper strata of water in the equatorial regions of the oceans. The only point that could be raised here against Galileo – though only if one credits him with recognising fully that, for the relativity principle to be correct, speed must be uniform and rectilinear – regards the legitimacy of his extension of this principle to the case of common tangential speed due to diurnal rotation, which, as such, cannot be simply regarded as being strictly analogous with rectilinear motion. The question involves the whole panoply of arguments that are marshalled in the Second Day to prove that it is impossible either to demonstrate or deny diurnal rotation by means of any terrestrial phenomena like, for example, the motions of projectiles or the flight of birds.

and dynamics – and in this sense they are no more central to his tide theory than to the fundamental physical principles of his mechanics.

Bearing in mind the distinction between the arguments in favour of the relativity principle set forth in the Second Day of the *Dialogue* and the question of the status of the Galilean concept of inertia – which regards its mechanics as well as its physical astronomy and is emphasised by his explanation of the trade winds given for the first time in the *Discourse* of 1616 – I would maintain that Galileo did not arrive at a clear awareness of the right to extend his concept of inertia to all natural elements. And, quite apart from the question of rectilinear or circular inertia – which I regard as somehow misleading insofar as it conceals precisely the problem of the physical and inertial properties of fluid bodies on the Earth's surface – I would also argue that, for him, there can be no doubt that the element of air must be carried along by some extrinsic mechanical agent.[35]

Yet, both in Galileo's understanding and in our own, relativity and composition of speeds lie sufficiently far from the dividing line between kinematics and dynamics – only beyond which can the concept of inertia be thought of – to justify our confining the investigations mainly to his tide theory. Later on in the course of this study, while discussing Galileo's probable experimental work with mechanical models that seek to simulate tide and wind motions, we shall see that further evidence of a lack of cohesion in Galileo's ideas on the property of inertia of all terrestrial matters is highlighted by a set of experiments described and carried out, partly by Galileo himself and partly by others, in the context of the notorious dispute on the nature of comets with the Jesuit Orazio Grassi, professor of mathematics at the Collegio Romano, which erupted in 1619 after the publication of the *Discourse on the Comets* by Galileo's disciple Mario Guiducci. On the other hand, we shall also see that Galileo's vacillation over the question of inertia is a sign of the internal coherence of his physical astronomy. For, in assuming that a still upper atmosphere, or even a higher celestial region beyond it, is the place where some sort of sublimated vapours or tenuous matter – whose provenance remains to be decided – could cause the reflection phenomena that we call comets, as well as in claiming that non-rotating air around the Earth is necessary to generate terrestrial trade winds in the equatorial regions, Galileo is to be credited with a partial success in furthering

[35] H.I. Brown studied the presence of an 'element theory' in Galileo and drew the same conclusion that I have drawn, though he started from a totally different point of view. See H.I. Brown, "Galileo, the Elements, and the Tides", *Studies in the History and Philosophy of Science*, v.7, 1976, pp. 337–351. It must be underlined that Brown's work is the first attempt to abandon the historiographic prejudice of a grave absence of coherence between the Second Day and the Fourth Day. He conceives of Galileo's 'doctrine of elements' as the missing aspect in Galileo's physics that "restores the consistency of these two central parts of the *Dialogue* [tide theory and relativity]" (*Ibid*., p. 338). Although it seems to me that part of the author's conclusions lacks sufficient evidence, particularly in the case of the element 'water' – to which, in Brown's view, Galileo would attribute only the ability to conserve circular motion while lacking any natural motion, i.e. the tendency to fall downwards after the manner of the element 'earth' – it is quite remarkable that starting from a totally different perspective he has arrived at a similar global vision of Galileo's mechanics and physics, inasmuch as their basic principles concern the connection between the tides and the arguments in favour of relativity set forth in the Second Day.

his purpose of combining his physics and astronomy in an intelligible and consistent whole.

Before going on to analyse the pernicious effects that Galileo's intuitive ideas about the 'primary cause' that sets the seas in motion – which we have called tide-generating acceleration – and his more or less arbitrary and hitherto forgotten application of the principle of composition of speeds have had on historians' attitudes toward his theory as a whole, a third, albeit less significant and more technical, characteristic of his concept of tide-generating acceleration must be considered. This feature has to do with a certain degree of ambiguity in the way in which he describes the distribution of the 'primary cause' on the Earth's surface. In Galileo's words:

> [...] the primary cause of the tides embodies a principle for moving the water only at twelve-hour intervals, that is, once for the maximum speed of motion and the other for the maximum slowness.[36]

That is to say, the 'primary cause', the tide-generating acceleration, acts mainly at points B and D (Fig.1.1), where real acceleration – as we have seen – should be zero.

Now, let us assume – in line with our foregoing conclusions about Galileo's technical handicap regarding the mathematics involved in his woad-grindstone model – that, purely because of his inability to handle the functional dependence of acceleration on speed, Galileo mistakenly assigns the extreme values to midnight and noon; yet, his new claim that the "primary cause [tide-generating acceleration] of the tides embodies a principle for moving the water *only* at twelve-hours intervals" seems in contradiction with his previous statement that each part of the Earth's surface 'moves with different speeds at different times of the day', which, on the other hand, entails an unmistakable understanding of the continuity of action of tide-generating acceleration. Anticipating a few conclusions, it must be noted here that one of the main characteristics of Galileo's oscillatory model – the second half of Galileo's theory, which accounts for the fundamental contribution of the geometry of the sea beds to the tidal motions (see Sect. 3) – refers precisely to this question, and he unequivocally asserts that there is continuity of action.

My interpretation is therefore that Galileo says 'only' at those points for two reasons: first, he wants to stress the fact that there exist just two points on the Earth's surface where tide-generating acceleration reaches its maximum and minimum, its peaks of intensity; second, he is about to tackle the problem of the difference between the diurnal periodicity of his 'primary cause' and the most commonly observed six-hour periods of the Mediterranean, and he wants to emphasise the apparent discordance between the facts and the picture presented by his model before going on to destroy the objection. It is a rhetorical device that Galileo uses with consummate skill. And let us not forget that Galileo had really been engaged in an oral *Discourse on the Tides* in Rome ten days

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[36] G. Galilei, *Discourse*, *op. cit.*, p. 127–128.

before he wrote it.[37] An amusing testimony in a letter by Monsignor Querengo[38] refers to this extraordinary ability. The setting was an oral dispute held in the house of Federico Ghisilieri where, having been invited to discuss with others his theses

> before answering the opposing reasons, he [Galileo] amplified them and fortified them himself with new grounds which appeared invincible, so that, in demolishing them subsequently, he made his opponents look all the more ridiculous....[39]

As a conclusion, I would argue that although the idea of describing the tide-generating acceleration's action as being mainly regulated by twelve-hour peaks of intensity may have been prompted by the strongly felt recollections of the recent oral discussions and contests – which Galileo had been holding in Rome during the weeks between December and January 1616 – we have to recognise that a certain ambiguity remains in Galileo's language, a language that he frequently uses very freely, without bothering unduly to stick to precise definitions. Let us also remember that Galileo's scientific prose and the highly structured rhetoric that it embodies are among his most powerful intellectual weapons, and his vernacular ranks among the most captivating in the whole of Italian literature.

1.3 Tide-generating acceleration as a historiographical stumbling block

The idea of tide-generating *acceleration* appears to have been the conceptual stumbling-block to any real progress hitherto in the Galilean historiography of science. To take for granted that what follows in Galileo's exposition is not worth studying simply because what precedes it, i.e. the Galilean tide-generating *acceleration*, does not coincide with what has been established by Newton, namely, the correct notion of a tide-generating *force*, is to deprive Galileo's whole theory of its most important ideas and distort it beyond recognition. For, the points that Galileo discusses thereafter (the oscillatory model) represent the second half of his tide theory – without which the entire construction would be severely compromised- and so merit our closest scrutiny. We will devote our attention to these ideas in *Sect. 3* and further in *Sect. 5*, where we deal with the experimental work that in all probability underlies Galileo's brilliant intuition of the true dynamic nature of tidal phenomena.

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[37] At the beginning of the *Discourse* Galileo states that he is about "to put into writing what I [Galileo] explained to you [Cardinal Orsini] orally ten days ago" (G. Galilei, *Discourse, op. cit.*, p. 119).

[38] Monsignor Antonio Querengo, was at that time minister of the Duke of Modena in Rome. The letter was written on 20th January 1616 and refers to an event that had took place a few days earlier.

[39] The Italian text is "prima di rispondere alle ragioni contrarie, le amplificava e rinforzava con nuovi fondamenti d' apparenza grandissima, per far poi, nel rovinarle, rimaner più ridicoli gli avversari..." (*Le opere di Galileo Galilei*, XII, p. 227). I have borrowed the translation of the passage from G.de Santillana, *The Crime of Galileo*, Chicago, The University of Chicago Press, 1955, p. 113. See especially pp. 110–124 for a vivid description of the intellectual atmosphere in Rome during the days that led to the Decree of the Index on 5th March 1616.

The originator of part of the modern historiographical attitudes towards Galileo's supposedly erroneous – not to say banal and naïve – conception was to all intents and purposes the physicist Ernst Mach, who gave a résumé of the Galilean ideas in his *The Science of Mechanics*,[40] first published in German in 1883 and which was to influence scholars for many years to come, and indeed still does so today.[41]

Briefly, Mach's account is based upon a simplified kinematic model of a fluid sphere rotating about its polar axis while moving in a straight line along a plane perpendicular to its rotational axis. In Mach's view, the difference between that portion of the circumference along which the Earth travels in 24 hours and the chord which it subtends is so insignificant that the Earth's annual motion can be regarded as rectilinear and uniform with respect to a time interval of one day. Thus, there can be no influence of the annual motion of the Earth on phenomena such as the tides, which occur at the surface of the planet.[42]

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[40] E. Mach, *Die Mechanik in ihrer Entwickelung historisch-kritisch dargestellt*, Leipzig, F.A. Brockhaus, 1883. My quotations are from E. Mach, *The Science of Mechanics*, La Salle Il., Open Court Pub. Co., 1960. See particularly pp. 262–264.

[41] See, as an example among professional historians, W. Shea, *Galileo's Intellectual Revolution, op. cit.*, where he says on p. 175 "As Mach pointed out in *The Science of Mechanics*, [...] Galileo makes the error of mixing two different frames of reference". P. Rossi echoes the same words in his contribution devoted to Galileo in P. Rossi [ed.], *Storia della Scienza Moderna e Contemporanea*, Torino, Utet, 1988, 3 v.. The author states that "Galilei (come ha notato E. Mach) integra illecitamente due sistemi di riferimento", *ibid.*, 1st v., pp. 218–220. It is worth noting that the idea of the mixing of two different frames of reference was not Mach's at all; it was another historian, E. Aiton, who later on in 1954 used these confusing words without obviously attributing them to Mach. See E. Aiton, "Galileo's Theory of Tides", *op. cit.*, p. 46 where he says that "The error in Galileo's theory is in the mixing of two different frames of reference". A. Koestler, in his *The Sleepwalkers*, London, Hutchinson, 1957, p. 466, claimed that the tides represent an ersatz proof of the Earth's motion contrived by Galileo so as to compensate for the lack of observational evidence of the stellar parallax. His ensuing reasoning clearly shows its resemblance to Mach's ideas. See also F. Minazzi, *Il Flauto di Popper, op. cit.*, p. 254–255, who accepts Koestler's position. For an instance of oversimplification by scientists, see the discussion in the next footnote.

[42] My account of Mach's model makes explicit the hypotheses implicit in his representation of the double circular movements of the Earth. Mach asserts that "in his theory of the tides [Galileo] treats the first dynamic problem of space", but in "the most naïve manner he considers the fixed stars as the new system of reference", Mach, *op. cit*. [p. 264]. Nevertheless, Mach himself does not discuss the consequences of the assumptions underlying his model. Evidence of Mach's influence on scientists who have recently contributed to the history of science can be found in J.B. Barbour, *Absolute or Relative Motion?*, Cambridge, Cambridge University Press, 1989, pp. 396–402. In the author's view, "the curvature of the Earth's orbit can be ignored" so that the Machian model can be applied to Galileo's theory, even though the licitness of this simplifying hypothesis is not called into question. The author concludes that Galileo's theory of tides is "a demonstration of how a conceptual way of thinking that, despite successful application, has no ultimate anchor in empirical observations can take hold of the imagination and lead one to an entirely false conception of the world", *ibid*., p. 400. As will be made clear later on in this study, notwithstanding the lack of an adequate theory of gravitation, which prevented him from developing a dynamic theory of tides, Galileo was nevertheless able to achieve a deep insight into the complexity of the phenomenon that is far from being an 'entirely false conception of the world' and is indeed deeply anchored 'in empirical observations'.

The twofold simplification introduced by Mach's interpretation of Galileo's model may appear to be insignificant, but is in fact of consequence. Firstly, if a body moves in a straight line at a constant speed and does not rotate, the motion is inertial – and therefore no mechanical effect that is detectable on the body can be generated by this motion. In other words, in Mach's model, an observer attached to the Earth's centre and, therefore, moving in a straight line, cannot decide whether he is affected by a progressional motion or at rest by virtue of the occurrence of terrestrial phenomena such as the ebb and flow of the sea. Secondly, Mach's 'inertial model' – as regards the annual motion – fails to take into account the central thrust of the Galilean tide theory, namely, its claim to have proven the Copernican System as well as to have explained in detail the movements of the waters in the ocean basins.[43] Let us consider the latter point first.

It is worth remembering that historians – with few exceptions, as we shall see – have hitherto taken for granted that Newton was the first to provide a satisfactory account of the tides as a consequence of his discovery of the universal law of gravitation. The erroneous corollary to this point of view has always been that Newtonian theory of tides makes clear, once and for all, not only the origin, but actually the whole of the phenomena associated with the tidal motions of the seas. In their uncritical acceptance of this conclusion, historians have failed to appreciate fully how far tide theory has developed since Newton first gave his explanation – though only partially correct – of the mechanism of the tide-generating force in the *Principia* , and, above all, how far removed the actual dynamics required to account for the so-called dynamic theory of tides is from the foundations laid by him. This has been mirrored in their tendency to assume that Newton's universal gravitation theory is the yardstick by which to disprove Galileo's complex model of the tides and shelve it in the cabinet of curiosities of science history.

Although Newtonian mechanics must be accepted as the corner stone on which all modern theories of dynamic oceanography are based, I would argue that, in order to understand fully what in Galileo's conception was, on the one hand, 'erroneous' (and in

[43] According to Mach's opinion: "That on his [Galileo's] theory the tides rise only once a day did not, of course, escape Galileo's attention. But he deceived himself with regard to the difficulties involved, believing himself able to explain the daily, monthly, and yearly periods by considering the natural oscillations of the water and the alterations to which its motions are subject", see E. Mach, *op. cit.*, p. 263. This is absurd, simply because it is untrue that, according to Galileo's theory, the tides should rise only once a day. It is true that the periodicity of the tide-generating acceleration is diurnal, but this has nothing to do with the daily, monthly and yearly periods. The same error is repeated by W. Shea who even claims that "there is a direct observational consequence" of Galileo's model "which he cannot be excused for overlooking", namely, "the high water should occur at noon, at the time of greatest retardation, and low-water at midnight, the time of greater acceleration", see W. Shea, *op. cit.*, p. 176. Yet, Galileo never distinguishes between a supposed accelerative power making the water pile up and an opposite decelerative power making it subside. For him, what matters most is the simple, basic effect of setting the waters in motion, 'commuovere le acque' – which is due to tide-generating acceleration. The fact is that Galileo's tide model is a dynamic model, where the sea motions are not the direct consequence of tide-generating acceleration, which is at variance with all the traditional explanations then current and with Newton's tide-generating force, which is the direct cause of the waters swelling under the combined attraction of the Moon and the Sun.

what sense) and, on the other hand, a sign of an extraordinary insight into the phenomena, the historian cannot ignore the decisive theoretical developments that came after Newton and the subtle, though fascinating complexities that the mathematics involved reveals, as well as the most recent numerical supercomputer models, which a few years ago started to show quantitatively and in realistic cases the dependency of the tide periods on the geometry of the basins.

My first thesis is, therefore, that Galileo's 'misconception' is mainly due to his lack of an adequate explanation for what is known today as tide-generating force; he has to make do with the tools to hand – the accelerations and decelerations inherent in the woad-cruncher model, which result in what we have called his tide-generating acceleration – in order to come up with a periodic cause necessary to set the seas in motion. And Galileo's ingenuity lies in his insightful ability to connect an incorrect tide-generating cause with a basically correct mechanism, even though fundamentally qualitative, namely, the natural response of the sea basins to the external forces acting upon the waters – in the final analysis, in his intuition that a sea basin is nothing more than a natural oscillating system with its own natural frequency of oscillation.

Let us go back now to the second aspect of Mach's interpretation. Do we have the right to 'straighten' the Earth's orbital path around the Sun? In other words, would Galileo have accepted the complete coincidence of his woad-grindstone model with Mach's inertial one? We cannot answer this question, but we can formulate another, even cruder, question, which we *can* answer. Did Galileo succeed in demonstrating that the tides are the most spectacular observable effect of the Copernican double motion of the Earth? Insofar as this question does not take into account Galileo's explanation of the monthly period, which he wants to depend not only on the Earth's motions but on those of the Moon as well, it is equivalent in part to asking whether Galileo eventually succeeded in proving that tide phenomena are caused by the Copernican motions of the Earth-Moon system around the Sun. This is the most general and fundamental question. Finding an answer has turned out to be surprisingly complicated.

In the end, Galileo did not succeed. He was not able to prove that the tides are the manifestation, the visible effect, of the composition of the annual motion due to the Earth-Moon systems orbiting the Sun with the Earth's diurnal rotation. Simply because in a Newtonian universe they are not, at least not in an elementary sense, which is the only sense we can attribute to Galileo's physics. Even his mechanical model of the monthly period – an analogy with the balance-wheel of the regulator mechanism of wheel-clocks and the most 'advanced' of his speculations on the connection between Earthly phenomena and celestial movements – though lending itself to being regarded as an adequate 'clockwork' of the Sun-Earth-Moon system, cannot break the laws of Newtonian universal gravitation, and the workings of this elegant mechanism remain without any influence whatsoever on the ebb and flow of the seas, as they do on other similar terrestrial phenomena.

Yet, we shall see that the mathematical and physical structure of both the woad-grindstone model of the Earth's double motion and the wheel-clock model of the Earth-Moon system is intertwined with the nature of Newton's laws of motion in such a way that, were it not for the 'simplicity' of the universal gravitation law (the inverse square law) and for its entirely relative nature (i.e., its dependence on relative distances between bodies), the tide phenomena would depend even on the Earth's motions in space. In other words, were it not precisely for the relative-positional nature of the gravitation law of the universe in which we live, tide phenomena would be a perfectly adequate proof of the Earth's travelling in space. And such a possible universe would still be a fully Newtonian universe – i.e., one where the three laws of motion of Newtonian mechanics would be totally unchanged – even though its pseudo-gravitational force would have a slightly, but not much, more complicated nature than its real counterpart in our real universe. It is no more than a 'physical coincidence' that the simplicity of our gravitation law rules out the participation of the Earth's annual motion in determining the tide effects. From this perspective, it is no more than a historical coincidence that Galileo's tide-generating acceleration has, since Newton, been thrown on the scrap heap of science history.

Nevertheless, my second thesis is that the difficulty one experiences in attempting to dismantle Galileo's celestial wheel-clock theory, as well as that generated by his woad-grindstone model – i.e. the difficulty in countering the arguments on which those two models and their modern reconstruction are based[44] – is a significant measure of the richness of his thought, which went so far as to hint at dynamic problems, whose solutions were to be found only after Newton, well into the second half of the 18th century. In the final analysis, if the historian is to answer the question why Galileo failed to furnish a physical proof of the twin motions of the Earth, and eventually of Copernican astronomy, he must needs be able to explore the intricacies of the mathematical and physical arguments required to demonstrate that the terrestrial phenomenon of the flux and reflux of the seas remains totally undisturbed by these motions. And these arguments are only available in a fully-fledged Newtonian conceptual frame of reference.[45]

By the same token, the very criteria required to perceive the inherent truth of Galileo's precursory intuitions are to be sought in the importance that modern oceanography attaches to what Galileo called the "secondary causes" of the tides, namely, the general shape and the geometrical characteristics of the sea beds, which determine or at least affect the periods of oscillations of the waters quite independently of the periods of the external perturbations generated by the apparent motions of the Moon and the Sun. This corresponds to the Galilean oscillatory model, which has so far been virtually neglected by the historiography of science.

To sum up, it is a prerequisite of any appreciation of what is at stake at the heart of Galileo's view of tidal phenomena that a brief glance be cast both at the so-called

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[44] We shall see in *Sect. 4* that in the 1950s Galileo's woad-grindstone model was variously reconstructed by historians, and different interpretations, depending on how much mathematics had been 'lent' to Galileo's verbal descriptions, were proposed.

[45] This point of view is opposed to the conviction that Galileo's error was recognisable within the framework of his own physics, particularly by contrasting it with his clear statement of the classical relativity principle. See P. Rossi, *La Scienza e la Filosofia dei Moderni, op. cit.*, p. 126. See also W. Shea, *op. cit.*, p. 176, who points out that the non-technical criticism that was levelled at Galileo by a group of French physicists – whom Jean-Jacques Bouchard mentioned in a letter to Galileo dated 5th December 1633 – was "within the compass of pre-inertial physics". Although I am not sure about the meaning of the expression 'pre-inertial physics', the passage in the letter – in my opinion – shows only that Galileo's argument was rather poorly understood by its contemporary French critics. They were not able to accept the surprising idea of composing two speeds that refer to two different frames of reference.

Newtonian ‘equilibrium theory’, which is also known as the ‘bulge theory’ and whose real and lasting breakthrough is the formulation of the concept of tide-generating force, and at a few relevant aspects of modern dynamic theory. By comparing them, one should be able, on the one hand, to arrive at an ideal, objective perception of the real meaning of revolution brought about by Newton’s theory of gravitation in the particular context of the tide problem, and, on the other, to appreciate the enormous distance that separates our current approach to the theory of tides from that outlined in the *Principia*. Conversely, the striking parallel between Galileo’s qualitative ideas and our modern computational ability to calculate in detail tide phenomena that Galileo was only able to grasp in an intuitive manner should serve to restore to the Galilean theory of tides its historical integrity and complexity.

The next section is devoted to Newton’s achievements in the field of tide theory, insofar as they concern the present study, and the influence that they have had on the historiography of science in establishing an absolute reference point. The section thereafter is given over to an attempt to reconstruct Galileo’s oscillatory model in the light of a few modern concepts that can help us to understand better what Galileo was effectively trying to do.

2. Newton’s tide-generating force

2.1 Newton’s dynamic model

Newton established the attraction of the Moon and the Sun as the fundamental cause of tidal ebb and flow by defining the tide-generating force acting upon a water particle as the difference between the force with which the disturbing body – in this simple case the Moon or the Sun – attracts the fluid particle and that with which it would attract the same fluid particle if it were collocated at the centre of the Earth.[1] Despite the fact that he was able for the first time to predict quantitatively the rise and fall of the waters with some accuracy,[2] and that his theory of the tide-generating force became the basis of all subsequent attempts to explain the tides up to Laplace, the basic assumption that the water piles up under the gravitational attraction of the disturbing body is not correct. What deter-

[1] I. Newton, *Principia*, tr. Motte-Cajori, Berkeley, University of California Press, 1934, pp. 185–187, pp. 435–440 and pp. 581–594. The explanation of the tide-generating force is outlined in the fundamental Proposition LXVI of Book I, particularly in the corollaries 19, 20.

[2] An important work partly devoted to the study of Newton’s theory of tides is E.J. Aiton, “The Contribution of Newton, Bernoulli and Euler to the Theory of the Tides”, *Annals of Science*, XI, 1955, pp. 206–223. See also J. Proudman, *Newton’s work on the theory of the tides*, in W.J. Greenstreet [ed.], *Isaac Newton, 1642–1727*, London, G. Bell and Sons, 1927, pp. 87–95; G. Tabarroni, “The Tides and Newton”, *Memorie della Società Astronomica Italiana*, LX, 1989, pp. 769–782. The most complete commentary on Newton’s propositions regarding tides is S.Chandrasekhar, *Newton’s Principia for the Common Reader*, New York, Clarendon Press, 1995, pp. 399–417. On Newton’s early speculations on tide theory see J.E. McGuire, M. Tamny [eds.], *Certain Philosophical Questions: Newton’s Trinity Notebook*, Cambridge, Cambridge University Press, 1983, pp. 175ff and p. 405.

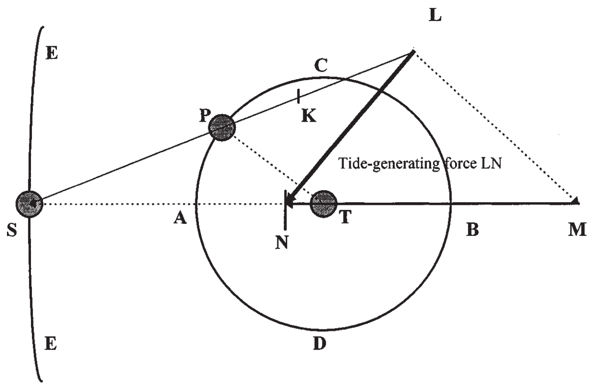

Fig. 2.1. Newton's three-body diagram

mines the main dynamic characteristics of the tidal waves is the horizontal motion of the waters, which in turn depends on the horizontal component of the tide-generating force.

Nevertheless, it is interesting to note that Newton's first approach to the question is different and far less well-known. It draws upon a surprising dynamic analogy which he does not expand on.[3] Let us refer to Fig. 2.1.

In proposition LXVI of Book I, which is given over to the analysis of the famous 'three body problem', he compares the motion of the third body P around the central body T with the motion of "many fluid bodies contiguous to each other" so as to form "a fluid annulus, or ring, of a round figure, and concentric to the body T", and claims that the latter would perform "their motion by the same law as the body P".[4] By imagining enlarging the central body T and cutting a channel all around its circumference, so that the 'fluid annulus' is contained within it, Newton deduces that the water "being accelerated and retarded [...] will be swifter at the syzygies, and slower at the quadratures, than the surface of the globe, and so will ebb and flow in its channel after the manner of the sea".[5] The awkward consequence of this model is that if the total volume of the ocean is to remain constant, then low tide should occur at the meridian passage of the disturbing body when the water is moving faster than at the quadratures; instead of the bulge, therefore, a depression would appear beneath the Moon.[6] Newton refrains from making

[3] See E. Aiton, "The Contribution of Newton, Bernoulli and Euler to the Theory of the Tides", *op. cit*, p. 206, where he recognises that Newton's theory was composed of two distinct hypotheses, the first of which explained the tides "by an analogy with the motion of satellites under the influence of a disturbing body", while the second gave rise to the "equilibrium theory" and was to be the basis on which Newton was to develop his quantitative work.

[4] I. Newton, *op. cit*, p185.

[5] *Ibid.*, p. 185–186.

[6] This conclusion is also drawn by E. Aiton, "The Contribution of Newton, Bernoulli and Euler to the Theory of the Tides", *op. cit*, p. 209.

this prediction and abandons the dynamic model.[7] It is beyond the scope of this study to explore further the reasons that made Newton give up his brilliant dynamic analogy, but it should be noted that the lack of comprehension, or perhaps the underestimation, of the importance of the local, geometrical conditions of the basins in determining the tidal phenomena mirrors the dual difficulty experienced by Galileo in finding the origin of the tide-generating force.[8]

2.2 Asymmetric tide-generating force and asymmetric tide periods

Having discarded the dynamic model, Newton resorts to the hypothesis that the difference between the force exerted by body S upon an ocean particle P (which may be described as the combined action of the two components, MS and LM, in which he has resolved the total force of attraction SL exerted by disturbing body S) and that exerted by body S on Earth's centre T (force SN), induces a vertical motion on water particle P (Fig. 2.1). The net force LN is what we nowadays call tide-generating force (to see why tide-generating force is just a *difference*, refer to the discussion below). Tide-generating force certainly governs the motion of Newton's fluid annulus but, being at a loss as to how to tackle such a complex mathematical problem, he simply supposes that a swell must occur somewhere underneath body S as a consequence of the differential action of tide-generating force. Nevertheless, even if one were willing to admit that the swell of the sea responds to the tide-generating force in a quasi-static way, one could not help recognising that its real behaviour would only roughly fit the description given in the *Principia*. Let us have a closer look at what would happen in such a model.

In Newton's rather confusing words:

> [. . .] the force LM attracts the waters downwards most in the quadratures, and the force KL or NM – LM attracts it upwards most in the syzygies. These forces conjoined cease to attract the water downwards and begin to attract it upwards in

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[7] In Aiton's view, "Newton regarded the development of the equilibrium theory as following naturally and logically from his kinetic theory", E. Aiton, *ibid.*, p. 209. See also U. Forti, *Introduzione Storica alla lettura del Dialogo sui Massimi Sistemi del Mondo di Galileo Galilei*, Bologna, Zanichelli, 1931, pp. 179–188, who considers Newton's theory as an organic whole, making no distinction between the theory outlined in Book I, Proposition LXVI and the subsequent equilibrium theory proposed in Book III, Proposition XXIV, of the Principia. In my opinion, Newton's first dynamic approach was simply not consistent with his second attempt to deduce the sea motions directly from the tide-generating forces.

[8] S. Drake defines Galileo's theory as a 'flow-theory of tides', contrasting it with Newton's 'bulge-theory', and claims that it is not "inconsistent with any possible dynamics". Even though I do not entirely accept the intuitive conclusions that the author draws from this argument – namely, that a Galilean effect, however weak, must exist, provided that the Earth is kept in its annual orbit by some sort of non-gravitational force (see my discussion in Sect. 4) – I agree that the terms he chooses fairly epitomise the main difference between Galileo's ideas and Newton's. See S. Drake, *Galileo: Pioneer Scientist*, Toronto, University of Toronto Press, 1990, p. 73.

the octants before the syzygies; and cease to attract the water upwards and begin to attract the water downwards in the octants after the syzygies.[9]

Now, referring to the previous diagram in Fig. 2.1, it is quite clear that, in Newton's view, force LM attracts the water downwards most at the quadratures, supposedly because at the syzygies part of it is cancelled out by force SM. On the other hand, force LN attracts the water upwards most at the syzygies, where it is clearly directed towards disturbing body S. As regards force KL, Newton is only adding to the confusion. Force SK is the force exerted on P when P is located 'at the mean distance' from S.[10] Force KL is therefore the difference between the actual force SL exerted on P by body S and force SK exerted by S on P when P is at the *mean distance* from S (which – to cap it all – is by no means the *mean force* exerted on P by body S).

Now, apart from the baffling conclusion that refers to 'these forces conjoined' without specifically distinguishing which of them must be taken into consideration, the awkward point is that Newton would appear to fail to separate the effects due to each of the forces he has come up with because he takes into simultaneous account tide-generating force LN as a whole but only one of its components, force LM, whereas he should have considered either tide-generating force alone or its two components independently.

This obscurity notwithstanding, I assume that with the expression 'these forces conjoined' Newton refers to tide-generating force LN, the plural being justified by his regarding it as being given by NM – LM. Yet, in spite of the qualitative validity of Newton's intuitive conclusion, the 'conjoined action' of the two components, whose resultant is force LN, by no means performs so well as to cease to exert its downward pull either precisely at the point where it crosses the border line between the octant after the quadrature and that preceding the syzygy or at some point thereafter. Nor does it do the opposite – namely, cease to make the seas pile up – where it leaves the octant after the syzygy and enters the octant before the quadrature.

In other words, Newton's 'equilibrium theory' is simply unable to predict two six-hour periods of high water and two six-hour periods of low water during the day (solar or lunar, depending on disturbing body in question). This point has hitherto escaped the notice of the historians of science, who have paid scant attention to the articulated process by which Newton, in the *Principia*, first proposes a dynamic model whose mathematical structure he is at a loss to approach, only to discard it in favour of a second one that is no less deficient than the first and whose physical consequences he is merely able to develop intuitively.

Nonetheless, a little dollop of analytical geometry that is commonly ladled out in High School is in itself sufficient to enable us to understand why the 'equilibrium theory' predicts in reality two longer periods of high tide and two shorter periods of low tide. To be precise, if we neglect the variability of the Earth-Sun (or Earth-Moon) distance over the solar year (or the lunar month), we have approximately solar high-tide periods of 7 h 20 min (7 h 12 min lunar) and solar low-tide periods of 4 h 40 min (4 h 48 min lunar), instead of the four equal six-hour intervals, championed by most commentators. The following sketch (Fig. 2.2) is an adaptation of Newton's diagram for the three-body

9 I. Newton, *op. cit*, p. 187.
10 *Ibid.*, p. 174.

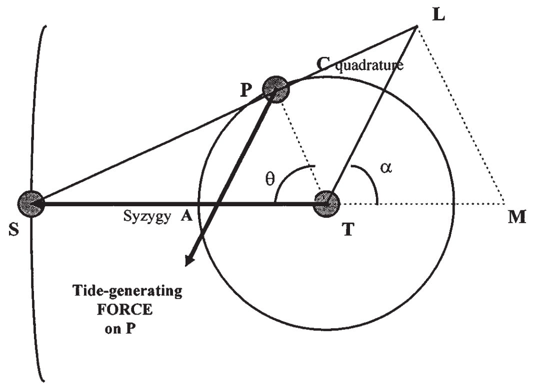

Fig. 2.2. An adaption of Newton's diagram

problem in which, in order to simplify the mathematics involved without any loss of generality, I have made the force of attraction of S on a particle P placed in T equal to the distance ST. Consequently, the Sun's attractive force acting upon P on the Earth's surface must be equal to SL, where $SL = ST \cdot ST^2/SP^2$.

By definition, the tide-generating force acting upon P on the Earth's surface is $LT = LM + MT$, where it goes without saying that here the symbol + means the diagonal-of-the-parallelogram rule for adding vector quantities. Now, let us remember that, by this very rule, force SL may be thought of as composed by LM and SM. Force ST, which is the common portion that SM shares with ST, acting equally and in the same direction both on the particle P and the Earth T does not "change their situation with respect to each other", and "the motion of the bodies between themselves" is the same, "as if those attractions did not act at all, by Corollary VI of the Laws of Motion".[11] Newton is referring here to the extension of the Galilean principle of relativity to translational accelerating systems that he has formulated at the beginning of the *Principia*, according to which

> If bodies, moved in any manner among themselves, are urged in the direction of parallel lines by equal accelerative forces, they will all continue to move among themselves, after the same manner as if they had not been urged by those forces.[12]

Corollary VI is of paramount importance if we are to understand in its entirety the subsequent discussion contained in the fundamental Proposition LXVI, particularly in

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[11] *Ibid.*, p. 175. Here, Mottes translation is to be regarded as slightly inaccurate, al least in the context of our discussion. Newton's Latin words are "ac si hae attractiones tollerentur", which could be more appropriately rendered "as if those attractions were taken away". This conveys more decisively the notion of the Earth as being devoid of the Sun's attraction, and therefore as being at rest or travelling uniformly in a straight line. See I. Newton, "*Philosophiae Naturalis Principia Mathematica*", 2 volumes, ed. A. Koyré and I.B. Cohen, Cambridge, Massachusetts, Harvard University Press, 1972. The passage quoted is in v.1, p. 280.

[12] I. Newton, *Principia*, tr. Motte-Cajori, *op. cit.*, p. 21.

Corollaries 20 and 21 where the third body P is interpreted as a fluid particle (or an entire 'fluid annulus') flowing freely within the circular channel on the surface of the Earth. Although it is not explicitly stated in the text of the *Principia*, the condition defined by Corollary VI must, in Newton's view, be considered as having the function of what I would call a 'special principle of equivalence'. Thus, providing that the Earth and the seas can be regarded, in their annual orbit around the Sun, as a system of particles subject to equal accelerative forces acting in parallel directions, the problem of the motion of a water particle P may legitimately be dealt as if P were moving on a stationary Earth and not, as it is in reality, on an orbiting Earth.[13] Later on in this study, we shall see that failure to distinguish between the restrictive condition imposed by Corollary VI and the more general principles of relative dynamics has been at the root of many a historians inability to appreciate the complex relationship between the Earth's motions and terrestrial phenomena, such as the tides.

Let us now turn our attention to the diagram given in Fig. 2.2. The direction of the tide-generating force LT acting on a water particle P, which is measured by the angle α, varies while P moves from quadrature C to the syzygy opposite S, where LT is parallel to line ST. The component of LT tangent to the surface of the Earth has no effect in Newton's equilibrium theory and therefore does not enter into this discussion, whereas the normal component attracts the waters downwards, where it is directed towards the centre of the Earth, and attracts them upwards, where it is directed outward from the centre. Because in quadrature C total force LT acts towards the interior of the Earth and in syzygy A towards S, there must be a point where its normal component is zero and where, in consequence, force LT is tangential. This is the very point where the tide-generating force *ceases to attract the waters downwards and starts to attract them upwards*, which Newton mistakenly believes either to coincide with the border line between the octant after the quadrature and that before the syzygy, namely, $\theta = 45^\circ$, or to lie somewhere inside this octant. The location of this transition point depends on the radius 'r' of the Earth T and on the distance 'a' between the Earth and disturbing body S, so that not only does it turn out to be different for the solar and the lunar tide, but it varies because the distance from the disturbing body varies too. The calculations carried out in *Appendix 1* prove that for the average distance between the Earth and the Sun $\theta = 55^\circ$, whereas for the Moon $\theta = 54^\circ$, which accounts for the above asymmetric values for the low-tide and high-tide periods. The following two illustrations given in Fig. 2.3 clarify the situation.

The figure on the left-hand side gives the unexpectedly asymmetric distribution over the Earth's surface of Newtonian tide-generating force, namely, the component of the total force that is perpendicular to the Earth's surface. The left and right sectors represent the high tide zones, whereas the upper and lower sectors represent the low tide zones. The figure on the right-hand side depicts the correct situation. This new distribution of force is

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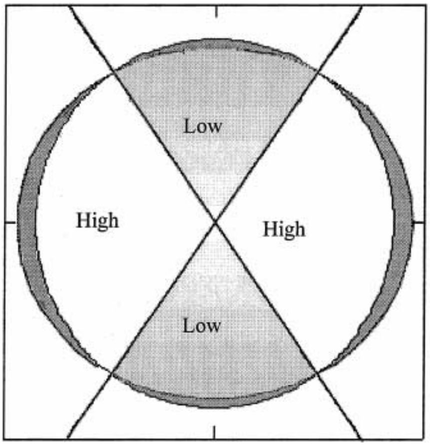
[13] Even though Newton does not declare explicitly that removing the attractive force of the body S from the Earth T amounts – assuming that the third body has a negligible effect on it – to stopping the Earth (or to letting it move uniformly in a straight line), this is the conclusion that we are bound to draw from his discussion. If not, if we were to accept his treatment of the entire problem without such a premise, we should question the legitimacy of his a priori disregarding any possible effect of the Earth's annual motion on the particle P.

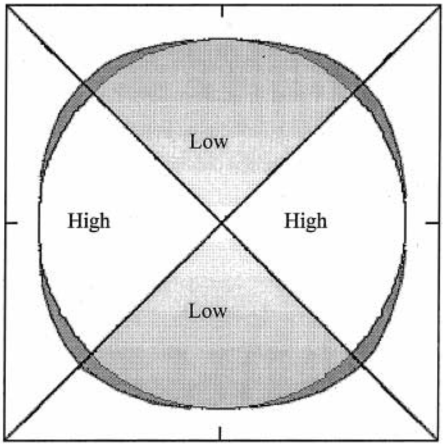
obtained by assuming as tide-generating force the tangential component (not considered by Newton) of the total tide-generating force. As expected, there are four equal zones, two high-tide sectors and two low-tide ones. In both cases, the disturbing body must be thought of as placed far away on the horizontal axis of symmetry of the figures, either on the left or on the right. Referring to the Earth's surface, the ordinates of the external lines represent the intensity of tide-generating force and the resulting curves describe the distribution of the normal and tangential components of tide-generating force.[14]

It is worth noting that the 'bulge theory' is the natural consequence of assuming the perpendicular component of the tide-generating force to be the very force that is directly responsible for the sea water piling up beneath the attracting body. Even if Newton had been able to calculate the mathematical properties of his model and had been faced with the dilemma of choosing which of the two components could actually be the real cause of the tides, we must admit that he would have had to select the perpendicular component, the only one which, in his global perspective on tide phenomena as a great swelling towards the disturbing body, would have made sense. The alternative would have been considered in all probability as unable to produce the required effect, unless the entire idea of an enormous mass of water, a quasi-static bulge placidly following the motions of the Moon and Sun had been rejected in favour of a totally new dynamic vision (the 'fluid annulus').

To sum up, the Newtonian 'equilibrium theory', which should be described more precisely as a 'tide-generating force theory', was a largely tentative approach to the formidable problem of the tidal motions in the oceans – in which the major success was the full recognition for the first time in history of the role played by the gravitational forces – than, as historians have so far tended to believe, the complete, long overdue solution to a question which until then had had generations of natural philosophers racking their brains in vain. In addition, I would argue that, in the instance of the tide theory, the 'visual component' of Newton's reasoning, and its possibly having led him astray (see *Appendix 1*, the graphic scale of the three-body diagram) has not been fully recognised, perhaps just because, in our assessment of the structure of the Newtonian tide-generating force, we have never gone beyond the qualitative developments given in the *Principia*. If this structure had been scrutinised more carefully, the inherent weakness, or at least the awkwardness, of the consequences of the very founding principle of the so-called 'equilibrium theory' would immediately have been apparent. Such an analysis seems to me crucial if we are to cast new light upon the one aspect of the issue with which the historiographical debate has so far concerned itself – and this, before we present Galileo's case, resting as it does on his endeavour to arrive at a comprehensive

[14] Referring to the second diagram in Fig. 2.3, it must be noted that the tangential component reaches its maximum four times exactly at 45°, 135°, 225° and 315°, on the border lines between the octants, while being zero at 0°, 90°, 180° and 270°. The direction of this force is towards the disturbing body on the side opposite to it, and away from it on the other side, so that a bulge could in principle occur as a consequence of the flow of the sea waters towards the Sun or Moon, and *vice versa*. Nevertheless, this would be an entirely new dynamic behaviour and, as such, would be subject to the laws of hydrodynamics, so that even the so-called equilibrium theory turns out to be in reality a truly dynamic, undulatory process. In other words, the equilibrium theory is only valid on the assumption that the motion of the sea water occurs so slowly that no dynamic effect can arise.





Normal component intensity distribution Tangential component intensity distribution

Figure 2.3

explanation of the motions of the seas by furnishing at the same time a new physical proof of Copernican astronomy.

It is also true that Galileo's struggle to explain by means of dynamic causes (i.e. the motions of the Earth-Moon system) the main regularities of tidal phenomena – regularities which he saw in the three fundamental periods of the tides daily, monthly and annual accepted by him and by most of his contemporaries – was not, as we shall see, only against his inability to dominate quantitatively the kinematics of circular motion, but also against his lack of an adequate concept of the physical causes of motion in the universe. And this, to a certain extent, bears a dual resemblance – as in a photographic negative – to Newton's inability to calculate the unequal periods that his tide-generating force led to, an oversight which perhaps made him over-confident and eventually made him neglect the contribution of the geometry of the sea basins to the tides.

3. The oscillatory model

3.1 A simple oscillating system

Although Newton quite correctly recognised that the attraction exerted by the Moon and the Sun over water particles (the tide-generating force) has the ability to accelerate sea waters within basins, his tide model was unable to 'adjust' and 'tune' the predicted basic periodicities due to the periodical motions of the 'disturbing bodies'to real and local conditions. According to the hypotheses of the so-called 'equilibrium theory', the water masses of the oceans respond to the tide-generating force without any time delay and, above all, without interfering with the 'rhythm' set by the alternating configurations that the Moon and the Sun display on the celestial sphere. In accordance with this model, the swell of the sea smoothly follows the moving Sun and Moon while the Earth rotates about its polar axis. Yet, in spite of the regularity of celestial motions, tide phenomena are so diverse, and manifest so wide a range of periodic characteristics, that no 'equilibrium theory' can account for the whole of them. According to this theory, water slowly swells under the action of the tide-generating force in a way known to physicists as 'quasi-static' or 'static', whereas real tidal waves are dynamic phenomena whose global behaviour is controlled not only by external forces like the tide-generating force, but also by more 'internal' parameters, intrinsic to the system itself. An example of dynamic system is the simple pendulum – a bob hung by a thread – that oscillates owing to the action of gravity and whose period of oscillation is at the same time 'regulated' by the length of the thread. If you push the bob, the pendulum begins to swing, but this action of yours is soon 'forgotten' (however forceful) and its motion becomes entirely controlled by intrinsic characteristics (in this case gravity and the length of the thread). On the other hand, a quasi-static system is well represented by the absorption mechanism of a cushion whose stuffing slowly compresses under the weight of your body. It is this latter force that sets the cushion in slow motion, and as soon as it ceases the motion of the cushion ceases too.

Although the wave-like nature of tidal motions was first recognised in the second half of the eighteenth century by the French mathematician and physicist Laplace,[1] who wrote the fundamental equations of the so-called dynamic tide theory, it was not until the beginning of the 1980s that oceanographers,[2] thanks to the impressive development of more and more powerful supercomputers and of sophisticated methods for the approximate numerical integration of complicated differential equation systems, began to furnish abundant quasi-empirical evidence[3] that the frequency of oscillation of tidal waves in the sea basins depends markedly on the geometry of the basins and, more

[1] See P.S. Laplace, *Oeuvres Complètes*, 14 volumes, Paris, Gauthier-Villars, 1878–1912. Laplace's first paper on tides was published under the title "Recherches sur quelques points du systéme du monde" in *Mémoires de l'Académie royale des sciences de Paris* for the year 1775. See *Oeuvres Complètes*, tome 9, p. 88ff. Laplace's research programme is clear: "Il ne s'agit point de chercher une nouvelle cause du flux et du reflux de la mer, mais de bien faire usage de celle que nous lui connaissons incontestablement, et qui, comme l'on sait, consiste dans l' inégale pesanteur des eaux de la mer et du centre de la Terre vers le Soleil et la Lune. Je me propose d'assujettir à une analyse plus rigoureuse qu'on ne l'a fait encore les effets de cette inégalité de pesanteur et *les oscillations qui en résultent*" (*ibid.*, p. 88). Italics are mine. The most complete scholarly work devoted to the whole history of tide theory from Newton to the end of the nineteenth century is still R.H. Harris, *Manual of Tides*, Part I, Appendix n.8 – Report for 1987, Washington, Government Printing Office, 1898. The best study on Laplace's theory of tides is to be found in this work (*ibid.*, pp. 422–437).

[2] The account on which I have drawn is G.I. Marchuk, B.A. Kagan, *Dynamics of Oceans Tides*, Kluwer Academic Press, Dordrecht, 1989, p. 88ff. See further bibliography on recent studies on tides at the end of the book. Classic texts on this topic (belonging to the pre-computer era) are H. Lamb, *Hydrodynamics*, 6th ed., New-York, Dover Publications, 1945, pp. 250–362 and L.M. Milne-Thomson, *Theoretical Hydrodynamics*, 5th ed., New-York, Dover Publications, 1968, pp. 426–463. See also J. Pedlosky, *Geophysical Fluid Dynamics*, 2nd ed., New York, Springer, 1987, pp. 336–489.

[3] The expression finds justification in the common point of view according to which a numerical simulation is, to a certain extent, equivalent to a laboratory experiment.

generally, on the global geometry of the so-called 'World-Ocean' (the entire ocean system on planet Earth). Although it would be extremely interesting to follow in detail the impact that supercomputers have had on tide theory, what we are mainly concerned with in our study is the fact that these recent studies have for the first time proved that, quite apart from the periodicities due to 'forced oscillations' (i.e. oscillations induced by the periodic nature of tide-generating force, which is due to the combined attraction of the Sun and the Moon – an equivalent of your hand pushing the bob in the above-mentioned pendulum), the World Ocean may be described as a dynamic oscillating system basically characterised by a set of 'free oscillation' periodicities – the equivalent of the bob's swinging being controlled only by gravity and the length of the thread – that shows a 'strong' concentration of frequencies near the diurnal and semi-diurnal frequencies of forced oscillations due to the tide-generating force. 'Free oscillation' is the motion of water inside a basin subjected only to the action of gravity, so that the water is free to reciprocate with its own natural frequency (like a pendulum that oscillates with its own period – i.e. the natural period or natural frequency – without external forces acting upon it). This close proximity between free and forced oscillation frequencies is the key to the understanding of the role of basin geometry in tide phenomena. The interaction between tide-generating force and tidal waves in the World Ocean is dominated by this merely accidental proximity. It is this proximity that is responsible for generating the phenomenon known as 'resonance'.

The ability to resonate is a common phenomenon well known to physicists. For example, thanks to electronic resonance phenomena you can tune your radio equipment and receive your favourite broadcast. This is due to the ability of your tuner to respond to the radio wave that carries the message you want to listen to. When wave frequencies are beyond the reach of your tuner, you are unable to tune your radio, which means that your equipment has lost its resonance ability (because the arriving frequency is either too low or too high). This kind of resonance is useful. But not all resonances are such. For example, if your car's suspension system – a basically oscillating system in which the spring is responsible for absorbing the shocks due to the energy input from the roughness of the road surface, while the damper is responsible for dissipating that unwanted energy – had poor damping performance and at the same time were tuned to the very frequencies generated by the disturbances from the road, a 'mechanical' resonance would soon start making you bounce up and down! This is a kind of resonance you could well do without.

The World Ocean itself has the ability to resonate according to the frequencies of the tide-generating force (also called tidal potential). And the mechanism whereby the Sun and the Moon act on the sea waters is simply that of 'exciting' the intrinsic 'harmonic content' (the set of free oscillation periodicities) of the sea basins. The harmonic content can be seen as the ability of the basin to manifest motion of a periodic nature at particular and fixed frequency values. It is precisely this harmonic content that is mainly responsible for a basin's manifesting these well-defined and fixed tide periods. And the harmonic content is in turn determined by the geometry of the basin. Different basins have different harmonic contents, so that they respond to external forces with different tide periods. Basins are like receivers that can only be satisfactorily tuned to the frequencies of particular broadcasters.

Now, even though the World Ocean cannot be described as a 'discrete system', namely, a system describable by a finite number of variables, its harmonic content – which is also called spectrum of oscillation – has been shown to have a 'clearly defined discrete nature' where 'the only exception is the low-frequency region where the spectrum is almost continuous".[4] This means that only well defined particular values of resonance exist in the World Ocean. And nearly sixty are significant (i.e. they have 'energy content' – importance with respect to others – high enough to be comparable with one another[5]). In addition, in the region of the 'semi-diurnal and diurnal spectral bands are present modes the periods of which differ only slightly from those of the major tidal potential harmonics'.[6] This means that the very geometry of the World Ocean has an intrinsic ability to respond to the diurnal and semi-diurnal periodicities of the tide-generating force (these periodicities, which are nowadays also called *the harmonic content of tidal potential*, are subdivided according to three main periodic components of tide-generating force: a) long period, i.e. more than one day; b) diurnal period, i.e. 24 hours; c) semidiurnal period, i.e. 12 hours).[7] The best way to understand how this combination of internal and external parameters determines the behaviour of oscillating systems is to study a system simplified enough to reduce to a minimum the mathematics involved, though significant enough to be able to exhibit all these characteristics. Such a system may be represented by the one-dimensional motion of a particle P, because such a motion is described by one single variable.[8]

Let us consider a fluid particle P oscillating horizontally on the surface of the sea along a straight line x, and let m_p be its mass, k a constant describing the elastic property of its motion (as if P were attached to a spring of your car suspension system) and c a constant describing the energy dissipation function due to friction (as if P were attached to the damper too). Let F(t) be a function describing the external tide-generating force acting on P. F(t) depends only on time. Newton's equation of P's motion is

$$\mathrm{m} \cdot \frac{d^2}{d\mathrm{t}^2}\mathrm{x} = \mathrm{F(t)} - \mathrm{c} \cdot \frac{d}{d\mathrm{t}}\mathrm{x} - \mathrm{k} \cdot \mathrm{x} \qquad (1)$$

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[4] G.I. Marchuk, B.A. Kagan, *Dynamics of Oceans Tides, op. cit.*, p. 107. A continuous spectrum means that whatever the frequency of the external tide-generating force, the basin has the ability to respond to it (the 'quantity' of this ability is nevertheless determined by the amplitudes of the low frequency 'modes of oscillation' relative to the dominant modes).

[5] It is assumed that significant 'energy content', i.e. 'importance', amounts to 2.5% of the maximum value in the 'spectrum of oscillation'. (See *ibid.*).

[6] *Ibid.*, p. 107–108. 'Mode' refers to the spatial distribution of motion on the surface of the World Ocean. 'Modes' show how tidal waves actually appear to evolve on the surface of the sea (*ibid.*, p. 109ff). 'Spectral band' is a common expression much used to refer to spectral 'zone', rather than to a specific value of frequency.

[7] The harmonic content of tidal potential depends on the various combinations of the motions of the Sun and the Moon, so that there are, for example, seven diurnal components and five semidiurnal components. See is G.I. Marchuk, B.A. Kagan, *Dynamics of Oceans Tides, op. cit.*, p. 11.

[8] For a more formal and technically complete (i.e. that also takes into account systems with more than one variable) presentation of the following analysis see A.F. D'Souza, V.K. Garg, *Advanced Dynamics, op. cit.*, p. 161ff.

where $-(\mathrm{k} \cdot \mathrm{x})$ is the force exerted by the 'spring' and $-\left(\mathrm{c} \cdot \frac{d}{d\mathrm{t}}\mathrm{x}\right)$ the force exerted by the 'damper'. A common way to study equation (1) is to re-write it in a slightly different form by positing $\frac{\mathrm{c}}{\mathrm{m}} = 2 \cdot \xi \cdot \omega$, $\frac{\mathrm{k}}{\mathrm{m}} = \omega^2$ and $\mathrm{I(t)} = \frac{\mathrm{F(t)}}{\mathrm{m}}$, so that equation (1) may be re-written as

$$\frac{d^2}{d\mathrm{t}^2}\mathrm{x} + 2 \cdot \xi \cdot \omega \cdot \frac{d}{d\mathrm{t}}\mathrm{x} + \omega^2 \cdot \mathrm{x} = \mathrm{I(t)} \tag{2}$$

where ω is generally called 'natural frequency' and ξ 'absolute damping coefficient'. Now, 'natural frequency' and 'absolute damping coefficient' are the intrinsic parameters of P's motion, while I(t) is the external input. There is a common mathematical 'trick' – named 'Laplace transform' after its inventor – that allows us to rid ourselves of the differential nature of equation (2) by transforming it into a simple algebraic equation. The trick amounts to substituting time itself with a new so-called Laplace variable 's' – 's' is basically a complex number whose meaning will be clear in a moment – and the simple and double derivatives with respect to time with powers of the new variable (one for the first derivative and two for second derivative). The new 'algebraic' equation (its elements X(s), I(s) and 's' are complex numbers) is

$$\mathrm{s}^2 \cdot \mathrm{X(s)} + 2 \cdot \xi \cdot \omega \cdot \mathrm{s} \cdot \mathrm{X(s)} + \omega^2 \cdot \mathrm{X(s)} = \mathrm{I(s)}$$

which gives the solution X(s) as the ratio – also known as 'transfer function' – of the motion X(s) to the input I(s) in the following form

$$\frac{\mathrm{X(s)}}{\mathrm{I(s)}} = \frac{1}{\mathrm{s}^2 + 2 \cdot \xi \cdot \omega \cdot \mathrm{s} + \omega^2} \tag{3}$$

Transfer function (3) represents the ratio of P's motion to the external action of tide-generating input I(t). Now, we are interested in what happens when the external input I(t) (tide-generating force) is a periodic function. Thanks to our trick, we may represent the external periodic force (let Ω be its varying frequency) in a very simple way, providing we interpret the complex number 's' in such a way that $\mathrm{s} = \mathrm{j} \cdot \Omega$ (where j is the 'imaginary unit') and compute the modulus of the complex number (3) (remember that (3) is a ratio between two complex numbers).

Now, although with recourse to our College math we might easily find the modulus of $\frac{\mathrm{X(j \cdot \Omega)}}{\mathrm{I(j \cdot \Omega)}} = \frac{1}{(\mathrm{j} \cdot \Omega)^2 + 2 \cdot \xi \cdot \omega \cdot (\mathrm{j} \cdot \Omega) + \omega^2}$, we do not actually need to perform such a tedious operation, because all we want is to see how the input $\mathrm{I(j \cdot \Omega)}$ 'excites' P's motion. And to compute numerically that modulus our small personal computer is adequate. Let us remember that ω is the natural frequency of P's oscillating motion, while ξ is the damping coefficient; in other words, ω is the very frequency that might be tuned to the external periodic input, while ξ is the dissipating factor (due to friction) that prevents P from resonating 'too much', that is, from being made to 'escape' too far away when an external tide-generating force is finely 'tuned' to its natural frequency.

The following diagram (Fig. 3.1) reveals the secrets of P's behaviour.

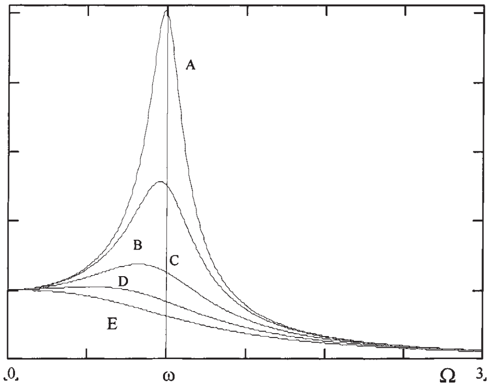

Figure 3.1

The abscissa represents the varying frequency Ω of the external input, while the ordinate gives the ratio of P's periodic motion (also known as P's periodic response[9]) to input $I(j \cdot \Omega)$. Each curve (A, B, C, D, E) represents a possible value for ξ (which is a pure number, independent of the choice of unit), namely, for P's ability to dissipate external energy (respectively $\xi = 0.1$, $\xi = 0.2$, $\xi = 0.4$, $\xi = 0.6$, $\xi = 0.8$). When ξ is low, P's motion 'explodes' very near the value of its natural frequency ω, that is, when $\Omega \cong \omega$ (the 'resonance' condition), whereas, when ξ is higher, P's 'resonance' is kept under control. The vertical line represents the 'resonance' point given by $\Omega = \omega$. (If it were $\xi = 0$ then a true resonance would occur, i.e. exactly when $\Omega = \omega$).

Figure 3.1 is the best way to visualise P's dynamic behaviour when an external tide-generating force acts on it and causes it to oscillate as if it were driven by frequency Ω. And the position on the abscissa of the point $\Omega = \omega$ (i.e. ω itself) is due to the intrinsic characteristics that regulate P's motion, namely, the intrinsic parameters of particle P's equation of motion, its mass m_p, its elasticity k and its dissipating ability c, as given in equation (1). What is of paramount importance is to realise that Figure 3.1 shows exactly how P's motion is 'magnified' by the tuning of the external periodic tide-generating force to the internal 'natural frequency'. The nearer the *external* periodicity to the *internal* natural periodicity, the finer the tuning and, therefore, the more visible and more 'amplified' P's motion. This is how an external forced oscillation due to a tide-generating force merges with the internal free oscillation due to internal parameters and results in a global motion whose amplitude is governed by how fine the tuning is.

There is a second important aspect of P's motion that has to be understood. We have so far described P's motion in the light of the hypothesis that tide-generating force F(t) acts on P. This is called forced oscillation. Now, if you let $F(t) = 0$ in equation (1), you have what is called by physicists 'free oscillation'. According to this second hypothesis, P's motion is still governed by relation (1), even if no external force exerts its action on it. If you 'push' P (viz the pendulum example), P starts to oscillate freely under the control of

[9] It can be easily shown that, providing ξ is not zero, then after a certain time the dissipation due to ξ 'equalises' P's periodic motion with the external input Ω. See A.F. D'Souza, V.K. Garg, *Advanced Dynamics, op. cit.*, p. 246ff.

its intrinsic characteristics (mass m, stiffness k and damping c, or, more simply, ω and ξ). Now, the question arises as to what the 'total' motion of P might be when both an external force F(t) and an initial 'push' are acting together. Well, physicists claim that under these circumstances, P moves as if it were driven by the simple sum of both the effect of an initial push and of the external force P. Insofar as the system (1) is 'linear' – i.e. insofar as 'stiffness' and 'damping' are described by simple laws proportional to position x and velocity $\frac{dx}{dt}$ – no 'strange effects' result from the superimposition of the two causes of motion. In other words, P's motion is in turn a superimposition of the two independent motions caused by the initial 'push' and by F(t), so that to deduce P's total motion we must simply add forced oscillation to free oscillation. And given that we have learned how to treat forced oscillation, all we need to do now is to learn how to treat free oscillation.

If we regard P's motion as it is described by the original differential equations (1) or (2), that is, if we consider P's motion as it occurs in what is sometimes referred to as 'time domain' (to distinguish it by the 'Laplace domain' represented by the Laplace variable 's'), and if we let $F(t) = 0$, then $x_{FR}(t)$ (the subscript FR means 'free') simply turns out to be a function of the known parameters m, k, c or, ω and ξ. We do not need to be concerned with the actual procedure to work out the expression $x_{FR}(t)$,[10] and in order to simplify things a bit more, we can put $\xi = 0$. Hence we simply have $x_{FR}(t) = x_0 \cdot \sin(\omega \cdot t)$, where x_0 represents the initial position (the point at which P's motion starts – we also assume that initial speed is zero). Now, it is not difficult to find out that, when the tide-generating force is periodic and its frequency is Ω (i.e. when $F(t) = F_0 \cdot \sin(\Omega \cdot t)$, where F_0 is the amplitude of tide-generating force), P's forced oscillation $x_{FO}(t)$ (subscript FO means 'forced') may be expressed in the time domain by the following expression[11]

$$x_{FO}(t) = \frac{F_0}{k} \cdot \frac{1}{1 - \left(\frac{\omega}{\Omega}\right)^2} \cdot \left(\sin(\Omega \cdot t) - \left(1 - \frac{\omega}{\Omega}\right)\sin(\omega \cdot t)\right)$$

so that the total motion of particle P becomes

$$\begin{aligned} x(t) &= x_{FR}(t) + x_{FO}(t) \\ &= x_0 \cdot \mathrm{xin}(\omega \cdot t) + \frac{F_0}{k} \cdot \frac{1}{1 - \left(\frac{\omega}{\Omega}\right)^2} \cdot \left(\sin(\Omega \cdot t) - \left(1 - \frac{\omega}{\Omega}\right)\sin(\omega \cdot t)\right) \end{aligned} \quad (4)$$

whose meaning, although at first sight it may well appear a bit complicated, is in fact quite straightforward. The right-hand side of the previous equation tells us simply that particle P's total motion under the action of both external tide-generating force $F(t) = F_0 \cdot \sin(\Omega \cdot t)$ and internal free motion $x_{FR}(t) = x_0 \sin(\omega \cdot t)$ is simply the sum of the effects due to initial position x_0 and to external force F(t). Now, whether x_0 is present or not, P's response contains 'two frequencies: the natural frequency ω and forcing frequency Ω. When ω and Ω are close to each other, the response exhibits a beat phenomenon and when $\omega = \Omega$, it is obvious that x(t) is infinite (i.e., there is resonance)'.[12]

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10 See A.F. D'Souza, V.K. Garg, *Advanced Dynamics, op. cit.*, p. 235ff.

11 *Ibid.*, p. 241.

12 *Ibid.*. I have used different symbols, so that I have slightly modified the text quoted.

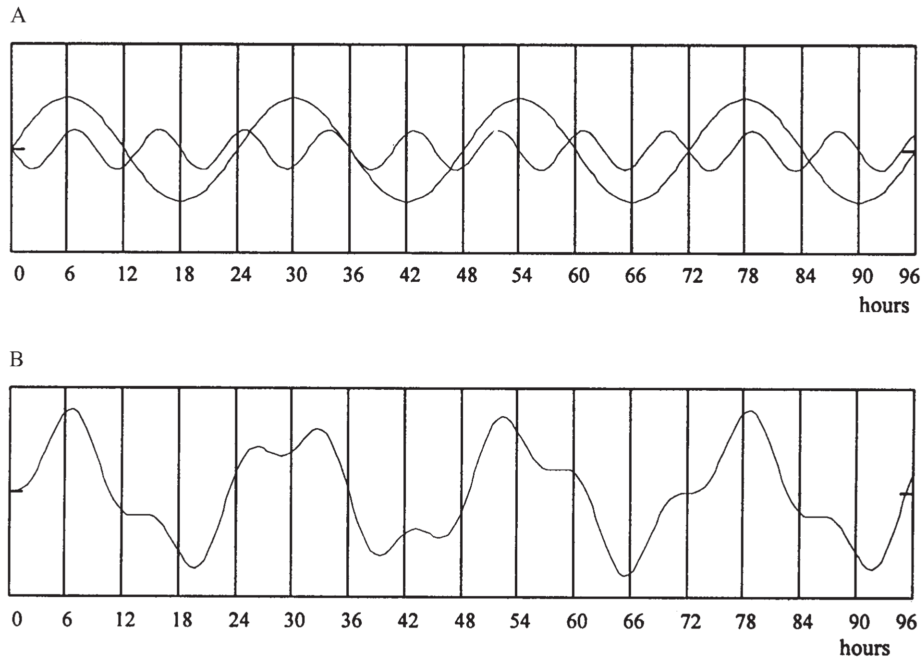

Fig. 3.2. Parts (A, B)

The two parts (A, B) of Fig. 3.2 show how equation (4) works. Here, in the above part, we have two sine functions whose amplitudes satisfy relation (4) – so as to represent correctly the two distinct components of P's combined oscillation – and, in the part below, we have P's combined oscillation, i.e., the sum of the two distinct waves represented above.

In the example above, we have assumed that $T_\Omega = 24$ hours and $T_\omega = 9$ hours, and have illustrated P's total periodic oscillation over four days. By varying ω and Ω one obtains many combinations of single waves due to P's internal property ω and to tide-generating force $F(t) = F_0 \cdot \sin(\Omega \cdot t)$, which leads to different total waves of P.

Now, although our one-dimensional oscillating system is very simple, its basic characteristics, that is, its dynamic behaviour, may be considered, if not a satisfying approximation, at least an analogy of the technically much more complicated dynamic behaviour of the World Ocean. For, although the World Ocean is a 'continuous system' – i.e. a system where it is not possible to describe the behaviour of all the water particles by means of a finite number of variables – physicists have developed sophisticated techniques that allow them to represent such continuous systems by means of a finite set of 'intelligent' super-coordinates. It is these techniques that are at the core of the methods based on the computer simulations that – as we have seen – led at the beginning of the 1980s to the discovery that the 'spectrum' of natural frequencies of the World Ocean is *finite*, namely, that there are only a *finite* number of natural frequencies that characterise the dynamics of the World Ocean. And this is what enables us to maintain that, for our 'didactic' purposes, our one-dimension model is good enough to represent qualitatively the typical oscillating character, both forced and free, of the actual behaviour of true waves within sea basins.

3.2 Galileo’s model: laws of basins and superimposition of waves

There are three basic characteristics inherent in any oscillatory system: a) the ability of the system to oscillate between two extreme positions, after being excited by some external cause, in such a way that the spatial characteristics of the wave and its frequencies are governed only by internal parameters; b) an external cause that is responsible for setting the external ‘pace’ of motion; c) a mechanism by means of which the external cause and the ability of the internal system to respond to that cause can be satisfactorily explained.

Galileo’s oscillatory model of the time and spatial response of sea basins to tide-generating acceleration is possessed of all three characteristics: the ability of water to ‘reciprocate’, by virtue of its being heavy, and the geometry of the basin are the two elements that satisfy requirement a); tide-generating acceleration is the periodic external cause (its periodicity being 24 hours) that satisfy requirement b); finally, a simple principle of superimposition of waves within basins is the mechanism that explains the interaction between external tide-generating acceleration and the internal properties of the oscillating system (gravity and geometry), which satisfy requirement c). We now need to examine with close attention points a) and c), given that point b), i.e. tide-generating acceleration, has already been dealt with in Sect. 1.

> [. . .] whenever the water, thanks to some considerable retardation or acceleration of motion of its containing vessel, has acquired a cause for running toward one end or the other, it will not remain in that state *when the primary caused has ceased*. For, *by virtue of its own weight* and its natural inclination to level and balance itself, it will speedily return of its own accord; and being heavy and fluid, *it will not only return to equilibrium but will pass beyond it*, pushed by its own impetus, and will rise at the end where first it sank. But it will not stay there; by *repeated oscillations of travel* it will make known to us that it does not want the speed of motion it has received to be suddenly removed and reduced to a state of rest. It wishes this to be slowly reduced, abating little by little.[13]

When Galileo’s ‘primary cause’ – tide-generating acceleration – has ceased to act, it is ‘by virtue of its own weight’ that water does not cease to move[14] (as it would do

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[13] G. Galilei, *Dialogue on the Two Chief World Systems, op. cit.*, p. 428. Italics are mine.

[14] The expression ‘by virtue of its own weight’ suggests that Galileo attributes here to the water’s weight the property of being the actual cause of its to and fro motion. It is well beyond the scope of my enquiry to enter into the vexed question of Galileo’s conception regarding ‘gravity’ and ‘heaviness’ and their being causes of motion, particularly causes of accelerated motion, for example, free-fall motion. Although many an interpretation has been proposed by Galilean scholars, two theses appear to summarise the point at issue: a) A. Koyre’s thesis (see A. Koyré, *Ètudes Galiléennes*, 3 vols., Paris, Hermann, 1939; see volume III, pp. 230–231), according to which Galileo did not see gravity as the cause, or, at least, a source, of downward movement, and b) a more recent thesis, according to which Galileo made many attempts to establish a connection between accelerated downward motion and its physical cause (see, for example, R. Westfall, “The problem of Force in Galileo’s Physics”, in C.L. Golino [ed.], *Galileo Reappraised*, Berkeley, University of California Press, 1966, pp. 67–95; S. Drake, *Galileo’s Studies, op. cit.*, p. 249ff,

according to the hypotheses of the Newtonian equilibrium theory, where there is no need for a dynamic response of water, given that the whole phenomenon, which is controlled exclusively by tide-generating force, is quasi-static, i.e. in essence, extremely slow). On the contrary, water passes beyond the ‘equilibrium point’ and only by ‘repeated oscillations of travel’ does it reduce itself to ‘a state of rest’. What Galileo has in mind here is the image of a swinging pendulum. In fact the passage quoted above goes on to explain that

> In exactly this way we see that a weight suspended by a cord, once removed from the state of rest (that is, the perpendicular), returns to this and comes to rest by itself, but only after having gone to and fro many times, passing beyond this perpendicular position in its coming and going.[15]

And this image becomes the *trait d’ union* with the statement of the laws of motion of water within basins that immediately follows

> [. . .] the reciprocations of movement just mentioned are made and repeated with greater or less frequency (that is, in shorter or longer times) according to the various lengths of the vessels containing the water. *In the shorter space, the reciprocations are more frequent, and they are rarer in the longer,* just as in the above example of the plumb bobs the reciprocations of those which are hung on long cords are seen to be less frequent than those hanging from shorter threads. [. . .] it is not only a greater or lesser length of vessel which causes the water to perform its reciprocations in different times, but a greater or lesser depth does the same thing. It happens that *for water contained in vessels of equal length but of unequal depth, the deeper water will make its vibrations in briefer times, and the oscillations will be less frequent in the shallower*.[16]

Thus, Galileo knew exactly that: a) thanks to gravity – be it an internal tendency or an external accidental cause – water contained in vessels continues to oscillate freely after having been excited; b) the frequency of free oscillation depends on the width and depth of the vessel, i.e. on its geometry; c) the frequency increases in relation to the depth of the vessel and decreases in relation to its width. What he almost certainly did not know, or, at least, what he was not able to work out satisfactorily enough to support his claims, was the quantitative relations by means of which the two laws of basins might be expressed in an appropriate mathematical language. This lack of ‘quantitative’ refinement

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where the author discusses the question in relation to Galileo’s concept of inertia and projectile motion; P. Galluzzi, *Momento. Studi Galileiani*, Roma, Edizioni dell’ Ateneo & Bizzarri, 1979, particularly pp. 309–329, where Galileo’s different theories of ‘acceleration’ and its cause are discussed). However that may be, Galileo’s analysis of the oscillatory behaviour within basins appears to be totally independent of the question as to the physical cause that determines its behaviour. And whether ‘gravity’, or ‘heaviness’, acts as an internal principle ‘naturally’ common to all heavy bodies, or as an external ‘accidental’ cause, so that weight is simply proportional to this cause, Galileo’s description of oscillating phenomena emerges unscathed.

[15] G. Galilei, *Dialogue on the Two Chief World Systems, op. cit.*, p. 428.

[16] *Ibid.*, pp. 428–429. Italics are mine.

notwithstanding, Galileo's clear statements are the very 'minimum' of knowledge that is absolutely required in order to credit him with part a) of an oscillatory model.

The fact that Galileo was not able to quantify his laws is a significant clue to how his research method worked (or did not work) and an interesting question about the origin of his discovery arises. What is more, given that the origin of this discovery must have been intimately tied up with Galileo's astonishing description (which, from a qualitative point of view, virtually coincides with our modern description) of what a *stationary wave* inside a rectangular basin – i.e. a wave whose spatial profile may be described by a function that does not depend on time – actually is and how it behaves, this question turns out to be even more intriguing, particularly as it forces us to turn a spotlight upon the stage of Galileo's laboratory. Here is his description of the profile of the water, i.e. of the spatial characteristics of the water wave:

> [. . .] such vibrations [of water] produce two effects in water which are worthy of being noticed and observed carefully. One is the alternate rising and falling at either extremity; the other is the horizontal moving and running to and fro, so to speak. *These two different motions inhere differently in different parts of the water*. The extreme ends of the water rise and fall the most; the central parts do not move up and down at all; and other parts, by degrees as they are nearer to the ends, rise and fall proportionately more than those farther from the ends. On the other hand, the central parts move a great deal in that other (progressive) movement back and forth, going and returning, while the waters in the extreme ends have none of this motion.[17]

This description does not differ from how a physicist would nowadays describe the relation between *horizontal current* and *vertical displacement* in a stationary wave. Figure 3.3 should clarify the situation. If we consider – as Galileo's words overtly suggest – what is called a single node oscillation (i.e. one in which there exists a point, like node N in Figure 3.3, which is precisely located in the middle of the vessel and remains fixed) and suppose that (a) is the wave at time zero, then, after a quarter of a cycle, the wave profile, as given in (b), is a horizontal straight line, while (c) represents the mirror image of (a) as it appears after half a cycle. The horizontal arrows represent the water current (magnitude is proportional to their lengths, while the direction of the flow is indicated by the arrows) at different points along the vessel. In (a) the wave has reached the 'rest' configuration; the water is 'instantaneously' at rest (the phenomenon is continuous and there is no finite time of rest and therefore the speed of every water particle at this instant is zero) and current is zero at every point. In (c), vertical displacement is maximum at the extremes, where current is zero, whereas, at the centre N, where vertical displacement is zero, current is maximum towards the left hand side. In (b), where all vertical displacements are zero, flow is maximum everywhere, even though it remains greater at N than at any other point and it is obviously zero at the extreme ends.

Now, how might one come up with such an extraordinarily precise description of such a complex interaction between vertical and horizontal motion? By imagining the oscillating free surface of the water as if it were a sort of a rigid straight line turning

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[17] *Ibid.*, p. 429. Italics are mine.

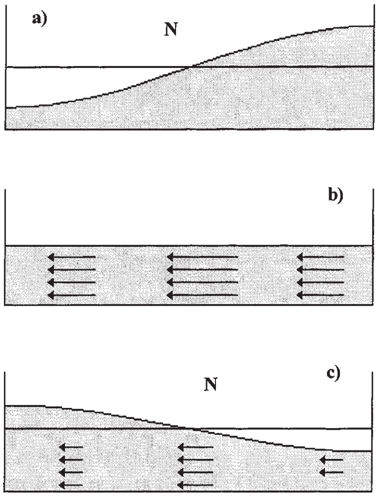

Figure 3.3

around a hub (node N) located at the centre of the vessel, one can easily visualise the vertical motion component. But what about the horizontal current? Although it is evident that water cannot disappear inside the vessel and that therefore, when the level of the water decreases on the left-hand side and increases on the right-hand side, there must be a net mass transfer from left to right, it is not at all obvious that such a mass transfer gives rise to a velocity distribution like that so neatly described by Galileo. My suggestion is that Galileo established, if not discovered, that this is the way stationary waves behave in rectangular basins by means of experiments with artificial vessels – experiments about which he speaks at length in the Fourth Day of the *Dialogue*. The evidence I can give to support my suggestion is simply what I myself have ascertained upon repeating a few of these experiments (see Sect. 5 for a more detailed description of my experiments with a parallelepipedal glass tank and the significance of the results obtained). This mixed behaviour is exactly the first thing that a beginner who sets out to investigate the nature of these phenomena learns about waves: at the extreme ends of the vessel waves appear to rise and descend vertically, whereas at the centre they clearly appear to flow more or less horizontally. A small piece of buoyant material is all that is needed to see with a fair degree of precision how current and displacement interact with each other, especially at node N.

Now, even though it remains open to debate whether Galileo anticipated his preliminary conclusions regarding waves by means of deductive reasoning or drew them on the basis of empirical evidence (like that plainly manifested by simple experiments akin to those I have re-performed), what appears to me impossible is that he could have formulated, by means of pure intuition or pure reasoning, or by means of analogy, at least one of the two laws of basins, namely, the 'depth' law. Although I seriously doubt whether, in the light of Galileo's clear understanding of the mixed nature of wave-like motion, even the 'width' law may be considered somehow 'intuitive' or deducible by reasoning (it would be intuitive if one admitted that something like a narrow wave peak travels back and forth between the vessel's end walls, but this possibility is definitely ruled out by Galileo's neat description of stationary waves), there is no escaping the textual evidence, according to which Galileo was supposedly guided by the analogy with the pendulum – given that he more or less explicitly advances a comparison between length of the the pendulum and the width of the vessel.

Although the pendulum in itself, as a fundamental mechanical problem that also serves as an analogy, is a central element in the whole of Galileo's science of mechanics, and although it may well have been the inspiration of his 'width' law, one thing is clear: pendulum-like motions cannot have furnished Galileo with a completely satisfactory analogy with water motion inside a basin. The reason is that – as Galileo knew – the pendulum's frequency of oscillation is governed by a sole parameter: the length of the suspending thread.[18] And although, as is well known, pendulums were a fertile source of research interest throughout the whole of Galileo's career, in all probability much more fertile than he would have desired and than he was keen to make public,[19] the 'depth' law cannot have been generated, as a serendipitous by-product, even by this most 'proliferous' of Galileo's conceptual and practical research tools. It must have been formulated on the ground of evidence based on experiment (see Sect. 5).

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[18] Apart from the isochronism law, which Galileo discovered early in his career and to which scholars have traditionally paid great attention, Galileo knew the so-called second law of pendulums, according to which the ratio of the frequencies ω_B, ω_A of oscillation of pendulums of lengths B and A is $\frac{\omega_B}{\omega_A} = \sqrt{\frac{A}{B}}$. It is not clear when and by whom the pendulum law of length was discovered. The first testimony regarding the isochronism law is Galileo's letter to Guidobaldo Del Monte in 1602 (*Le opere di Galileo Galilei*, X, pp. 97–100), while Galileo's first testimony relating to the second law is a much later letter, dated June 1637, to the Dutch admiral Laurens Reael (*Le opere di Galileo Galilei*, XVII, pp. 96–105), in which Galileo expounds at length his proposal for a time 'numerator' (a rigid pendulum in the form of a brass circular sector of amplitude about 12, 15 degrees used as a vibration counter) to be used as a tool to find longitude at sea. Now, given that this second letter is the first testimony of Galileo's knowledge of the second law – which he thinks to hold true for both simple and rigid pendulums – and given that it tells us about Galileo's probable work with complex rigid pendulums, it might be possible to hypothesise that he found this second law precisely in connection with his attempt to devise his time 'numerator'. If so, we have to rule out the possibility that such research was carried out in relation to the problem of the basin laws. And even if the discovery of the second law was earlier, it is hard to see how working with simple or rigid pendulums, like the brass 'circular sector' described in the letter to Reael, he could have come up with an analogy between the width and depth of rectangular vessels and the length of the thread of the simple pendulum or the length and angular amplitude of rigid pendulums.

[19] By repeating Galileo's measurement procedures and experiments, R. Naylor demonstrated not long ago that Galileo knew a lot more about the non-perfect validity of the isochronism law, indeed more than appears from what he decided to publish in his *Dialogues Concerning Two New Sciences*. See R. Naylor, "Galileo's Simple Pendulum", *Physis*, XVI, 1974, pp. 23–46. More recently, D.K. Hill, in his article "Pendulums and Planes: what Galileo didn't publish", *Nuncius*, IX, 1994, pp. 499–515, which is based on a detailed analysis of an extant working draft of Galileo's, came to the same conclusion as R. Naylor. Even though this proves that Galileo effectively carried out a lot of experimental work with pendulums that only now do we begin to unearth, nothing suggests that this was possibly in connection with the problem of tides. Evidently Galileo was not able to establish an analogy between vessels and pendulums, be they simple or rigid. Both the studies mentioned above are entirely devoted to the isochronism law.

In order to define an ‘oscillatory model’ a third element is required – a mechanism whereby waves interact and give rise to a global system’s response to external input. Galileo’s principle of superimposition of waves played this indispensable role. Galileo reminds the reader of the two main causes of tides, i.e. tide-generating acceleration and the regulating basin geometry:

> [...] it remains now for us to make another important reflection upon the two principal causes of the tides, thereafter compounding them and mixing them together. *The first and simplest of these* [...] *is the definite acceleration and retardation of the parts of the earth* [...] *within the space of twenty-four hours*. The other depends upon the water’s own weight, *which* [the water], *once moved by the primary cause, tries then to restore itself to equilibrium by repeated oscillations* which are not determinate as to one pre-established time alone, but which have differences of duration according to different lengths and depths of the containers and basins of the oceans. In so far as they depend upon this second principle, some would flow and return in one hour, some in two, in four, in six, in eight, in ten, etc.[20]

Then, he goes on to explain his *principle of superimposition of waves*:

> Now, if we commence *to add the first cause*, which has an established period of twelve hours, *to the second* when it has for example a period of five, then it will sometimes happen that *the primary and secondary causes agree in making their impulses both in the same direction* [...]. At other times it happens that the primary impulse becomes in a certain sense *contrary to that brought by the secondary* [...]. At still other times, when the two principles *are not in opposition nor yet entirely unified*, they cause other variations in the rise and fall of the tides.[21]

The best way to appreciate fully the effectiveness of Galileo’s principle of superimposition and, above all, to see that it works precisely as Galileo claims, is to transform his vivid description into real images. Let us consider a wave $A = \sin(\Omega \cdot t)$ whose period is 24 hours and let it be the response due to the Galilean primary cause. A second wave $B = \sin(\omega \cdot t)$ with a different period may represent the response generated by the secondary cause. Given that Galileo’s reasoning is merely qualitative and that there is no evidence that he made any attempt to investigate the quantitative aspect of the phenomenon, we cannot credit him with a full understanding of the relationship between the amplitudes of free and forced oscillation, as it is given by relation (4). Thus, we define a Galilean response $x_G(t)$ to tide-generating acceleration as a simple

[20] G. Galilei, *Dialogue on the Two Chief World Systems, op. cit.*, p. 434. Italics are mine. There is a small discrepancy between this passage and the previous one, given that, in relation to the period of the primary cause, Galileo speaks in the latter of a 24 hour period, while in the former of a 12 hour period. We have to bear in mind that Galileo’s concept of period is not defined rigorously and might refer to what we nowadays define as period (the entire cycle) or semi-period (half the cycle). He evidently uses language more freely than physicist nowadays do.

[21] G. Galilei, *Dialogue on the Two Chief World Systems, op. cit.*, pp. 434–435. Italics are mine.

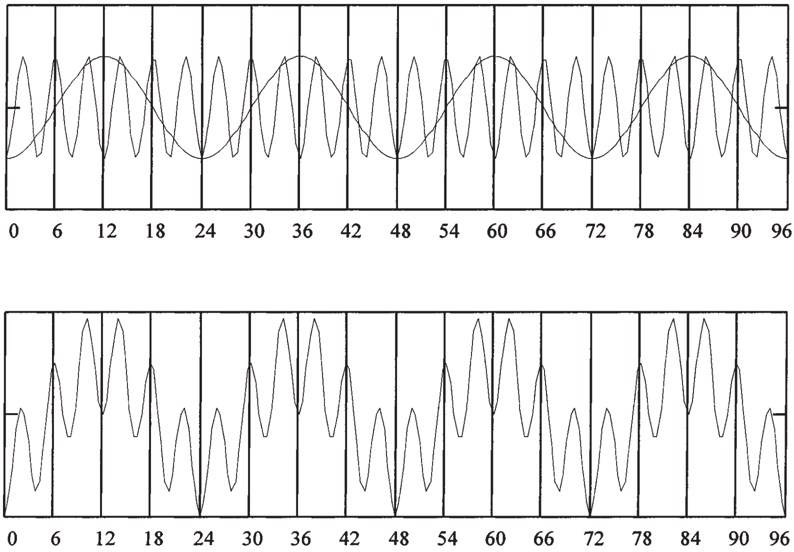

Fig. 3.4. $T = 4$ hours

sum of the responses to the primary and secondary causes – exactly as Galileo claims – as $x_G(t) = \sin(\Omega \cdot t) + \sin(\omega \cdot t)$. We now assign different values to ω, in accordance with Galileo's description. Figure 3.4 (period T = 4 hours), Fig. 3.5 (period T = 8 hours), Fig. 3.6 (period T = 12 hours) and Fig. 3.7 (period T = 18 hours) represent the two distinct waves in the upper part, while their superimposition is plotted in the lower part.The total period displayed is four days (x-axis represents hours).

Figure 3.6 (T = 12 hours) shows that at t = 0 and at t = 12 tide is low, whereas at t = 6 tide is high and at t = 18 tide is high. Although the amplitudes are not uniform – so that the first low is greater than the second, while the two highs are equal to each other, though their absolute value is less than that of the lows – it must be recognised that this is a good explanation why, in the Mediterranean Sea, particularly in the Venetian lagoon, tide periods are of six hours. Galileo claims that although

> there resides in the primary principle no cause of moving the water except from one twelve-hour period to another [. . .], the period of ebbing and flowing nevertheless commonly appears to be from one six hour period to another [. . .]. Such a determination, I say, can in no way come from the primary cause alone. The secondary cause must be introduced for it; that is, the greater or lesser length of the vessel and the greater or lesser depth of the water [. . .]. These causes, although they do not operate to move the water [. . .], are nevertheless the principal factors in limiting the duration of the reciprocations [. . .]. *Six hours, then, is not a more proper or natural period for these reciprocations than any other interval of time, though perhaps it has been the most generally observed*

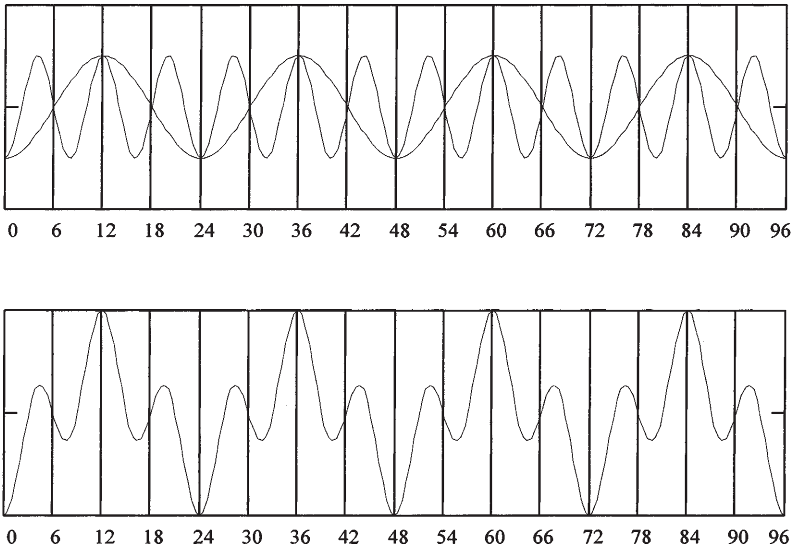

Fig. 3.5. $T = 8$ hours

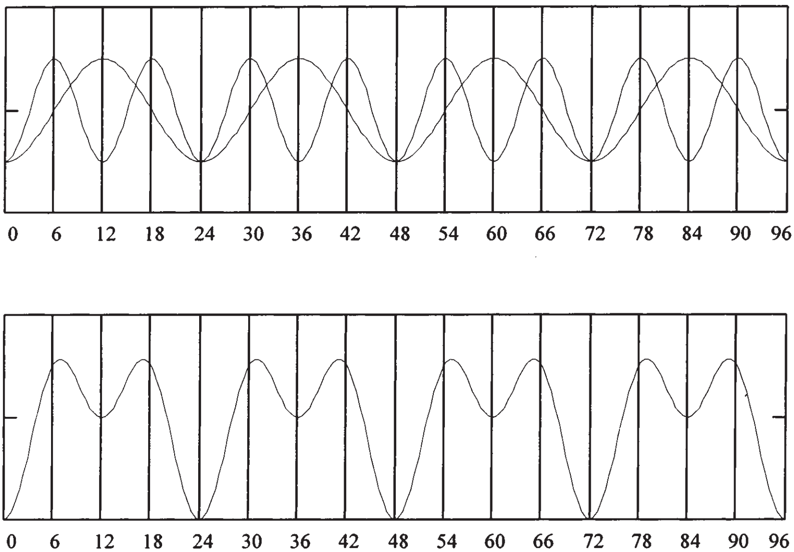

Fig. 3.6. $T = 12$ hours - Mediterranean tides

because it is that of our Mediterranean, which has been the only place practicable for making observations for many centuries.[22]

[22] G. Galilei, *Dialogue on the Two Chief World Systems, op. cit.*, p. 432. Italics are mine.

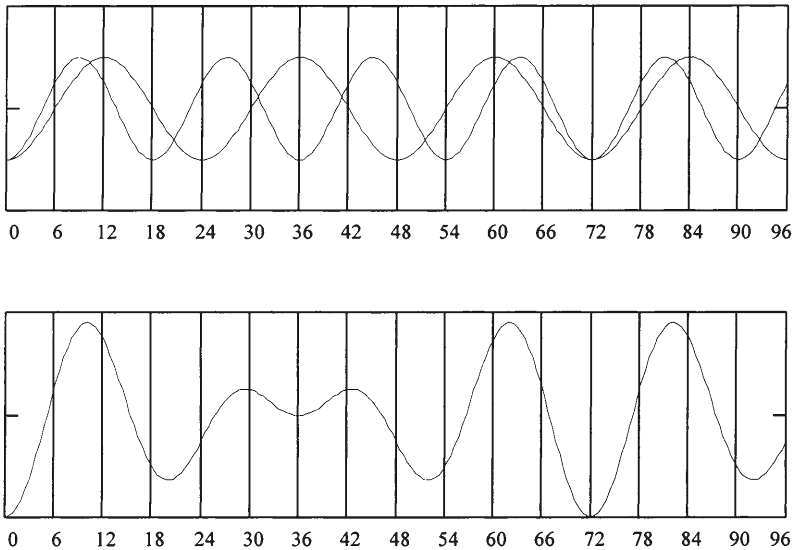

Fig. 3.7. $T = 18$ hours

As we shall see in Sect. 7, in his late years Galileo once again became deeply interested in the phenomenon of tides, and, in order to further his knowledge of the actual daily and monthly variations occurring in the Venetian lagoon, besieged his correspondents in Venice with precise requests for information regarding details about differences between the *absolute high* and *absolute low* in different periods of the year and in different parts of the lagoon. Might all this have somehow been a revival of his preoccupation with the asymmetric nature of the superimposition of waves? Might he have performed new calculations in order to work out numerically the effects predicted by his principle of superimposition?[23]

An oscillatory model is a powerful tool and Galileo intended to take all the advantages he could from it. He tried, therefore, to derive logical consequences from his oscillatory model in order to explain facts. He must have conceived of these consequences as a good test of the predictive power of his model. If his model were able to account for even strange or apparently abnormal phenomena, then it would mean that his tide theory was effectively theoretically more fertile than traditional theories based on the Moon's occult quality of attracting sea water. There were two major consequences that Galileo could not refrain from deducing.

First, if the Moon attracted sea water, then there was no reason why it should not attract them equally in all parts of a sea basin, yet for Galileo there were particular seas such as the Red Sea that did not manifest tides (Galileo was wrong: the Red Sea

[23] My conjecture may be more than a figment of imagination, especially if one takes into account the evidence recently brought to light by D.K. Hill, in his article "Pendulums and Planes: what Galileo didn't publish", *op. cit.*, regarding the huge number of numerical computations Galileo performed in an attempt to test the isochronism law, a methodology that reminds us of the modern numerical analysis carried out by computers.

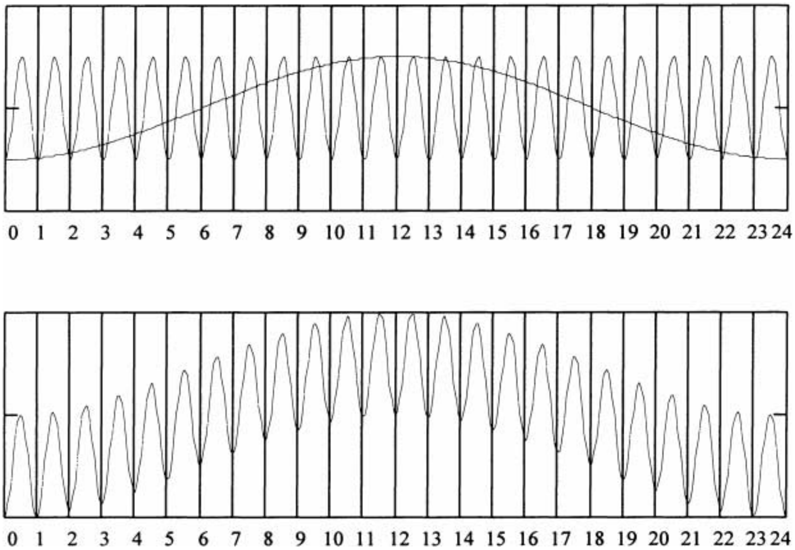

Fig. 3.8. Tides in lakes and ponds

has a tide). Second, it was hard to accept that a supposed occult ability to attract sea water should not exert the same action on the fresh water of small lakes or ponds, yet for Galileo small ponds and lakes did not manifest tides. The oscillatory model had therefore to prove its theoretical superiority by reasonably allowing for these two particular cases. As regards the second point, according to Galileo, it was not hard to understand why in lakes, pools and even small seas there appeared to be no tide:

> There are two impelling reasons for this. One is that because of the shortness of their basins they acquire at different hours of the day varying degrees of speed, but with little difference occurring among their parts; they are uniformly accelerated and retardated as much in front as behind; that is, to the east as to the west. And they acquire such alterations, moreover, little by little, and not through the opposition of a sudden obstacle and hindrance, or a sudden and great acceleration in the movement of the containing vessel. [...]. Consequently the signs of rising and falling or of running to one extremity or the other are exhibited only obscurely. *This effect is also clearly seen in small artificial vessels* [...]. The second reason is the reciprocal oscillation of the water instituted by the impetus already received from the motion of the container, which oscillation [...] makes its vibrations with high frequency in small vessels. [...] Acting contrary to the first cause [tide-generating acceleration], this [the free oscillation] perturbs and removes that without ever allowing it to attain the height, or even the average of its motion. Any evidence of ebbing and flowing is entirely annihilated by this conflict, or is very much obscured.[24]

[24] G. Galilei, *Dialogue on the Two Chief World Systems, op. cit.*, pp. 431–432.

Figure 3.8 (T = 1 hour) is an illustration of Galileo's explanation. The simulation runs for 24 hours. It is not entirely clear whether Galileo only attributed to the much higher frequency of oscillation of smaller lakes and ponds the ability to counter the 'primary cause', or, whether he intended that even vertical displacement should be in consequence less than in great seas and oceans.

And, although Galileo is certainly correct in claiming that higher frequency may result in a sort of confused riddle of rapidly evolving waves vibrating inside the small basin, I nonetheless cannot see how this should prevent a tide, though certainly very tiny, from being appreciable and hence measureable. As Fig. 3.8 shows, an average tide should occur during the day.

As regards the first point, Galileo's explanation, though by way of a foregone conclusion, nonetheless affords an interesting insight into to a very specific phenomenon:

> the Red Sea, although very long, is nevertheless quite devoid of any tide. This is so because its length does not extend from east to west, but runs from the southeast to the northwest. The movement of the earth being from west to east, the impulses of the water are always aimed against the meridian and not from one parallel to another. Hence in seas that extend lengthwise towards the poles and are narrow in the other direction, there is no cause of tides – *unless it is that of sharing those of some other sea with which they may communicate and which is subject to large movements*.[25]

As is clear from the passage, apart from the direct and obvious consequence derived from the fact that tide-generating acceleration mainly acts on the plane of the terrestrial orbit and therefore cannot have any component in the north-south direction, what is important to note is the concluding remark about the possible occurrence of an 'induced tide' in particular seas where tides are normally not expected (as in the Red Sea) owing to a connection between two basins, one of which has a significant ebb and flow. This consequence is a direct by-product of the oscillatory model. Only if water ebbs and flows can it transmit this flux and reflux to another sea's basin through a passage with which the two seas communicate.

On top of this, Galileo added to his model a 'complication principle', which, interestingly, he claimed could not be tested in a laboratory by means of experiments with small artificial vessels, because, in order to compel such unknown phenomena (supposedly due to this surprising principle) to manifest themselves, a more sophisticated 'tide-machine' was required. As we shall see (see Sect. 5 for the history of Galileo's experiments concerning tides), in all probability such a tide-machine was never built or, if it was built, it did not work properly.

Although Galileo was not able to derive further consequences from it, his 'complication principle' deserves attention, insofar as it proves that Galileo's vision of undulatory phenomena – even though bound to remain detached from any attempt to quantify the laws governing these phenomena – was able to take into account all the potential 'degree of predictivity' of his theory. Referring to Fig. 3.9

[25] G. Galilei, *Dialogue on the Two Chief World Systems, op. cit.*, p. 433. Italics are mine.

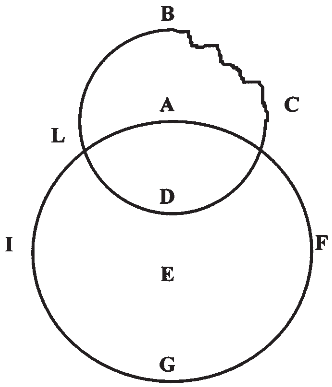

Figure 3.9

> Let us suppose a stretch of sea to be as long as one quadrant; the arc BC, for instance. Then the parts near B are [...] in very swift motion because the two movements (annual and diurnal) are united in the same direction, and the parts near C are at the same time in retarded motion, since they lack the forward movement depending upon the diurnal motion. If we suppose, I say, a sea bottom as long as the arc BC, we shall see at once that its extremities are moving very unequally at a given time. A stretch of sea as long as the semicircle and placed in the position of the arc BCD will have exceedingly different speeds, since the extremity B would be in very rapid motion, D in very slow motion, and the parts in the middle around C in moderate motion.[26]

First of all, this clarifies what in Sect. 1 has been referred to as the 'continuity of action' of tide-generating acceleration. Galileo evidently sees tide-generating acceleration as a continuous function distributed along the Earth's circumference, BLDC. The 'principle of complication' is, therefore, a direct consequence of what has been called in Sect. 1 'woad-grindstone' model. Even though Galileo is not able to derive any theoretical consequences from this principle, and although he evidently must have failed to simulate its possible effects in a laboratory, it is nevertheless of extreme importance from a conceptual, and more specifically, epistemological point of view, since it introduces the spatial element in Galileo's oscillatory model. And this adds to the completeness of this model, in the sense that it acknowledges the potential effectiveness of the spatial distribution with respect to the basin of the exciting cause responsible for setting the pace of the flux and reflux of the sea.

This piece of physics is something more than rhetorical gamesmanship; in spite of its predictive sterility (once again Galileo's physics was striding away and his mathematics was hard put to keep up), it must nonetheless be regarded as another step towards a full comprehension of the nature of wave-like motion, especially with regard to multi-dimensional fluid motion. We have seen that if one wishes to describe the motion of a water particle P, all that is needed is a simple equation for what physicists call a

[26] G. Galilei, *Dialogue on the Two Chief World Systems, op. cit.*, pp. 430.

one-degree-of-freedom system. Yet, this is only a very crude approximation of motion when fluids are involved. If one stops to think for a moment, one quickly realises that waves in rectangular basins and, more generally, on the surface of the sea must be the result of the complex behaviour of a virtually infinite number of particles like P. This is why fluid-dynamicists and oceanographers have come up with the idea of representing certain types of fluid motions (the so-called stationary waves) by means of two-facet functions, which demonstrate the influence of temporal variables and spatial variables. In other words, they have learned to represent fluid motions like the Galilean wave in a sea basin as long as a quadrant (a fourth of the Earth's circumference), in a manner that originally is wholly Galileo's. Let Ω be the angular speed of diurnal rotation and y a spatial coordinate along the Earth's circumference. If R is the Earth's radius, then the Galilean spatial-temporal tide-generating acceleration distribution along the Earth's circumference, $A_{\Omega Y}$ (t,y), can be represented by

$$A_{\Omega Y}(t,y) = A_0 \cdot \sin(\Omega \cdot t) \sin\left(\frac{2 \cdot \pi}{R} \cdot y\right) \quad (5)$$

namely, as the product of two sine functions, the first giving the *diurnal temporal periodicity* and the second the *spatial periodicity* (A_0 being a constant value).

The 'principle of complication' – given by relation (5) – was unable to generate further insight into the nature of fluid motions within basins, so that Galileo had to accept this principle as a simple 'cautionary' guiding rule, something which had to be taken into account as a probable cause able to both 'moderate' and 'complicate' the much simpler effects predicted by his tide theory. And having failed to test the effects of such a 'marvellous' cause of 'commotion' in his laboratory he simply warned:

> What must we suppose would happen in a vessel so remarkably situated [see Fig. 3.9] that a retardation and an acceleration of motion are conferred very unevenly upon its parts? Certainly we cannot help saying that there would necessarily be perceived still greater and more marvellous causes of commotion in the water, and stranger ones.[27]

4. The 'warping' of history

4.1 Galileo's claim: tides prove Copernicus

> After having many times examined for myself the effects and events, partly seen and partly heard from other people, which are observed in the movements of the water; after, moreover, having read and listened to the great follies which many people have put forth as causes for these events, I have arrived at two conclusions which were not lightly to be drawn and granted. Certain necessary assumptions having been made, these are that if the terrestrial globe were immovable, the ebb and flow of the oceans could not occur naturally; and that *when we confer upon the globe the movements just assigned to it, the seas are necessarily*

[27] *Ibid*.

subjected to an ebb and flow agreeing in all respects with what is to be observed in them.[1]

Although Galileo's second conclusion may sound extravagant in boldly claiming to agree with the whole of the effects to be observed in tides, the first part of it – i.e. if we impart to Earth the double motion assigned to it by Copernicus, then an ebb and flow will necessarily follow – is the most perspicuous formulation to be found in his texts of the problem of the dependence of the flux and reflux of the sea on the Earth's motions in space. In this Section we shall be concerned with the fascinating balance of complexity and simplicity inherent in this mind-challenging problem.

Warps – also called space or time warps – are imaginary deformations in space-time that allow science-fiction characters to travel in the universe, in blissful disregard of the known laws of physics. Now, unlike science fiction writers, historians are not free to alter the 'space-time of history' and must be aware that their 'up-to-date perspective' posits a threat to the impartial interpretation of historical facts. Yet sometimes the sheer genius of their real characters requires them to shift their ground. Galileo's lack of an 'appropriate mathematical language' in which he might adequately formulate the whole problem of the dependence of tidal motions on Earth's motions demands that we do not stubbornly insist on just the one point of view, but that we take full advantage of a few developments in classical mechanics that have occurred since Newton wrote the *Principia*. These developments constitute the minimum language powerful enough to describe complex kinematic models such as those Galileo proposed.

It is my conviction that we have to take the calculated risk of 'warping history' in order to talk in an appropriate language about the kinematic consequences of the Galilean woad-grindstone and so tease out its secrets. In other words, we have to recognise that, in tackling the issue of the diurnal and, as we shall see, monthly periodicity of the tides, Galileo pitted his wits against the limitations of his mathematical resources to cope with the complexity of the relationship between tide phenomena and Earth's motions. Thus, we have to escape the framework of his geometrical methods in order to find an answer to this apparently simple question: did Galileo succeed – and, if so, to what extent – in furnishing a physical proof of the Copernican double motion of the Earth, or was his claim no more than a vain and mind-boggling pretension to such a proof?[2]

Galileo contributed a vital 'piece of physics without the necessary mathematics' to this formidable problem. If formulated mathematically, this 'piece of physics' affords

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[1] G. Galilei, *Dialogue Concerning the Two Chief World Systems*, *op. cit.*, p. 417. The italics are mine.

[2] Even historians who claim that Galileo's pretension to an explanation of the tides based on the Earth's motions is patently unacceptable, implicitly recognise that we must use a more advanced language to counter his arguments. As we have seen, a prime example is E. Mach's interpretation, which inaugurated a tendency followed by many a historian, who have spoken of 'frames of reference', 'inertial systems', 'mixing of two different frames of reference', 'principle of relativity', 'inertia' and so forth. Evidence of this attitude is also implicit in the widely accepted, though biased, assumption that Newton's was the correct and natural standard of comparison to which Galileo's theory had to be referred. See the discussion on tide-generating acceleration in *Sect. 1*.

a term indispensable to tide equations. A second term present in these equations – and which stems from the mathematically simple form of Newton's law of universal gravitation (the inverse square law) and is made visible only by means of a mathematical 'trick' (a power series expansion[3] of the attraction force between bodies) – surprisingly cancels out the former term. Thus, this second term, the so-called Newtonian tide-generating force, prevents the former term – which we will call the 'quasi-Galilean term' so as to distinguish Galileo's contribution from a Newtonian superstructure he would not have recognised as his own – from being recognised as fully responsible for generating a tide effect. The aim of this Section is to show how to'disassemble' the whole set of tide equations of motion in order to isolate each individual term forming a part thereof and so weigh up its particular purely physical meaning.

It was not until the mid 1950s that historians first accepted the need to risk 'warping history', thereby sparking off an interesting debate that furthered the state of knowledge of the tide-motion-of-the-Earth affair, at least as regards its historical development from Galileo's tide theory in the seventeenth century to the first half of the eighteenth century. At that time, three contributions to tide theory were published that made significant breakthroughs with respect to Newton's theory. The works were submitted to the Paris Academy of Sciences, which in 1738 had proposed the problem of tides as the subject of a competition, and were awarded the prize two years later, in 1740.[4] Although the historiographical debate represented the most technically-sophisticated attempt by

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[3] For our purposes, a 'power series expansion' of a function F(x) may be thought of as being no more than a very useful tool to manipulate F(x). This tool allows us to re-write F(x) as $F(x) = a_0 + a_1 \cdot x + a_2 \cdot x^2 + a_3 \cdot x^3 + \ldots$ where a_0, a_1, a_2, a_3, are numeric coefficients that depend on F(x) and its derivatives. This expansion may be used to highlight which powers of x are actually contained in F(x). For example, a common function like sin(x) may be 'expanded' into the infinite series $\sin(x) = x - \frac{1}{3\cdot 2} \cdot x^3 + \frac{1}{5\cdot 4\cdot 3\cdot 2} \cdot x^5 + \ldots$, which shows that only odd powers of x are present 'inside' sin(x). A remarkably extensive book, in which a summary of elementary and advanced methods of the calculus and of mathematics applied to physics is given, is L.A. Pipes, L.R. Harvill, *Applied Mathematics for Engineers and Physicists*, 3rd ed., (1st ed., 1946), New-York, McGraw-Hill Book Company, 1971. See pp. 819ff.

[4] The authors were Colin McLaurin, Daniel Bernoulli and Leonard Euler (a fourth paper by Antoine Cavalleri shared the award, but it was based on Cartesian physics so that it is of little interest to our story, which is concerned with Newtonian tradition).The first three contributions marked substantial theoretical progress with respect to Newton's *Principia*, progress that must have been perceived as such by contemporary scholars, too. For example, T. Le Seur and F. Jaquier, editors of a monumental edition with commentary of Newton's *Principia*, at the end of the first part of the third volume, decided to re-publish the three papers by Bernoulli, McLaurin and Euler, which were based on Newton's universal gravitation law (the work by Cavalleri was based on Descartes's physics of vortexes). In a brief note in volume three on p. 132, they point out that after first having thought to abridge the papers and re-cast them in their notes, they had second thoughts and recognised that these dissertations were so important as to deserve to be re-printed unabridged. See I. Newton, *Philosophiae Naturalis Principia Mathematica auctore Isaaco Newtone, perpetuis commentarii illustrata, communi studio PP. Thomae Le Seur et Francisci Jaquier Ex Gallicana Minimorum Familia, Matheseos Professorum,* 3 volumes, Genevae, Typis Barrillot & Filii Bibliop. & Typogr., 1739–1742. See also E.J. Aiton, "The Contribution of Newton, Bernoulli and Euler to the Theory of the Tides", *Annals of Science*, XI, 1955, pp. 206–223.

historians to study Galileo's diurnal periodicity model, it failed to settle the question of his pretension to a physical proof of Copernican astronomy, foundering instead on the apparently insoluble dilemma of the Earth's double motion effect on tides – which H.L. Burstyn and V. Nobile claimed existed and adequately represented Galileo's theory, whereas E.J. Aiton denied its influence altogether. Nonetheless, these pioneering articles placed on a sounder footing historical research into this notorious predicament.[5] Since then, virtually nothing has been contributed to the discussion.[6] As will become clear,

[5] In his article "Galileo's Attempt to Prove that the Earth Moves", *Isis*, LIII,1962, pp. 161–185, H.L. Burstyn draws a felicitous analogy between the Earth's double motion as it is conceived of by Galileo and a revolving merry-go-round that carries a 'daily rotating turntable' representing Earth. The merry-go-round model amounts to what we have defined in *Section 1* as a woad-grindstone model of the Earth's double motion (see *Section 1*, Figure 1). Burstyn's image may be more familiar to us moderns than Galileo's woad-grindstone, but the merry-go-round was to form the crux of a dispute between H.L. Burstyn and E.J. Aiton as to whether Galileo's tide theory succeeded in furnishing a proof of the Earth's double motion and whether, even in the broader context of Newtonian physics, tides are able to furnish the very proof long sought by Galileo (see E.J. Aiton, "On Galileo and the Earth-Moon System", *Isis*, LIV, 1963, pp. 265–266 and K.L. Burstyn, "Galileo and the Earth-Moon System: Reply to Dr. Aiton", *ibid*., pp. 400–401. A second round was played two years later: E.J. Aiton, "Galileo and the Theory of Tides, *Isis*, LVI, 1965, pp. 56–61 and Reply By L. Burstyn, *ibid*., pp. 61–63). Each author recognised implicitly that the woad-grindstone required historians to apply mathematical analysis in order to dissect a problem whose complexity cannot be handled adequately by means of common language and verbal descriptions. Nevertheless, the first to state the need for a fresh start in studying Galileo's claim was an Italian scholar, V. Nobile, in his article "Sull' argomento galileiano della quarta giornata dei 'Dialoghi' e sue attinenze col problema fondamentale della Geodesia", *Rendiconti della Accademia Nazionale dei Lincei, Classe di Scienze Fisiche, Matematiche e Naturali*, XVI, 1954, pp. 426–433. This article concluded a four-part series devoted to the broader theme of the conflict between Aristotelians and Copernicans; see V. Nobile, "Il Conflitto tra copernicisti e aristotelici nella sua essenza e nel pensiero di Galileo. Part I", *Rendiconti della Accademia Nazionale dei Lincei, Classe di Scienze Fisiche, Matematiche e Naturali*, IX, 1950, pp. 299–306, V. Nobile, "Il Conflitto tra copernicisti e aristotelici nella sua essenza e nel pensiero di Galileo. Part II", *ibid*., X, 1951, pp. 337–343 and V. Nobile, "Il Conflitto tra copernicisti e aristotelici nella sua essenza e nel pensiero di Galileo. Part III", *ibid*., XI, 1951, pp. 311–319. Nobile's analysis of Galileo's woad-grindstone turns out to be 'technically' equivalent to Burstyn's merry-go-round model. The latest attempt to approach Galileo's tide theory from a 'technical' point of view is M.Cimino, *Il 'Dialogo sui due massimi sistemi' di Galileo Galilei ed il 'De Revolutionibus orbium coelestium' di Niccolò Copernico: un diretto confronto*, in *Giornate lincee indette in occasione del* 350° *anniversario della pubblicazione del 'Dialogo sopra i massimi sistemi' di Galileo Galilei*, Roma, Accademia Nazionale dei Lincei, 1983, pp. 21–54. Nonetheless, the author does not tackle the question of the dependence of the tides on the Earth's double motion.

[6] In order to test Galileo's diurnal periodicity, S. Drake, in an article in 1961, proposed a qualitative thought experiment that – as far as I am able to interpret it – can be seen as equivalent to Burstyn's merry-go-round. See S. Drake, "Galileo Gleanings – X. Origin and Fate of Galileo's theory of Tides", *Physis*, III, 1961, pp. 185–193 and S. Drake, *Galileo's Studies, op. cit.*, pp. 206–207, where the author clarifies his thought experiment with a few drawings. Although the author does not discuss the 'mathematical' analysis of Galileo's model, which had been produced in the 1950s, in a later contribution in 1983 he quotes V. Nobile's article of 1954 and puts forward an

the main reason why the discussion between the upholders of a possible Galilean tide effect and their lone opponent came to a standstill is that, instead of taking into account a complete set of tide equations, they considered only separate terms: the former, the tide-generating term stemming from their purely kinematic analysis of Galileo's woad-grindstone; the latter, the gravitational term stemming from his focusing mainly on the attractive force acting upon bodies in the universe. We shall see, however, that the key to the understanding of the whole problem is the 'mix' of these terms – and the way this 'mix' works depends on the fact that, on a travelling Earth, motion cannot be described as if it occurred on a stationary Earth.

Insofar as part of the evidence that allows us to credit Galileo with the 'piece of physics' essential to the tide equations is based on his insight into what we call 'non-inertial nature of circular motion', we shall also have to cast a glance at Galileo's ideas about the motion of bodies around a centre. In other words, in order to 'lend' him the voice of the mathematics he lacked, and to avoid overestimating the importance of his technical handicap, we have to test the soundness of his physical principles regarding motion along a circle.[7] My investigations have led me to the following conclusions:

a) Galileo knew enough about the non-inertial nature of circular motion for us to be able to credit him with the knowledge of the 'piece of physics' (which he was not able to describe mathematically) that expresses the interaction between circular motion and the effects generated by its accelerated nature. This point must be kept separate from the historiographical dispute over the interpretation of the Galilean concept of inertia, i.e. whether or not he attained to a full understanding of the inertia principle;

b) a quasi-Galilean tide-generating acceleration enters into the tide equations of motion – it being the very term that describes the contribution of the Earth's annual motion to the total tide-generating force;

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analogy to show that the double circular motion of devices commonly found in entertainment parks – such as a 'seat that rotates about its centre, while being carried around a central column at the end of a long beam' – manifests the Galilean effect. See S. Drake, *The Organising Theme of the Dialogue*, in *Giornate lincee indette in occasione del* 350° *anniversario della pubblicazione del 'Dialogo sopra i massimi sistemi' di Galileo Galilei*, *op. cit.*, pp. 101–114. The quotation is from p. 112.

[7] As is well known, circular motion is not inertial motion (in Newton's classical mechanics 'inertial motion' is uniform straight motion), i.e. it cannot continue without the action of external forces that 'bend' the moving body along its circular pathway. Whether Galileo achieved a full understanding of the principle of inertia as we know it is open to question. It is beyond the scope of this inquiry to enter into this debate – which in any event I regard as rather abstract, given that it tells us very little about what Galileo was actually able to 'demonstrate' regarding the properties of circular motion. One may have achieved a general understanding of uniform straight motion and yet be unable to say anything either about what causes the 'forces of extrusion' that manifest themselves on rotating wheels or slings to vary with wheel diameter and rotation speed or even about what prevents earthly objects from being thrown up into the air by Earth's diurnal rotation. What tells us a great deal more about Galileo's knowledge of how inertia 'works', is precisely how he managed to solve a few extremely interesting problems on motion around a centre, precisely in wheels and slings.

c) the annual motion of the Earth must be taken into account in formulating the equations that govern tidal motions, because the Earth's basins are not attached to an inertial frame of reference[8];

d) the specific relative-positional form[9] of the law of universal gravitation is responsible for the cancellation of the Earth's annual motion contribution embodied in the quasi-Galilean term;

e) tides might even be considered a perfectly adequate proof of the Copernican double motion of the Earth, in accordance with the hypothesis that universal gravitation is not a relative-positional force, but an absolute force (i.e. depending on absolute motion). And, to do justice to the scientist's endeavour, one has to go so far as to study what would happen in a different Newtonian universe[10] – it being assumed that a Newtonian universe is a universe where the three laws of motions are valid – if the force of universal gravitation had a different form from what we have learned from physics textbooks.

As we shall see, in our universe tides do occur on Earth regardless of the Earth's motions, because of the very 'simplicity' of the relative-positional nature of the law of universal gravitation. But if we 'warped' the Newtonian universe in such a way as to allow for a 'gravitational deformation' that would turn gravity into a force depending on absolute parameters,[11] we would observe that tides on a travelling Earth were different from those occurring on a stationary Earth. Hence, to all intents and purposes tide phenomena could be seen as a physical proof that the Earth orbits the Sun. This 'warped gravitation' acting on sea waters would still underpin a correct theory of tidal ebb and flow, even though, contrary to what occurs in our 'normal' universe, the contribution of the Earth's double motion would not be deleted by the elegant simplicity of 'normal' gravitation. In this 'warped universe' a quasi-Galilean tide-generating acceleration would

[8] An 'inertial frame of reference' is a frame of reference in which the second of Newton's laws of motion – which in its simplest form for a point of mass m can be expressed as $m \cdot \vec{a} = \vec{F}$, where $\vec{a}$ is the point's acceleration and $\vec{F}$ the sum of the forces acting upon it – holds true. It is also sometimes called a frame of reference 'attached' to the fixed stars. In any other frame of reference, other terms expressing the forces due to the motion itself of the frame of reference must be introduced on the right hand-side of the previous equation. In this case, the equations of motion become much more complicated.

[9] Generally speaking, a force is called 'positional' if the function that expresses its law depends on distances, but not on velocities. In other words, gravitation is said to be positional because the force between two bodies placed at distance $\vec{r}$ is given by a function of form $\vec{F}(\vec{r})$, whereas a non-positional force would be given by a function of form $\vec{F}\left(\vec{r}, \frac{d\vec{r}}{dt}\right)$, where the derivative of vector $\vec{r}$ would be present too. In the case of gravity, only distances between bodies relative to each other are involved, hence its 'relative' nature.

[10] It must be stressed that the laws of motion are quite independent of the form of the law of universal gravitation. In other words, the motion laws do not imply any particular law of gravitation.

[11] For example, a two-term function of form $\vec{F}\left(\vec{r}, \frac{d\vec{r}}{dt}\right) \cdot G\left(\vec{R}, \frac{d\vec{R}}{dt}\right)$, where the universal gravitation constant G has been turned into the scalar function G(...), which depends on absolute distance $\vec{R}$, as well as on absolute speed $\frac{d\vec{R}}{dt}$, so as to turn the relative-positional nature of gravitation into an 'absolute' nature.

therefore remain part of the equations as a necessary component of a more general tide-generating force.

In this sense, the Galilean tide model – though not yet able to furnish correct quantitative results, insofar as it does not take into account the pull of gravitation – would nonetheless afford proof of the Copernican double motion of the Earth. And, viewed in this light, it must be recognised that what finally prevented Galileo from achieving his grand objective of finding the proof of Copernican astronomy in the great natural laboratory of the Earth's oceans turns out to have been not so much a theoretical 'error' as a mere physical coincidence.

4.2 Galileo's notions on bodies that move around a centre

The purpose of this section is to prove that Galileo had a clear understanding of the existence of a complex relationship between the kinematic variables of circular motion, i.e. tangential speed, radius of the circular trajectory and angular velocity, and the dynamic effect due this peculiar kind of motion, which, in our language, is centrifugal force. Contrary to what has generally been accepted by science historians, Galileo's notions on circular motion, far from being no more than a fuzzy set of ideas, deserve their place in the front ranks of his achievements. On this ground we can attribute to the Pisan scientist a sufficient comprehension of the non-inertial nature of circular motion and substantiate the thesis expressed by the above point a).

One of the chief objections to the notion of the Earth's diurnal rotation was what Galileo called 'Ptolemy's argument', according to which, if the Earth were truly spinning around, then buildings, trees and all heavy bodies standing on its surface would be thrown up into the air and our very breath would be snatched from our lips. Galileo tackles this issue in the Second Day of the *Dialogue Concerning the Two Chief World Systems* and apparently tries to demonstrate the 'impossible', namely, that heavy bodies on the Earth's surface would never be ejected by Earth's diurnal rotation – regardless of the rotational speed ! In referring to a body, even as light as a feather, resting on the surface of the spinning Earth – and assuming that said body has a tendency to move downwards (whatever the reason for this may be) – Galileo asserts that

> there is no danger, however fast the whirling and however slow the downward motion, that the feather (or even something lighter) will begin to rise up. For the tendency downward always exceeds the speed of projection.[12]

And this holds true in spite of the fact that he knows perfectly well that

> [. . .] heavy bodies, whirled quickly around a fixed centre, acquire an impetus to move away from that centre even when they have a natural tendency to go toward it. Tie one end of a cord to a bottle containing water and, holding the other end firmly in your hand (making your arm and the cord the radius, and your shoulder knot the centre), cause the vessel to go around swiftly so that it

[12] G. Galilei, *Dialogue Concerning the Two Chief World Systems, op. cit.*, p. 197.

> describes the circumference of a circle. Whether this is parallel to the horizon, or vertical or slanted in any other way, the water will not spill out of the bottle in any event; rather, he who swings it will always feel the cord pull forcibly to get farther away from his shoulder. And if a hole is made in the bottom of the bottle, the water will be seen to spurt forth no less toward the sky than laterally or toward the ground.[13]

Nowadays we know that Earth's diurnal rotation does not wreak havoc on the Earth's surface because the rotational speed is too low, though most of us probably would agree that if the speed were great enough, then some significant and possibly catastrophic effect might well occur and that therefore Galileo must have gone quite wrong. In fact, as we shall see, what apparently should occur does not occur at all, nor was Galileo so naïve as to believe that whatever the rate of spin might be, 'extrusion' would never happen as long as heavy bodies have a tendency downwards (we would simply say as long as heavy bodies are heavy, i.e. are affected by gravity).

Well, clear as it may appear, the case of the spinning bottle is not the point at issue. For, the analogy of the spinning bottle does not adequately reflect the rotation of the Earth. The point is: what would happen if Earth's diurnal rotation were progressively increased so much as to reach a significant value, significant enough to make the centrifugal force comparable with the force of gravity? The answer is not as straightforward as it might seem to be at first sight. And, as Galileo himself well knew, what is reported in the *Dialogue* in relation to water spurting from swinging bottles – a report which rhetorically reinforces the opponent's argument before the rebuttal of the same – would not happen. Galileo knew a great deal about the difference between wheels, bottles and the rotating Earth, even though it was not enough to enable him to 'tame' the disconcerting effects due to centrifugal forces. As a prerequisite to the discussion of Galileo's arguments against Ptolemy's objection – arguments that are based on his claim to have rid himself of the whole issue of 'extrusion' effects manifested by spinning bodies, and which are of paramount importance because they embody the bulk of his knowledge of the physics of circular motion – we first need to review what would actually happen if the Earth were made to rotate at an increasing speed until it reaches the point where effects on bodies standing on its surface would be detectable.

Let us imagine a heavy body P standing on the Earth's surface. With respect to a frame of reference attached to the Earth and rotating with it, the body is at rest. In other words, it does not accelerate with respect to the frame of reference that has been chosen. Now, this roughly means that the sum of all forces acting upon the body must be equal to zero. To simplify things, let us consider a simple model in which the Earth is represented by a rotating circle on a plane (we are not interested in three-dimensional effects). Let m be the body's mass, ω the Earth's angular velocity, g gravity acceleration at the Earth's surface, R the Earth's radius and Φ the reacting force acting upon P on the Earth's surface. Assuming the positive direction to be oriented towards the sky, it follows that for a body standing on the circle's rim,

$$\Phi - \mathrm{m} \cdot \mathrm{g} + \mathrm{m} \cdot \omega^2 \cdot \mathrm{R} = 0$$

[13] *Ibid.*, p. 190.

where, as is well known, $(m \cdot \omega^2 \cdot R)$ is the centrifugal force acting on P due to the Earth's rotation. Now, imagine increasing ω. When $m \cdot g = m \cdot \omega^2 \cdot R$, i.e. when $\omega = \sqrt{\frac{g}{R}}$, then $\Phi = 0$, which means that P has 'lost' its whole weight (remember that a balance attached to the Earth measures Φ and not simply $m \cdot g$). This is not because the weight has disappeared – in fact it is obviously still there – but because the centrifugal force balances the force of gravity. And, if one looks at this simplified Earth-P system from space, P continues to travel along its circular trajectory totally undisturbed, remaining at rest with respect to the Earth: both go around the centre of motion with a rotational speed of $\omega = \sqrt{\frac{g}{R}}$. At the same time, if one looks at the Earth-P system from Earth, no 'extrusion' occurs. Why not? Well, simply because the pull of gravity is still there and prevents P from doing anything other than circulating along its curved pathway. In other words, it is gravity that makes P remain at rest on the Earth's surface.

What is the difference with respect to wheels, full bottles and slings? The answer is obvious. If P were attached to the Earth by means of some sort of mechanical constraint locally acting between P and the Earth's surface, such as a cord or glue, things would be totally different, because once the constraint has been destroyed by the centrifugal force, it would cease to exert its action on P, which would therefore continue its journey through space in a straight line tangential to the circle at the very point where P lost its link and at the same speed it was doing at that very moment. The fact is that gravity does not act on P by means of mechanical constraints, so that it never ceases to act. This is why, even if the Earth were rotating at speed $\omega = \sqrt{\frac{g}{R}}$, nothing similar to what is predicted by Ptolemy's argument would follow.

Now, what we learn about circular motion from the previous analysis is at the very least, that our intuition may easily be led astray by the powerful influence of reasoning based on analogies with oversimplified terrestrial mechanical systems. Moreover, it should be clear from the analysis that studying the effects that would occur in the 'weightless' situation has little to do with an in-depth knowledge of the Newtonian principle of inertia. I have not used this principle in my foregoing discussion, and even if I had not known anything about the mathematical form of centrifugal forces, I would somehow have come to the same conclusion without actually formulating the 'weightless' condition $\omega = \sqrt{\frac{g}{R}}$; what I have actually used is the basic idea of equilibrium of forces. It is true that I have asserted that P would continue along a straight line at the same speed at which it was travelling at the moment of separation. But this does not necessarily mean that P would travel forever. We shall see that Galileo was fully aware of this important consequence – i.e. that P would continue along a straight line – and, what is more, recognised its crucial significance in relation to circular motion around a centre.

My thesis is that what Galileo was actually trying to do while studying the nature of motion around a centre was not so much to investigate in abstract terms the nature of inertial motion – he was evidently not interested in arriving at a theoretical formulation of the principle of inertia as we find it expressed in Newton's first law of motion, which is tantamount to stating that perennial uniform motion in a straight line is not distinguishable from rest. No, given that he knew perfectly well where bodies 'thrown' by rotating devices tend to go,[14] I would maintain that what he was after was basically this: how to work out the dependance the mechanical behaviour of circular motion on the geometric parameters that characterise it. This, particularly in relation to the effects of centrifugal forces on heavy objects attached to spinning devices. And his major objective was to show that a 'tendency centrewards' however small (or gravity itself, if indeed he regarded gravity as the cause of this tendency – a question we are not concerned with here), is all that is needed to prevent bodies from being 'extruded', whether in slings or wheels, from the surface of a rotating Earth, which would have countered Ptolemy's argument for good and all.

Of course, he was asking too much, and went wrong. To maintain a body in circular motion it requires not an infinitesimal (i.e. however small) quantity of 'centreward tendency' but exactly one, and only one, value of this quantity. In the example above, P cannot keep pace with the spinning Earth, unless ω has the exact value given by $\omega = \sqrt{\frac{g}{R}}$. Now, in his first theorem on circular motion (as will be evident in a moment) Galileo astonishingly stumbled upon the exact description of the 'extreme condition' expressed in modern language by $\omega = \sqrt{\frac{g}{R}}$, even though he did not recognise it as a very special case. Quite the contrary, he interpreted his demonstration as a geometric proof that 'however fast the whirling and however slow the downward motion' a body like a feather (or even something lighter) would not begin to rise up. This first theorem amounts to what I would like to define as the 'equilibrium solution' theorem (which Galileo expresses by means of his geometric and kinematic language). Nevertheless, we shall see that he also had a clear understanding of the dynamic effects generated by circular movements, namely, of the effects we nowadays call 'centrifugal forces' and, what is more impressive, a clear awareness of the necessity to link these effects in some

[14] Although it is beyond the scope of this paper to enter into the debate on Galileo's principle of inertia, A. Koyré's famous *Ètudes Galiléennes* must be recalled here, insofar as Galileo's theorems on circular motions used in the *Dialogue* to counter Ptolemy's argument are concerned. I find myself at variance with the analysis proposed by A. Koyré, which may be summarised in his judgement of Galileo's attempt to prove that no extrusion can occur at the Earth's surface. According to Koyré, the error "que commet Galilée n'est pas une simple inadvertance. Il sait fort bien que le mouvement rapide de la roue (ou de la fronde) peut rompre le lien qui y attache la pierre.[...] S'il n'admet pas cette possibilité pour le cas de la rotation terrestre, et ne remarque même pas la contradiction qui il comet ainsi (à nos yeux elle est flagrante), c'est que, pour lui, la force naturelle de la pesanteur qui attire – ou qui pousse – les graves vers le centre de la terre, ne peut pasêtre mise sur le même plan que l' action extérieure – adventice, violente – d' un lien attachant une pierre à une roue". And in Koyré's view, this is why Galileo failed to attain to full understanding of the law of inertia. My quotation is from A. Koyré, *Ètudes Galiléennes, op. cit.*, volume III, p. 259. The third volume has the significant subtitle *Galilée et la loi d'inertie*. My point is that Galileo did not distinguish between terrestrial devices like wheels or slings and the rotating Earth. On the contrary, such a distinction would not have been coherent with his 'grand' plan to test Copernican astronomy in a laboratory and to prove the Earth's double motion by means of terrestrial phenomena like tides. And his ambition to rule out once and for all the effects due to Earth's rotation originated in his only partial understanding of the kinematic parameters that affect centrifugal force in circular motion. See discussion below.

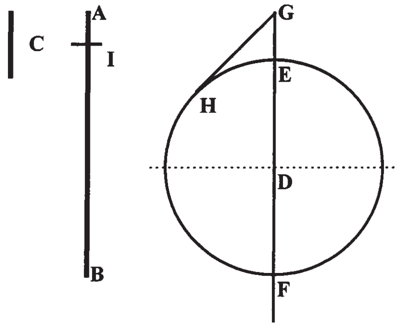

Figure 4.1

way with the geometric parameters of circular motion. Let us begin with the 'equilibrium solution' theorem.

The first step towards understanding the mechanics of circular motion is to realise that when a body is thrown off by a spinning device, then

> the projectile acquires an impetus to move along the tangent to the arc described by the motion of the projectile at the point of its separation from the thing projecting it.[15]

Referring to Fig. 4.1, let C be to AB as the downward tendency is to the speed of projection, i.e. let the ratio $\frac{C}{AB}$ be arbitrarily small. Galileo demonstrates that a circle exists whose centre is D and whose diameter is FE, such that tangent HG is to secant GE as C is to AB, namely, such that a body P coinciding with point H has tangential speed to downward speed as HG is to GE. Under these circumstances, projection cannot ensue, because P moves from H to G by virtue of its tangential speed, while it moves from G to E by virtue of its downward tendency.[16] The proof is perfectly correct, but the conclusion is awkward. What Galileo loses sight of is the fact that the circle he builds

[15] G. Galilei, *Dialogue Concerning the Two Chief World Systems*, *op. cit.*, p. 193. Galileo does not furnish any mechanical or geometric proof of this basic fact. Rather, the dialogue by which Galileo's spokesman Salviati leads the Aristotelian Simplicio to agree that this is necessarily true is claimed by Simplicio to resemble Plato's opinion that 'nostrum scire sit quoddam reminisci', which Salviati comments on ambiguously by saying that 'how I feel about Plato's opinion I can indicate to you by means of words and also by deeds. In my previous arguments I have more than once explained myself with deeds. I shall pursue the same method in the matter at hand...' (*ibid*., p. 191). A few lines below, when Simplicio – thinking about Salviati's question 'When the stone escapes from the stick, what is its motion?' – runs out of time, Salviati adds this ironic remark 'Listen to that, Sagredo; here is the *quoddam reminisci* in action, sure enough' (*ibid*.). In the *Dialogue*, Sagredo, the third actor, represents the learned layman, a sort of freethinker ready to absorb new knowledge, not committed to any philsophical school. The Venetian Giovanfrancesco Sagredo was an amateur of science who studied privately under Galileo at Padua.

[16] G. Galilei, *Dialogue Concerning the Two Chief World Systems*, *op. cit.*, p. 198. The strategy employed in Galileo's demonstration is to look for the third proportional AI with respect to BA and C and then to make the circle's diameter to EG as the ratio $\frac{BI}{IA}$. The rest of the demonstration is quite straightforward.

is not arbitrary at all. In other words, there cannot exist an arbitrary circle – i.e. whose radius R is arbitrary – having the property that on its circumference, given the arbitrary ratio $\frac{C}{AB}$, then $\frac{C}{AB} = \frac{\text{downward motion}}{\text{projection motion}}$. If the ratio $\frac{C}{AB} = \frac{\text{downward motion}}{\text{projection motion}}$ is given, then the circle's radius is given too, so that one and only one circle can exist for a given ratio $\frac{C}{AB} = \frac{\text{downward motion}}{\text{projection motion}}$. Galileo is able to find this circle in a correct way, though he is probably led astray by the intrinsic characteristics of his mathematical language, which is totally based on the geometric theory of proportions. Galileo stumbles upon the 'equilibrium solution' without realising that such a solution is no more than a special case.

To see why there cannot exist more than one circle that may satisfy the geometric conditions of Galileo's theorem, let R = HD, A = HG, B = GE. If we apply Pythagoras' theorem to right-angled triangle DHG it follows that

$$R = \frac{A^2 - B^2}{2 \cdot B} \tag{1}$$

In other words, R is not independent of A, B, which means that only one circle has the property of representing the 'equilibrium solution'. It is not difficult to understand now why Galileo might have overlooked the fact that only one circle satisfies his geometrical construction. If one follows verbatim Galileo's demonstration – which is what is called by mathematicians a 'constructive' demonstration, precisely because it leads to the actual construction of the mathematical object that is being sought – and starts with plotting on a piece of paper C and AB with arbitrary lengths, one can easily get the false impression that this method leads necessarily to the construction of a circle independent of C and AB.[17]

Let us now show that Galileo's 'equilibrium solution' amounts to the same extreme situation where $\omega = \sqrt{\frac{g}{R}}$. In so doing, we shall also be able to throw light on a very interesting aspect of the whole Galilean conception of circular motion that might help us to further our understanding of his 'geometric physics' and explain why, in the final analysis, he needed to prove the 'impossible', that is, however 'slow the downward motion', the 'extrusion' effect due to the swift swirling of the 'projector' would never be able to throw the projectile into space.

According to the hypothesis that $\omega = \sqrt{\frac{g}{R}}$, body P 'falls' towards Earth in one second covering a distance that is given by $B = \frac{g}{2}$ (this is the elementary formula of uniformly accelerated motion when time is taken to be equal to one second), while during the same second it moves along the tangent travelling a distance given by $A = \sqrt{\frac{g}{R}} \cdot R$ (which is the elementary formula for tangential speed in circular motion when $\omega = \sqrt{\frac{g}{R}}$).[18]

[17] As regards Galileo's language of proportions and the limits imposed by geometric methods on the kind of physical laws that may be adequately expressed by them, see E.Giusti, *Euclides Reformatus*, Torino, Bollati Boringhieri, 1993, particularly on pp. 35–82 and pp. 163–174. See also the introductory essay by the same author in G. Galilei, *Discorsi e Dimostrazioni Matematiche Intorno a Due Nuove Scienze Attinenti alla Meccanica e al Moto Locale*, Torino, Einaudi, 1990, pp. IX-LXIII.

[18] Galileo knew both the laws, even though he would not have used our algebraic language to express them. In fact, he would have used proportions. For example, the free fall law would have

Now, imagine a time ΔT: Galileo knew that in this case $B = \frac{g}{2} \cdot \Delta T^2$, even though he would have written the same law as $\frac{B1}{B2} = \frac{\Delta T1^2}{\Delta T2^2}$. If we apply Pythagoras' theorem to the right-angled triangle of Figure 1, the following relations hold true

$$R^2 + A^2 = R^2 + B^2 + 2 \cdot R \cdot B$$

$$A^2 = \frac{g^2}{4} \cdot \Delta T^4 + R \cdot g \cdot \Delta T^2$$

$$\omega^2 \cdot R^2 \cdot \Delta T^2 = \frac{g^2}{4} \cdot \Delta T^4 + R \cdot g \cdot \Delta T^2$$

and if we allow ΔT to become smaller and smaller, namely $\Delta T \to 0$, then $\omega = \sqrt{\frac{g}{R}}$, which is exactly the 'weightless' solution.

It must be stressed that, although the only significant condition that Galileo would probably[19] not have recognised as part of his mathematics is that infinitesimals like ΔT^4 are 'smaller' than others like ΔT^2, and hence negligible, the foregoing proof is totally ours and not Galileo's. In other words, we have shown that the situation described by Galileo's theorem of the 'equilibrium solution' is equivalent to a situation where the Earth rotates swiftly enough to balance centrifugal force. It should be clear by now why Galileo needed to prove that 'however fast' the circular motion, any 'downward motion', however small, suffices to guarantee that body P will remain attached to the circle. Having missed the opportunity to seize on the 'equilibrium condition' and so establish it as a special case in which any heavy object rotating around a centre is balanced by the centrifugal force – so that, contrary to Ptolemy's argument, there is no question of its escaping its circular pathway – Galileo naturally has to rule out all possible eventualities, so as to be able finally to demonstrate 'the impossible', namely that

> if the rock thrown from a rapidly moving wheel had any such natural tendency to move toward the centre of the wheel as it has to go toward the centre of the Earth, it might very well return to the wheel, or rather never leave it. For the distance travelled being so extremely small at the beginning of its separation (because of the infinite acuteness of the angle of contact), any tendency that

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been expressed as $\frac{s1}{s2} = \frac{T1^2}{T2^2}$, where S1 and S2 are distances and T1 and T2 times, so that our gravity constant g would have disappeared.

[19] It is not sure whether Galileo would have refused any attempt to compare infinitesimals; given that a few paragraphs later – in a proof we are not concerned with, because it does not refer to his understanding of circular motion in itself – he succeeded in showing precisely that the magnitude of the 'contact angle' at the contact point between tangent and circumference is to be deemed lower than that formed between any straight line passing through the contact point and the tangent. See the proof and the associated diagram in G. Galilei, *Dialogue Concerning the Two Chief World Systems*, *op. cit.*, p. 199.

> would draw it back toward the centre of the wheel, however small, would suffice to hold it on the circumference.[20]

Yet, this is not the whole story. For, Galileo knew perfectly well that 'tangential motion' is not the parameter that controls the dynamic effects of circular movement, and that his proof was deficient insofar as it was only 'geometry', not 'physics'. The 'equilibrium theorem' was in the end a purely geometric-kinematic proof. And if one assumes that the ratio $\frac{\mathrm{C}}{\mathrm{AB}} = \frac{\text{downward motion}}{\text{projection motion}}$ is given, still there remain different ways of making a wheel rotate at such a speed as to generate a particular 'tangential motion'. One way is to increase the wheel's radius and a second one is to increase the wheel's angular velocity. The resulting 'tangential motion' is exactly the same either way, but the 'extrusion' effect is quite different. For, centrifugal force in the former example differs from that generated by the latter. In other words, while the 'equilibrium theorem' is based on the kinematic properties of motion along a circumference, the 'extrusion' effects due to centrifugal force (Fc) depend on a slightly more complex relation between radius and angular velocity ($\mathrm{Fc} = \mathrm{m} \cdot \omega^2 \cdot \mathrm{R}$). And from this it is clear that the 'equilibrium theorem' lacked the necessary generality to overcome the circular motion problem, even though Galileo came so close to the formulation of the 'weightless condition', which, had it been properly understood by him, would have resulted in the first mathematically correct proof that Ptolemy's argument is totally erroneous.

> It is true that if I make a comparison between speeds of the *same wheel*,then that which is turned the more rapidly will hurl stones with the greater impetus, and *when the speed increases the cause of projection will increase also in the same ratio*. But now suppose the speed to be made greater not by increasing the speed of a given wheel (which would be done by making it have a larger number of revolutions in the same time), but by increasing the diameter and enlarging the wheel, preserving the same time for each revolution of the large wheel as of the small one. *The velocity would now be greater in the large wheel merely by reason of its greater circumference*. *Let no-one believe that the cause for extrusion increases in the ratio of the speed of its rim to that of the smaller wheel*.[21]

[20] G. Galilei, *Dialogue Concerning the Two Chief World Systems, op. cit.*, p. 194. In a comment on this passage S. Drake asserts that 'Galileo was prevented from going farther by his error in supposing that no tangential velocity could overcome any centripetal attraction' (*ibid.*, p. 478). The point is that – dubious as a sentence may appear in which a 'centripetal acceleration' is supposed to be able to overcome a 'tangential speed' – in Galileo's subsequent proof, as we have seen, there is no error whatsoever. What Galileo does quite correctly is to compare 'tangential motion' with 'downward motion', and what he assumes erroneously is in all probability that his demonstration is valid regardless of the dimension of the circle. Galileo's proof would have been wrong if he had fixed an arbitrary circle and, at the same time, an arbitrary ratio between 'tangential motion' and 'downward motion'.

[21] G. Galilei, *Dialogue Concerning the Two Chief World Systems, op. cit.*, p. 211. The italics in the last sentence are mine. I have also slightly changed Drake's translation of this sentence, which was 'No one would suppose...", because the Italian original " non sia chi creda che..." is a very strong imperative form. See the original text in *Le opere di Galileo Galilei*, VII, pp. 238.

As is clear from the passage, Galileo knows full well that: a) for wheels A, B of the same diameter but rotating at different speeds ω_A, ω_B, $\frac{\text{cause of projection A}}{\text{cause of projection B}} = \frac{\omega_A}{\omega_B}$; b) for wheels of different diameters rotating with the same angular velocity $\frac{\text{tangential velocity A}}{\text{tangential velocity B}} = \frac{R_A}{R_B}$. What he does not know in relation to case b) is the ratio between the causes of projection; on the other hand, what he asserts is that $\frac{\text{cause of projection A}}{\text{cause of projection B}} \neq \frac{V_A}{V_B}$. Nonetheless, Galileo knows that, at least in one particular case, it must hold true that

> we *can* throw a stone with a stick a yard long, *whereas we cannot* with one six yards long, even if the motion of that end of the long stick where the stone is stuck is more than twice as fast as the motion of the end of the shorter stick – as it would be if the speeds were such that during one complete revolution of the larger stick, the smaller one made three turns.[22]

To summarise: let 6 be the length of the longer stick, A, and 1 the length of the shorter one, B. Stick A's speed of revolution is 1 and stick B's is 3. Thus, Galileo knows that $\frac{V_A}{V_B} = \frac{\omega_A}{\omega_B} \cdot \frac{R_A}{R_B} = \frac{1}{3} \cdot \frac{6}{1} = 2$, namely, that tangential speed A is twice speed B. Yet, projection does not ensue. If he had known Huygens' formula, he would have known that projection does not ensue because according to his hypotheses the ratio between centrifugal forces F_A, F_B would be given by $\frac{F_A}{F_B} = \frac{\omega_A^2}{\omega_B^2} \cdot \frac{R_A}{R_B} = \frac{1}{9} \cdot \frac{6}{1} = \frac{2}{3}$. Thus, in spite of the fact that stick A has a tangential speed twice that of stick B, which has angular velocity three times greater, what we have is $\frac{\text{cause of projection A}}{\text{cause of projection B}} = \frac{2}{3} < 1$! But Galileo did not have to hand the tool that Huygens was to invent.

So, how on earth did he know that projection does not ensue? My hypothesis is that Galileo performed experiments with wheels and slings or similar spinning devices, even if he does not furnish any clue to this puzzle. Whatever the reason for his quite correctly believing that projection does not ensue, Galileo's proposed experiment with sticks shows that he knew beyond doubt that the 'cause of projection' is not tangential speed, but a more complex combination of the geometric parameters of circular motion.[23]

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[22] G. Galilei, *Dialogue Concerning the Two Chief World Systems*, *op. cit.*, p. 211–212. Italics are mine. I have corrected Drake's translation because otherwise the meaning would be lost. Drake translates the Italian " tal pietra potremo noi scagliare con una canna lunga un braccio, che con una lunga sei braccia non potremo " with "we throw a stone can better with a stick a yard long than with one six yards long". But, Galileo says clearly that 'we can...' and 'we cannot...'. The Italian word for 'stick' is 'canna', so that it must also be remembered that by 'canna' is meant a hollow 'stick'. See the original in *Le opere di Galileo Galilei*, VII, pp. 238.

[23] I disagree here with E.Strauss's stance relative to this passage. In the notes to his German translation of the *Dialogue* he hypothesises that Galileo a) probably assumes for the centrifugal force the expression $\frac{F_A}{F_B} = \frac{\omega_A}{\omega_B}$, namely, $F = \frac{V}{R}$; and b) by applying it to the second case is able to draw the conclusion that projection does not ensue. If Strauss' position were correct, Galileo would have stated that in the second case (same angular velocity but different radiuses) the cause of 'extrusion' was equal, whereas, in fact, he does not know what the cause of extrusion might be at all and simply claims that $\frac{\text{cause of projection A}}{\text{cause of projection B}} \neq \frac{V_A}{V_B}$. In other words, if Galileo had

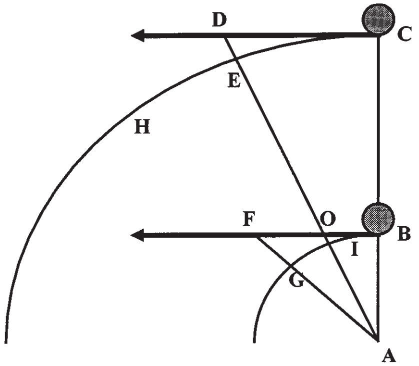

Figure 4.2

This is of paramount importance because, while the notion of tangential speeds reveals nothing significant about one's knowledge of the accelerated nature of circular motion around a centre, the awareness that 'extrusion' and 'projection' depend on a complex combination of geometric and kinematic parameters – like angular speed and radius – shows clearly that one has a very advanced understanding of that phenomenon. Indeed, it shows that, at the very least, one knows that only under particular circumstances does circular motion produce 'extrusion' effects, and that these effects are the very manifestation of its non-inertial nature. The point is how to quantify these phenomena, when one has no dynamic laws of motion, no formula of centrifugal force and just a rudimentary mathematical language, in which everything has to be subjected to the 'non-dimensional' rules of theory of proportions[24] and has to be expressed by means of simple ratios. Well, Galileo somehow managed to overcome these limitations and turned his 'equilibrium theorem' into a more sophisticated 'dynamic theorem', even though his language was not powerful enough to enable him to formulate a truly dynamic law.

Referring to Fig. 2:

> Let there be two unequal wheels around this centre A, BG being on the circumference of the smaller, and CEH on that of the larger, the radius ABC being vertical to the horizon. Through the points B and C we shall draw the

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assumed that *in general* $F = \frac{V}{R}$, then he would also have had to maintain that centrifugal force would *remain constant* when angular velocity *remains constant too,* regardless of the change in radius. Now, Strauss consistently attributes this belief to Galileo, though only 'hypothetically': "Wenn er [Galileo] hingegen, obgleich nur vermutungweise, den Satz ausspricht, dass bei gleichen Winkelgeschwindigkeiten die Centrifugalkraft konstant bleibe, also unabhängig von dem radius sei, so irrt er."(G. Galilei, *Dialog über die beiden hauptsächlichsten Weltsysteme,* [*ed. and tr.* by Emil Strauss], Leipzig, Teubner, 1891, pp. 531–532). But this belief is nowhere to be found in Galileo's text, neither dogmatically asserted nor hypothetically advanced. Nevertheless, Strauss overtly recognises that Galileo was quite correct in believing that in circular motion the cause of projection diminishes in the case in which radius increases and tangential speed remains constant (*ibid*., p. 531).

[24] Ratios between homogeneous magnitudes are 'non-dimensional', namely pure numbers, because physical dimensions are cancelled.

tangent lines BF and CD, and in the arcs BG and CE we take two arcs of equal length, BG and CE. The two wheels are to be understood as rotating about their centre with equal speed in such a way that two moving bodies will be carried along the circumferences BG and CE with equal speeds. Let the bodies be, for instance, two stones placed at B and C, so that in the same time during which stone B travels over the arc BC, stone C will pass the arc CE.[25]

[25] G. Galilei, *Dialogue Concerning the Two Chief World Systems*, *op. cit*., p. 217. This passage is of extreme importance because here Galileo quite correctly proves simply that the cause of 'extrusion' decreases in greater wheels rotating with equal tangential speed. Yet, even recently, Galileo's text has once again been misunderstood. I am referring to the analysis of this passage proposed by D.K. Hill in "The Projection Argument in Galileo and Copernicus: Rhetorical Strategy in the Defence of the New System", *Annals of Science*, XLI, 1984, pp. 109–133, an analysis which probably is also biased owing to the author's relying on a deficient English translation. According to Hill, after Salviati's demonstration, Sagredo appears to be convinced and advances the suggestion that 'for globes of different sizes and constant *angular* velocity centrifugal force will be the same' (*ibid*., p. 128). This conclusion draws on the following incomplete quotation of a concluding remark put forward by Sagredo: "...it might be supposed that the whirling of the earth would no more suffice to throw off stones than would any other wheel, as small as you please, which rotated so slowly as to make but one revolution every twenty-four hours"(G. Galilei, *Dialogue Concerning the Two Chief World Systems*, *op. cit*., p. 217). Unfortunately, the preceding English text presumably read by the author, from which he quotes just the final part, lacks a sentence that is essential to the exact comprehension of the reasoning. Here is the English text of the complete passage in S. Drake's translation: "Since the casting off diminishes with the enlargement of the wheel [as Galileo has proved in the 'dynamic theorem'], it might be true that to have the large wheel extrude things as does the small one, its speed would have to be increased as much as its diameter, which would be the case when their entire revolutions were finished in equal times. And thus it might be supposed that the whirling of the earth would no more suffice to throw off stones than would any other wheel, as small as you please, which rotated so slowly as to make but one revolution every twenty-four hours"(*ibid*.). Here is the original text in Italian " [. . .] ma di più vo considerando, che scemandosi la proiezione nell' accrescersi la ruota, *tuttavolta che si mantenga la medesima velocità in esse ruote*, forse potrebbe esser vero che a voler che la gran ruota scagliasse come la piccola, bisognasse crescerle tanto di velocità, quanto se le cresce di diametro, che sarebbe quando le intere conversioni si finissero in tempi uguali: e così si potrebbe stimare che la vertigine della Terra non più fusse bastante a scagliare le pietre, che qualsivoglia altra piccola ruota che tanto lentamente si girasse, che in ventiquattr' ore desse una sola rivolta" (*Le opere di Galileo Galilei*, VII, p. 244). The sentence in italics is missing in Drake's English text. Now, although Galileo did not grasp the concept of centrifugal force or of its quadratic dependence on angular speed, he was not so naïve as to believe that, whatever the angular speed may be, centrifugal force is the same. In fact 'projection' in greater wheels decreases '*tuttavolta che si mantenga la medesima velocità in esse ruote*', namely, if the tangential speed of the wheels is the same (the 'dynamic theorem'). Sagredo's *conjecture* is that the causes of projection in the two wheels might be 'equalised' by increasing the greater wheel's angular velocity in proportion to its diameter (which is of course a wrong hypothesis). On the other hand, Sagredo's above-quoted conclusion is not a 'suggestion' but a mere opinion ('si potrebbe stimare che...'). According to such an opinion it 'might be supposed that the whirling of the earth would no more suffice to throw off stones than would any other wheel, as small as you please, which rotated so slowly as to make but one revolution every twenty-four hours', because the cause of extrusion (which

Galileo knows that 'the cause for extrusion' does not increase according to the ratio of the speed of the rim of the larger wheel to that of the smaller wheel, so that he naturally feels justified in now trying to investigate what would happen if tangential speeds are equal, the diameters of the wheels being different. He hopes to divine the law of centrifugal force by testing the case in which the parameter he has well understood not to be part of the 'cause of projection' is set aside, namely, in the language of proportions, kept constant. Thus, in Fig. 2, where A is the larger wheel and B the smaller one, we have the following condition:

$$V_A = \omega_A \cdot R_A = V_B = \omega_B \cdot R_B$$

Here is the statement of his dynamic theorem:

> Now I say that the whirling of the smaller wheel is much more powerful at projecting the stone B than is the whirling of the larger wheel at projecting the stone C.[26]

At this point, Galileo's mechanics enters the *Dialogue* to prove the dynamic theorem. As Galileo's argument does not concern our main objective, I do not propose to follow his demonstration in detail – interesting as it may be since, in appealing to the dynamic law of the *stadera* (Roman lever scales or 'steelyard') a few paragraphs earlier, it confirms that he is seeking proof in a fully-fledged dynamic context – rather, I shall focus on the significance of the result.[27] Assuming that the stones in B and C remain on their circumferences, and taking DE and FG as direct measures of the centrifugal forces according to his mechanical law of the steelyard (an embryo of the virtual speeds principle), it follows that

> much more force is needed to hold the stone B joined to its small wheel than the stone C to its large one, which is the same as to say that a smaller thing will hinder projection from the large wheel than will prevent it on the small one.[28]

Let us take stock. Galileo's 'dynamic theorem' starts from the hypothesis that $V_A = \omega_A \cdot R_A = V_B = \omega_B \cdot R_B = V$ and concludes that $\frac{Fg_A}{Fg_B} = \frac{DE}{FG}$, Fg_A, Fg_B being what

according to the foregoing *conjecture* would be the same as the small wheel only if the Earth's angular speed were increased in proportion to its diameter) could simply not suffice 'to throw off stones', i.e. the Earth's angular speed could not be in proportion to its diameter. But the speed of diurnal rotation is in the end a matter of fact, so that Salviati subsequently concludes that "we shall not look further into this right now" (G. Galilei, *Dialogue Concerning the Two Chief World Systems*, *op. cit.*, p. 218).

[26] G. Galilei, *Dialogue Concerning the Two Chief World Systems*, *op. cit.*, p. 217.

[27] "In the steelyard, the lesser weight moves the greater only when the latter moves very little, being weighed at the lesser distance, and the former moves quite a way, hanging at the greater distance. One must say, then, that the smaller weight overcomes the resistance of the greater by moving much when the other moves little" (G. Galilei, *Dialogue Concerning the Two Chief World Systems*, *op. cit.*, p. 214). It is the embryo of what we call 'virtual displacements or speeds' principle. See the whole demonstration in *ibid.*, pp. 216–217.

[28] *Ibid.*

I propose to call Galilean 'quasi-centrifugal forces'. Recalling relation (1) and re-writing it as $B = -R + \sqrt{V^2 + R^2}$, we can formulate the Galilean quasi-centrifugal force[29] law as

$$\frac{Fg_A}{Fg_B} = \frac{-R_A + \sqrt{V^2 + R_A^2}}{-R_B + \sqrt{V^2 + R_B^2}} = \frac{DE}{FG} \tag{2}$$

whereas our (and Huygens') formula would yield $\frac{F_A}{F_B} = \frac{V_A^2/R_A}{V_B^2/R_B} = \frac{R_B}{R_A} = \frac{DE}{FG}$, (it being remembered that, in Galileo's hypothesis, tangential speeds are equal, namely $V_A = V_B$).

Well, relation (2) is Galileo's most advanced insight into the nature of circular motion.[30] It is a totally dynamic equation, which we have written in the form of a proportion, exactly as Galileo himself presents it in the above quoted passage. And quite apart from the fact that relation (2) – as we shall see[31] – is not so far from what is predicted by Huygens' formula according to the hypothesis of Galileo's 'dynamic theorem', we are bound to acknowledge that it is a relation between 'cause of motion' (i.e. force ratio $\frac{Fg_A}{Fg_B}$) and kinematic effect of motion (i.e. motion ratio $\frac{DE}{FG}$), a relation wherein 'something' is described that 'pulls' the stones in order to prevent them from escaping their circular trajectories along tangent lines. Of course, our discussion has avoided the notorious question of Galileo's concept of inertia. According to the above analysis, he was not in the least seeking to formulate an abstract principle regarding the difference between straight motion and circular motion around a centre (which as we conceive of it nowadays was virtually beyond the scope of his vision of motion in the universe). Yet, he was able to achieve a profound insight into something which was by far more important in the

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29 Let us remember that, in Galileo's formula, tangential speed V has the meaning of distance travelled in one second (or whatever time unity), so that there is nothing odd about adding speed V ('tangential motion') to radius R. It is all the more interesting that now Galileo overtly assumes DE and FG – which are the movements necessary for the stones to remain on the circumference – to be measures of the force that pulls stones B and C towards the centre, obviously regardless of the fact (unknown to him) that his previous 'equilibrium theorem' is valid only in one particular instance.

30 A. Koyré recognises that Galileo's demonstration is perfect. However, without taking into consideration what we have called the 'equilibrium theorem', he attributes to Galileo a strange theory of centrifugal force, claiming that "pour le rendre compréhensible [Galileo's proof], il [Galileo] a dû développer toute une théorie de la force centrifuge, et montrer tout d'abord que celle-ci n'est pas dirigée radialement, vers la circonférence, mais, au contraire, tangentiellment et perpendiculairment au rayon de la roue". A. Koyré, *Ètudes Galiléennes, op. cit.*, p. 254. I cannot see in what way Galileo's text may be interpreted in order to draw Koyré's conclusion that Galileo developed a tangential centrifugal force theory. Quite the contrary, Galileo understands that the 'cause of extrusion' is radial, though he is not able to arrive at Huygens' formula.

31 See Appendix 2. It must be stressed that Galileo's proof is absolutely correct and the theorem demonstrates that the greater the wheel's radius the smaller the quasi-centrifugal force. The paradox is only apparent, so long as it is assumed that *tangential* speed remains constant (which implies that angular velocity becomes inversely proportional to the radius of the wheel).

context of his grand plan to find a physical proof of the Earth's double motion, namely, the realisation that not only is circular motion characterised by a kinematic-geometric link with the centre of motion, but that it also bears a dynamic relation with the centre and that some agent has to be responsible for keeping the body 'attached' to its circular path. And in this sense he realised that circular motion is not 'inertial motion' at all. It is this distinction that allows us to overcome an apparent inconsistency with Galileo's famous statement in the First Day of the *Dialogue* of a supposed 'principle of circular inertia', according to which motion along the circumference of a circle,

> being the motion that makes the moving body continually leave and continually arrive at the end, [...] alone can be essentially uniform [...] From this uniformity, and from the motion being finite, there follow its perpetual continuation by a successive repetition of rotations, which cannot exist naturally along an unbounded line or in a motion continually accelerated or retarded.[32]

Under these circumstances, circular motion may well be perennial, even if some physical agent must make the body continuously deviate from its tendency to escape along the tangent line. In the end, what simply separates our understanding of circular motion from Galileo's is his pretension to the 'impossible' – namely, his dubious extension, to whatever case, of the dynamic theorem (which holds true only if tangential speeds are constant) according to which any 'tendency' or 'motion', however small, towards the centre is enough to keep the body on the circumference. Whereas, as we now know, one, and only one, value of centripetal acceleration is required to maintain the body on its trajectory. Moreover, the very fact that Galileo wants to prove the 'impossible' and directs the whole discussion towards his cosmological aim of demonstrating that Earth's diurnal rotation cannot 'extrude' the heavy objects that stand on its surface – contrary to the traditional belief embodied in Ptolemy's argument – appears to rule out any possibility that Galileo distinguished between the terrestrial mechanics of rotating wheels and whirling sticks and the celestial dynamics of planet Earth.[33]

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[32] G. Galilei, *Dialogue Concerning the Two Chief World Systems, op. cit.*, p. 31–32. Galileo's 'uniformity' clearly refers to the *tangential motion*, so that nothing in this passage is inconsistent with his subsequent recognition that circular motion is accelerated *towards the centre*. The question whether Galileo effectively intended to deny that motion along a straight line has perennial uniformity lies beyond the scope of this study.

[33] As regards the Burstyn-Aiton dispute over a Galilean 'supposed effect' of the motion of the Earth on tides, it must be said that, in 1965, Aiton, in his second note on Burstyn's article, denied that Galileo's analysis of rotating wheels and slings could indicate that Galileo had some understanding of the non-inertial nature of the circular motion of celestial bodies (repeating Koyré's argument) in order to weaken Burstyn's position. In his reply, Burstyn virtually accepted Aiton's criticism regarding the non-inertial nature of the Earth's rotation and celestial circular motions. Nevertheless, neither author took into account the theorems of the 'equilibrium solution' and the 'dynamic theorem'. See E.J. Aiton, "Galileo and the Theory of Tides, *op. cit*, p. 58 and 'Reply' by L. Burstyn, *ibid*., p. 63.

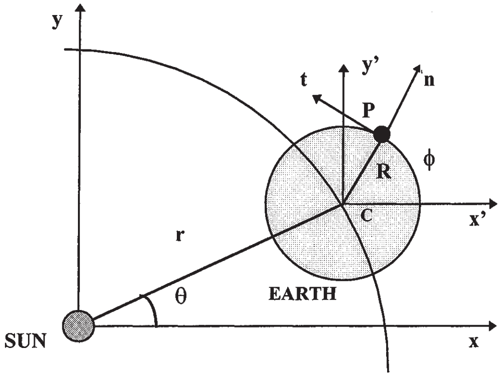

Figure 4.3

4.3 Tide equations: the quasi-Galilean term

As we have seen in Sect. 1 Galileo expresses the speed of the vessel 'Earth', i.e. the speed of a basin at point P on the Earth's surface in the form of a scalar function that we have written as $Vp = V + \omega \cdot R$ – whereas in fact the vector function $\vec{V}p = \vec{V} + \vec{\omega} \times \vec{R}$ is required to describe the motion of any point P that is rigidly attached to the surface of the revolving Earth. In so doing, Galileo loses sight of the 'tricky' nature of physical quantities that depend not only on scalar quantities, but also on directions (which must accordingly be taken into account as vectors).

Referring to Fig. 4.3, let us consider a Cartesian frame of reference in whose centre we imagine the Sun (fixed in space, i.e. fixed with respect to the fixed stars) and whose axes are (x,y). Once again, to simplify things, we disregard three-dimensional effects, in which we are not interested, and build a two-dimensional model of the Earth orbiting the Sun – we also assume, in accordance with Galileo, that the Earth's pathway is circular. Let the lesser circle be the Earth and the arc of the greater one a part of its trajectory along its annual orbit. Let us imagine a second frame of reference attached to Earth's centre C with axes (x', y'), but not rotating with the Earth – in other words, a translating frame of reference, which maintains its axes parallel to the axes of the first frame of reference. Let $(\vec{t}, \vec{n})$ be a couple of unit vectors, $\vec{t}$ tangent to the Earth's surface at point P, and $\vec{n}$ normal (and oriented towards the sky), respectively. This third frame of reference is sometimes called a system of 'tangential and normal co-ordinates'.[34] Let θ be the angle formed by the radius r (distance between the Sun and Earth's centre C) with x axis, and R the Earth's radius. Let ϕ be the angle formed by the Earth's radius R (distance between Earth's centre C and the Galilean vessel at P) with axis x', so that angular velocity ω of the vessel at P is $\omega = \frac{d\phi}{dt}$.

In order to compute the acceleration of a terrestrial 'vessel' placed on the Earth's surface and ideally coinciding with a generic point P on the Earth's surface, we have to determine the derivative of absolute speed $\vec{V}p = \vec{V} + \vec{\omega} \times \vec{R}$. Remembering that $\vec{V}p$ is the 'absolute' speed of P, i.e. speed with respect to the fixed stars, while $\vec{\omega} \times \vec{R}$ is the

[34] A.F. D'Souza, V.K. Garg, *Advanced Dynamics*, *op. cit*, pp. 12–15.

'relative' speed, i.e. speed with respect to the non-rotating terrestrial frame of reference (x', y') we have:

$$\frac{d}{dt}\vec{V}p = \frac{d}{dt}(\vec{V} + \vec{\omega} \times \vec{R}) = \frac{d}{dt}\vec{V} + \frac{d}{dt}(\vec{\omega} \times \vec{R}).$$

Let us first consider $\frac{d}{dt}(\vec{\omega}\times\vec{R})$. Thanks to our tangential co-ordinate system, and invoking the simple rule of the 'vector product' between two vectors, according to which a 'vector product' between two perpendicular vectors yields a vector that is normal to the plane formed by the former ones (in our case $\vec{\omega}$ is normal to the plane of this sheet), we may assert that $\vec{\omega} \times \vec{R} = R \cdot \omega \cdot \vec{t}$. Now, given that $\frac{d}{dt}\vec{t} = \vec{\omega} \times \vec{t}$ (i.e. the same 'vector product' rule applied to angular velocity $\vec{\omega}$ and vector $\vec{t}$ allows us to perform the differentiation), it follows that derivative $\frac{d}{dt}(R \cdot \omega \cdot \vec{t})$ yields $(-R \cdot \omega \cdot \vec{n})$.

This is all that is needed to understand a basic consequence of our model: velocity component $\vec{\omega} \times \vec{R}$, which is due to Earth's diurnal motion makes no contribution to Galilean tide-generating acceleration (GTGA), simply because GTGA is to be thought of as being tangential to the Earth's surface, whereas the term $(-R \cdot \omega \cdot \vec{n})$ is directed towards Earth's centre C, so that it is perpendicular to the Earth's surface. And this is why we must not confuse Galileo's 'scalar thought' about the composition of motions and speeds with the 'vector' operation $\frac{d}{dt}\vec{V}p$ we have performed, which – as we shall see in a moment – does nevertheless yield a quasi-Galilean contribution tangential to the Earth's surface. Galileo's non-vector formula for the motion of a vessel coinciding with point P on the Earth's surface is $Vp = V + \omega \cdot R$, which means that if one performs the derivative of this expression one finds the exact formula of GTGA, namely

$$\frac{d}{dt}V_P = \frac{d}{dt}\left[V \cdot \cos(\phi - \theta) + \omega \cdot R\right]$$

and

$$GTGA = -V \cdot \sin(\phi - \theta) \cdot (\omega - \Omega)$$

where Ω is the Earth's annual angular velocity $\Omega = \frac{d\theta}{dt}$, $\omega = \frac{d\phi}{dt}$ and $(\omega - \Omega)$ comes from differentiating the function $\cos(\phi - \theta)$. Now, GTGA is evidently a periodic function,[35] as Galileo predicted, though, being a sine function, it has its *maxima* and *minima* with a phase lag of 90° with respect to what Galileo thought (at the point where the sine function has its *maxima* and *minima*, exactly there the cosine function is zero). In other words, where Vp is maximum and minimum, GTGA is zero and *vice versa*.

This should suffice to show that Galileo's lack of knowledge of the vector nature of physical quantities like speeds and accelerations certainly played an important role in his 'oversight'. It was correct and perfectly legitimate to apply the principle of composition of speeds, though this rule had to be expressed in a poor language, so that, without being able to 'mathematize' his own intuition, Galileo was not able to see that the scalar

[35] The argument $(\phi - \theta)$ represents the 'solar' variable, because to complete a solar day P has to make up for the Earth's annual motion θ, which amounts to a little less than 1 degree per day and causes the solar day to be about 4 minutes longer than the sideral day.

$Vp = V + \omega \cdot R$ does not adequately represent the much more complex vector quantity $\vec{V}p = \vec{V} + \vec{\omega} \times \vec{R}$. Even more obvious is that the principle of composition of speeds is a purely kinematic tool that has nothing to do with the question of the principle of relativity set forth by Galileo in the Second Day of the *Dialogue*.

It is interesting to note that what we have defined in Sect. 3 as a qualitative (in the sense that the constant value A_0 was not specified) spatial-temporal distribution on the Earth's surface of tide-generating acceleration, i.e. the function $A_{\Omega Y}(t,y) = A_0 \cdot \sin(\Omega_{24} \cdot t) \cdot \sin\left(\frac{2 \cdot \pi}{R} \cdot y\right)$ – where Ω_{24} is diurnal angular speed (not to be confused with annual angular velocity Ω) – can now be more precisely described by function $GTGA = -V \cdot \sin(\phi - \theta) \cdot (\omega - \Omega)$, providing that we regard P as a generic point on the Earth's surface and V as the component of the annual speed $\vec{V}_C$ of Earth's centre C tangential to the Earth's surface exactly at point P. In other words, assuming $V = V_C \cdot \sin\left(\frac{2 \cdot \pi}{R} \cdot y\right)$ we are now able to specify constant A_0 as $A_0 = -V_C \cdot (\omega - \Omega)$, so that both the qualitative formula $A_{\Omega Y}(t,y)$ and the more precise GTGA consistently express the same concept.

Let us now define quasi-Galilean tide-generating acceleration (QGTGA).

We have seen that GTGA may be expressed in modern mathematical language by performing the differentiation of $Vp = V + \omega \cdot R$. We now want to maintain the vector nature of P's speed and to do so we define the quasi-Galilean tide-generating acceleration (QGTGA) as

$$QGTGA = \frac{d}{dt}\vec{V}$$

where we have abandoned the term $\left(-R \cdot \omega \cdot \vec{n}\right)$, because it is directed towards the Earth's centre and thus has no influence over P's tangential motion. The meaning of our terminology should be clear. Given that one of the two terms of GTGA has had to be abandoned, we can no longer refer to a *fully Galilean* tide-generating acceleration. In Galileo's conception, the variation in Vp stems from the composition of two distinct elements, whereas QGTGA stems from just one of these elements, namely, the absolute speed of Earth's centre C. Hence, the adjective 'quasi-Galilean'.

Let us consider now $\frac{d}{dt}\vec{V}$.

Let $(\vec{i}, \vec{j})$ be a couple of unity vectors parallel to x and y axes, respectively, and $x_c y_c$ Cartesian x and y coordinates of Earth's centre C. In the (x,y) frame of reference C's absolute speed and acceleration may be written, respectively, as

$$\vec{V} = \frac{d}{dt}x_c \cdot \vec{i} + \frac{d}{dt}y_c \cdot \vec{j}$$

and

$$\frac{d}{dt}\vec{V} = \frac{d^2}{dt^2}x_c \cdot \vec{i} + \frac{d^2}{dt^2}y_c \cdot \vec{j}$$

where the last formula represents the quasi-Galilean tide-generating acceleration in a form more suitable for our purposes. It is obvious that from the previous two relations a component tangent to the Earth's circumference must be present, because the direction of vector $\frac{d}{dt}\vec{V}$ is generic. We can also see now that if the Earth travels uniformly along a straight line, then the speed of Earth's centre C, $\vec{V}$, is constant, namely QGTGA $= \frac{d}{dt}\vec{V}_P = \frac{d}{dt}\vec{V} = 0$, as E. Mach's oversimplified model assumes.

We have learned how to treat speed and acceleration of a 'terrestrial vessel'. The next step is to build the equation of motion of a fluid particle P moving freely within a sea basin on the Earth's circumference (actually, in our two-dimensional model, the basin is no more than a portion of the Earth's circumference itself). Let ϕ now represent the angular displacement of fluid particle P, i.e. its 'flat' flux and reflux inside its basin. We no longer need to take into account Earth's diurnal rotation.[36] And, to all intents and purposes, it is as if the solar day has become an annual day.

Given that fluid particle P is free to move only on the Earth's circumference, writing down the equation for P's ebb and flow is tantamount to describing how the angular acceleration $\left(\frac{d^2}{dt^2}\phi\right)$ varies. To do that, we have to bear in mind that P ebbs and flows on the surface of a moving Earth, which means that in writing Newton's equation of the motion of P we have to take into account the so-called 'forces due to the non-inertial nature of the frame of reference'. In other words, the Earth's frame of reference, with respect to which we want to describe P's flux and reflux, is not inertial, so that Newton's laws have to be written down carefully (see footnote 8).

When a non-inertial frame of reference does not rotate – let us remember that to describe particle P's motion we have chosen a non-rotating frame of reference (x', y') – Newton's second law of motion may be written simply by adding to the right-hand side of the equation the product of P's mass by the acceleration of the frame of reference itself (i.e. the acceleration of the vessel) and changing its algebraic sign. Let m_p be the mass of particle P and $\vec{F}_{PS}$ the Sun's force of attraction acting on P, then, taking the tangential (to the Earth's circumference) component of P's acceleration, i.e. $r \cdot \frac{d^2}{dt^2}\phi$, Newton's second law of motion of particle P may be written as

$$r \cdot \frac{d^2}{dt^2}\phi = -\left(\frac{d^2}{dt^2}x_C \cdot \vec{i} + \frac{d^2}{dt^2}y_C \cdot \vec{j}\right) \cdot \vec{t} + \frac{\vec{F}_{PS}}{m_P} \cdot \vec{t} \qquad (3)$$

which, on the right-hand side, embodies the quasi-Galilean contribution (obviously taken in the direction tangential to the Earth's circumference[37]). To summarise: the quasi-Galilean contribution stems from the non-inertial nature of the vessel's frame of reference, with respect to which the fluid particle's ebb and flow occurs. The left-hand

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[36] In our two dimensional model, Earth's diurnal rotation has no effect on fluid particle P's ebb and flow. Earth's diurnal rotation effects are known as Coriolis or geostrophic effects. In our case, Earth's diurnal velocity – call it Ω_{24} – is perpendicular to the plane of this sheet (refer to Fig. 3) and P's speed is purely tangential to the Earth's circumference, so that non-inertial effects due to Coriolis acceleration (which is given by the vector product $2 \cdot \vec{\Omega}_{24} \times \vec{V}_P$) results in an acceleration directed towards the Earth's centre along the normal – $\vec{n}$.

[37] In order to obtain the component of a vector in the chosen direction, one has simply to perform the so-called 'scalar product' of the given vector quantity by the vector representing the chosen direction. Relation (3) is therefore a scalar relation in which all components are taken along the tangential direction given by vector $\vec{t}$.

side of equation (3) is P's tangential acceleration. Force $\vec{F}_{PS} \cdot \vec{t}$ is the 'projection' along the circumference of the Sun's gravitational force, namely, its tangential component.[38]

Now, all we need to do is make $\vec{F}_{PS} \cdot \vec{t}$ explicit and perform the scalar product $\left[-\left(\frac{d^2}{dt^2}x_C \cdot \vec{i} + \frac{d^2}{dt^2}y_C \cdot \vec{j}\right) \cdot \vec{t}\right]$. According to Newton's law of universal gravitation, the Sun's force of attraction acting on Earth may be written as[39]

$$\vec{F} = -G \cdot \frac{m_S \cdot m_T}{r_{PS}^2} \cdot \frac{\vec{r}}{\|\vec{r}\|} \tag{4}$$

where G is the constant of universal gravitation, m_S and m_T Sun's and Earth's masses, respectively, and r the Earth-Sun distance (vector $\vec{r}$ is directed from the Sun's centre to Earth's centre C). But relation (4) is formally general, so that it also describes the Sun-P system interaction. In this case, we have for fluid particle P (of mass m_P)

$$\vec{F}_{PS} = -G \cdot \frac{m_S \cdot m_P}{r_{PS}^2} \cdot \frac{\vec{r}_{PS}}{\|\vec{r}_{PS}\|}$$

and, given that we are interested in tangential motion, namely in $\vec{F}_{PS} \cdot \vec{t}$, we have to make this scalar product explicit too. The result is the following simple expression (the necessary calculations can be found in Appendix 2):

$$F_{PS_t} = -G \cdot m_P \cdot m_S \cdot \frac{-\sin\phi \cdot x_C + y_C \cdot \cos\phi}{r_{PS}^3}$$

So far, we have accomplished the task of rendering $(\vec{F}_{PS} \cdot \vec{t})$ explicit. Now we have to do the same with $\left[-\left(\frac{d^2}{dt^2}x_C \cdot \vec{i} + \frac{d^2}{dt^2}y_C \cdot \vec{j}\right) \cdot \vec{t}\right]$. To perform these scalar products, we simply have to carry out elementary trigonometric operations. Well, believe it or not (in the latter case, spend a minute in contemplation of Fig. 3 or refer to Appendix 2), the following holds true:

$$-\left(\frac{d^2}{dt^2}x_C \cdot \vec{i} + \frac{d^2}{dt^2}y_C \cdot \vec{j}\right) \cdot \vec{t} = \frac{d^2}{dt^2}x_C \cdot \sin\phi - \frac{d^2}{dt^2}y_C \cdot \cos\phi$$

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[38] Burstyn's model is equivalent to the following equation

$$r \cdot \frac{d^2}{dt^2}\phi = -\left(\frac{d^2}{dt^2}x_C \cdot \vec{i} + \frac{d^2}{dt^2}y_C \cdot \vec{j}\right) \cdot \vec{t}$$

namely, to the first part of the right-hand side of relation (3). Hence he failed to see that the quasi-Galilean contribution that this term yields vanishes because of the nature of gravity, which is 'contained' in the Sun's gravitational term. The same goes for Nobile. See H.L. Burstyn in his article "Galileo's Attempt to Prove that the Earth Moves", *op. cit.*, pp. 172, and "Sull' argomento galileiano della quarta giornata dei 'Dialoghi' e sue attinenze col problema fondamentale della Geodesia", *op. cit.*, p. 430ff.

[39] $\|\vec{r}\|$ means modulus of vector $\vec{r}$ (i.e. its measure). It is assumed that $\vec{r}$ is oriented from S towards T, hence the '-' sign in relation (4). Given that $\| \frac{\vec{r}}{\|\vec{r}\|} \| = 1$, the ratio $\frac{\vec{r}}{\|\vec{r}\|}$ is a common way to indicate that force $F_{PS} = -G \cdot \frac{m_S \cdot m_P}{r_{PS}^2}$ is in fact a vector.

so that the new relation expressing Newton's law of motion of fluid particle P eventually turns out to be:

$$\mathrm{r}\cdot\frac{d^2}{dt^2}\phi=\frac{d^2}{dt^2}\mathrm{x_C}\cdot\sin\phi-\frac{d^2}{dt^2}\mathrm{y_C}\cdot\cos\phi-\mathrm{G}\cdot\mathrm{m_S}\cdot\frac{-\sin\phi\cdot\mathrm{x_C}+\mathrm{y_C}\cdot\cos\phi}{\mathrm{r}_{\mathrm{PS}}^3}\qquad(5)$$

Let us now turn our attention to the motion of the vessel. To write Newton's equations of motion of the sea basin is to write Newton's equations of motion of Earth's centre C (in our simple model Earth's uniform diurnal rotation has no effect on P's motion). But relation (4) is only the first step towards these equations. Appendix 2 describes how to turn the knowledge embodied in vector relation (4) into the following two scalar equations, which are Newton's equations of motion of Earth's centre C with respect to our Cartesian frame of reference (x,y).[40]

$$\begin{cases}\dfrac{d^2}{dt^2}\mathrm{x_C}=-\mathrm{G}\cdot\dfrac{\mathrm{m_s}}{\mathrm{r}_{\mathrm{CS}}^3}\cdot\mathrm{x_C}\\[2mm]\dfrac{d^2}{dt^2}\mathrm{y_C}=-\mathrm{G}\cdot\dfrac{\mathrm{m_s}}{\mathrm{r}_{\mathrm{CS}}^3}\cdot\mathrm{y_C}\end{cases}\qquad(6)$$

The triangle formed by the Sun's centre, Earth's centre C and point P – namely triangle SCP, and let γ be angle $\angle SCP$ – must now be considered. The following relation holds true by virtue of what, in elementary plane trigonometry, is sometimes referred to as Carnot's theorem (a sort of trigonometric extension of Pythagoras' theorem):[41]

$$\frac{1}{\mathrm{r_{PS}}}=\frac{1}{\left[\mathrm{r}_{\mathrm{CS}}^2+\mathrm{R}^2-2\cdot\mathrm{R}\cdot\mathrm{r_{CS}}\cdot\cos\gamma\right]^{1/2}}$$

The latter can in turn be expanded as a power series in $\frac{1}{\mathrm{r_{CS}}}$ so as to yield the following infinite series, in which for the sake of simplicity only the terms up to the third power (namely up to power $\frac{1}{\mathrm{r}_{\mathrm{CS}}^3}$) are shown[42]

[40] For the sake of simplicity we assume that particle P's mass $\mathrm{m_p}$ is small enough not to influence the motion of the Sun-Earth system.

[41] The first to have used the triangle as a tool to 'deconstruct' gravitational forces was L. Euler. See L. Euler, *Inquisitio Physica in causas fluxus ac refluxus maris*, in I. Newton, *Philosophiae Naturalis Principia Mathematica etc., op. cit.*, tome III, pp. 297–299, where he carries out the subsequent series expansion to the fourth power. See also E.J. Aiton, "The Contribution of Newton, Bernoulli and Euler to the Theory of the Tides", *op. cit.*, p. 221ff. Aiton's study is still the only specific scholarly contribution devoted to the works on tides by Bernoulli and Euler. See also R.A. Harris, *Manual of Tides, op. cit.*, pp. 415–420.

[42] To see how this works, simply let $y=\frac{1}{\mathrm{r_{CS}}}$. It follows that $\frac{1}{\mathrm{r_{PS}}}=\frac{y}{\sqrt{1+\mathrm{y}^2\cdot\mathrm{R}^2-2\cdot\mathrm{R}\cdot\mathrm{y}\cdot\cos\gamma}}$. To find the coefficients of expansion (7) 'near' the point $\mathrm{y}=0$, calculate the first derivative of function $\frac{1}{\mathrm{r_{PS}}}$ with respect to y, the second, the third…, and so on. If you let $\mathrm{y}=0$ in all the derivatives, you will get all the coefficients of expansion (7). One of the clearest explanations I have found of this most elegant mathematical 'trick' is that given in H.Poincaré, *Leçons de Mécanique Céleste*, 3 vols., Paris, Gauthier-Villars, 1910. See tome III, *Théorie des Marées*, pp. 52–55. See also

$$\frac{1}{\mathrm{r_{PS}}} = \frac{1}{\mathrm{r_{CS}}} + \frac{\mathrm{R}\cdot\cos\gamma}{\mathrm{r_{CS}^2}} + \frac{\mathrm{R}^2\cdot\left(3\cdot\cos^2\gamma - 1\right)}{2\cdot\mathrm{r_{CS}^3}} + \cdots \tag{7}$$

Now we have an expression that can be usefully substituted for $\frac{1}{\mathrm{r_{PS}}}$ in relation (5). For our purposes, we only need to show the first term of series (7) (so that we end up with an equation where the third term of the right-hand side is an infinite series whose lowest term is $\frac{1}{\mathrm{r_{CS}^3}}$). The substitution yields

$$\mathrm{r}\cdot\frac{d^2}{d\mathrm{t}^2}\phi = \frac{d^2}{d\mathrm{t}^2}\mathrm{x_C}\cdot\sin\phi - \frac{d^2}{d\mathrm{t}^2}\mathrm{y_C}\cdot\cos\phi - \mathrm{G}\cdot\mathrm{m_S}\cdot\frac{-\sin\phi\cdot\mathrm{x_C}+\mathrm{y_C}\cdot\cos\phi}{\mathrm{r_{CS}^3}} + \cdots$$

and, bearing in mind equations (6), namely, $\begin{cases}\frac{d^2}{d\mathrm{t}^2}\mathrm{x_C} = -\mathrm{G}\cdot\frac{\mathrm{m_s}}{\mathrm{r_{CS}^3}}\cdot\mathrm{x_C}\\ \frac{d^2}{d\mathrm{t}^2}\mathrm{y_C} = -\mathrm{G}\cdot\frac{\mathrm{m_s}}{\mathrm{r_{CS}^3}}\cdot\mathrm{y_C}\end{cases}$, we eventually obtain relationship (8):

$$\begin{aligned}\mathrm{r}\cdot\frac{d^2}{d\mathrm{t}^2}\phi &= \underbrace{-\mathrm{G}\cdot\frac{\mathrm{m_S}}{\mathrm{r_{CS}^3}}\cdot\mathrm{x_C}\cdot\sin\phi + \mathrm{G}\cdot\frac{\mathrm{m_S}}{\mathrm{r_{CS}^3}}\cdot\mathrm{y_C}\cdot\cos\phi}_{\text{Quasi−Galilean contribution}}\\ &\quad \underbrace{-\,\mathrm{G}\cdot\mathrm{m_S}\cdot\frac{-\sin\phi\cdot\mathrm{x_C}+\mathrm{y_C}\cdot\cos\phi}{\mathrm{r_{CS}^3}} + \mathrm{R}\left(\frac{1}{\mathrm{r_{CS}^2}}\right)^3\cdot\mathrm{Q}}_{\text{Gravitational contribution}}\end{aligned}$$

where $\mathrm{R}\left(\frac{1}{\mathrm{r_{CS}^2}}\right)$ is the remaining infinite series, given by relationship (7), whose terms are powers of $\left(\frac{1}{\mathrm{r_{CS}}}\right)$ higher than or equal to two and $\mathrm{Q} = \left(-\sin\phi\cdot\mathrm{x_C}+\mathrm{y_C}\cdot\cos\phi\right)$. Relationship (8) shows that the first and second terms – the quasi-Galilean tide-generating contribution to the whole tide-generating-force (the whole right-hand side) – are cancelled out by the third and fourth, which are due to gravitational attraction cunningly 're-arranged'. In other words, the right-hand side of equation (8) no longer depends on the annual motion of the Earth which is represented by Earth's centre C's motion, i.e. by $\left(\frac{d^2}{d\mathrm{t}^2}\mathrm{x_C}\cdot\sin\phi - \frac{d^2}{d\mathrm{t}^2}\mathrm{y_C}\cdot\cos\phi\right)$.[43]

To sum up, the above analysis shows that only a quasi-Galilean tide-generating acceleration, given by $\frac{d}{d\mathrm{t}}\vec{\mathrm{V}}\mathrm{p} = \frac{d}{d\mathrm{t}}\vec{\mathrm{V}}$, enters into the equation of motion of fluid particle P. We have also seen that Galileo's understanding of the accelerated nature of circular motion is unequivocal. In his physics, fluid particle P's motion on the Earth's circumference, as well as the annual motion of the Earth itself, are to be conceived of as being accelerated motions around their centres, respectively Earth's centre C and the Sun. He would have

H. Lamb, *Hydrodynamics*, *op. cit.*, 358–359 and G.I. Marchuk, B.A. Kagan, *Dynamics of Oceans Tides*, *op. cit.*, pp. 1–15.

[43] Earth's diurnal rotation does not introduce any effects in our simplified two-dimensional model and merely accounts for the diurnal periodicity of ebb and flow.

accepted the quasi-Galilean tide-generating acceleration as a more precise description of his tide-generating acceleration and as being consistent with his understanding of the accelerated nature (towards the centre) of any circular motion. And this is the premiss upon which the historiographical debate, initiated in the 1950s, has to be re-opened if we are to ask an apparently simple question like ‘do the Earth’s motions influence tide phenomena in such a way that tide phenomena may be considerd at least a necessary consequence, if not an acceptable proof, of these motions?’. Even though Galileo’s mathematics is far removed from the sophisticated language that is required to find the answer to this question, and even though his pre-Newtonian physics remains in turn far removed from the complex conceptual framework supporting the whole foregoing analysis, we have to credit him with a profound insight (the ‘piece of physics without mathematics’) into the role that the Earth’s Copernican double motion plays in the complex interaction between tidal ebb and flow within a sea basin and motion of the basin itself.

4.4. Tides in a non-Newtonian universe

Referring to Fig. 3, let Sun S be free to move in space. According to this hypothesis, frame of reference (x,y) has to be thought of as being attached to absolute space’, for example to the fixed stars, if we assume that the fixed stars somehow represent absolute space. Given that the only purpose of this section is to prove that ‘relative motion’ phenomena like terrestrial tides may depend on Earth’s absolute motion in space according to particular hypotheses, we may assume that bodies S, C, P are simply free points in space and that fluid particle P orbits centre C (as if it were a moon), ‘floating’ in the neighbourhood of Earth without being ‘constrained’ to its surface (which does not exist in this three-point system). Under these circumstances, if P turns out to be subjected to different accelerations that depend on the motion of the Earth, then it is proved that tide phenomena also depend on Earth’s absolute motion (this model is more general than the original one presented in Figure 3, because the three-point system can be ‘reduced’ to the former by complicating P’s equation, so as to take into account the constraint due to the Earth’s circumference).

Let O be the origin of frame (x,y), $\vec{r}$ vector distance, m mass and $\vec{\mathrm{F}}$ gravitational force (where subscripts S, C, P identify bodies). Let us assume that P’s mass is small enough not to attract S and C (merely to simplify equations without loss of generality). Newton’s equations of motion of S-C-P system are

$$\begin{cases} \dfrac{d^2}{dt^2}\vec{\mathrm{r}}_{\mathrm{SO}} = \dfrac{\vec{\mathrm{F}}_{\mathrm{CS}}}{\mathrm{m}_{\mathrm{S}}} \\ \dfrac{d^2}{dt^2}\vec{\mathrm{r}}_{\mathrm{CO}} = \dfrac{\vec{\mathrm{F}}_{\mathrm{SC}}}{\mathrm{m}_{\mathrm{C}}} \\ \dfrac{d^2}{dt^2}\vec{\mathrm{r}}_{\mathrm{PO}} = \dfrac{\vec{\mathrm{F}}_{\mathrm{CP}}+\vec{\mathrm{F}}_{\mathrm{SP}}}{\mathrm{m}_{\mathrm{P}}} \end{cases} \tag{9}$$

Motion of point P relative to Earth’s centre C is given by difference $\vec{\xi} = (\vec{\mathrm{r}}_{\mathrm{PO}} - \vec{\mathrm{r}}_{\mathrm{CO}})$, where $\vec{\xi}$ is the vector distance between P and C, so that the equation of motion of point P relative to Earth’s centre C is

$$\frac{d^2}{dt^2}\vec{\xi} = \frac{d^2}{dt^2}\vec{r}_{PO} - \frac{d^2}{dt^2}\vec{r}_{CO} = \frac{\vec{F}_{CP} + \vec{F}_{SP}}{m_P} - \frac{\vec{F}_{SC}}{m_C} \tag{10}$$

Let us recall Newton’s corollary VI to the laws of motion:

> If bodies, moved in any manner among themselves, are urged in the direction of parallel lines by equal accelerative forces, they will all continue to move among themselves, after the same manner as if they had not been urged by those forces.[44]

According to this corollary, the motion of bodies relative to each other does not change if an arbitrary acceleration $\vec{a}$ is superimposed on the whole system, that is, if all bodies belonging to the system accelerate with acceleration $\vec{a}$. If we apply this corollary to our system and write equations (9) as

$$\begin{cases} \frac{d^2}{dt^2}\vec{r}_{SO} = \frac{\vec{F}_{CS}}{m_S} + \vec{a} \\ \frac{d^2}{dt^2}\vec{r}_{CO} = \frac{\vec{F}_{SC}}{m_C} + \vec{a} \\ \frac{d^2}{dt^2}\vec{r}_{PO} = \frac{\vec{F}_{CP}+\vec{F}_{SP}}{m_P} + \vec{a} \end{cases} \tag{11}$$

it is clear that the equation expressing the motion of P relative to C does not change, because $\vec{a}$ disappears from it. We can now take advantage of this fact and make the Earth ‘stop’ – without introducing changes in the relative motion of bodies S-C-P – by assigning to $\vec{a}$ the particular value $\vec{a} = -\frac{\vec{F}_{SC}}{m_C}$, so that equations (11) become:

$$\begin{cases} \frac{d^2}{dt^2}\vec{r}_{SO} = \frac{\vec{F}_{CS}}{m_S} - \frac{\vec{F}_{SC}}{m_C} \\ \frac{d^2}{dt^2}\vec{r}_{CO} = 0 \\ \frac{d^2}{dt^2}\vec{r}_{PO} = \frac{\vec{F}_{CP}+\vec{F}_{SP}}{m_P} - \frac{\vec{F}_{SC}}{m_C} \end{cases} \tag{12}$$

According to equations (12), Earth’s centre C is now at rest with respect to the absolute space (if it is at rest at initial time t_0; if it moves, it will continue uniformly with the same speed). Given that equation (10) is not affected by $\vec{a} = -\frac{\vec{F}_{SC}}{m_C}$, equations (9) (i.e. Earth’s moving system) and (12) (i.e. Earth at rest system) describe relative phenomena (i.e. equation (10)) independently of the superimposed common acceleration $\vec{a} = -\frac{\vec{F}_{SC}}{m_C}$, i.e. *relative phenomena are the same irrespective of the Earth’s state of motion, be it absolute rest or absolute motion*. This implies that P’s motion relative to C may depend on C’s absolute motion if, and only if, the right-hand side of equation (10), namely, $\frac{\vec{F}_{CP}+\vec{F}_{SP}}{m_P} - \frac{\vec{F}_{SC}}{m_C}$, somehow already depends on C’s absolute motion – i.e. both equation systems (9) and (12) yield the selfsame relative-motion equation (10) if, and only if, the right-hand side of the equation describing P’s motion relative to C does not contain absolute coordinates and/or their derivatives. Now, gravitational forces – the right-hand sides of equations (9), (12) – are precisely ‘relative’ quantities because they depend only

[44] I. Newton, *Principia*, tr. Motte-Cajori, *op. cit.*, p. 21.

on the distance between bodies (and, of course, on masses). Under these circumstances, the right-hand side of equation (10) is totally independent of C's absolute motion in space.

Yet, if gravitational forces between bodies depended on any absolute quantities – i.e. quantities only describable with respect to absolute space – equation (10) would necessarily depend on these quantities too. Suppose, for example, that gravitation constant G (let us remember that Newton's formula of the inverse square law for bodies 1 and 2 may be written as $\vec{F} = -G \cdot \frac{m_1 \cdot m_2}{r^2} \cdot \frac{\vec{r}}{\|\vec{r}\|}$) becomes a function of the Sun's absolute position and/or absolute speed,[45] i.e. let $G = G(\vec{r}_{SO}, \frac{d}{dt}\vec{r}_{SO})$. If we now re-write equation (10) so as to show the presence of universal gravitation function $G = G(\vec{r}_{SO}, \frac{d}{dt}\vec{r}_{SO})$, we obtain: $\frac{d^2}{dt^2}\vec{\xi} = \left(\frac{\vec{F}_{CP}+\vec{F}_{SP}}{m_P} - \frac{\vec{F}_{SC}}{m_C}\right) \cdot G(\vec{r}_{SO}, \frac{d}{dt}\vec{r}_{SO})$, and P's motion relative to C, namely, $\vec{\xi}$, turns out to depend on absolute gravitation' $G = G(\vec{r}_{SO}, \frac{d}{dt}\vec{r}_{SO})$, which in turn depends on the Sun's absolute motion, which motion must obviously be different when the Earth is at rest from when the Earth moves. This is also clearly shown by equations (9) and (12). For the sake of clarity, we re-write them here:

$$\begin{cases} \frac{d^2}{dt^2}\vec{r}_{SO} = \frac{\vec{F}_{CS}}{m_S} \\ \frac{d^2}{dt^2}\vec{r}_{CO} = \frac{\vec{F}_{SC}}{m_C} \\ \frac{d^2}{dt^2}\vec{r}_{PO} = \frac{\vec{F}_{CP}+\vec{F}_{SP}}{m_P} \end{cases} \quad (9) \qquad \begin{cases} \frac{d^2}{dt^2}\vec{r}_{SO} = \frac{\vec{F}_{CS}}{m_S} - \frac{\vec{F}_{SC}}{m_C} \\ \frac{d^2}{dt^2}\vec{r}_{CO} = 0 \\ \frac{d^2}{dt^2}\vec{r}_{PO} = \frac{\vec{F}_{CP}+\vec{F}_{SP}}{m_P} - \frac{\vec{F}_{SC}}{m_C} \end{cases} \quad (12)$$

When the Earth moves – i.e. when system S-C-P is governed by equations (9) – then the Sun's motion is described by equation $\frac{d^2}{dt^2}\vec{r}_{SO} = \frac{\vec{F}_{CS}}{m_S}$, whereas, when the Earth is at rest – i.e. system S-C-P is governed by equations (12) – then the Sun's motion is described by equation $\frac{d^2}{dt^2}\vec{r}_{SO} = \frac{\vec{F}_{CS}}{m_S} - \frac{\vec{F}_{SC}}{m_C}$, which is different from the former one.

In other words, when motion depends on absolute parameters – i.e. parameters that can only be measured with respect to absolute space – Newton's corollary no longer holds true, and it follows that the motion of bodies relative to each other no longer occurs *as if acceleration* $\vec{a}$ *were not acting*. And this proves that fluid particle P's motion *with respect to a moving Earth* no longer occurs *as if the Earth were at rest*. In such a curiously 'warped' universe, tide phenomena would therefore be a perfectly adequate proof (or, at the very least, a necessary consequence) of the Earth's motion in space! This should suffice to show why, and in what sense, the very relative-positional nature of our normal gravitation law cancels out the Earth's motion contribution to tide equations. And why, in our Newtonian universe, any terrestrial phenomena like tidal ebb and flow cannot attain to the status of a physical proof of the Earth's annual motion.

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45 This means that gravitational forces acting between two bodies in the universe are no longer easily describable in relation to each other, i.e. irrespective of the place where the bodies effectively are in the universe and irrespective of their 'state' of motion or rest (which are defined with respect to absolute space). Other possibilities, for example, may be: G depends on the absolute motion of the S-C-P system's centre of mass; G depends on any variable which in turn depends on a privileged point in the universe; any other more complicated function of absolute quantities in the universe.

5. Simulate the winds and the sea

5.1 Comets and winds

Of the many questions which have been raised regarding the motivations behind the long argument about the nature of comets that was sparked off in 1619 by Galileo's *Discourse on Comets*[1] – in which he opposed the scientific view of the Jesuit Orazio Grassi, professor of mathematics at the Collegio Romano, and which was to lead to the publication of *The Assayer* in 1623 and eventually to Grassis final answer, the *Ratio Ponderum Librae et Simbellae* – the importance of the connection between the series of experiments on the motion of fluids inside a rotating bucket and the theory of tides and trade winds has so far been virtually overlooked[2].

[1] The work was published in 1619 by Galileo's follower Mario Guiducci under the title *Discorso delle Comete di Mario Guiducci fatto da lui nell' Accademia Fiorentina nel suo medesimo consolato*, Firenze, Nella Stamperia di Pietro Cecconelli, 1619. On the important question of the role that Galileo played in the actual writing of the *Discourse on Comets* and on the reasons that allow us to consider the whole *Discourse* as being virtually Galileo's, see A. Favaro's introduction in *Le opere di Galileo Galilei*, VI, pp. 5–19. In particular, as a manuscript of the entire work with Galileo's comments and corrections has been preserved, A. Favaro had Galileo's hand-written notes and passages printed in characters of different size in the National Edition.

[2] Despite the common view that Galileo had a theory of his own to put forward in order to attack Orazio Grassi's *De Tribus Cometis Anni MDCXVIII Disputatio Astronomica* published in Rome in 1619, it must be said that nowhere in the *Discourse on Comets* is a Galilean cometary theory to be found. Galileo's main concerns are to counter Tycho Brahe's proposal that comets are to be ranked among celestial bodies on the ground of the absence of any parallax, to deal with optical questions relating to the use of the telescope and to argue against Aristotle, who maintains that comets are earthy exhalations rising up to the borderline of the elemental region and ignited by the rotational motion of the concave surface of the Lunar orb. This is clearly recognised by S. Drake in his, *Galileo at Work. His Scientific Biography, op. cit*, pp. 276–288. See also S. Drake, *Galileo: Pioneer Scientist*, Toronto, University of Toronto Press, 1990, pp. 179–191. A. Fantoli puts forward the same argument in A. Fantoli, *Galileo. For Copernicanism and for the Church*, 2nd ed. Revised and Corrected, The University of Notre Dame Press, Notre Dame, 1996 (1st ed. 1994), pp. 276–280. Although Galileo speaks of 'cometary matter' that could have come down to the elemental region and then been raised up again as well as of 'sublimated fumes, vapours and exhalations', he is very cautious in advancing different – and, for him, equally possible – hypotheses on the nature and origin of the matter of comets. See the most 'explicit' passage of Galileo's *Discourse on Comets, in Le opere di Galileo Galilei*, VI, p. 93. On Galileo's 'theory of comets' see also J. Casanovas, "Il Padre Orazio Grassi e le Comete dell' Anno 1618", P. Galluzzi [ed.], *Novità Celesti e Crisi del Sapere*, Firenze, Giunti Barbera, 1984, pp. 307–313; M.L. Soppelsa, *Genesi del metodo galileiano e tramonto dell'aristotelismo nella Scuola di Padova*, Padova, Antenore, 1974, pp. 46–65; W. Shea, "Galileo and the Controversy of Comets", *Physis*, XII, 1970, pp. 5–35. See also S. Drake's Introduction in S. Drake [ed.], *The Controversy on the Comets of 1618*, Philadelphia, University of Pennsylvania Press, 1960, pp. VII–XXV. The book contains translations into English with useful notes of the main texts on the controversy by Galileo, O. Grassi and M.Guiducci, with the exception of Grassis answer to *The Assayer*, the *Ratio Ponderum Librae et Simbellae* because, according to the editor, the *Ratio* deals "mostly with

Yet, the relationship between these experiments and Galileo's theory of both tides and trade winds reveals a great deal about his attitudes regarding not only his philosophical assumption that physics has to establish truth through hypotheses that seek to explain ascertained facts, but also the methodological function of simplified theoretical and mechanical models as conceptual bridges between more complete and abstract theories and their singular and empirically verifiable consequences. Referring to Aristotle's ancient theory of comets as earthy vapours ignited by the swift revolution of the concave of the Lunar orb in the region that borders on the elemental sublunary world, Galileo asserts that:

> [...] Aristotle's discourse [on comets] is, if I am not wrong, full of suppositions, which, even though not manifestly false, are nevertheless much in need of being proved: yet, what one supposes in science should be quite apparent.[3]

At the hub of Aristotle's theory is, first and foremost, his assumption that hot and dry exhalations can be set in motion by the revolution of the concave of the Lunar orb, inside which they are contained. Now, Galileo proposes a simple, though powerful and ingenious mechanical model and an experiment to assess, in the light of observations that are now made possible by his new experimental apparatus, Aristotle's pretensions to a physical mechanism whereby comets are formed.

> If we make a hollow vessel, which is perfectly round and with smooth surfaces, rotate about its own centre at whatever speed, the air therein will remain at rest, as is clearly shown by the flamelet of a small burning candle placed in the concave of the vessel, which not only will not blow out, but will not even be bent by the air contiguous to the vessels surface. Yet, if the air were moved at high speed, any, even greater, flame should blow out.[4]

The importance of the mechanical model – whose origin, according to the testimony of Mario Guiducci,[5] may have been an incidental observation made in a potter's workshop[6] – was first, though implicitly, recognised by Galileo's opponent, Orazio Grassi, in his *Libra Astronomica ac Philosophica*,[7] which was published as an answer to the *Discourse on the Comets*. Later on, the same experiments were discussed again in Galileo's

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minutiae of the dispute" (p. XX). Nevertheless, the *Ratio* has turned out to be very important in relation to the long history of the 'bucket experiments'.

3 G. Galilei, *Discourse on Comets*, in *Le opere di Galileo Galilei*, VI, p. 53.

4 *Ibid.*, VI, p. 55.

5 After the publication of Grassi's *Libra Astronomica ac Philosophica*, Galileo's pupil Mario Guiducci felt that he himself had to reply, and in 1620 he published a brief answer in the form of a letter to the Jesuit Father Tarquinio Galluzzi, whose title is *Lettera al M.R.P. Tarquinio Galluzzi della Compagnia di Giesu, di Mario Guiducci nella quale si giustifica delle imputazioni dategli da Lotario Sarsi Sigensano nella Libra Astronomica e Filosofica*, Firenze, 1620. See the complete text of Guiducci's letter in *Le opere di Galileo Galilei*, VI, pp. 187–196, and his reference to the potter's workshop on p. 193.

6 An identical device was described a few years earlier by Antonio Santucci, a contemporary of Galileo's, in a tract on comets first published in 1611. See the discussion below.

7 The complete title is *Libra Astronomica ac Philosophica qua Galilaei Galilaei opiniones de cometis a Mario Guiducio in Florentinae Academia expositae, atque in lucem nuper editae,*

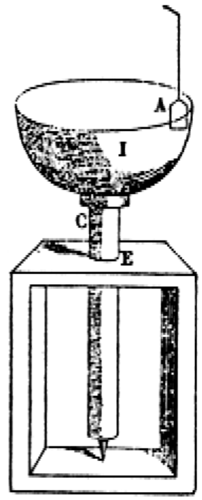

Fig. 5.1. Grassi's first 'contrivance', a piece of paper hanging from a thread. In *Le Opere di Galileo Galilei*, VI, p. 156. In Galileo's experiment there is a small candle instead of the piece of paper

Assayer and eventually in Grassi's final reply, the *Ratio Ponderum Librae et Simbellae*.[8] In the *Libra Astronomica ac Philosophica* a long and careful discussion is given over to the interpretation of Galileo's apparatus and to the different results Grassi claimed to have obtained after repeating the experiments, with even more sophisticated equipment, at the Collegio Romano before many witnesses, among whom he cites the Jesuit Fathers themselves and one of Galileo's most intimate friends in Rome, Virginio Cesarini.[9]

The bone of contention was simply the position inside the vessel in which what we could call the 'detecting device' had to be placed. As we have seen, the detector was the flamelet of a small candle in Galileo's original version of the experiment. In Grassi's, it was a small piece of paper hanging from a thin thread (Fig. 5.1) or, in a second case (Fig. 5.2), a more complicated balance-like device, consisting of a light horizontal arm

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examinantur a Lothario Sarsio Sigensano, Perusiae, 1619. The whole text is printed together with Galileo's postils in *Le opere di Galileo Galilei*, VI, pp. 111–180. Lothario Sarsio Sigensano is an anagram for Horatio Grassi Salonensi, who was from Savona.

[8] It is worth noting that of the many objections from Grassi's *Libra* which Guiducci claims he could counter, he chooses the discussion of the experiment of the rotating vessel and maintains that the proof was incidentally originated by an observation he had made in the workshop of a potter and that this had been seen afterwards by Galileo himself. See *Le opere di Galileo Galilei*, VI, pp. 193–195. See also the relevant passages of the *Assayer* in *Le opere di Galileo Galilei*, VI, pp. 325–329 and Grassi's final intervention in *Ratio Ponderum Librae et Simbellae: in qua quid e Lotharii Sarsii Libra Astronomica, quidque e Galilei Galilei Simbellatore, De Cometis statuendum sit, collatis utriusque rationum momentis, Philosophorum arbitrio proponitur*, Lutetiae Parisiorum, 1626, in *Le opere di Galileo Galilei*, VI, pp. 471–475.

[9] See O. Grassi, *Libra Astronomica ac Philosophica*, in *Le opere di Galileo Galilei*, VI, pp. 155–160.

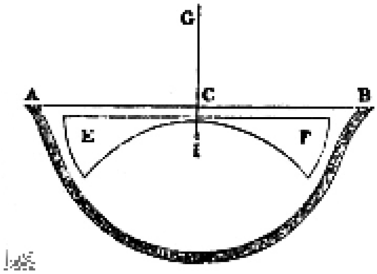

Fig. 5.2. Grassi's balance-like device. From *Le Opere di Galileo Galileo*. VI, p. 159. AB is the vessel closed by a lid and EF is the rotating detector

suspended by a thread and able to rotate inside the vessel – which was closed by a lid – thanks to small vanes of papyrus placed at its tips near the internal surface of the vessel. Obviously, the nearer to the surface the tips of the detector are, the more sensitive they are to its impulsion.

This is more or less what was observed and described by Giovanni Ciampoli in another important document on this question, a letter to Galileo dated 24th August 1619, just before the publication of Grassi's *Libra Astronomica ac Philosophica*.[10] The phenomenon is due to the induced motion of the thin air stratum contiguous to the internal rotating surface, which in turn depends on the ability of the vessel's surface and the nearest air strata to interact reciprocally with shear forces. It is a well known phenomenon of fluid dynamics caused by viscous property of real fluids, whose effects become manifest inside what is nowadays called the 'boundary layer'- the thin layer of fluid near the bodies' surface where the speed of the fluid current decreases until, right at the surface, it becomes zero.

Galileo's interpretation of these phenomena was basically correct and perfectly coherent with his previous explanation of the trade winds, which had been circulated three years earlier in 1616 in his *Discourse on the Tides*. He thinks that the nearer thin air layers could well be dragged by the rotating surfaces of the vessel, whose roughness, even though scarcely detectable to the naked eye, he holds responsible for the macroscopic

[10] The testimony is significant because it tells us about the intensive experimental activity in which Grassi was evidently engaged and underlines the importance attached to these experiments by both parties. After having reported on the experiments performed with the rotating vessel, firstly full of water and secondly of air – experiments to which Ciampoli had been witness and that had turned out to be very dubious – he goes on to describe the second, balance-like apparatus and says that it was devised by Grassi after the observations made with the first one had not produced any conclusive results. Ciampoli says also that, having recently been informed by the Jesuit himself that the experiments with the more sophisticated model had been repeated successfully, he wanted to see them immediately and went to the Collegio Romano, where he was finally able to observe that the balance-like arm actually rotated in the same direction as that of the vessel. See *Le opere di Galileo Galilei*, XVIII, pp. 423–425.

effect.[11] Nevertheless, he argues, if one places the detecting candle at a sufficient distance from the vessel's internal wall, then there can be no doubt that, being undisturbed by the motion of the apparatus, which is definitely unable to set the inner air in motion, its flame will remain at rest. In like manner, he concludes, the concave of the Lunar orb could not excite any circular motion in the upper fluid strata of the elemental region, especially as, being a crystalline sphere, it must be deemed to be very smooth and lacking any kind of surface texture.[12] In other words, the upper strata of the elemental region are absolutely calm and, as such, could well constitute a stable region through which vapours of some sort after rising from below are transported, or even be the very place where they come to rest, having emanated from the celestial space above. And these vapours – whether rising from Earth or emanating from the celestial regions – could generate the cometary effects by partially reflecting and refracting the Sun's light.

At the same time, a stationary atmosphere is precisely what Galileo needs to explain the trade winds on a rotating Earth.[13] The potter's wheel that turns the pot, perhaps

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[11] See the *Assayer* in *Le opere di Galileo Galilei*, VI, p. 329, where Galileo also suggests that the boundary layer of the small vessel is not to be supposed thinner than that which would be generated by the Lunar concave. Thus, one should not be misled into believing that, owing to the small scale of the experimental apparatus, the greater Lunar orb could produce greater effects. It has to be said that there is indeed no correlation between the thickness of the critical stratum and the extension of the surface upon which it develops. An analogous and even more detailed observation was made by Galileo in a postil to Sarsi's *Libra*; see *Ibid.*, p. 157, where he stresses the fact that if the thickness of the boundary layer inside the vessel is, say, one digit, which could amount to a hundredth of the total diameter, then one must not deduce that the thickness of the layer near the concave of the Moon will be in the same proportion; on the contrary, it is just as thick as the other, namely, one digit.

[12] Galileo's way of proceeding is here merely hypothetical because he did not believe in the existence of crystalline spheres, let alone in any difference between the celestial and elemental regions. This is a good example of experimental physics applied to a standard astronomical subject. While annotating Grassi's *Libra,* he advances an interesting optical reason for refuting the alleged difference between celestial and earthly substance, noting that if one were willing to admit, as Grassi was, that the Lunar concave could be rough so as to improve its 'pulling ability', this should be revealed by the different refraction of light through this uneven medium, which in turn should make the fixed stars appear so erratic as to change their position every hour. Of course, Grassi's reasoning was a curious mixture of stuffy old arguments and crisp, newly-discovered facts, given that he invoked Galileo's telescopic findings, which had once and for all demonstrated the rough nature of the surface of the Moon, while at the same time pretending that the Lunar sphere, as being less noble than the Moon itself, could consequently be thought of as being more rough and less smooth. See Galileo's postil in *Le opere di Galileo Galilei*, VI, pp. 152–155 and the *Assayer* in *Ibid.*, p. 320, where the same reasoning is put forward.

[13] Galileo was thus able to deliver a comprehensive rebuttal of a criticism levelled at his theory by Grassi in the *Libra*. The Jesuit had claimed that, owing to the continuous blowing of winds and the resultant turbulent nature of the air near the Earth, terrestrial vapours could not rise into the upper atmosphere. Although his objection was ineffectual because Galileo had not specified the origin of the vapours – whose provenance, in his view, might even be celestial – in a postil in Latin to Grassi's text he rejected the objection by simply noting that the fluctuations in the air induced by the winds were scarcely perceptible above the higher mountains, where he supposed that, not being dragged by its natural vessels on the Earth's surface, the atmosphere must be calm

once observed by Galileo in a workshop, has now become in his mind a perfect working mechanical model not of the inexistent crystalline orbs but of one of the more tangible effects of Earth's diurnal rotation, the trade winds.

It is worth noting that in a tract published in 1611 by Antonio Santucci, which has recently been studied by Thomas Settle,[14] an experiment, similar to those set up by Galileo and thereafter repeated by Grassi, was proposed with the aim of showing that upper regions of the sublunary world are not subject to diurnal rotation – which for Santucci was not Earth's rotation, but the traditional whirling of the solid, crystalline spheres. Santucci's thesis on comets was explicitly set forth in the title of his work and amounted to maintaining that these phenomena were produced not in the elemental region as Aristotle had claimed, but in the heavens.[15] Apart from the welter of new and old ideas cobbled together by Santucci,[16] it is not clear what his experiment is supposed to have demonstrated relative to his thesis on the celestial origin of comets. Still, what seems clear, and is of concern to us, is his attempt to use an experiment in order to prove that the region of air and fire cannot move with the diurnal motion of the heavens. He claims to have set up a round, drum-shaped wooden vessel, made it rotate on its vertical axis and verified that if a small a lamp is hung inside the vessel 'not only is it not extinguished, it shows no flickering at all'.[17] Now, whether Santucci or an anonymous potter was the inventor of the apparatus later on 're-invented' by Galileo,

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and quiet, as he had explained earlier on in the *Discourse on the Tides*. See *Le opere di Galileo Galilei*, VI, p. 136. Galileo maintains that the air around the Earth is at rest with respect to a non-rotating terrestrial observer, although a small portion of it is set and kept in motion near the surface of the planet by its diurnal rotation.

[14] T. Settle, "Antonio Santucci, his 'New Tractatus on Comets', and Galileo", in P. Galluzzi [ed.], *Novità Celesti e Crisi del Sapere, op. cit*, pp. 229–238.

[15] The complete title in Italian is "*Trattato nuovo delle comete, che le siano prodotte in Cielo, e non nella regione dell' aria, come alcuni dicono, . . . Con l' aggiunta che le Sphere del Fuoco, e dell' Aria non si muovono di moto circolare delle 24 hore*", Firenze, Giovanni Antonio Caneo, 1611. See, T. Settle, "Antonio Santucci, his 'New Tractatus on Comets', and Galileo", in P. Galluzzi [ed.], *Novità Celesti e Crisi del Sapere, op. cit*, p. 229. A second, identical edition was published in 1619. My quotations are from this second edition. The ultimate cause of comets was, in Santucci's view the creative power of God, which confers on some parts of the heavens "the ability to receive from the Sun, by means of the second causes of the other constellations, the shining form that commonly is named comet", (A. Santucci, *op. cit*, p. 23).

[16] In Settle's opinion, Santucci's [...] arguments are rambling and repetitious and tend to stumble over each other...", *Ibid*., p. 235. Indeed, the same arguments, and even the same sentences, are repeated again and again throughout the short tract. The 'radical' Aristotelian Santucci sets out to prove that Aristotle himself was wrong in admitting the circular motion of the upper region of the elements in order to justify the fact that comets, though being earthly exhalations and vapours, nevertheless appear to rotate diurnally. His main argument against the earthly nature of comets and their originating in the sublunary world relies on the absence of any significant parallax which should exist if one assumes – as he does – that there is a great difference between the radius of the sphere of the elemental region and that of the sphere of the fixed stars. Notwithstanding his Aristotelianism, he does not hesitate to apply an experiment of terrestrial physics to astronomy in order to dismiss the possibility of elemental circular motion.

[17] A. Santucci, *op. cit*, p. 115.

what matters most is that we do not find in Santucci any attempt to interpret the mechanics of the dragging phenomenon – which was at the heart of the much more sophisticated dispute between Galileo and Grassi over the correct interpretation of the experimental findings. Nor could he suspect that such experiments would represent in Galileo's mind an effective tool with which to attack the traditional conception of the separation between terrestrial physics and celestial astronomy, rather than merely furnishing a few empirical observations with which to settle a dispute on the origin of comets.[18]

Rather, it looks as if Santucci, who would appear to have been converted to Copernicanism in the last phase of his career,[19] is nevertheless at a loss as to how to rid himself of his preconceptions regarding the traditional dogma of the crystalline orbs. In this, he somehow reminds us of Father Grassi's final intellectual conclusions regarding the physics of fluid motions inside vessels, which culminated in the publication of the *Ratio Ponderum Librae et Simbellae* in 1626. Grassi's last word on the question – as it emerges from his continual discussion of the disputed experiment (and to which, in the last analysis, he contributes little fresh insight) – is precisely an attempt to re-focus the entire question on the solid orb's ability to carry along the fluids contained therein by virtue of their being contained in enclosed spaces.

First of all, Grassi does not understand Galileo's interpretation of the boundary layer because he concedes that it is correct only in an open vessel, where all the upper part of the contained air is in contact with the air of the external environment, but he disputes that the same goes for a closed vessel. Recalling a third version of the experiment he had proposed in the *Libra*, consisting of a rotating glass sphere inside which a circular, flat detector was hung, he now introduces a further modification by proposing to collocate the Galilean 'detecting device', the burning candle, directly inside the sphere. Having 'adjusted' his apparatus, he claims openly that only if contained in enclosed spaces are the entire fluid masses set in motion by the revolving of the receptacles, as he asserts can be verified if the experiment is carried out in a glass sphere. Thus, since the enclosed air is

> separated from the air of the surrounding environment, it will not be necessary to place the candle near the wall; for even in locations removed from the wall and near the middle, the flame's flickering will indicate that the parts of air far from the surface also rotate.[20]

As is evident, Grassi is stressing the fact that the Galilean experimental conditions do not adequately represent the real phenomena; he maintains that a more complex apparatus such as the glass sphere – a really working model of the crystalline orb – will demonstrate that the entire internal air mass moves along with the vessel. Whether he is busily adapting the new experimental physics to an old cosmological tenet is open to question; what, however is certain is that he does not refrain from countering his

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[18] *Ibid.*, p. 236. T. Settle is not sure whether Galileo heard of Santucci's drum-shaped vessel and asserts that he "may have taken the idea of the experiment from Santucci or both may have depended on some third source or the common knowledge of the day", but he does not mention Guiducci's precise testimony.

[19] *Ibid.*, pp. 234–235.

[20] O. Grassi, *Ratio Ponderum Librae et Simbellae..., op. cit*, in *Le opere di Galileo Galilei*, VI, p. 474.

adversary's theses on the new ground of experimental philosophy. And, as the war of interpretation makes clear, the actual ability of the experiment to furnish a definitive proof is here at stake and, more importantly, its being a legitimate tool to investigate even physical phenomena that occur in the heavens is evidently acknowledged. Although Grassi does not avoid of somehow naïvely 'distorting' the data – as he goes so far as to assert that 'even near the middle' the flame will flicker, whereas clearly in the middle there can be no motion at all – his reports on the experiments are always clear and convincing.

What is more important and primarily concerns our study is to recognise that the long history of the dispute as to the correct theoretical analysis of the experimental findings – ranging, as it did, from Galileo's first proposal of the rotating vessel experiment in the *Discourse on Comets* in 1619, through Grassi's *Libra* and Galileo's *Assayer*, up until Grassi's final reply in the *Ratio Ponderum* in 1626 – illustrates the very complexity of the empirical background against which the debate rages and highlights the force with which experiment and observation impinge upon the philosophical attitudes of the contenders.

While new and more sophisticated experiments were carried out by Father Grassi at the Collegio Romano, Galileo refrained from replying to the new findings of his opponent and apparently stuck to his initial position. Whether Galileo had himself tried to repeat the experiments proposed by Grassi in the *Libra*, we do not know. In the *Assayer* he gives no obvious clue to the truth of the matter, even though he does let fall an indirect hint in saying that he is not going to discuss Grassi's complete set of experiments simply because they were carried out before the selfsame person to whom the work is dedicated, Virginio Cesarini. For Galileo, the experiments are simply a matter of fact, and, as such, cannot be called into question. What he calls into question is Grassi's misrepresentation of his own experimental results and the Father's attributing to him what he never thought.[21] There is little doubt that Galileo's judicious shift of ground can be construed as an attempt to get away with having recourse to a rhetorical strategy to oppose the new arguments stemming from the formidable series of experiments carried out at the Collegio Romano.

Not only are these experiments crucial to a correct historiographical evaluation of the opposing claims of the two contenders – both of whom demonstrate thereby both an embryonic understanding of the motion of fluids inside buckets and/or spheres and the ability to come up with sophisticated theoretical models by which to grapple with complex experimental data – they are also relevant to Galileo's trade winds theory and, consequently, to his tide theory, a fact that has hitherto gone largely unheeded.

This connection is explicitly set forth by the Aristotelian Simplicio in the Fourth Day of the *Dialogue* in an attempt to reverse Galileo's argument regarding the trade winds. According to the Aristotelian School

> the element of the fire and a large part of the air are carried around in the diurnal rotation from east to west by contact with the lunar sphere as their containing vessel. [...] Thus, as you [Salviati-Galileo] declared that the air surrounding the mountain ranges is carried around by the roughness of the moving Earth, we [Aristotelians] say the converse – that all the element of the air is carried around

[21] The *Assayer*, in *Le opere di Galileo Galilei*, VI, p. 325–327. Galileo points out that Grassi falsely attributes to him the idea that even the water inside the vessel would not be carried along by the vessel's wall, such an example never having been put forward in Galileo-Guiducci's *Discourse*.

> by the motions of the heavens [...]. And as this celestial movement is powerful enough to carry the free air with it, we may say quite reasonably that it contributes this same motion to the moveable water. [...] Perhaps from that same coursing of the water, tides also may arise....[22]

In an Aristotelian context, a stationary Earth and a rotating heaven could well account for the constant breeze of the equatorial regions without displacing the Earth from the confortable position at the centre of the cosmos, were it not for a simple physical argument. Even assuming that an 'element of fire' and a 'lunar sphere' exist

> there is no reason for us to believe that fire, *by simple contact with a surface that you yourself* [Simplicio] *consider remarkably smooth and even*, should in its entire extent be carried around in a motion foreign to its own inclination. This has been proved throughout *The Assayer*, and demonstrated by *sensible experiments*. Beyond this, there is the further improbability of such motions being transferred from most subtle fire to the air, which is much denser....[23]

Here Galileo's answer to the Aristotelian objection refers evidently to the set of experiments discussed mainly in *The Assayer*, but which, as we have seen, are part of the complex controversy on comets. There can be no doubt about the importance of this snippet of knowledge – the impossibility of motion of entire masses of fluid by means of simple contact with smooth rotating surfaces – which Galileo had acquired a few years earlier from the background of 'sensible experiments' regarding the effects of whirling vessels on the air contained therein. It is also true that, despite the crucial point raised by Simplicio – the celestial movement through fire and air 'contributes this same motion to the moveable water' – and despite the fact that it posits a serious threat to the whole of Galileo's tide theory – 'tides also may arise...' – he does not bother to give the reader any further detail about these experiments, let alone about the sequel of objections and possible interpretations which fuelled the dispute with Father Grassi.

Yet, these experiments are the only evidence presented by Galileo to disprove Simplicio's 'not only entirely improbable, but absolutely impossible and false' idea. Were there any good reasons on Galileo's part not to emphasize the role of these experiments? Was Galileo's attitude just a tactic suggested to him by prudence, in order not to awake an old quarrel with one of the best scientists of the Catholic Church, right at the moment in which he was about to publish a Coperincan treatise, notwithstanding the prohibition decree issued in March 1616 by the Congregation of the Index? Or, were there other purely physical motivations behind his reluctancy? Although, in the light of the events which followed the publication of the *Dialogue* in 1632 and which eventually led to Galileo's trial and abjuration in 1633, it may be natural to draw the conclusion that Galileo simply wanted to adopt a cautious stance, there is no reason to rule out the possibility that other, more 'technical' reasons – which might have been lurking in Galileo's mind since receiving the letter from Giovanni Ciampoli in 1619 that reported on the impressive results of the experiments carried out at the Collegio Romano – contributed to his decision. If

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[22] G. Galilei, *Dialogue Concerning the Two Chief World Systems, op. cit*, p. 442.

[23] G. Galilei, *Dialogue Concerning the Two Chief World Systems, op. cit*, p. 443. The Italics are mine.

so, in order to discover their bearing on the whole question, it is important to understand better what that set of experiments may have revealed or, perhaps, only suggested about the nature of the dynamic behaviour of air, rotating buckets and glass spheres.

Therefore, with a view to acquiring firsthand knowledge so as to weigh up the experimental evidence confronting the two scientists, and also to gain further insight into the development of Grassis theories with the gradual refinement of his apparatus, I decided to reconstruct the devices described by Galileo and the Jesuit mathematician and repeat the entire set of experiments on motion of air inside whirling vessels.

5.2 Rotating buckets and the terrestrial atmosphere

I built a simple rotating device with a turntable on which I placed a Plexiglas vessel and a glass sphere in order to simulate the Galilean open vessel and Grassi's model of the crystalline closed orb. Using small candles on a wooden support and rotating balance-like detectors hanging from a thread and shaped after the sketches given in Grassi's *Libra*, I carried out four experiments,[24] namely, 1) Galileo's original open vessel proposal with the candle as detector, 2) Grassi's modified version of it with a closed vessel and the balance-like detector, 3) Grassi's sphere with the balance-like detector and 4) Grassis sphere with the candle. The results are as follows:

Experiment 1). Galileo's claim that the candles flamelet inside an open vessel does not move at all and, by inference, that the air inside the vessel does not move either, is correct. The only significant effect of the vessels motion can be observed in close proximity to the vessels wall.

Experiment 2). Grassi's claim that his air-propelled detector starts moving along with the vessels motion after a few rotations is correct. The effect is clearly observed, however, only when the vessel is closed with a lid, exactly as Grassi himself maintained in the *Libra*.

Experiment 3). Grassi's second apparatus – the model of the crystalline orb – works precisely as he described in the *Libra*. The detector moves, as it does in the closed vessel of experiment 2).

Experiment 4). The candle inside the model of the crystalline orb bends clearly in the direction of the vessel's motion even when it is located as much as three fifths of the way from the vessel wall to the axis of rotation. If placed closer to the centre, it no longer bends in one direction or another but it is still evidently agitated by the air and oscillates irregularly. This agrees with what Grassi reported in the *Ratio Ponderum* in 1626.

The most important of all the experiments are doubtless the first and the fourth. In a sense, they epitomize the entire development of the scientific dispute. For, what we have here is both the confirmation of Galileo's plain interpretation of a straightforward piece of experimental evidence – the bending of the flamelet as a consequence of the motion of the air inside the whirling vessel – and an equally canny adjustment of the empirical conditions on the part of Father Grassi in order to shift the focus of his dissension. As

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[24] See *Appendix 3* for some photographs of the apparatuses I set up and more details on the actual operations involved in carrying out the tests.

regards the fourth experiment, the Jesuit did not mention it at all in the *Libra*, but proposed it only later on in the *Ratio Ponderum*. A delay which tells us about the substantial development of his interpretation of the phenomena and also about his apparently disconcerting double role in, on the one hand, opening up the debate by contributing further experimental evidence, and, on the other, clinging to or resuming the old cosmological axiom of the solid orbs.[25]

Now, although putting a model of a celestial orb at work in a laboratory may sound extravagant and although it stresses a noticeable difference between Galileo's reasonably coherent position in the context of his new physics – embracing both the terrestrial and the celestial world – and Grassi's difficulty in dealing with a *new* physics and an *old* astronomical tradition, yet, there is no getting away from the fact that such an awkward attempt is based on solid experimental grounds. It may be hard to believe that crystalline orbs exist, but if they exist and if glass spheres are a good model of their behaviour, it is impossible to question their ability to drag the air they contain. To all intents and purposes, the objection of the Aristotelian Simplicio in the Fourth Day of the *Dialogue* could boast an experimental basis as reasonable as that furnished a few years earlier by the Aristotelian Father Grassi.[26]

In Galileo's physical astronomy there is no place whatsoever for any sort of solid orb. His physics applies equally well both to the celestial region and to the Earth, so that he must have felt justified in no longer discussing Grassi's extension to the crystalline orb of his original experiment. The open vessel is for him a working model of the rotating Earth, around which the atmosphere is still, except in the lower regions near the surface where it is carried along by the natural orographic features of our planet. No other surface encloses the upper layers of the air. On the other hand, Grassi's glass sphere is in Galileo's view a fictitious model of an unreal orb. Let us not forget that in the years of the dispute with the Jesuit Father, Galileo, like everyone else, was forbidden to write on Copernican astronomy. Yet, his earlier Copernican *Discourse on the Tides* predicted a physical effect on the Earth's surface – the trade winds – whose physical mechanism was now corroborated by a new experiment, the essential outcome of which was hardly questionable, even though it was perhaps not perceived by his critics as being directly connected with Copernicus.

Galileo's physics was gradually chipping away at the axioms of Aristotelian cosmology and at the old tenet of Earth's immobility. He confounded his best opponents, not only by taking the lead in excogitating simple mechanical and 'terrestrial' models (rotating buckets) to test the consequences predicted by more theoretical 'celestial' models (the woad grindstone's double circular motion) but also by making his adversaries adopt ex-

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[25] See S. Drake, *Galileo at Work. His Scientific Biography, op. cit.*, pp. 277–278, who maintains that Grassi was now compelled by the intervention of higher Jesuit authority "to pledge overt allegiance to Aristotle". Grassi's initial *Dissertatio* did not espouse Aristotle's theory of comets. On the contrary, it is well known that the Roman Jesuits at that time favoured Tycho's astronomical system.

[26] One could also go so far as to argue that not only did Galileo deliberately disregard the results of those experiments, of which he must have been well aware, but that his intention in this passage of the *Dialogue* was to mock his Jesuit opponent under the guise of the Aristotelian Simplicio, certainly not a very brilliant intellectual.

perimental philosophy and terrestrial physics as the new paradoxical ground upon which they must now compete to save the traditional separation of astronomy and physics.

Having interpreted correctly the phenomenon occurring very near the internal wall of the vessel as a local phenomenon, however, Galileo was not able to make a second and fundamental step forward, namely, the linking of his concept of inertia – circular or rectilinear, as it may have been conceived of in his mind – with the problem of the presence or absence of motion in the air around the Earth. Nevertheless, it must be recognised that, in this regard, his correct intepretation of the experimental evidence, as suggested by the behaviour of fluids inside revolving vessels, led him astray. In Galileo's physics the mechanism responsible for dragging the air along near the internal surface is to be attributed to the roughness of the walls. The inner air must rest. Accordingly, the fluid mass of the greatest portion of the atmosphere is at rest because there is no physical cause that could impart a rotational motion to its superior strata. In other words, air, despite its being unquestionably heavy, cannot be thought of as sharing fully the property of inertia with ordinary earthly substances – that is to say, both the ability to resist being accelerated and the ability to maintain the motion imparted to it. Strange as it may appear to our Newtonian mental outlook, we are bound to conclude that in Galileo's physics the element air clearly possesses the former ability, but lacks the latter.

There is an objection that can be raised here against Galileo and which is legitimate in that it is well 'within the compass' of pre-Newtonian mechanics. If the entire air region does not rotate along with the Earth's diurnal rotation, why does it orbit the Sun, together with the planet it girdles? From a modern point of view, one cannot fail to see a patent lack of internal coherence in Galileo's physics. Yet, it must be said that, for him, all fluids, like water and air, while lacking any ability to resist being divided – that is to say, having only the ability to act on bodies with forces that depend on their velocity – nevertheless possess an internal tendency to resist being pushed. In other words, in Galileo's fluid-dynamics they are endowed with only one half of the content we attribute today to the concept of inertia, namely, the property of resisting being 'moved faster or slower' (we talk of being 'accelerated'). Now, preserving motion is the second half of the concept of inertia that Galileo cannot attribute to the element 'air', though he attributes this second property to the element 'water'. From such a point of view, the orbitingEarth 'pushes' the air – at least the part in front of it – and, at the same time, drags its superficial strata and makes them rotate, preventing them from being left behind.[27]

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[27] Galileo's main ideas on fluids and fluid resistance to being divided are to be found in his treatise on *Bodies That Stay Atop Water, or Move In It* and in the numerous comments and replies he made during the development of the dispute on buoyancy with a few Aristotelian philosophers, which arose in 1611 as a 'court dispute' at the table of Grand Duke Cosimo II. See G. Galilei, *Discourse on the Bodies in Water* [ed. S. Drake], Urbana, Illinois University Press, 1960 – which is a facsimile edition with a commentary by S. Drake of an early English translation by Thomas Salusbury, published in London in 1665 – and, more recently, a translation in modern English by the same S. Drake in S. Drake, *Cause, Experiment and Science. A Galilean dialogue incorporating a new English translation of Galileo's Bodies That Stay Atop Water, or Move In It'*, Chicago, The University of Chicago Press, 1981. The original is published in *Le Opere di Galileo Galilei*, IV, pp. 61–141. As regards the meaning of 'court dispute', see M. Biagioli, *Galileo Courtier*, Chicago,

The atmosphere girdles the Earth, but the whole of it cannot revolve along with the solid terrestrial globe in its diurnal rotation. To achieve this, the upper air would need to be impelled by vast mountains reaching practically to the outer limits of the Earth's atmosphere and ideally oucropping all round the planet's surface, just as the thin air stratum near the wall of the whirling vessel is dragged round by its microscopic roughness inside the small layer that today we call the 'boundary layer'.[28]

The trade winds are a very interesting effect of a cause that Galileo dovetails neatly with the whole conceptual frame of his tide theory. In the Fourth Day of the *Dialogue* he has this amazing consequence of the Earth's diurnal rotation presented by his mouthpiece Salviati to the incredulous Aristotelian Simplicio, but in fact it was not derived from his brilliant grindstone model of flux and reflux, rather it was simply an effect of the 24-hours motion of the terrestrial globe about its polar axis. Unfortunately, as Galileo must well have realised, the trade winds are not a consequence of the double motion of the Earth – as his tide-generating acceleration unquestionably is – they are purely a consequence of diurnal rotation. And, as such, they cannot attain to the status of empirical proof of the double Copernican motion but merely to that of a singular physical effect coherent with the whole of his astronomical system. Salviati can only conclude his discussion of the trade winds by linking the effects manifested in the atmosphere by the air's 'primary current' with those visible in the sea waters. All in all, the outcome of the discussion, the strong cohesion between the Copernican astronomical system and the terrestrial physics of phenomena like the tides and the trade winds, intriguing as it may be, cannot efface a slight sense of disappointment. In the end, Galileo's science is about physical causes and experiments, whilst coherence is a matter of logic and no coherence in logic could make up for a lack of experimental evidence.

The University of Chicago Press, 1993, pp. 159–209, where the complex social dynamics at the Medicis court is described.

[28] In Galileo's conception of the physical interaction between fluids and bodies, the ability of a vessel's surface to drag air turns out not to depend on shear forces that arise between the surface and the fluid, but simply on what can be called a 'pushing effect', which is due to microscopic or macroscopic 'mountains and valleys' – on the Earth's surface or on vessels' walls, as the case may be – generated by the natural roughness of physical bodies. In other words, for him, drag comes to depend only on the speed of the body moving in the fluid and, consequently, there is no such thing as fluid 'ability to resist absolute or simple division', which is the language Galileo uses to refer to his opponents' claim that buoyancy partly depends on the shape of bodies. On the contrary, resistance experienced by bodies moving in fluids can only stem from the inertia of fluid particles, which resist being displaced and accelerated in different directions around the penetrating body. The clearest formulation of this principle is given in the massive *Answer to the objections of Lodovico delle Colombe and Vincenzo di Grazia against Galileo Galilei's Treatise 'Bodies That Stay Atop Water, or Move In It'*, originally published in 1615 and reprinted by Antonio Favaro in *Le Opere di Galileo Galilei*, IV, pp. 451–789. See especially pp. 670ff., 699ff. and 717ff. The *Answer*, which is nearly four times as long as the *Bodies*, is literally an hydrodynamics and physics laboratory where one can watch Galileo at work live.

> Now you see how the actions of the water and the air show themselves to be remarkably in accord with celestial observations in confirming the mobility of our terrestrial globe.[29]

What was urgently needed was either a mechanical model of the tide motions themselves or a model of at least some of their most spectacular effects, some ingenious device that was able to show, on the one hand, how the tides are effectively generated and why and, on the other, the workings of their 'amazing combinations of motions'.

5.3 Tide experiments: artificial vessels and tide-machines

To anticipate some of the conclusions, it must be stressed that Galileo almost certainly did not succeed in constructing a fully-operative working model that could simulate the tides precisely as they were predicted by his grindstone model – or at least he failed to make it work properly. For, although there is evidence that he had been trying to build some mechanical devices and that, throughout his long career, he was interested in similar tide-machines – about which he was duly informed by his correspondents – on the whole he was never able to capitalize satisfactorily on the bulk of experimental knowledge that he must have acquired by working both with smaller and simpler 'artificial vessels' and with possibly more pretentious machines. Nevertheless, it behooves the historian not to neglect any suggestion that may come from stories of even the most resounding failures but to try to understand why they did not turn out to be success stories and, therefore, never became part of history as we normally conceive of it, namely, as testimony presented in documentary form, be it printed treatises, correspondence, laboratory notes or whatever.

Indeed, Galileo's experimenting with mechanical models of the tides must have been a story of dispiriting alternations between hope and frustration, a doomed attempt to reproduce, here on Earth, the double motion of our globe and its effects on the sea waters. A behind-the-scenes story, which was eventually to fail to produce the long-awaited experimental evidence that Galileo had been striving for and whose sole partial success may have been the discovery – or possibly the confirmation – of the two fundamental laws of oscillation of basins. As one might expect, when the objectives are not achieved, the whole endeavour tends to be set at nought. What remains may be a few scattered textual fragments, and even if one collates all of them, one can only hope to reconstruct hypothetically what really occurred. Yet, in the case of Galileo's tide theory these fragments are all the more revealing because not only do they afford insight into the great diversity of empirical and theoretical approaches he adopted in doing his research, but they also cast new light on the relationship between his 'mental laboratory' and his workshop – where his mental processes were translated into mechanical devices and bench-tested.

In trying to reconstruct that behind-the-scenes story, I shall consider both what I call 'direct evidence', namely, that which can be deduced from the various texts, and what I propose to call 'indirect evidence', derived from reconstructions of some of his experiments.

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[29] G. Galilei, *Dialogue Concerning the Two Chief World Systems, op. cit*, p. 440.

My thesis is that from both the direct and the indirect evidence it is reasonable to infer, firstly, that Galileo carried out a lot of experimental work in order to work out his basic tide theory, secondly, that he found and/or possibly verified at least one of the oscillation laws – the law of depth – but, finally, that he was eventually forced to renounce in part the experimental ground upon which he hoped to be able to base the results he had obtained, being baffled by the very complexity of the undulatory nature of the tidal phenomena. Let us consider the direct evidence first. In April, 1626, a correspondent of Galileo's in Bologna, Cesare Marsili, wrote to him in Florence in order to inform him that an engineer had turned up in Bologna who claimed to be able to show by means of round-bottom glass flasks containing salt water that the flux and reflux of the sea was due to 'celestial and intrinsic virtue'.[30] Although Marsili himself had not seen the experiment, he assured Galileo that he would soon meet this engineer. A few days later, Galileo replied and said that the apparent flux and reflux in the flasks could simply be due to air being at a higher temperature during the day than during the night. He also added that, in his opinion, the salt water used by the engineer was simply an artifice to impress the audience and that the same effect would occur with fresh water as well. And then, this intriguing aside:

> And, I performed such a trick ['scherzo'] twenty years ago in Padua, but it has nothing to do with the flux and reflux of the sea, except as regards the name arbitrarily imposed on it by this experimenter.[31]

Galileo therefore carried out his 'scherzo' twenty years earlier in Padua in 1606, ten years before the *Discourse on Tides* in 1616. It has been demonstrated by Antonio Favaro that the 'scherzo' belongs to a series of experiments on heat and heat measurment that Galileo had performed in Padua in the first decade of the sixteenth century.[32] Although it is not possible to assert that this kind of experiment was somehow related to the study of the motions of the sea, the 'scherzo' could possibly have been performed in connection with Galileo's theory of tides or some other tide theory.

In 1610 Galileo was informed by Giuliano de' Medici, at that time ambassador in Prague, that a 'Fleming from England' was claiming to have invented a perpetual motion machine in the form of a crescent-shaped glass tube, inside which water could move to and fro between the two ends. Galileo was evidently interested and asked the ambassador to look into on the device.[33]

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[30] See *Le Opere di Galileo Galilei*, XIII, pp. 313–317.

[31] See *Le Opere di Galileo Galilei*, XIII, pp. 320. In June, Marsili wrote again to Galileo to confirm that, just as the latter had guessed, the quack's claim to have explained the flux and reflux was groundless. See *Ibid*., pp. 327–328

[32] Galileo's 'scherzo' must refer to some sort of experiment in relation to the series conducted in Padua in 1606, or even a few years earlier, on the effects of heat over the expansion of air and water inside glass flasks and which would eventually lead him to 'discover' the thermometer. See A. Favaro, *Galileo Galilei e lo studio di Padova*, 2 v., Padova, Editrice Antenore, 1966 (1st ed. Firenze, successori Le Monnier, 1883), v.1, pp. 193–212. According to Favaro, the experiment Galileo refers to as a 'joke' in his letter to Marsili is to be distinguished from those more clearly related to heat measurements.

[33] See *Le Opere di Galileo Galilei*, X, pp. 448 and 478.

Two years later Daniello Antonini, a pupil of Galileo who studied mathematics in Padua and later embarked on a military career, wrote to his master from Brussels, apparently referring to the same mysterious Fleming[34] from the Court of England and to his invention. Antonini told Galileo that the motion of the water inside the glass tube was claimed to be like that of the 'flux and reflux of the sea'. However, Antonini pointed out that the true cause of the motion was simply the 'variation in the air, namely, the heat and the cold' – and that this was well known to Galileo, for the latter had performed similar experiments earlier together with his pupil in Padua. The letter goes on to describe a new device invented by Antonini himself in order to show that the effects were clearly due to the expansion and contraction of the air caused by variations in temperature.[35] In a second letter written a week later, Antonini sent a complete sketch of the said Fleming's apparatus 'which is lodged with the king of England'.[36]

Exactly the same machine was described again many years later, in a letter to Galileo from Nicole Fabri de Peiresc dated 17th April 1635, and was now attributed to Cornelius Drebbel. On this occasion, the machine was explicitly depicted as a mechanical tide model. In Peiresc's own words

> [...] there are some people who claim that in the water contained in circular glass tubes, invented by Drebbel from Holland, there can be a natural motion [...] and these waters move twice in 24 hours, almost like the flux and reflux of the sea; [...] but when I saw one of these tubes, I could not find any relationship that was either regular or in proportion to the flux and reflux of the sea.[37]

To conclude, although we do not have any answer from Galileo to Antonini, nor any comment on Drebbel's device in what appears to be Galileo's reply to Peiresc – which has relatively recently been published – we can infer that, at the very least, Galileo was known by his correspondents to be interested in such devices and also that this sort of mechanical model was frequently related at that time to the phenomenon of the flux and reflux of the sea.[38] In other words, even though we cannot bring forward any explicit textual evidence of his working with the kind of tide-machines described by Antonini and Peiresc, experimental investigation of tide phenomena must have been doubtless amongst Galileo's research interests since his early period at Padua University – where, in all probability, he had been involved in experiments which had to do with the condensation and rarefaction of air and water inside glass flasks and tubes.

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[34] This Fleming is probably the inventor Cornelius Drebbel who stood high in the favour of king James I.

[35] See *Le Opere di Galileo Galilei*, XI, pp. 269–270.

[36] See *Ibid*., pp. 275–276. The machine and its workings are accurately described by Antonini.

[37] See *Le Opere di Galileo Galilei*, XVI, pp. 259–261.

[38] A letter from Galileo to Peiresc came to light in 1962 and was published by Stillman Drake along with a study of the context of the correspondance in his article "Galileo Gleanings XII: An unpublished letter of Galileo to Peiresc", *Isis*, Vol. 53, 1962, pp. 201–211. It is not clear whether the letter is the answer to Peiresc's second letter dated 17th April or to a previous letter dated 1st April that did not mention Drebbel's tide circular tube. If Galileo's was really the answer to the former, he does not comment on the tide model, but only on the workings of the magnetic-hydraulic clock that was the main subject of both the letters. He describes in detail a similar clock he had built many years before.

Whether Galileo performed experiments with various types of tide-machine akin to those mentioned above – whose effects were mainly due to temperature and pressure variations – is not at all clear; what is clear is that he invariably attributed these effects to their real physical causes without hesitation. It was always evident to him that their utility lay in the field of thermometry. Nor did he fail to distinguish carefully between this sort of apparatus and his own mechanical models of tidal motions. Let us recall that the word he used to refer to the Bologna engineer's round-bottomed glass flasks – in which salt water was contained only 'to impress the audience' – was 'scherzo'. What *he* had in mind was far from being 'scherzo'. It was nothing less than the idea of reproducing all the phenomena in his laboratory by means of artificial vessels and machines that were effectively able to show the complexity of the amazing combinations of motions that his grindstone model entailed and his mechanics explained.

The 'grand idea' of simulating the tides and their causes here on Earth was clearly expressed for the first time in the *Discourse on the Tides* of 1616. In this regard, between the text of the *Discourse* and that of the Fourth Day of the *Dialogue* there are a few telling differences that must be duly taken into account in order to illuminate the background against which Galileo played the role of an eager experimenter in pursuit of terrestrial evidence of Copernicus' double motion.

Galileo posits the problem of how to simulate the motions of the tides in a laboratory by means of mechanical models not from a generic point of view but in relation to the law of non-uniform distribution of the tide-generating acceleration on the Earth's surface.

> The fifth detail [the non-uniform distribution law] must be considered much more carefully, insofar as it is at least very difficult, if not impossible, to reproduce it experimentally and practically. The effect is this. In artificial vessels which, like the boats mentioned above, move now more and now less swiftly, the acceleration... [Galileo goes on to formulate the non-uniform distribution law]. [...] Though many will consider it impossible that we could experiment with the effects of such an arrangement by means of machines and artificial vessels, nevertheless it is not entirely impossible; I have under construction a machine, which I shall explain at the proper time, and in which one can observe in detail the effects of this amazing combination of motions.[39]

As is clear from Galileo's words, he is thinking about constructing a special 'machine' that will be able to test specifically the unknown effects due to the non-uniform law according to which tide-generating acceleration is distributed on the Earth's surface. At the same time, he also distinguishes such a machine from the artificial vessels, which a few lines earlier he has compared with the boats that carry fresh water from one place

[39] G. Galilei, *Discourse on Tides, op. cit*, pp. 126–127. W. Shea has read this passage and the similar one in the Dialogue – which lacks the words 'which I shall explain at the proper time' – as evidence that Galileo must have 'had no more than a vague idea what a mechanical model would really look like'. See W. Shea, *Galileo's Intellectual Revolution, op. cit*, pp. 176–177. Nevertheless, Shea does not distinguish between 'machine' and 'artificial vessels'. As we shall see, although there are serious doubts that Galileo constructed a working tide-machine he must have experimented with more simple 'artificial vessels'.

to another over the sea – sixteen years later, in the *Dialogue*, he will evoke the vivid and famous image of large barges like these continually arriving from Lizzafusina filled with fresh water for the use of Venice. The passage quoted above is recast in the Fourth Day of the *Dialogue* and the words 'which I shall explain at a proper time' are deleted. Still, Galileo maintains he has a 'machine'.[40] In all probability, either such a machine was never built or, if it was built, it was never made to work adequately.

Yet, the story was not one of complete failure. For, although sixteen years after the *Discourse* he had to admit that a tide-machine was still more of a project than a working mechanical model, a few hints in the *Dialogue* suggest that a great amount of experimental work had been carried out with simpler 'artificial vessels'. When explaining the reasons why tides occur neither in small seas nor in lakes and ponds, Galileo asserts that as the containing vessel gradually acquires acceleration, so the water therein also receives the same acceleration with little or no resistance. Such an effect

> is also clearly seen in small artificial vessels in which the contained water is impressed with the same degrees of speed whenever the acceleration or retardation is made in slow and uniform increments.[41]

This passage is not present in the *Discourse on the Tides*.

Again, there is a second similar comment, which is not present in the *Discourse*, regarding a few particular points or 'events' – 'accidenti' is the Italian word – constituting in part what we have called the bench test of the oscillatory model's predictive power and which Galileo explains at length both in the *Discourse* and in the Fourth Day. He sums up as follows:

> It is a simple thing, I say, to understand the cause of these events [accidenti], because we have examples of them easily observable in all sorts of artificially manufactured vessels, in which the same effects are seen to follow naturally when we move them unevenly; that is, now accelerating and now retarding them.[42]

What exactly is Galileo here referring to? Well, whether he intends to refer strictly to the previous four 'events' he has described, or whether he wants to extend his comment to all the remaining 'events' thereafter referred to as 'accidenti', we cannot assert with any certainty; what we can learn from the *Dialogue*, at least with respect to the situation outlined in the *Discourse* in 1616, is that he must have been working with artificially manufactured vessels in order to verify the validity of his arguments as to 1) why tides do not occur in small seas, lakes and ponds; 2) why the tides are for the most part made in periods of six hours as in the Mediterranean sea; 3) why seas such as the Red Sea have no tides, unless they communicate with other larger seas; 4) why the tides are more marked at the innermost shoreline of a gulf and lowest at its centre. These are the four 'events' to which Galileo refers explicitly in the text of the last passage quoted above.[43]

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[40] G. Galileo, *Dialogue Concerning the Two Chief Worlds Systems, op. cit*, pp. 430–431.

[41] *Ibid*., p. 431.

[42] *Ibid*., p. 433.

[43] *Ibid*., pp. 431–433. As regards the remaining events, Galileo asserts that: 1) 'water moving slowly in a spacious channel must run very impetuously when it has to pass through a narrow

Let us now turn our attention to an amazing *lacuna*. As we have seen, what we find in the Fourth Day of the *Dialogue* of 1632 is Galileo's explicit and accurate assertion that at least four particular 'events' have been verified by means of experiments with small artificial vessels, which experiments he claims to have carried out. However, what we do not find, either in the *Dialogue* or in the *Discourse on Tides* of 1616, is any attempt to corroborate his two totally unintuitive laws of oscillation of water in an ocean basin. The astonishing absence of any such attempt speaks volumes about the exasperation Galileo must have felt at his inability to finally clinch matters since making his 'scherzo' about temperature and pressure at Padua University, some thirty years earlier. For, we have the correct answer to an amazingly complex problem, but we do not have any precise idea as to what mental pyrotechnics and/or what experimental procedures eventually led him to the solution to the problem. Nor do we understand why, after all, Galileo should not have emphasized – as he normally does in such cases – the importance and the novelty of his findings.

To sum up, I would argue that there is textual evidence that Galileo performed experiments with his small artificial vessels in order to investigate some of the major effects predicted by his oscillatory theory. On the other hand – strange as it may appear – he does not claim that such experiments formed the empirical foundation upon which the two laws regulating the vibrations of water inside an ocean basin were based. Why not? This omission is even more striking given that these laws are at the core of his oscillatory theory, and, as such, should have deserved much closer attention than that paid to the simpler 'events', whose epistemological status is – according to the *Dialogue* and the *Discourse* – that of being mere 'effects', whereas the former are 'laws'. Finally, we can assume that Galileo also worked with some sort of more advanced mechanical tide-machine – which in the end turned out to be unsuccessful. Unfortunately, he gives us no details whatsoever regarding the construction of such a device.

To remedy this omission we must return to the laboratory and have a glance at what I have called 'indirect evidence', namely, my attempts at simulating a few of his tide experiments. The following three conclusions summarize my findings, i.e. what I re-

place'; 2) a secondary cause of variation in the flux and reflux is the 'great quantity of water from the rivers that empty into the seas which are not vast, for which reason the water is seen to run always in the same direction in channels or straits through which such seas communicate, as happens in the Thracian Bosphorus, below Constantinople'. He also draws a third interesting conclusion from his principle of superimposition of waves according to which it may happen that 'two very large seas which are in communication through some narrow channel are found to have [...] a cause of flood in one at the very time the other is having the contrary movement. In this case extraordinary agitations are made in the channel through which they communicate, with opposite motions and vortexes and most dangerous churnings' (*ibid.*, pp. 434–436). The first author who has noticed all these consequences of Galileo's tide model appears to have been H.L. Burstyn in his article "Galileo's Attempt to Prove that the Earth Moves", *op. cit,* pp. 184–185. He concluded his paper with a brief list of notes aimed at comparing Galileo's observations and ideas 'with modern ones' (*ibid.*, p. 184).

discovered in performing some of the experiments that must have been the simplest of those that Galileo carried out with his numerous ‘artificially manufactured vessels’.[44]

1) First and foremost, I ascertained that, to a certain extent, the oscillation laws can actually be observed and easily formulated on the basis of simple measurements of periodicity. Galileo may have formulated these laws himself – particularly the depth law – and/or he may have verified them by performing analogous experiments.

2) I found a simple answer to the question as to why Galileo did not invoke his experimental findings as justification for the oscillation laws, and it may well explain the omission in question. I ‘saw’ that my accelerating and decelerating vessel could furnish under *some* circumstances empirical proof of the oscillation laws, but not in *all* circumstances. By filling the vessel with different quantities of water, that is, by varying the depth of the basin, I found that it is virtually impossible to describe the vibrations of shallow water – as Galileo strived to do – with some accuracy, even though qualitatively, in terms of one simple oscillation of the free surface (the surface in contact with air). The multidimensional undulatory nature of the phenomena – the superimpositions of what we nowadays call ‘the modes or patterns of vibration’ or ‘natural modes’ – is the most evident effect that strikes the observer, and the associated ‘modal confusion’ (due to the different components that contribute to the profile of the free surface) literally prevents the experimenter from perceiving any regularly reciprocating shape in the free surface. The so-called ‘fundamental mode of oscillation’ – which vibrates with the lowest frequency – does not emerge strongly enough to be recognizable; what one observes, therefore, is a serried mass of peaks travelling to and fro between the opposite walls of the vessel without virtually interfering with one other.

In other words, only with water above a certain depth is the law of depth verifiable, for only under these circumstances does the free surface of the fluid maintain a well-defined shape, which is not all that far from a truly rectilinear profile, such as Galileo had imagined.

This model shows clearly that the complexity due to the composite nature of the tidal waves defies any description in terms of a simple ‘alternating’ motion, in which the surface of the fluid remains acceptably flat. With no mathematical tools to handle the bizarre behaviour of these phenomena, Galileo may well have decided to abandon partially that empirical evidence that had emerged from his experiments.

3) Of the four ‘events’ to which Galileo refers explicitly, the second and the fourth – i.e. tides are a) ‘for the most part made in periods of six hours’, as in the Mediterranean basin, and, b) are ‘highest at the extremities of gulfs and lowest in their central parts’ – are readily observed and come under the heading of qualitative results.

To demonstrate the second ‘event’, I filled the tank with different quantities of water and obtained many different periods of oscillation; the conclusion that any sea basin could be subject to its basic periodicity – as in the case of the six-hour period of the Mediterranean – therefore appears quite reasonable.

[44] Details on the experiments, the experimental apparatus – a small parallelepipedal glass vessel – and some photographs of different shapes of tidal waves are given in the *Appendix 4*, where I have also discussed a few more technical questions.

As regards the fourth 'event', it must be said that although the Galilean thesis – being a merely qualitative assertion – turns out to be acceptable under much broader circumstances than those which limit the validity of the laws of oscillation, it is nevertheless not exact under all circumstances. For, when there is little water inside the vessel, so that the shape of the free surface is the result of the combination of many waves and its profile becomes much more complex than a straight line, many peaks travel back and forth and there is no justification for asserting that at either end the tidal wave is greater than in the middle. These peaks generate appreciable high and low tides even in the middle of the tank.

With regard to the first 'event' – why tides do not occur in small seas, lakes and ponds – the following qualitative conclusion can be inferred from my experiments. Whereas by smoothly accelerating and decelerating the tank, after introducing a wall in different positions to shorten the length, it is possible to decrease the amplitude of the tidal motions and virtually cancel them out, in experiments involving longer and more capacious vessels (so that their fundamental frequency is low enough) any modest variation in speed results in a very clearly observable – though, as we have seen, sometimes very complex – periodic alteration of the free surface's profile of the water. In other words, it is virtually impossible to move a long enough tank without causing slow vibrations to appear. My thesis is that these simple experiences were the only basis upon which Galileo could arguably have founded his assertions as regards the tide effects in small basins, such as lakes and ponds.

The third 'event' – why seas such as the Red Sea have no tides, unless they communicate with other larger seas – is a consequence of the foregoing observation as long as tide-generating acceleration is directed sideways with respect to the main dimension of the basin. I have not the faintest idea as to how Galileo could possibly have tested the effects due to the connection with a larger sea. My experiments tend to prove more or less adequately the first 'event', but nothing can be said about the third.

Let us take stock and summarize.

From the foregoing we can see that what these experiments have revealed was not a foregone conclusion. We now begin to understand why – if such experiments were really carried out, as I believe they were – the story of Galileo's work in his laboratory must have been one of baffling ups and downs and, moreover, why, in the end, he had virtually shelved his findings. Small 'artificially manufactured vessels' undoubtedly illustrated the most important effects of his oscillatory model, but they also demonstrated, most clearly, and no doubt to his great consternation, that beneath the surface of these simple phenomena lay the whole fearful complexity of the infinitely various motions of waves. The very experiments on which he had pinned his hopes of finding an empirical proof, under controlled conditions, of his oscillatory model eventually revealed that his grand ambition – that of reproducing the spectacular 'combinations of motions' of the sea in a laboratory was bound to remain unfulfilled. In which case, we must recognize Galileo's intellectual integrity in accepting the verdict of experience and renouncing his most advanced but, to his utter dismay, imperfect and, in the final analysis, unconfirmable insights into the laws governing tidal waves.

6. Celestial wheel clock

6.1 Monthly and annual periodicities

Tide-generating acceleration was the *external* cause that accounted for the fundamental diurnal periodicity of tide phenomena. The geometry of the basin was the *internal* key to explaining the multiplicity of short-period regularities. And thanks to his 'principle of complication', Galileo was even able to introduce a spatial effect due to the non-uniform distribution of tide-generating acceleration along the Earth's surface (even if this was to remain a sterile concept and Galileo failed to draw further significant conclusions from it). All in all, his tide theory, mainly based on tide-generating acceleration and the oscillatory model, was powerful enough to predict a great variety of motions in the seas. Since ancient times, tide phenomena had been acknowledged as displaying monthly, annual and possibly longer regular variations that were associated with the various combinations of positions of the Sun and the Moon on the celestial sphere.[1] Thus, it comes as no surprise that Galileo, who was well aware of the necessity to furnish a tide model capable of competing with the traditional explanations based on the ability of the Moon and the Sun to attract or influence sea waters, wanted to prove that his model's predictive power was far superior to other tide models and able to encompass the three basic periodicities of tide phenomena – diurnal, monthly and annual.

Rather than adopting the more precise expression for GTGA (see Sect. 4), we can express it more simply using the expression (5) given in Sect. 3:

$$A_{\Omega Y}(t,y) = A_0 \cdot \sin(\Omega \cdot t) \cdot \sin\left(\frac{2 \cdot \pi}{R} \cdot y\right), \qquad (1)$$

remembering that Galileo's simple idea is to search for two possible causes of variation in $A_{\Omega Y}(t,y)$, so as to make $A_{\Omega Y}(t,y)$ recur in one month and in one year. Given that $A_{\Omega Y}(t,y)$ is due to the composition of the Earth's orbital speed with the tangential speed at its surface caused by diurnal rotation, Galileo points out that if these two speeds are varied – for whatever reason – then a variation will result in the sum of the two and, therefore, a variation will result in $A_{\Omega Y}(t,y)$.[2] Yet, both Earth's diurnal speed and its annual speed are uniform. So that

[1] For example, in Book II, Chapter 97, of Pliny's *Natural History*, a complete luni-solar theory of tides is expounded at length. Galileo probably possessed a copy of Pliny's scientific encyclopaedia, given that it appears in an inventory list of books belonging to Sestilia Bocchineri Galilei (the wife of Vincenzio Galillei, the only son of Galileo) at the time of her death. The books were found by Vincenzo Viviani on 23rd January 1668, and – according to A. Favaro – it would appear unlikely that Vincenzio Galilei added to his father's library. Moreover, according to Antonio Favaro's testimony, an Italian edition of Pliny's *Natural History* with Galileo's postils (eventually lost) should have existed, apparently in a library in Siena. Galileo's personal library is catalogued in A. Favaro, "La libreria di Galileo Galilei", *Bullettino e Bibliografia delle Scienze Matematiche e Fisiche*, Rome, XIX, 1886, pp. 219–290. The note on Pliny is on p. 262. Pliny's one-hundred-lunar-month period is described in R.Almagià, "La dottrina della marea nell' antichità classica e nel Medio Evo. Contributo alla storia della Geografia scientifica", *op. cit.*, pp. 405–407.

[2] G. Galilei, *Dialogue on the Two Chief World Systems*, *op. cit.*, p. 448.

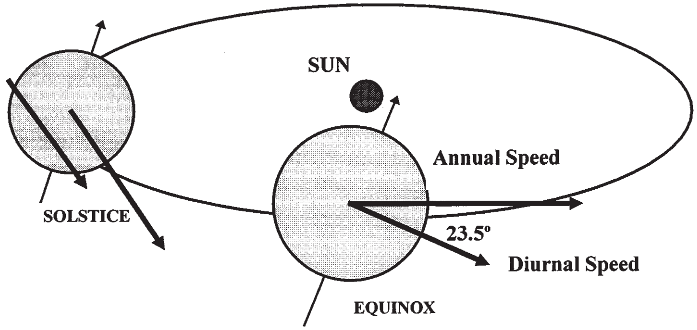

Fig. 6.1. The annual periodicity

> [...] it was indeed a laborious task for me to discover how such effects could be accomplished in nature. Yet, I finally found something that served me admirably. In a way it is almost unbelievable. I mean that is astonishing and incredible to us, not to Nature; for she performs with the utmost ease and simplicity things which are even infinitely puzzling to our minds, and what is very difficult for us to comprehend is quite easy for her to perform.[3]

Indeed, Galileo must have devoted his best efforts to this 'laborious task' for quite a long time, given that his final explanations of the monthly and annual inequalities were not present in the *Discourse on the Tides* of 1616. In Galileo's view, the annual variation in tide-generating acceleration simply turns out to be due to the 23.5° degree tilt of the Earth's polar axis with respect to the plane of the ecliptic. His model predicts higher tide-generating acceleration at the solstices than at the equinoxes. The reason is simple. At the solstices the Earth's tangential speed at midnight or midday is parallel to the annual speed, while at the equinoxes it is inclined by 23.5° degrees, so that, since a part of the tangential speed (to be considered a vector) is normal to the annual speed, it has no effect on the composition of the two, i.e. on tide-generating acceleration. Thus, tide-generating acceleration must be maximum at the solstices and minimum at the equinoxes (Fig. 6.1). Now, Galileo's musings on annual periodicity allow us to add to relation (1) the annual periodicity term:

$$\mathrm{A}_{\Omega\mathrm{Y}}(\mathrm{t,y}) = \mathrm{A}_0 \cdot [\sin(\Omega \cdot \mathrm{t}) + \sin(\Omega_A \cdot \mathrm{t})] \cdot \sin\left(\frac{2 \cdot \pi}{\mathrm{R}} \cdot \mathrm{y}\right)$$

where Ω_A is the frequency of the annual period.[4]

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[3] *Ibid.*, p. 448.

[4] Although Galileo might have found in Pliny's *Natural History* that tides are higher at equinoxes than at the solstices (R. Almagià, "La dottrina della marea nell' antichità classica e nel Medio Evo. Contributo alla storia della Geografia scientifica", *op. cit.*, p. 406), it must be once again pointed out that Galileo's tide-generating acceleration does not directly cause tidal ebb and flow because his model is not the equivalent of a quasi-static theory (like Newton's equilibrium theory), and tide phenomena are therefore the effect of *both* tide-generating acceleration *and* the natural response of sea basins. W. Shea's contention, according to which Galileo's model of the

Eventually, even the Moon made its entrance into Galileo's theory of tides. But far from being a recantation of his quasi-dynamic tide model, it marked a step forward in Galileo's understanding of the relationship between the Earth-Moon system's orbiting of the Sun and the physical effects that the motion of a two-body system in space should have both on the phenomena occurring on the surface of the Earth and on the motion of the Earth itself.

> [...] the terrestrial globe, always accompanied by the moon, goes along the circumference of its orbit about the sun in one year, in which time the moon revolves around the earth almost thirteen times. From this revolution it follows that the moon is sometimes close to the sun (that is, when it is between the sun and the earth), and sometimes more distant (when the earth lies between the moon and the sun). [...] Now, *if it is true* that the force which moves the earth and the moon around the sun always retains the same strength, and *if it is true* that the same moving body moved by the same force but in unequal circles passes over similar arcs of smaller circles in shorter times, *then* it must necessarily be said that the moon when at its least distance from the sun (that is, at conjunction) passes through greater arcs of the earth's orbit than when it is at its greater distance (that is, at opposition and full moon). *And it is necessary also that the earth should share in this irregularity of the moon*.[5]

First of all, it is clear that Galileo's explanation depends on two hypotheses that he is at a loss as to how to prove a) that the Earth-Moon system is moved around the Sun by 'the same strength' and b) that circular motion occurs in such a way that 'the *same moving body* moved by the *same force* but in unequal circles passes over similar arcs of smaller circles in shorter times'. Whereas in Galileo's physics and astronomy there is no evidence whatsoever to uphold hypothesis a), he can at least make do with what has been called the 'first law of astronomy'[6] to support hypothesis b), even though the empirical evidence he could marshal to justify his 'first law of astronomy' was merely derived from terrestrial pendular motions and the alternating motion of the regulator mechanism of ancient wheel clocks.

Yet, although his whole argument was somehow weakened by the evident lack of support for the 'cosmological' hypothesis a), and by the fact that the pendulum was a mechanical system whose 'transference' to the heavens could hardly have been accepted

annual inequality is erroneous, given that on average tides are greater at the equinoxes than at the solstices, must surely be considered untenable, for Galileo's annual periodicity model accounts just for the periodicity of the external cause of tides, and within the framework of his model it is not legitimate to derive any direct consequences from only tide-generating acceleration (W. Shea, *Galileo's Intellectual Revolution, op. cit.*, p. 183).

[5] G. Galilei, *Dialogue on the Two Chief World Systems, op. cit.*, pp. 452–453. Italics are mine.

[6] "Thus I say that one true, natural, and even necessary thing is that a single moveable body made to rotate by a single force will take a longer time to complete its circuit along a greater circle than along a lesser circle. This is truth accepted by all, and in agreement with experiments, of which we may adduce a few" (*ibid.*, p. 449). The definition 'first law of astronomy' is by P.E. Ariotti on p. 353 of his article "Aspects of the Conception and Development of the Pendulum in the 17th Century", *Archive for History of Exact Sciences*, VIII, 1972, pp. 329–410.

as being philosophically painless, we shall see that the clock regulator was a stand-alone analogy *totally untainted* by the pendulum's gravity-dependent ability to furnish evidence that 'similar arcs of smaller circles [are passed over] in shorter times' and, as such, must be regarded as being a masterpiece of reasoning purely based on analogy. Galileo himself must have been aware of the difficulty that was intrinsic in his attempt to make the pendulum law of length a cosmological principle, given that, in what was clearly the most delicate passage of his whole argument – why the Earth partakes of the Moon's periodic irregularity – he wisely resorted to the less problematic of his two analogies. Thus, he claimed that it was necessary that 'the earth should share in this irregularity of the moon', for

> if we imagine a straight line from the centre of the sun to the centre of the terrestrial globe, including also the moon's orbit, this will be the radius of the orbit in which the *earth would move uniformly if it were alone*. But if we locate there also another body carried by the earth, putting this at one time between the earth and the sun and at another time beyond the earth at its greatest distance from the sun, then in this second case the common motion of both *along the circumference of the earth's orbit* would, because of the greater distance of the moon, have to be somewhat slower than in the other case when the moon is between the earth and the sun, at its lesser distance. *So that what happens in this matter is just what happened to the rate of the clock, the moon representing to us the weight which is attached now farther from the centre, in order to make the vibrations of the stick less frequent, and now closer, in order to speed them up*.[7]

The bold extension of the irregularity of the Moon's motion to the Earth's orbital motion is based solely on the analogy with the horizontal and gravity-independent motion of the regulator mechanism of ancient wheel clocks known as balance-wheel (or balance-stick). In spite of the appeal that the idea of 'transferring' earthly gravity from terrestrial pendulums to heavenly bodies exerts on our ingrained post-Newtonian mode of thought, the balance-stick's moveable weight (Fig. 6.2) and its quality of being 'gravity free' in its regulating action on the rate of the clock (and yet able to show in detail the relationship between temporal regularities and mechanical properties) makes it a more adequate term of comparison with the Earth-Moon system than the pendulum's bob. This is simply because the pendulum needs gravity to perform its swinging motion, unlike the clock regulator which functions without gravity, since gravity is merely the *engine* of ancient wheel clocks, while the 'regulating' ability rests exclusively with the rotational-inertial property of the balance-stick, a property mainly determined by the distance from the hub at which its two weights are located (see Fig. 6.2). Thus, since the balance-stick's property of exerting a regulating action is independent of the constant action of the clock engine, no force is actually needed to account for this kinematic ability to regulate the rate of the clock. This is clearly recognised by Galileo himself:

> In order to regulate the time in wheel clocks, especially large ones, the builders fit them with a certain stick which is free to swing horizontally. At

[7] G. Galilei, *Dialogue on the Two Chief World Systems*, *op. cit.*, p. 453. Italics are mine.

> its ends they hang leaden weights, and when the clock goes too slowly, they can render its vibrations more frequent *merely by moving these weights somewhat towards the centre of the stick* . On the other hand, in order to retard the vibrations, *it suffices to draw these same weights out towards the ends,* since the oscillations are thus made more slowly and in consequence the hour intervals are prolonged. *Here the motive force is constant* – the counterpoise – and the moving bodies are the same weights; but their vibrations are more frequent when they are closer to the centre; that is, when they are moving along smaller circles.[8]

There is a direct consequence of the fact that gravity merely *powers* the clock, while its rate is influenced only by the horizontal balance-wheel. Although Galileo does not make it explicit it is clearly entailed by his argument. It was probably only later on that he became conscious of it when his interest in tide phenomena was revived by the discovery of a striking coincidence between the periodic regularities of tide motions with those of the so-called optical librations of the Moon. On closer observation of the behaviour of the balance-stick he noted that, by merely shifting the position of the leaden weights, the period of the horizontal motion of the balance-stick and, consequently, the rate of the clock is speeded up or slowed down. Now, if the Moon behaves like one of these two weights, then, by simply being respectively in conjunction or in opposition, it must speed up and slow down the pace of the celestial clock, while on nearing the quadratures it gets the clock at an intermediate pace. Thus, tide-generating acceleration turns out to be continuously varying from quadrature to conjunction and from quadrature to opposition. This entails likely variations in tide amplitude during the whole lunation.

The distinction between the gravity-powered engine of the whole mechanism of the clock and the purely kinematic ability of the balance-stick to regulate the rate by merely varying its inertial property may at first sight appear to be insignificant, but is in fact of consequence. For, if we conceive of the motion of the Earth-Moon system as being driven by a sort of pre-gravitational cause (be it a magnetic force or some other kind of physical relationship between the sun and the planets similar to terrestrial gravity) acting on bodies, we run the risk of attributing to Galileo an extensive grasp of the nature of gravitation (or some form of it) both on Earth and in the Universe – which is certainly problematic[9] – while at the same time losing sight of the much simpler and more important fact that, in the context of his physics, he was eventually able to produce a model that explained quite adequately the monthly inequality of tide-generating acceleration, without any need for a pre-gravitation force or 'virtue'. What is more, such a model was 'fertile', in the sense that it allowed Galileo to draw from it an important and, at least in principle, verifiable empirical conclusion. In the *Dialogue* it is the learned layman Sagredo who states this obvious conclusion from the monthly inequality model:

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[8] G. Galilei, *Dialogue on the Two Chief World Systems, op. cit.*, p. 449. Italics are mine.

[9] Galileo's mention of 'the force which moves the earth and the moon around the sun' notwithstanding, if there were such a precursory intuition of a *universal* force governing the motions of the planets, it certainly remained 'sterile' in Galileo's physics and he did not succeed in drawing from any such principle further significant conclusions.

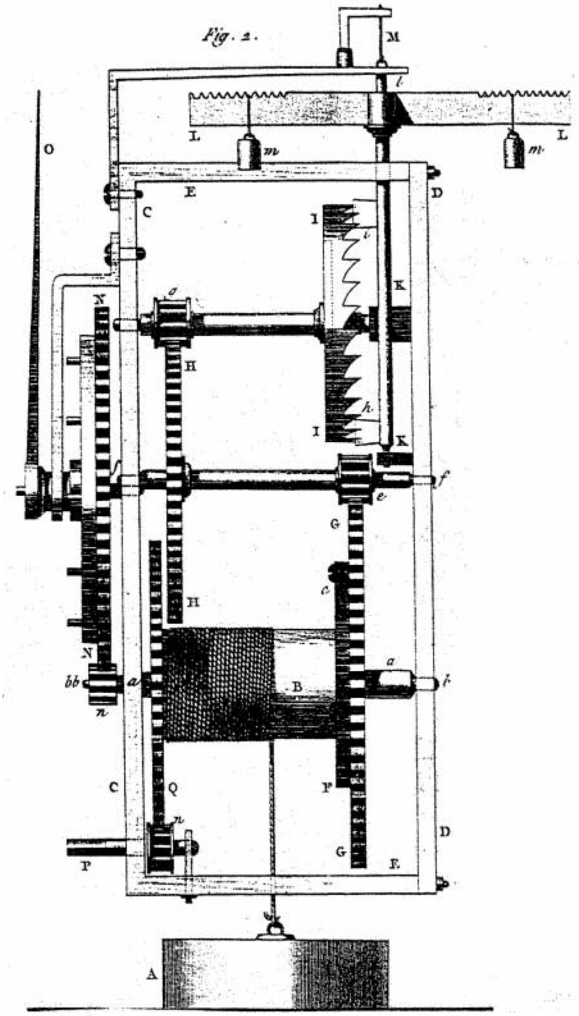

Fig. 6.2. A wheel-clock example (F. Berthoud, *Histoire del la mesure du temps par les horologes*, Paris, 1802). LL is the balance-stick and *m* represents the moveable bob (there are two bobs). A is the weight that powers the clock

> If the movement of the earth around the zodiac in company with the moon is irregular, such an irregularity ought to have been observed and noticed by astronomers, but I do not know that this has occurred.[10]

Now, this is exactly what should happen: in our language, imbued as we are with the Newtonian idea that forces are ultimately responsible for motion, we would say that if the Earth is alternately pulled and pushed by the Moon, it follows that the Earth's

[10] G. Galilei, *Dialogue on the Two Chief World Systems*, *op. cit.*, p. 454.

orbital speed cannot be constant along the zodiac. In Galileo's pre-force language, it is the simple alternating movement of the Moon along the celestial balance-stick, formed by the line drawn from the Sun's centre to the Earth's centre, that is responsible for 'disordering' Earth's otherwise uniform orbital motion with its periodic pulsating rhythm. It is precisely this irregularity that should in principle be observable and should have been observed by astronomers. Yet,

> [...] although astronomy has made great progress over the course of the centuries in investigating the arrangement and movements of the heavenly bodies, it has not as such arrived at such a state that there are not many things still remaining undecided, and perhaps still more that remain unknown [...]. Now, to get down to our particular point; that is, to the apparent motions of the sun and the moon. In the former there has been observed a certain great irregularity, as a result of which it [the sun] passes the two semicircles of the ecliptic (divided by the equinoctial points) at very different times, consuming about nine days more in passing over one half than the other; a difference which is, as you see, very conspicuous.[11]

Galileo's spokesman Salviati has not yet answered Sagredo's objection. He has simply pointed out that in the history of astronomy not all interesting phenomena have been observed at the same time (he also gives a list of conspicuous cases: the order of the planets, their laws of revolution and especially Mars' orbit which has 'caused modern astronomers so much distress').[12] Thus Galileo honestly admits that

> It has not been observed whether the sun preserves a regular motion in passing through very small arcs, as for example those of each sign of the zodiac, or whether it goes at a pace now somewhat faster and now slower, as would necessarily follow if the annual motion belongs only apparently to the sun and really to the earth in company of the moon. Perhaps this has not even been looked into. [. . .] the fact that there is no obvious irregularity is insufficient to cast doubt upon the possibility that the earth and the moon are somewhat accelerated at new moon and retarded at full moon in travelling through the zodiac . . . .[13]

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[11] *Ibid.*, pp. 455–456.

[12] This passage is clear. Galileo has not yet answered Sagredo's objection and he has simply made Sagredo say that a nine-day irregularity has been observed, so that – as Salviati is going to explain – the possibility that other, possibly smaller, irregularities will be discovered by means of more accurate observations cannot be ruled out. I cannot see how this excerpt could be invoked to uphold W. Shea's thesis that "An irregularity of nine days is completely unexpected on Galileo's hypothesis of monthly variations, and it contradicts the explanation of the annual variation he proceeds to give with the aid of his geometrical model" (W. Shea, *Galileo's Intellectual Revolution*, *op. cit.*, p. 182). Apart from the fact that it is hard to see how this irregularity should contradict Galileo's annual inequality explanation, the nine-day irregularity *is not a consequence* of Galileo's monthly inequality model, but just an example that Galileo puts forward to show that there remain many astronomical problems, solutions to which have not yet been found.

[13] G. Galilei, *Dialogue on the Two Chief World Systems*, *op. cit.*, p. 456.

The monthly inequality model entails an empirical consequence: a corresponding monthly variation in the Sun's motion along the zodiac – which in Galileo's Copernican astronomy amounts to a variation in the motion of the Earth. This irregularity has not yet been observed, says Galileo, but it must in principle be observable, if his model holds true. Now, Galileo obviously has only a vague idea of the quantity of this inequality, and, given that he is not able to perform any calculations with his balance-wheel model, he cannot predict whether this phenomenon is significant enough to be detected by ordinary means of observation. Nor 'is there any need for the irregularity to be very large in order to produce the effect that is seen in the alterations of the size of the tides', given that on the whole tides are 'small with respect to the magnitude of the bodies in which they occur, though with respect to us and to our smallness they seem to be great things'.[14] Thus, Galileo could only predict from a qualitative point of view that a periodical acceleration did not need to be great in order to produce its effect on tides.

Given that, as regards monthly tidal periodicity, the scant historiographical attention paid to Galileo's model has hitherto stressed only the precursory intuition of universal gravitation or misinterpreted his explanation,[15] I would like to demonstrate in the rest of this Section

a) that the regulator mechanism based on the balance-wheel mechanical model does indeed fulfil its duty of accelerating and decelerating the Earth monthly without any need for a precursory force of attraction that anticipates universal gravitation,

b) that the approximation of the effect accounted for by the balance-wheel model is quantitatively and surprisingly close to the real effect – which is due to Newtonian universal gravitation – and, therefore,

c) that the excogitation of such a mechanism, which is both adequate to explain the monthly inequality of tide-generating acceleration and perfectly consistent with his own physics and astronomy, must be regarded as one of Galileo's achievements, well within the compass of his mechanics.

An important point must be stressed here. Even the monthly and the annual inequalities are simply to be conceived of as being irregularities in the external cause of tides, namely, in what Galileo called 'primary cause' and we have called 'tide-generating acceleration', i.e. $A_{\Omega Y}(t,y)$. Thus, the Galilean tide-generating acceleration may be expressed by the following simple formula:

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[14] *Ibid.*

[15] Referring to the passage quoted above and to Galileo's conclusion that 'the earth should share in this irregularity of the moon', H.L. Burstyn in his article "Galileo's Attempt to Prove that the Earth Moves", *op. cit.*, p. 179, asserts that "This is perhaps the most remarkable passage of the *Dialogo*, for here Galileo comes close to the idea of gravitational attraction". The connection between pendulums and Earth's motion is investigated by the same author in his article "The Deflecting Force of the Earth's Rotation from Galileo to Newton", *Annals of Science*, XXI, 1966, pp. 46–80. See also L. Sosio's important comment on Galileo's example of the wheel clock regulator in G. Galilei, *Dialogo sopra i due massimi sistemi del mondo*, Torino, Einaudi, 1970, p. 536. For the 'misinterpretation' see footnote 11 and R.H. Harris, *Manual of Tides*, *op. cit.*, p. 400, who speaks of 'a *supposed* acceleration and retardation in the Earth's orbital motion' (the italics are mine).

$$A_{\Omega Y}(t, y) = A_0 \cdot [\sin(\Omega \cdot t) + \sin(\Omega_A \cdot t) + \sin(\Omega_M \cdot t)] \cdot \sin\left(\frac{2 \cdot \pi}{R} \cdot y\right) \quad (2)$$

where $\Omega_M$is the monthly frequency. Relation (2) is made up of the three contributions to the 'primary cause' stemming from the diurnal, monthly and annual periodicities, i.e. Ω, Ω_A, Ω_M, respectively. But relation (2) remains simply a 'summary' of the frequencies present in tide-generating acceleration; by no means may it be interpreted as being directly responsible for the oscillatory effects on the actual tide motions. Tide motions are waves, and waves can only be accounted for by the composition of an external pulsating cause that sets the pace and the internal response to that cause, which, in the case of tidal waves, is entirely due to the physical properties of water and to the geometrical characteristics of the Earth's basins. In his later years, Galileo explicitly recognised that tide motions are simply waves and we shall examine his final speculations on the nature of these spectacular phenomena in the last Section of this work.

Nevertheless, there is a final question regarding the influence on tides of the Earth's monthly irregularity that must be briefly touched upon. Although, as will be clear, Galileo was quite right in his view that the Moon acts on the Earth-Moon system in the same way as the movable weight of the balance-wheel acts on the rate of the clock, and although such a phenomenon really *exists* in nature, this real periodic wobbling of the Earth's annual acceleration is by no means able to induce any motion in the water of the sea. Why not? Simply because – as we have seen in Section 4 in the case of the gravitational interaction between particle P and the Sun – this effect is due to the lunar component of the total Newtonian tide-generating *force*. In other words, the Sun's force of attraction on particle P can be expressed by the following formula:

$$\vec{F}_{PS} = -G \cdot \frac{m_S \cdot m_P}{r_{PS}^2} \cdot \frac{\vec{r}_{PS}}{\|\vec{r}_{PS}\|}$$

and by adding to this relation the Moon's contribution we may build the entire force of attraction acting on particle P as:

$$\vec{F}_{PSL} = -G \cdot \frac{m_S \cdot m_P}{r_{PS}^2} \cdot \frac{\vec{r}_{PS}}{\|\vec{r}_{PS}\|} - G \cdot \frac{m_M \cdot m_P}{r_{PM}^2} \cdot \frac{\vec{r}_{PM}}{\|\vec{r}_{PM}\|}$$

where the subscript M stands for 'Moon'. Now, *whatever* the effect induced by the presence of the Moon on the actual motion of the Earth (i.e. acceleration and retardation), the mathematical structure of the previous relation is not altered by this effect, so that the selfsame reasoning that led to the conclusions of Sect. 4 – according to which, as long as such a motion is the result of universal gravitation, it cannot affect the tides – is still valid. In consequence, the Earth's monthly variation in speed – which, as we shall see, is predicted by the Newtonian equations of motion of the three-body system Sun-Earth-Moon – once again *cannot cause any effect on tide phenomena*.

At the same time, given that Galileo's mechanical model – based on the analogy with the balance-stick of ancient wheel clocks – predicts a similar variation in the Earth's orbital speed *without* any need for a force of gravity or 'virtue' acting on a cosmological scale between the planets and the Sun, it being simply the kinematic effect of the *regulating ability* of the celestial balance-stick formed by the Earth-Moon system, we can regard his model – in which the Moon ideally slides along the line joining the

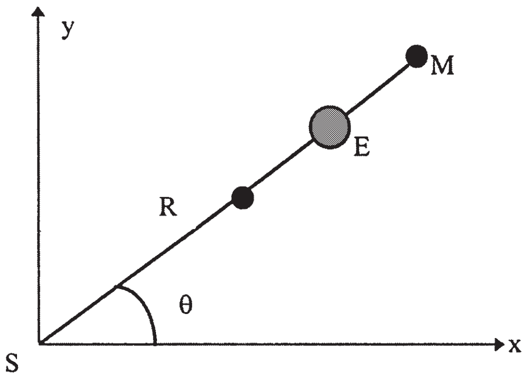

Fig. 6.3.

Earth's centre and Sun's centre – *as a perfectly legitimate* (in Galileo's physics) *explanation* of the monthly irregularity of the Earth's orbital motion and, consequently, of the monthly irregularity of the Galilean tide-generating acceleration. We shall examine in the following section exactly how the Moon as a 'generator of wobbling' works in the heavens (according to Galileo) and how (according to our Newtonian mechanics) the entire phenomenon as conceived of by Galileo may be seen as a good approximation of the real one due to universal gravitation.

6.2 The celestial balance-stick regulator

In Galileo's astronomy orbits are circles, hence, in the following description, both the Earth's orbit around the Sun and the Moon's around the Earth must be represented as circles. Let us consider (Fig. 6.3) a Cartesian frame of reference whose axes are (x,y) with the Sun fixed at the centre, namely, fixed at the origin of the frame of reference. Let R be the Sun-Earth's centre distance and ρ the radius of the Moon's orbit around the Earth; both Earth E (let its mass be m) and Moon M (let its mass be μ) are regarded as points. We shall consider – as Galileo did – the Earth-Moon system as representing 'half' of the balance-wheel regulator (we do not need the other part to study this phenomenon). In other words, Earth E orbits the Sun along the circumference whose radius is R and Moon M ideally slides along the line joining the Sun and the Earth's centre. When the Moon is at its maximum distance from the Sun it is in the full Moon phase, whereas when it is at its minimum distance it in the new Moon phase. Let θ be the angle formed by radius R and x-axis. We simply express the sliding motion of Moon M by imposing the following kinematic constraint:

$$R(\theta) = \rho \cdot \cos(N \cdot \theta) \qquad (3)$$

where N gives the number of lunations per year (we assume N = 13). Now, this system is called by physicists a 'one degree of freedom system' because the sole variable θ is all what is required to represent E's motion.

There are two basic ways of working out the equation of E's motion: one involves Newton's laws of motion, the other Lagrange's technique, which for our purpose we can

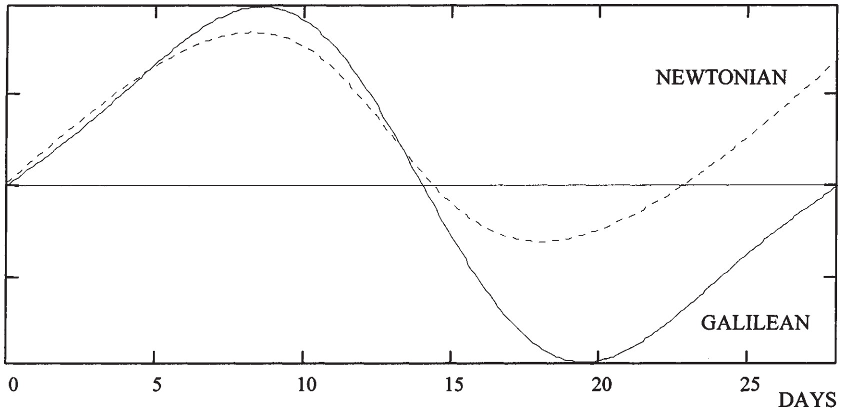

Fig. 6.4. The Earth's wobbling

simply regard as a useful mathematical tool generally used to simplify the procedure needed to write down Newton's equations, especially when kinematic constraints like that given by relation (3) are part of a system. Given that Lagrange's technique is based on the ability to work out the kinetic energy of the whole system, all one needs to do is write down the expression for the speed of points E and M. Since it is just a matter of tedious calculation, we do not need to be concerned here with the actual development of the equation of Earth's motion (see Appendix 5). We can therefore simply give the equation of the motion of Earth E (let it be relation (4)):

$$\frac{d^2}{dt^2}\theta = \frac{-\rho^2 \cdot [-\mathrm{N} \cdot \sin(\mathrm{N} \cdot \theta) \cdot \cos(\mathrm{N} \cdot \theta) + \mathrm{N}^3 \cdot \sin(\mathrm{N} \cdot \theta) \cdot \cos(\mathrm{N} \cdot \theta)] + \mathrm{R} \cdot \rho \cdot \mathrm{N} \cdot \sin(\mathrm{N} \cdot \theta)}{\frac{\mathrm{m}}{\mu} \cdot \mathrm{R}^2 + \mathrm{R}^2 + \rho^2 \cdot [\cos^2(\mathrm{N} \cdot \theta) + \mathrm{N}^2 \cdot \sin^2(\mathrm{N} \cdot \theta)] + 2 \cdot \mathrm{R} \cdot \rho \cdot \cos(\mathrm{N} \cdot \theta)} \times \left(\frac{d}{dt}\theta\right)^2 \tag{4}$$

and go on to simulate relation (4) in order to find out whether the effect claimed by Galileo exists. To do so we simply need to assign numerical values to the constants that appear in relation (4). Given that our main purpose is merely to show that equation (4) does indeed exhibit the Galilean effect (irrespective of the quantitative aspect of the phenomenon in which we are only partially interested) and given that our secondary purpose is to compare the Galilean effect with the real one so as show that the former may be seen as an approximation of the real effect due to universal gravitation, I have chosen modern astronomical values for the average distances both between Earth and Moon and Earth and Sun, as well as for the Earth and Moon masses.

The best way to visualise this periodic phenomenon, and to realise in what sense it may be considered a very good approximation to reality, is to represent in the same diagram (Fig. 6.4) not only the variation occurring during a lunar month (taken to be 28 days) in the Earth's tangential acceleration, and which is due to the *Galilean wobbling*, but also the variation that is due to *real wobbling* generated by the Moon in a Newtonian model based on universal gravitation. Referring to Fig. 6.4, the continuous line represents the

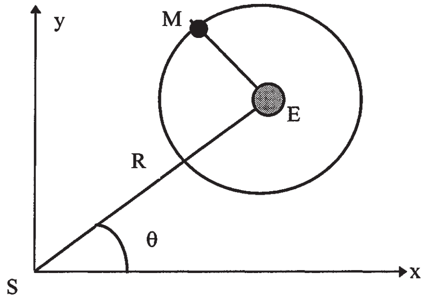

Figure 6.5

tangential acceleration of the Earth as it occurs in the Galilean celestial balance-wheel, while the dotted line represent the same effect as it occurs in a Newtonian system. The equations I have used for the Newtonian system, in which both the Earth and the Moon are free to move in space and are subject only to gravitational forces, are given in Appendix 5.

In order to read the diagram correctly, two points must be realised. First an increasing *scale factor* equal to 20 has been assigned to the Galilean curve, because the order of magnitude of the phenomenon to which the balance-stick model gives rise is in fact almost twenty times lower than the real phenomenon. Second, the Newtonian curve appears to be 'shifted' towards the higher part of Fig.6.4 because the Earth-Moon system, which is simulated according to the laws of universal gravitation, travels along an elliptical orbit and not along a circle. Thus, the centre of mass of the whole system accelerates and decelerates along the orbital pathway in such a manner that, nearing the perihelion, it progressively accelerates, while nearing the aphelion it progressively decelerates. Since at starting time t = 0 the Earth-Moon system was located at an average distance from the Sun and the whole system was made to run towards the perihelion, this explains why in my simulation a net positive acceleration appears to be 'superimposed' to the periodic

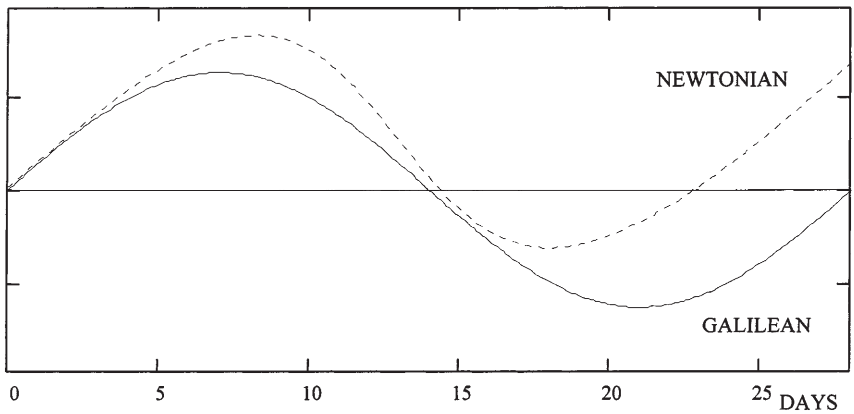

Fig. 6.6. The Earth's wobbling: The quasi-balance-stick model

wobbling due to the variation of the position of the Moon with respect to Earth. The elliptical pathway also accounts for the difference in shape between the two curves.

There is a second possibility within the 'compass' of Galileo's physics. Referring to Fig. 6.5, let Moon M now be free to rotate in its circular orbit around the Earth's centre. In this second model, the Moon no longer slides along the balance-stick constituted by the line joining the Sun and the Earth but is totally free to rotate along a circular trajectory whose radius r is the average distance Earth-Moon. At first sight, the system may appear to be just the same as, or very similar to, that of the previous model. But, it is not so at all. This second 'rotating Moon' system affords a surprisingly accurate approximation of the Newtonian effect.

Now the equation of motion of Earth E simply turns out to be (Appendix 5):

$$\frac{d^2}{dt^2}\theta = \frac{\mathrm{m}\cdot\mathrm{R}\cdot\rho\cdot\mathrm{N}\cdot(\mathrm{N}-1)\cdot\sin((\mathrm{N}-1)\cdot\theta)}{\mathrm{m}\cdot\mathrm{R}^2+\mu\cdot\{(\mathrm{R}^2+\rho^2\cdot\mathrm{N}^2)+2\cdot\mathrm{R}\cdot\rho\cdot\mathrm{N}\cdot\cos((\mathrm{N}-1)\theta)\}}\cdot\left(\frac{d}{dt}\theta\right)^2 \quad (5)$$

where the symbols have the same meaning as in equation (4). Figure 6.6 is the spectacular result of this quasi-balance-stick model, again compared with the Newtonian effect. Let us note that this time *no scale factor* is needed to correct the quasi-balance-stick wobbling in order to compare it with the real effect: a remarkable 'proof' that Galileo's 'kinematic intuition' was indeed on the right track, even though he did not have at his disposal the formidable computational tools of modern computers.

7. Moon and waves

7.1 Lunar trepidations and tides: a new research programme

On 5th November 1637 Galileo wrote to his correspondent and great admirer in Venice, father Fulgenzio Micanzio, to inform him that, although his left eye was worsening (in July the right one had been lost totally), he had nonetheless recently carried out '*new* observations of the Moon's face' and verified beyond any doubt that the Moon, orbiting the Earth, runs along its 'dragon' (Galileo's expression for the Moon's apparent trajectory on the heavenly vault) while maintaining the Earth's centre as the centre of its trajectory around the Earth. This fact and the circumstance of the distance of the terrestrial observer – whom Galileo thinks of as standing on the surface of the rotating Earth – from the planet's centre could explain all the '*apparent* variations' of the Moon.[1] With the expression 'apparent variations', Galileo is referring here to his discovery of what astronomers today call Moon's optical trepidations.[2] He had briefly announced

[1] In Galileo's language, the 'dragon' is represented as a line lying on the celestial sphere. Because this line lies partly south and partly north of the Ecliptic, it shows a serpent-like or s-like shape. See *Le opere di Galileo Galilei*, XVII, pp. 212. The italics are mine.

[2] The most complete account of observations of the Moon and of the attempts to map its surface in the seventeenth century is E. Whitaker, *Selenography in the seventeenth century*, in R. Taton, C. Wilson [eds.], *Planetary astronomy from the Renaissance to the rise of astrophysics. Part A: Tycho Brahe to Newton*, Cambridge, Cambridge University Press, 1989, pp. 119–143. As regards Galileo's observation of the Moon's optical trepidations, Whitaker's article is confused

his findings a few years earlier in the First Day of the *Dialogue Concerning the Two Chief World Systems* in the context of a discussion on the extraordinary novelties that the newly introduced telescope had brought about in the field of lunar astronomy.[3]

Owing to the delicacy of this point in Galileo's thought regarding both the discovery of the Moon's optical trepidations and a possible, fascinating connection with his tide theory – which may be suggested by an ambiguous concluding remark in a second letter to Fulgenzio Micanzio, which we shall examine briefly, on the correspondence between tidal periodicy and the Moon's libration periods – we must stop for a moment to consider what exactly Galileo has meant by trepidation motions and to explore in some detail the whole context in which his late preoccupation with the Moon might have been sparked off. We shall see that Galileo's resurgent interest in the Moon and tide theory does not lead him to abandon his former dynamic perspective and undulatory conceptions in favour of some old-fashioned explanation based on occult lunar influence. Quite the opposite: evidence founded on a few important letters written about the years 1637–38 shows that Galileo reinforced his vision of tide phenomena as wave-like phenomena, and, what is more, that he arrived at an explicit formulation of tidal ebb and flow as a single wave travelling to and fro between the coastline and the sea. In order to assess better this late furtherance of Galileo's science of tides and its alleged correlation with the Moon's trepidations we need to cast a glance at what the so-called trepidation 'motion' actually is.

As is well known, optical trepidation phenomena are not motions at all.[4] On the contrary, they are simply a mirror image of the changing position of the terrestrial observer's viewpoint. In other words, the Moon's trepidation motions, or librations, are due to the simple fact that, because we observe the Moon orbiting the Earth while standing on the surface of a rotating planet – which in turn orbits the Sun – we can spot different, small portions of the lunar hemisphere alternately, so that in reality, we can observe from Earth roughly sixty per cent of the lunar surface. For example, both when the Moon rises and when it sets, a terrestrial observer could in theory catch sight of its surface beyond the boundaries that limit his geometric perception when the Moon transits across his meridian. This phenomenon is called 'diurnal libration', but it is so small that it is hardly detectable even with our modern telescopes and astronomical photography, and therefore it cannot have been observed by Galileo.[5] This is illustrated in Fig. 7.1, where circle E represents the Earth and circle M the rising Moon. O is a

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and contains one error that seriously compromises any understanding of Galileo's thought. See the discussion below.

[3] G. Galilei, *Dialogue Concerning the Two Chief World Systems*, *op. cit.*, pp. 65–67.

[4] In order to understand Galileo's position, optical trepidations, or librations, must be distinguished carefully from true, physical librations, which are, of course, true wobbles of the Moon. Newton advanced the hypothesis, in proposition 38 of Book III of the *Principia*, that, besides an optical libration in longitude, there could also be a physical libration due basically to the Moon's non-spherical shape. It was Lagrange who studied quantitatively the phenomenon of the Moon's physical librations and achieved a full insight into this complex phenomenon. See C. Wilson, *The work of Lagrange in celestial mechanics*, in R. Taton, C. Wilson [eds.], *Planetary astronomy from the Renaissance to the rise of astrophysics. Part B: Tycho Brahe to Newton*, Cambridge, Cambridge University Press, 1995, pp. 109–112.

[5] E.A. Whitaker, *Selenography in the seventeenth century*, *op. cit.*, p. 125.

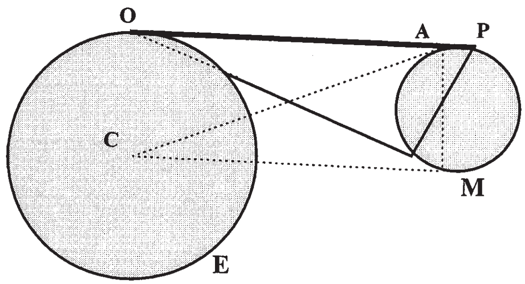

Figure 7.1

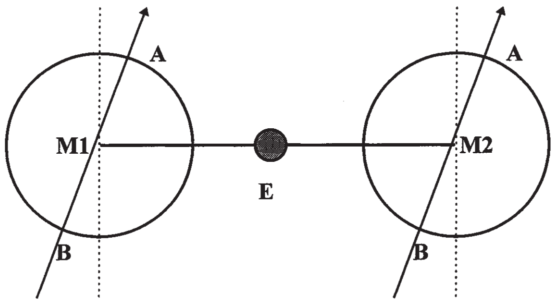

Figure 7.2

terrestrial observer standing on the Earth's surface. The observer can spot point P, beyond A, which would be the furthermost point of his horizon if he looked at the Moon from the Earth's centre C. This phenomenon is called 'diurnal or parallactic trepidation'.

But, there are two major and, quantitatively, more significant librations. One is called 'libration in longitude' and the other 'libration in latitude'. Regarding the latter, since the monthly lunar pathway around the Earth rises above the Ecliptic and drops below it – i.e., the plane of the lunar orbit is on average tilted 6° 41" with respect to the plane of the Ecliptic – we observe a variation in the visible regions near the lunar north and south poles.[6] This is very similar to what happens here on Earth with the seasons, which are due to the tilt of the terrestrial polar axis. Figure 7.2 gives a sketch of the situation. Two circles M1 and M2 represent the Moon with respect to Earth E in two positions that are one half of the Moon's sidereal month apart. A and B are the lunar poles. When the Moon is in M1, the region near A is visible from Earth, while the region near B is visible when the Moon is in M2.

The former 'libration in longitude' is due to a different phenomenon, namely, on the one hand, the variation in lunar orbital speed due to its elliptical trajectory around the Earth, and, on the other, the uniformity of the Moon's rotation about its own polar

[6] Actually the plane of the lunar orbit around the Earth is tilted on average about 5°9" with respect to the Ecliptic, whereas the lunar polar axis has in turn an average further tilt of its own.

axis. The composition of a uniform rotational speed with a non-uniform orbiting one causes slight variations during the lunar month to be detectable from Earth in the regions near the western and eastern borders of the Moon's hemisphere. Again, this is simply a change in viewpoint and has nothing to do with any physical oscillations of the Moon. We can set aside this third type of libration, for it is entirely due to phenomena that could not even have been suspected by Galileo, given his firm belief in the circularity of celestial bodies' orbits.

To sum up, although we still refer to these complex optical phenomena in terms of 'trepidation' or 'libration', we have to bear in mind that the Moon does not have any oscillating motion whatsoever – except, of course, for real, physical librations which do occur, but are due to completely different causes. Let us now turn our attention to Galileo's interpretation of the Moon's librations so as to throw into relief what was his real scientific interest in the Moon in his late age. As we shall see, although his intellectual curiosity about the Moon's librations might have released a new flurry of theoretical research and engendered an eagerness to gather fresh information in support of new empirical evidence, his thinking regarding the tides remained as far removed as ever from being contaminated by the notion that the Moon might somehow influence directly the motion of sea waters. And the key to the physical role the Moon plays in the libration affair is probably 'magnetical philosophy', even though the cultural context of the affair may well have been a broader 'selenographical programme'[7] aimed at mapping the surface of the Moon and carried out in France by Pierre Gassendi, Nicolas Claude Fabri de Peiresc and the painter Claude Mellan, and to which Galileo had contributed the lenses for the telescope with which new observations were to be made.[8]

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[7] See the discussion in E.A. Whitaker, *Selenography in the seventeenth century,* in R. Taton, C. Wilson [eds.], *Planetary astronomy from the Renaissance to the rise of astrophysics. Part A: Tycho Brahe to Newton, op. cit.*, pp. 126–127. The main goal of the Moon programme was to measure the longitude difference between Paris and Aix-en-Provence by "noting the local times of the occultations and reappearances of small lunar spots during lunar eclipses, for which a good lunar map was needed" (p. 126).

[8] Although there is no evidence that Galileo was directly involved in the programme, he was requested by Gassendi to furnish his superior lenses for the telescope, with which new and a more accurate observations, not only of the Moon but also of the planets, were to be made in France. Gassendi received the optical parts at the end of 1634 in a package tied with a thread exactly the length required for the telescope's tube (see *Le opere di Galileo Galilei*, XVI, p. 117 and p. 153). Later on, in November 1636, he informed Galileo that Mellan was observing the Moon and working with 'paintbrush and burin' (*ibid.*, p. 212). In February 1637, Peiresc wrote to Galileo and updated him on Mellan's work, promising that as soon as the first engraving proofs were ready they would be sent to Galileo (see *Le opere di Galileo Galilei*, XVII, p. 35). The French programme was eventually aborted in 1637 because of the death of Peiresc. Nevertheless, Mellan turned up again in Rome – where he had lived for ten years before going back to France – and was engaged by Galileo's pupil Benedetto Castelli, who – falling short of money – and despite his willingness to keep the painter, suggested to Galileo that the artist should go to Florence. In the meantime, Mellan had produced some proofs or drawings that Castelli sent to Galileo. After receiving these, Galileo answered in a letter to Castelli in October 1637 that the 'drawings made with chalk and pencil' were good enough, but lacked some very important features of the Moon's surface, which he

Two days after writing his first letter to Fulgenzio Micanzio on lunar trepidations, Galileo sent a second letter to the Servite Father in Venice and gave an enthusiastic account of his recent observations along with a curious, possibly ironic, remark about the stunning coincidence of the triplicity of the Moon's periodic wobbling with the the triple periodicity of tide motions. Although it is a long passage, it is worth while citing it almost entirely.

> I [Galileo] have discovered a very marvellous observation in the face of the moon, in which body, though it has been looked at infinitely many times, I do not find that any change was ever noticed, but the same face was always seen the same to our eyes. This I find not to be true; rather, it changes its aspect with all three possible variations, making for us those changes that are made by one who shows to our eyes his full face, head on so to speak, and then goes changing this in all possible ways, that is, turning now a bit to the right and then a bit to the left, or else raising and lowering [his face], or finally, tilting his left shoulder to right and left. [. . .] Add, moreover, another marvel, which is that these three different variations have three different periods; for one of them changes from day to day, and thus comes to have the diurnal period; the second goes changing from month to month, and has its period monthly; the third has the annual period in which it finishes its variations. *Now, what will you say in confronting these three lunar periods with the three periods, daily monthly and annual, of the movements of the sea, of which by common agreement of everyone the moon is arbiter and superintendent* ? [9]

The letter's scientific content ends with that sibylline question regarding the congruity between the three periods of the Moon's librations and the tidal three main periods. If one reads the passage without taking into consideration the whole context in which Galileo has presumably carried out what I would call a fresh 'Moon libration research programme' – which in turn has to be seen in the broader cultural context of what has been called a 'selenographical programme',[10] to which he may well have wished to contribute new insights regarding the possibility of mapping much more of the lunar surface than was commonly reputed to be visible at that time – and, above all, the complete history of the problem, which goes back to 1632's *Dialogue* (where Galileo made an attempt to explain the Moon's ability to face towards Earth always with the same hemisphere by having recourse to magnetic philosophy), one risks 'crediting' Galileo with an unintelligible abrupt change of heart, a conversion to the religion of the Moon's occult influences that in fact had never taken place.[11]

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described with some detail. As regards Mellan's possible employment in Florence, Galileo wrote that the Grand Duke had already commissioned the map of the Moon (*Ibid*., p. 186 and p. 204).

[9] The passage is quoted from S. Drake, *Galileo at Work. His Scientific Biography, op. cit.*, p. 385. See the original in *Le opere di Galileo Galilei*, XVII, pp. 214–215. Italics are mine.

[10] See E.A.Whitaker, *Selenography in the seventeenth century*, in R. Taton, C. Wilson [eds.], *Planetary astronomy from the Renaissance to the rise of astrophysics. Part A: Tycho Brahe to Newton, op. cit.*, pp. 125.

[11] The idea that Galileo abandoned his tide theory in favour of a new one has been put forward by William Shea in his study *Galileo's Intellectual Revolution, op. cit.*, pp. 185–186. According

On the other hand, quite apart from the indirect suggestions of the recent 'selenographical' context, there is abundant clear and direct evidence that such a conversion never occurred. To understand why Galileo did not abandon his lifelong investment in his tide model, we have to compare the letter quoted above with two other essential documents: the *Dialogue*'s First Day, which reports what he had to say on the Moon's librations in 1632; and second, a scrupulous account of the whole question, given in the form of a letter to Micanzio's friend, Monsignor Alfonso Antonini, written in February, 1638, probably with an eye to having it published. The latter contains Galileo's final musings on the problem, which, although summarised after he became totally blind, in fact amount to a small tract on libration phenomena.

To anticipate a few conclusions, I would argue that what emerges is a complex picture in which Galileo's initial interests in the libration problem are probably induced by a desire to test the observable consequences of a possible magnetic interaction between the Earth and the Moon. Eventually, he concentrates his efforts in an attempt to focus his final energies – and his waning ability to use the telescope – on the measure of the periods of these apparent motions. In this context he noticed the coincidence of the triplicity of the Moon's libration periods with that of the tides. And, as regards his sibylline concluding remark, he may simply have wished to underline an unexplained concurrence of periodicities,[12] or even to express with irony and a streak of disillusion his feelings that new observations of the libration periods could paradoxically add weight to the arguments of the staunch believers in the ability of the Moon's occult quality to attract sea waters. For, at that time, he had in all probability abandoned the idea of an explanation of the Moon's tendency to keep a straight face towards the Earth's centre based on a magnetic 'natural agreement and correspondence' between the Earth and the Moon in favour of a more neutral geometrical relation between the Earth's centre and the Moon's centre. In other words, he refrained from hypothesising about the nature of such a correspondence and concentrated on quantifying its periodic behaviour. Let us now examine what evidence can be marshalled to uphold my conclusions.

The first résumé of Galileo's observations and meditations about the Moon's libration motions is given in the First Day of the *Dialogue* in 1632. In the following passage, both the basic idea and the reasoning structure of Galileo's spokesman, Salviati, must be given close attention, because it will allow us to reconstruct the development of Galileo's ideas by comparing this first exposition with the later letter to Alfonso Antonini.

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to the author, in 1637 Galileo "abandoned his theory of the tides for an other equally ill-fated explanation" (p. 185). The author goes on to cite Galileo's letter to Micanzio containing the odd remark on the coincidence between lunar libration periods and tide periods, but he does not actually say what such an 'equally ill-fated theory' might consist of. Nor is there in any text of Galileo's any evidence of his having drafted such a new theory. This has been recognised by S. Drake; see S. Drake, *Galileo At Work. His Scientific Biography*, *op. cit.*, p. 503. Now, attributing to Galileo the naïve idea that optical librations like those he had discovered (which were not motions at all, but changes of the terrestrial observer's viewpoint – as he beautifully describes at length in his letter to Alfonso Antonini, which we will examine below) could influence the tides here on Earth seems to me pure nonsense.

[12] This is S. Drake's thesis. See previous note.

> I [Salviati] happen to remember a specific event newly observed on the moon by our Academic [Galileo], *by means of which two necessary consequences may be inferred*. One is that we do see somewhat more than half the moon, and the other is that the moon's motion bears an exact relation to the centre of the earth. And what he observed was as follows. If the moon did have a natural agreement and correspondence with the earth, facing it with some very definite part, then the straight line which joins their centres would always have to pass through the same point on the surface of the moon [. . .]. Now the telescope has made it certain that this conclusion is in fact verified.[13]

Galileo goes on to describe the telescopic findings that support his claims, but he does not refer to his observational evidence as 'trepidation motions' of the Moon, or librations. Nor, obviously, is there any attempt to investigate the periods of phenomena that he has not yet fully realised to be periodic. What he has in mind is evidently an attempt to somehow prove that the Moon has a 'natural agreement' with the Earth – which explains why we actually observe variations in the position of particular spots near the line that we nowadays call 'terminator' – and that the physical nature of this 'natural agreement' – as he is going to stress in a concluding remark – is magnetic. It is a consequence of facts discovered by means of the telescope that 'the moon's motion bears an exact relation to the centre of the earth'. For Salviati-Galileo the conclusion is quite straightforward.

> [. . .] it is obvious that the moon, as if drawn by a magnetic force, faces the earth constantly with one surface and never deviates in this regard.[14]

As is well known, Galileo's interests in magnetic philosophy date back to his receiving William Gilbert's *De Magnete* from a peripatetic philosopher who presented him with the volume 'in order to protect his library from its contagion', as the scientist recounts in the Dialogue.[15] Galileo mastered the technique of fitting pieces of loadstone with metal caps so as to increase their power of attraction, and he had certainly read Gilbert's book with the most profound attention. The sixth book of the *De Magnete*,which was especially appealing to him because of its attempt to counter the traditional objections to the Earth's diurnal rotation by treating the terrestrial globe as a great loadstone, presented a clear statement of the Moon's face 'agreement' with the Earth's centre. In Gilbert's words:

> the moon alone of all planets directs its movements as a whole toward the earth's centre, and is near of kin to earth, and as it were held by ties to earth.[16]

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[13] G. Galilei, *Dialogue Concerning the Two Chief World Systems, op. cit.*, p. 66. Italics are mine.

[14] *Ibid.*, p. 67.

[15] *Ibid.*, p. 400. The Aristotelian philosopher was probably Cesare Cremonini.

[16] I have used the English translation by P. Fleury Mottelay, W. Gilbert, *De Magnete*, New York, Dover Publications, 1991, which is a re-publication of the first edition published by John Wiley & Sons, New York, 1893. See p. 345. A contribution devoted to the relationship between magnetic philosophy and astronomy in the first half of the seventeenth century is S. Pumfrey, *Magnetical Philosophy, 1600–1650*, in R. Taton, C. Wilson [eds.], *Planetary astronomy from*

This is exactly what Galileo wanted to verify by means of libration observations and – as we have seen – in 1632 or, possibly, even earlier on, he succeeded in discovering an optical oscillation of the Moon's face thanks to the telescope. The verification was so neat an 'empirical proof' that it scarcely warranted argument. What certainly warranted argument was, for Galileo, the physical nature of the alleged 'Moon's ties to Earth', because it is one thing to produce evidence based on empirical verification of facts, it is quite another to ascertain the physical causes of 'ties' such as those Gilbert attributed to the Earth-Moon system. Regarding Gilbert's putative, and, if so, ambiguous acceptance of Peter of Maricourt's 'fable' relating to the perennially rotating terrella,[17] Galileo says:

> I want to tell you one particular to which I wish Gilbert had not lent his ear. This is the concession that if a small sphere of lodestone were exactly balanced, it would revolve upon itself; for this no cause whatever exists.[18]

And, a few paragraphs earlier, he had said that what he could have wished for in Gilbert was

> a little more of the mathematician, and especially a thorough grounding in geometry, a discipline which would have rendered him less rash about accepting as rigorous proofs those reasons which he puts forward as *veraecausae* for the correct conclusions he himself had observed.[19]

Since the inception of Galileo's first interests in the loadstone's ability to attract other bodies, magnetism had remained for him more of a field in which experimental knowledge had to be painstakingly gleaned – it being necessary to separate carefully the true ascertained facts from a plethora of phenomena as treacherous as the quacks who

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the Renaissance to the rise of astrophysics. Part A: Tycho Brahe to Newton, *op. cit.*, pp. 45–53. Literature on Gilbert's influence on Galileo and on the relationship between magnetism and Galileo's cosmological ideas is scant. See M. Loria, *William Gilbert e Galileo Galilei. La terrella e le calamite del Granduca*, in C. Maccagni [ed.], *Saggi su Galileo Galilei*, Pubblicazioni del comitato nazionale per le manifestazioni celebrative del IV centenario della nascita di Galileo Galilei, Volume III, Tome 2, Firenze, G. Barbèra Editore, 1972, pp. 208–247. (This tome is the only one that has been published). Specifically on the relationship between magnetism and cosmological systems in Italy, see S. Magrini, "Il De Magnete di Gilbert e i primordi della magnetologia in Italia in rapporto alla lotta intorno ai massimi sistemi", *Archeion*, VIII, 1927, pp. 17–39, particularly on Galileo and Gilbert pp. 27–32.

[17] To reinforce his arguments in favour of the diurnal rotation of the Earth, Gilbert asserts: "I omit what Petrus Peregrinus so stoutly affirms, that a terrella poised on its poles in the meridian moves circularly with a complete revolution in twenty-four hours. We have never chanced to see this: nay, we doubt if there is such movement, both because of the weight of the stone itself, and also because the whole earth, as it moves of itself, so is propelled by the other stars" (*De Magnete*, *op. cit.*, p. 332). Peter of Maricourt or Petrus Peregrinus wrote a small tract on the magnet in 1269, which widely circulated as a manuscript until it was published in the sixteenth century. His infuence on W. Gilbert is examined in E. Zilsel, "The origin of Gilbert's scientific method", *Journal of the History of Ideas*, 2, 1941, pp. 1–32. Reprinted in P.P Wiener, A. Nolan, [eds.], *Roots od Scientific Thought*, New York, Basic Books, 1957, pp. 219–250.

[18] G. Galilei, *Dialogue Concerning the Two Chief World Systems*, *op. cit.*, p. 413.

[19] *Ibid.*, p. 406.

produced magnetic clocks and perpetual motion machines – than a mature branch of scientific research in which his ideal of 'certain proofs' and 'sensible experiences' could be applied.[20]

Six years after the *Dialogue*, the whole scenario had undergone a significant transformation. First of all, given that, in 1632, Galileo had briefly discussed the fundamentals of what he would later call trepidation phenomena, in what sense could he now claim that he was in fact doing *new* research? Why did he keep referring to his *new* findings? Why did his pupil Benedetto Castelli, who had evidently been updated by Galileo about the new findings and had them circulated in Rome, write to him that he was 'infinitely pleased with the *new* discovery regarding the Moon' and 'was looking forward to knowing the periods of these variations'[21]?

The answer is not hard to find. The new focus of attention was Galileo's newly posited problem of the libration periods. To achieve the quantitative measurement of the Moon's wobblings was now his last grand endeavour in the field of observational astronomy before his left eye too – the weaker one, as Galileo remarked after losing the right one – packed up for good. And this fear was to be transformed into reality very soon. Although the letter to Micanzio in November 1637 seems to suggest that Galileo had observed all the three periods of the Moon's trepidations, this is not entirely true, for – as he will explain later on in his letter to Alfonso Antonini, which we shall examine in a moment – his subsequent complete blindness prevented him from getting on with his research program into the libration periods, so that the period of the third libration actually was not observed. There can be little doubt that Galileo regarded his latest observations as completely new. The entire programme was seen as a fresh start, and one to be completed as soon as possible, not least in view of a potential dispute over

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[20] Salviati concludes his 'apology' for Gilbert's founding of magnetic science with these words: "I do not doubt that in the course of time this new science [magnetism] will be improved with still further observations, and even more by true and conclusive demonstrations" (*ibid*., p. 406). To savour what must have been the attitude amongst Galileo's followers towards the 'aura of mystery' that surrounded magnetism, it is worth quoting an amusing testimony of the most famous of Galileo's pupils, Evangelista Torricelli, who wrote to Galileo from Rome on 1st June, 1641, informing him that the Jesuit Ahanasius Kircher "has published a great volume on the loadstone; this book is enriched with a panoply of beautiful engravings. You [Galileo] will hear of astrolabes, clocks, anemoscopes, together with a handful of most extravagant words. [. . .] Amongst other beautiful things, there is the score of a piece of music that is purported to be an antidote against the tarantula's poison. Be it enough; Mr. Nardi, Mr. Magiotti and I have been laughing for quite a long time."(*Le opere di Galileo Galilei*, XVIII, pp. 332). The book Torricelli is referring to is Atanasii Kircherii, Fuldensis Buchonii, e Soc. Iesu, *Magnes, sive de arte magnetica, opus tripartitum*,ect., Sumptibus Hermanni Scheus, sub signo Reginae. Romae, ex typographia Ludovici Grignani, 1641. On the other hand, an idea of what Galileo might have conceived of as 'serious' research may be had from the investigations of another Galilean disciple, Benedetto Castelli, who, in 1639 or 1640, wrote a tract on the loadstone in which he put forward an interesting explanation of the property of some materials of acquiring and losing magnetisation based on a variable 'time response' of infinitesimal magnetic corpuscles. The short tract was published in the nineteenth century by A. Favaro under the title "Discorso sopra la calamita", in *Bullettino e Bibliografia delle Scienze Matematiche e Fisiche*, Rome, XVI, 1883, pp. 545–564.

[21] *Le opere di Galileo Galilei*, XVI, p. 216. Italics are mine.

prior claims, which, Galileo thought, could once again – as in the case of the sunspots quarrel with the Jesuit Father Christopher Scheiner – deprive him of the right to be considered the first to have made epoch-making astronomical observations.[22]

Here is the relevant passage from the letter to Alfonso Antonini reporting the statement of Galileo's libration research programme.

> [. . .] not long ago, I came up with the idea of ascertaining through precise observations whether the lunar globe always faces towards Earth, without any variation, in such a way that a line that joins the centre of the Moon with that of the Earth would perpetually pass through the exact same point on the Moon's surface. This would be an unfailing argument in favour of the fact that the Moon *does not have any tilt or trepidation of its own,* and always gazes upon Earth with the same part of its face. After taking this *assumption* as true, I started to draw certain conclusions and then to investigate accurately whether they really occur. [. . .] From this *hypothesis,* shall we say *assumption,* [. . .].[23]

First and foremost, what was looked upon as a factual consequence of empirical evidence in 1632 – that 'the moon, as if drawn by a magnetic force, faces the earth constantly with one surface and never deviates' – is now reckoned to be mere *hypothesis*, in which any reference to magnetism has vanished without trace, setting to nought any bold pretensions *vis-à-vis* furnishing a physical explanation. What Galileo wants to establish at this stage is simply the empirical verification that if one hypothesizes that the Moon always faces towards Earth without any variation, then *optical* libration motions are the obvious factual consequences one has to expect and observe.[24] And this in turn is a good argument in support of the notion that the Moon does not have any *physical* libration of its own. This second preoccupation is almost an explicit clue to Galileo's desire to exclude definitely the Moon from any causally unjustifiable 'interference' with

[22] Fulgenzio Micanzio warned Galileo twice against the risk that other Jesuits – as had happened in the case of the dispute with Christopher Scheiner – could steal his new discoveries and urged him to publish the results. See *Le Opere di Galileo Galilei*, XVII, p. 231 and p. 260. Galileo, though recognising such a risk, gave up the idea of publishing, in all probability because of the incompleteness of the work due to the total blindness that ended his observations. "I wanted to find many other details, not only in regard to the large spots that are observable with the naked eye, but also in regard to those that are dependent on small outcroppings and depressions [. . .], but I have been deprived of this by fate. . . ", (*ibid.*, pp. 295–296).

[23] *Le Opere di Galileo Galilei*, XVII, pp. 292–293. The italics are mine.

[24] According to E. Whitacker, in his letter to Fulgenzio Micanzio Galileo announced that "he was wrong in thinking that the Moon was fixed with respect to the Earth-Moon line", which is nowhere to be found in Galileo's text. This error compromises the understanding of the whole meaning of Galileo's letters to Micanzio and Antonini and, indeed, of his all ideas on the optical nature of Moon's librations. See E. Whitaker, *Selenography in the seventeenth century*, in R. Taton, C. Wilson [eds.], *Planetary astronomy from the Renaissance to the rise of astrophysics. Part A: Tycho Brahe to Newton, op. cit.*, pp. 125. Galileo does not abandon the conviction that the Moon faces towards Earth 'in such a way that a line that joins the centre of the Moon with that of the Earth would perpetually pass through the exact same point on the Moon's surface' (*Le Opere di Galileo Galilei*, XVII, pp. 292-293); quite the contrary, this hypothesis remains for Galileo the very necessary condition for seeing optical libration phenomena.

earthly phenomena like the tides. Had the Moon's librations been demonstrated to be in fact real and physical motions, such an 'interference' might have been at least suggested, if not mathematically justified. Still, as Galileo is going to explain, these motions are not at all real.

After introducing the reader to the elementary optical effect, according to which an observer placed somewhere along the line conjoining the centre of the Earth with that of the Moon can see circles of different radii of the Moon's visible hemisphere, depending on the distance from the Moon's centre, so that the less the distance the less the visible region, the letter to Alfonso Antonini goes on to describe the libration that has a diurnal period:

> [...] should the observer's eye lie in the plane defined by the circle described by the straight line conjoining the lunar and terrestrial centres, because of its being above this line during the daily rotation, it would discover some portion of the rising Moon's surface that would not be visible from the Earth's centre; whereas, at the Moon's setting, when the portion of the Moon that at its rising was situated above becomes the lower portion, the observer's eye would lose sight of the upper portion and would perceive a portion of equal size below .... [25]

A clearer explanation of the situation depicted in Fig. 7.1 could hardly be set down in words. Yet, although the passage is compelling in its clarity, Galileo cannot have observed the daily libration because it is too small and amounts to roughly only one degree.[26] Thus, what else might Galileo have observed? As we have seen, another similar optical phenomenon with similar effects is bound to occur when the Moon orbiting the Earth crosses the points named by Galileo as the dragon's 'bellies', namely, the points where the Moon reaches the maximum and minimum elevation after crossing the ascending or descending node. This variation has a period equal to the lunar sidereal month and is equivalent to what we nowadays call 'libration in latitude' and Galileo calls simply 'monthly libration'[27] . Now, according to his subsequent accurate description of a few telescopic observations, and given that he did not know anything about the tiny tilt of the lunar polar axis, Galileo must actually have observed only a global effect due both to diurnal libration – which, as has been pointed out by E. Whitacker,[28] could be observed

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[25] *Le Opere di Galileo Galilei*, XVII, p. 294.

[26] E. Whitaker suggests that Galileo, in 'observing the dark markings named *Grimaldi* and *Mare Caspium'*, in fact observed the 'libration in longitude' by noting the Moon spots' varying distances from the east and west limbs, but this is inconsistent with Galileo's subsequent description of what he actually had observed on the Moon's surface. The author quotes only the letter to Micanzio that misleadingly drops the hint that Galileo had observed all three kinds of libration. In fact, as Galileo states clearly in the letter to Antonini, he did not. See E. Whitaker, *Selenography in the seventeenth century*, in R. Taton, C. Wilson [eds.], *Planetary astronomy from the Renaissance to the rise of astrophysics. Part A: Tycho Brahe to Newton*, *op. cit.*, pp. 125. See the discussion below.

[27] *Le Opere di Galileo Galilei*, XVII, p. 294.

[28] See E. Whitaker, *Selenography in the seventeenth century*, in R. Taton, C. Wilson [eds.], *Planetary astronomy from the Renaissance to the rise of astrophysics. Part A: Tycho Brahe to Newton*, *op. cit.*, p. 125.

when the Moon is waxing in the west and two weeks later when the Moon is nearly full in the east, or, as I would suggest, when the waning Moon is in the last quarter or even after almost completing one cycle – and to the monthly 'libration in latitude'. Consequently, he must have attributed the whole variation to the daily libration.

Let us listen to Galileo's words reporting his findings:

> [. . .] when the Moon is collocated in the East, there is on it an oval-shaped spot, which is separate from the others and resembles an island, in such a way that it is near the farthest limb of the visible hemisphere; and its position is between the East and Auster, so that we can say that it falls 'below sirocco'. And this, which is amongst the old spots that are real and true, is also conspicuous to the naked eye. Diametrically opposed to this, there are two other spots, which are also separate from the surrounding ones and appear in a large, bright field. They are not far away from the extreme limb of the lunar face that is visible to us and, in relation to the previous spot, we can say they are between Boreas and the West, 'below maestro'. [. . .] Because the positions and orientations of these [spots] fall *between* the maximum circles of the Moon, which run from East to West and from South to North, they enable us to understand both variations, namely, the *diurnal* one and the *monthly* one.[29]

Although it is difficult to identify exactly which spots Galileo may actually have observed, what we can say is that the first one is visible to the naked eye. In all probability he is referring to the dark zone now called *Mare Imbrium*,whereas the identification of the two smaller spots remains open to question. One thing is clear, they are 'diametrically opposed' to the first, which means that, in their reciprocal motion – taking it in turns to appear and disappear – they can only serve the same purpose. As Galileo asserts, 'one catches sight of these [the two opposite spots] while they approach and depart from the Moon's circumference, *alternately corresponding* to the variations of the other spot'.[30] Hence, Galileo can only have observed a complex and mixed libration effect, and, given the accurate position of the spots he gives us – '*between* the maximum circles of the Moon', namely, neither too near the North nor too near the South – this must have been a combined effect due, on the one hand, to the addition of the monthly 'libration in latitude' with the 'diurnal libration', and, on the other, to the 'libration in longitude'. Thus he attributed the whole variation in latitude to what he called 'diurnal libration' and, possibly, from the same couple of diametrically opposed spots, detected a variation in longitude.

On the other hand, this variation in longitude was not consistent with Galileo's model of Moon's librations and their periods. For, in the first part of the letter to Antonini, Galileo expanded on a metaphor – first excogitated in his previous letter to Micanzio –

[29] *Le Opere di Galileo Galilei*, XVII, p. 295. The italics are mine.

[30] *Ibid.*, p. 296. Italics are mine. E. Whitacker thinks that Galileo observed both *Mare Imbrium* and crater *Grimaldi* near the eastern border of the Moon, so that he was able to detect two distinct libration effects, namely, one in latitude and the other in longitude. This is not consistent with Galileo's description. He never speaks of a single spot located near the eastern part of the lunar hemisphere and he was only able to observe a mixed effect. See E. Whitaker, *Selenography in the seventeenth century*, in R. Taton, C. Wilson [eds.], *Planetary astronomy from the Renaissance to the rise of astrophysics. Part A: Tycho Brahe to Newton, op. cit.*, p. 125.

that describes the three trepidations in terms of a head that makes a sort of ‘yes’ motion in a period of one day,[31] a ‘no’ motion in a period of one month and finally a swinging motion about its centre in a period of one year. Now, unfortunately he is not at all clear about the actual orientation of the anatomical details of the Moon’s face. Unnatural as it may appear, the only possible interpretation, according to this metaphor, is that the forehead and the chin lie along the Moon’s equatorial line, so that – as Galileo claims – when the Moon rises we see the Moon’s ‘hair’ and when the Moon sets we see below the ‘chin’; the ears, on the other hand, define the meridian line, so that one ear points northwards and the other southwards.[32] If this is the case, the variation in longitude is not consistent with the fact that Galileo attributes the ‘no’ motion to the Moon’s elevation above and below the ecliptic in its trajectory along the ‘dragon’, because such a motion can only determine, in Galileo’s model, a pure variation in latitude, as he himself overtly recognises.[33]

As regards Galileo’s ‘annual libration’, what we might qualify as the observational part of his research programme was terminated by blindness before he could make further attempts to investigate the annual periodicity of the Moon’s librations. After writing that his observations serve only to understand the diurnal and the monthly variations, he concludes the summary of his new findings in the letter to Alfonso Antonini asserting that

> I wanted to discover other details by means of more accurate observations. . . , but fate prevented me from doing so, [. . .] because six months ago a suffusion flew into my eye and two months ago I became totally blind. . . .[34]

Yet, his ‘speculative’ mind did not shy away from following new thoughts, almost as though his mind were in a sort of intellectual ‘frenzy of enthusiasm’ for new discoveries – as Galileo himself recognises in the letter to Fulgenzio Micanzio.[35]

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[31] E. Whitacker erroneously attributes a period of one month to the ‘yes’ motion. See E. Whitaker, *Selenography in the seventeenth century*, in R. Taton, C. Wilson [eds.], *Planetary astronomy from the Renaissance to the rise of astrophysics. Part A: Tycho Brahe to Newton, op. cit.*, p. 125.

[32] If one follows a more ‘natural’ interpretation, namely, that the forehead coincides with the North, the chin with the South and the ears define the West-East direction, one has to attribute to the ‘yes’ libration a period of one month and to the ‘no’ libration a period of one day. This is not acceptable on the base of Galileo’s text. See *Le Opere di Galileo Galilei*, XVII, p. 294.

[33] See *Le Opere di Galileo Galilei*, XVII, p. 294.

[34] See *Le Opere di Galileo Galilei*, XVII, pp. 295–296. Not only did Galileo’s observations cease, as is clear from the text, but his annual libration model – which we shall examine in a moment – has nothing to do with our 1.5° libration due to the tilt of the lunar polar axis. E. Whitacker’s position, according to which Galileo has probably not observed such a small annual libration, would therefore appear untenable. In 1638, far from looking for any such libration, Galileo was actually no longer capable of ocular observation. See E. Whitaker, *Selenography in the seventeenth century*, in R. Taton, C. Wilson [eds.], *Planetary astronomy from the Renaissance to the rise of astrophysics. Part A: Tycho Brahe to Newton, op. cit.*, p. 125.

[35] *Le Opere di Galileo Galilei*, XVII, p. 214. Galileo used *limbo* (limb) or *taglio* (cut) to indicate the line that separates the lunar hemisphere lit up by the Sun from that still in darkness, whereas ’terminator’ is the technical word used in present-day astronomy.

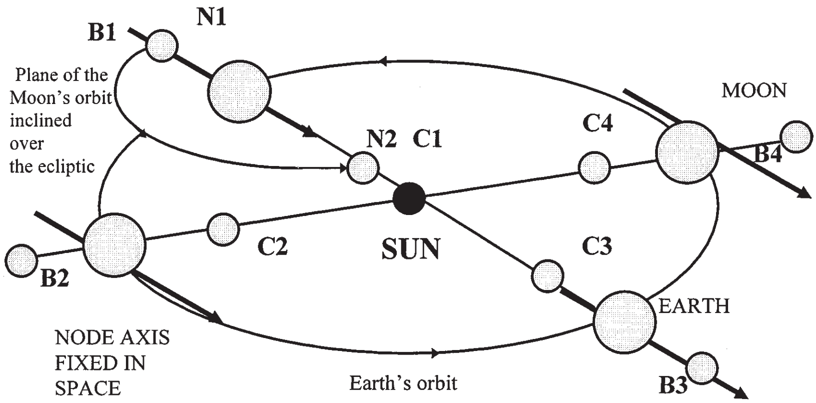

Figure 7.3

Indeed, he had devised an ingenious explanation for his annual libration, thus completing the Moon's trepidation model he had been working on for a few years.

> There will also be a variation regarding the hemispheres lit up by the Sun, because when the Moon leaves the Sun in its location near the nodes, the *limbo* – *taglio,* if you like – of the illumination will divide the Moon's face that is visible to us in a direction different from that observed when the Moon, on her first moving away from one of her bellies, leaves the Sun. This variation has a periodicity of one year, given that the Sun returns to the selfsame node in almost one year owing to the slowness of the motions of the nodes.[36]

Although it looks fairly complicated, Fig. 7.3 below should clarify what Galileo had in mind.

First of all, one has to bear in mind that the Moon's orbital plane in Galileo's model can be though of as being fixed (the thick arrow in the figure) in space with respect to the fixed stars, because – apart from the fact that he does not know about the apsidal motion of the Moon's elliptical orbit – Galileo makes the assumption that the motion of the nodes (N1 and N2), which he knows about, is sufficiently slow, so that the Sun 'returns' almost in one year to the same node – as is clear from the end of the passage just quoted.[37] Now, Galileo's premise is that the Sun must be near a node or coincide with it. This condition occurs once a year for each node, the ascending one or the descending one, precisely because in this simplified model the node line of the Moon's orbit is aligned to the Earth-Sun radius twice a year, owing to the fact that the lunar orbital plane defines a geometric plane that is fixed in space and can be thought of as proceeding 'attached to Earth' throughout the year while remaining parallel to itself.

As Galileo notes, while transiting across one node the Moon moves northwards or southwards at an angle of more or less 6° with respect to the ecliptic, whereas, when

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[36] *Ibid.*, p. 294.

[37] The period of the Moon's nodal motion is about 18 years. Galileo's hypothesis is acceptable within the scope of his model.

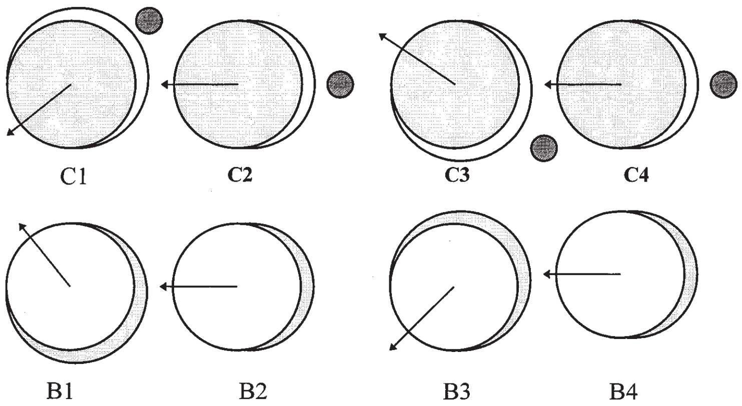

Fig. 7.4. The swinging motion of the Moon

the Moon, moving away from one of her 'bellies', leaves the Sun the direction of her motion is parallel to the Earth's orbital plane. In the former case, the shape of the first 'sickle' of the Moon in her waxing or waning phase is symmetrical relative to a diameter inclined by 6^o with respect to the Moon's equator, while in the latter the 'sickle' is perfectly symmetrical with respect to the lunar equator. This occurs three months after the transition across the node. These configurations are numbered 1,2,3,4 in Fig. 7.4 (where dimensions and angles have been purposely exaggerated, the arrows indicate the direction of the Moon leaving the Sun and the small circles represent the Sun, which is beyond the waxing Moon in C1,C2,C3,C4 and opposite the waning Moon in B1,B2,B3,B4). B1,B2,B3,B4 represent the waxing Moon, while C1,C2,C3,C4 represent the waning Moon. Configurations 3 and 4 give the situation, respectively, 6 and 9 months after configuration 1. The periodicity of the whole phenomenon is therefore one year, as Galileo states. In other words, what one should observe is a sort of a swinging motion of the Moon's face about her centre.

7.2 A single great wave

There can be no doubt that the coincidence of the trepidation model's periodicities with the three periods of the tide model must have aroused great astonishment. And it is hard to imagine Galileo's not being amazed at his discoveries. Especially in regard to the annual libration, my contention is that Galileo in his old age racked his brain to come up with some reasonable proposal whereby to reconcile phenomena as diverse as multifaceted tides and trepidations in one elegant model that would satisfy his innate sense of geometric symmetry. For a moment, he might also have dreamt of being able to establish a causal connection between the real motions of the seas and the apparent ones of the Moon; but these *were not motions*, they were simple changes of viewpoint, optical effects, whose regular appearances he could only capture in a coherent geometric whole. Even though not entirely correct, his models mastered the complexity of both tide and libration periodicities, but, aware of the impossibility of finding causal relationships, he had to abandon any idea of establishing cause and effect. Above all, Galileo's research into the Moon's periodic wobbles led him to renounce the quest for a confirmation of a supposed magnetic correspondence between Earth and Moon – a correspondence that, in his view, would, in the final analysis, amount to no more than an occult quality and which, as such, would be incapable of a rational explanation of the causes of phenomena like the motion of the Moon and planets and the ebb and flow of the seas. For, if such a magnetic 'natural agreement' between Moon and Earth were to be connected in any way with the causes of the tides, this should materialise precisely in physical motions of the Moon, whereas the sole librations that Galileo observed with the telescope are perfectly coherent with a model based on the hypothesis that the Moon's face is immobile relative to Earth, almost as if their centres were really conjoined by some sort of physical and rigid line. And the opening of the letter to Antonini honestly reveals that what Galileo had discovered bore no relationship whatsoever to anything he was able to think of:

> [. . .] the new discoveries I have made [about the Moon] are great, in the manner in which every lowliest effect in nature is great. Yet, I have not hitherto been able to draw from them any great conclusions, by contrast with what I have derived from other observations.[38]

The librations research programme might not have paid off in terms of the grand results Galileo had been expecting from it, but it is likely to have occasioned his flurry of renewed interest in the behaviour of tides, particularly in relation to their periodic characteristics, insofar as they could be observed in Venice, where his friend, Fulgenzio Micanzio, was only too eager to satisfy the scientist's requests for fresh information. Historiography has so far omitted to take these late sources into account and has therefore lost the opportunity to see the final confirmation of Galileo's insightful understanding of tide phenomena as waves.

On 30th January 1638, Galileo wrote to Father Fulgenzio asking him for information about possible differences between the sea's maximum and minimum levels with respect to the average.

> The ebb and flow of the sea is observed to be maximum during full moon and new moon, whereas it is observed to be minimum during quadratures, which is the reason for the local saying 'seven, eight and nine, the water does not move; twenty, twenty-one and twenty-two, the water goes neither up nor down', which are the quadrature times. Now, it being possible that ebb and flow is great in two ways, that is, since the water rises above average or drops below average, [. . .] what I would like to be informed about is whether these two different ways occur during both full and new moon, or whether, during either of these – for example, full moon – ebb and flow is great because of the water rising well above average, while during the other period – that is, new moon – ebb and flow depends on water dropping below the average level .[39]

As we have seen, one consequence of Galileo's celestial wheel-clock model is that the resulting tide-generating acceleration is greater at the syzygies and less at the quadratures.

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[38] *Le Opere di Galileo Galilei*, XVII, p. 292.

[39] *Le Opere di Galileo Galilei*, XVII, pp. 270–271. The saying in Italian goes 'sette, otto e nove l' acqua non si move; venti, ventuno e ventidù, l' acqua non va nè in su nè in giù'.

It should not therefore come as a surprise that Galileo now wants an empirical verification of such a consequence, which is derivable from his model of the monthly period but was never made explicit in the *Dialogue*. Although he does not reveal the purpose of his request for this information – which, in any event, Father Fulgenzio would hardly been able to appreciate – my conjecture is that this was precisely what he had in mind. The only way in which the Moon does fit Galileo's tide model is through the wheel-clock analogy. We can see how the answer to his request might have been enlarged on by his 'restless brain': if the behaviour of the water were symmetrical with respect to the full moon and new moon phases, then there should be no reason for doubting that any lunar effect must be accounted for by the Earth's acceleration at new Moon and by its half-month-later deceleration at full Moon. On the other hand, it is quite understandable that, after being momentarily 'lured' by the dazzling light of the triple periodicity of his newly-discovered optical trepidations, he should be eager to test a consequence that his model clearly entails.

After prompting his friend to gather information on this subject, Galileo goes on to describe the true aim of the inquiry. With this he puts the final stamp on a long series of investigations into the nature of tide phenomena. This time he is very explicit. And these are the final words he pronounced on his lifelong interest in the mind-boggling question of the flux and reflux of the sea.[40]

> I would also like to know about another detail, which is this. When the water comes in through the Malamocco cut, i.e. through Due Castelli, it makes the lagoon rise and surges beyond Venice, Murano and Marghera, as far as the distant shores towards Treviso; whereas, when the water ebbs, it starts to drop in Malamocco or Due Castelli before it starts to drop in Venice, Murano and in the farthest places. From this – if it occurs as I have described – I draw a conclusion, namely, that this phenomenon, which nature brings about, may be called by the selfsame name as other common water motions. In other words, the flow is *one single great wave* that moves in the same manner in which an infinite number of lesser waves – which are called white horses – are observed to travel towards the shore and invade the beach before receding, without resting one moment. And I have observed this phenomenon in Venice many a time.[41]

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[40] On 20th April 1641, Galileo wrote a letter to Francesco Rinuccini and, commenting on a recent book by the physician Giovanni Nardi entitled *On Subterranean Fire* – the tract was published in Latin under the title *De igne subterraneo* – ironically pointed out that "after reading for no more than a hour, you will see the solution to many marvellous natural effects, whereas just one has made me despair of understanding it, in spite of all my lifelong contemplations" (*Le Opere di Galileo Galilei*, XVIII, p. 323). The reference to the tide problem is clear. The full title of Nardi's tract is *De igne subterraneo, physica prolusio D. Ioannis Nardii Florentini, Serenis.mo Ferdinando II, M.D. Etr., Domino suo clementissimo dicata*. Florentiae, excidebant Amator Massa et Laurentius de Landis, 1641.

[41] *Le Opere di Galileo Galilei,* XVII, p. 271. Italics are mine. Malamocco is situated on the narrow strip of land that is nowadays called Litorale Lido and stretches southward bordering the lagoon. With the word 'cut', Galileo is probably referring to an old channel that up until the nineteenth century connected the lagoon with the see and has since then been filled up.

Father Fulgenzio duly made a few qualitative observations and reported on them to Galileo a fortnight later. Unknown to him, the Servite was to give what must have appeared to Galileo a baffling summary of the situation, for, not only did the asymmetrical behaviour of tides during new Moon and full Moon seem to be confirmed, the wave-like and progressive development of the phenomenon was confirmed as well:

> Sometimes, for example during October and November, the rise of the sea is greater than its fall, because water piles up above the foundations and damages wells, whereas sometimes it drops so little as to remain above the level that is reached when it runs in different periods. On the other hand, in the previous two months [December and January], the water dropped so much, that the canals were left dry. Nevertheless, I did not make any precise quantitative measurements. In addition, it is certain that after the water comes in through Due Castelli and the Malamocco cut a considerable time elapses before the lagoon starts to rise or fall. And once, while I was out in my gondola [. . .], I realised that the flow was like a continuous wave that slowly advances over a long period of time. I have not made any observation about the detail regarding a possible delay between the flux and reflux, in regard to which, Monsignor Aproino reported to me two observations. The river Sile in Casale, which is between the lagoon and Treviso, follows the flux and reflux periods in such a way, that the daily difference between the highest level and the lowest one is more than a *braccio*,following a time scale according to which when water drops in the lagoon, the river Sile is still rising, *et e contra*. This goes without saying. The second observation is more interesting. Even in Treviso and along all Sile, from source to mouth, one can observe a tidal phenomenon, though in Treviso it amounts to only one *palmo*. We thought that this could not be due to any impediment caused by sea water, because the river bed slopes more than eight feet; on the contrary, it must be due to the vessel's motion, given that the Sile always runs slowly from West to East.[42]

What Galileo might have made of all this is hard to say. Micanzio clearly asserts that there is an asymmetrical effect with respect to the Moon's phases and this was also to be confirmed by further information given to Galileo by Francesco Rinuccini,[43] though only from a qualitative point of view. Still, there can be little doubt that the evidence was significant enough to fuel further speculations by Galileo as to the nature of the dependence of tide phenomena on the Moon's orbiting Earth. One thing is clear. As Micanzio appears implicitly to have recognised at the end of the passage just quoted,

[42] *Le Opere di Galileo Galilei*, XVII, pp. 286–287. The letter is dated 13 February 1638. Micanzio wrote again a month later in March promising Galileo that he would observe the tide during an entire lunation (*ibid*., p. 317). Unfortunately no more reports survive, if there were any.

[43] See Rinuccini's letters on 13th February, 27th February, 6th March, 13th March, 15th May, 18th September 1638 (*ibid*., p. 288ff). Apart from the above-mentioned confirmation, the only significant thing we learn from this exchange of letters is that even Rinuccini was asked by Galileo to investigate the tides in Venice. Nevertheless, his fragmented and short reports gives us no further information on the tenor of Galileo's speculations.

Galileo did not disown his tide model based on the fundamental mechanism of the double motion of the Earth-Moon system, namely, the motion of the vessel.

Quite the contrary, though he was evidently interested in investigating the possibility that the Moon's motion around the Earth had a more complicated effect than that elegantly predicted by his simple wheel-clock model, he came to formulate overtly and unambiguously the basic principle of tidal ebb and flow, namely, its being 'one single great wave'. It was left to Laplace's mathematical genius, late in the second half of the eighteenth century, to demonstrate once and for all that the Newtonian tide-generating force does not have the 'banal' effect of making the oceans bulge beneath the Moon and the Sun, but the much more subtle one of powering the Galilean 'single great wave'.

Appendix 1. Newton's asymmetric tide-generating force

Let us draw the diagram presented previously in Fig. 2.2 of Sect. 2 in a slightly modified form (Fig. A1.1).

Here we have a Cartesian frame of reference, whose centre is Earth T and whose X and Y axes are, respectively, the line TM and the line upwards normal to TM. All we need to do is write down two expressions for the x and y coordinates of point L – let us call them xL and yL – by which we will be able to obtain the direction of the tide-generating force LT acting upon P simply as the tangent of the angle α, where $\mathrm{tg}\alpha = \frac{\mathrm{yL}}{\mathrm{xL}}$. As the reader has probably already guessed, the final step is to search for the transition point by imposing the condition that the direction given by $\mathrm{tg}\alpha$ be tangent to the Earth's surface. We shall also need a small computer in order to solve the non-linear equation for the transition point.

If we define ST $=$ a, SP $=$ b, PT $=$ r and the angles $\theta =$ PTS and $\phi =$ PST as shown in Fig. A1.1, then the following relations must hold true:

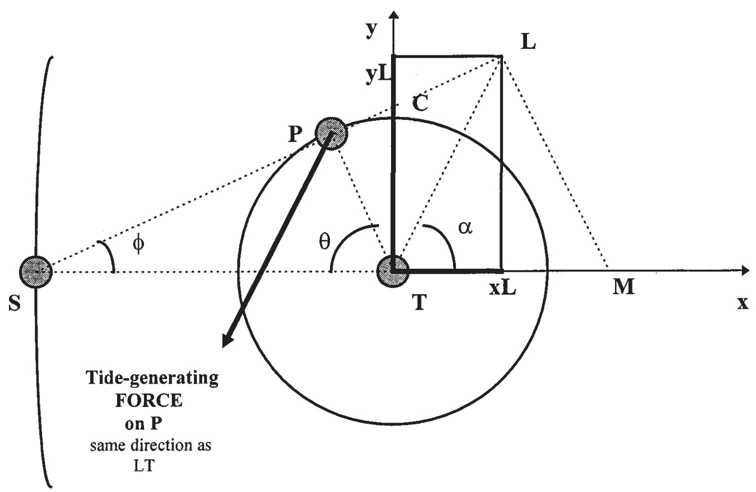

Figure A1.1

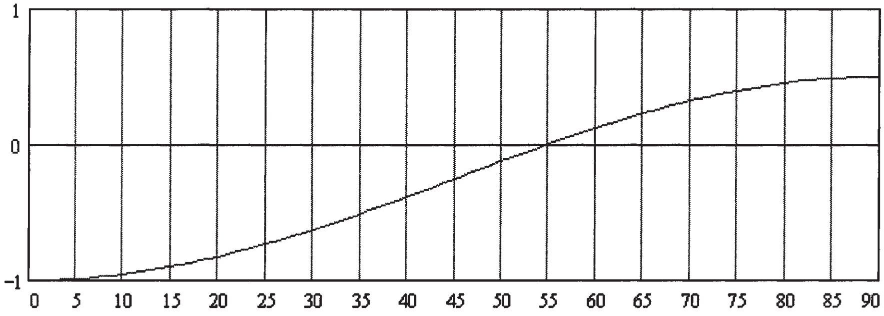

Figure A1.2

$$\mathrm{SL} = \frac{\mathrm{a}^3}{\mathrm{b}^2}, \mathrm{xL} = \mathrm{SL} \cdot \cos\phi - \mathrm{a}, \mathrm{yL} = \mathrm{SL} \cdot \sin\phi, \mathrm{l} = \mathrm{r} \cdot \sin\theta$$

$$\mathrm{w} = \mathrm{a} - \mathrm{r} \cdot \cos\theta, \cos\phi = \frac{\mathrm{w}}{\mathrm{b}}, \sin\phi = \frac{\mathrm{l}}{\mathrm{b}}$$

$$\mathrm{b}^2 = \mathrm{l}^2 + \mathrm{w}^2 = \mathrm{r}^2 + \mathrm{a}^2 - 2 \cdot \mathrm{a} \cdot \mathrm{r} \cdot \cos\theta$$

so that:

$$\mathrm{xL} = \frac{\mathrm{a}^3}{\mathrm{b}^3} \cdot (\mathrm{a} - \mathrm{r} \cdot \cos\theta) - a \tag{1}$$

$$\mathrm{yL} = \frac{\mathrm{a}^3}{\mathrm{b}^3} \cdot \mathrm{r} \cdot \sin\theta$$

and the required tangent turns out to be:

$$\mathrm{tg}\alpha = \frac{\mathrm{a}^3 \cdot \mathrm{r} \cdot \sin\theta}{\mathrm{a}^3 \cdot (\mathrm{a} - \mathrm{r} \cdot \cos\theta) - \mathrm{a} \cdot (\mathrm{r}^2 + \mathrm{a}^2 - 2 \cdot \mathrm{a} \cdot \mathrm{r} \cdot \cos\theta)^{\frac{3}{2}}} \tag{2}$$

which gives the direction of the tide-generating force. Now, as is clear from the diagram shown below in Fig. A1.2, the transition point occurs where force LT is tangent to the Earth's surface, namely, where it is normal to radius r, so that, since in this case the angle PTL forms a right angle, the following must be true by virtue of simple trigonometric relations:

$$\mathrm{tg}\alpha = \cot\mathrm{g}\theta \tag{3}$$

which is the equation we have been looking for. By substituting from expression (1) in (2) a new equation containing just the angle θ is obtained. To solve equation (2) involves looking for the value θ that corresponds to the point on the Earth's surface we have named 'transition point'. In order to do so, we simply need the ability of modern 'number-crunching' machines, for the non-linear nature of equation (2) prevents us from simply applying the standard rules by which normal linear equations are solved. In other words, we shall simply make a computer plot the function $\mathrm{F}(\theta) = \mathrm{tg}\alpha - \mathrm{cotg}\ \theta$, over

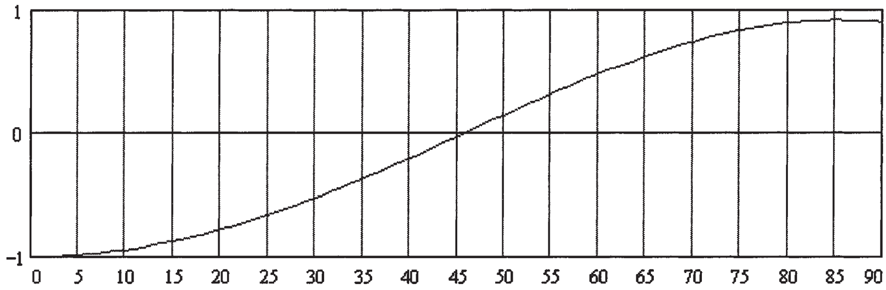

Figure A1.3

the variable θ, and from the plot determine the point where it crosses the zero axis. Fig. A1.2 shows the curve when the Sun is taken as the disturbing body. The Earth's radius has been given a modern value of 6370 km and the average Earth-Sun distance has been taken to be equivalent to 23 million Earth's radii, but it should be noted that even significant variations in these values have a very modest influence on the results, so that no dispute should arise about my decision to adopt modern astronomical values as the basis for calculation:
The horizontal axis ranges from 0 to 90 degrees, the horizontal line at the centre is the zero line and the crossing point is very close to 55 degrees. Since there are 360 degrees in a circle and 1440 minutes in a day, one degree corresponds to 4 minutes and it follows that the solar high tide period T is:

$$T = 4\,\text{min} \cdot 2 \cdot 55 = 7\text{ h }20\text{ min}$$

as stated above (the same procedure goes for the Moon, but I shall spare the reader tedious, identical computations).

Now an interesting question arises. What could possibly have caused Newton to be led astray in his evaluation of the phenomenon? The key to the answer is perhaps hidden in the potentially misleading scale of the diagram drawn in Fig. A1.1. Locating the disturbing body S so close to the Earth, in other words assuming the Earth-Sun distance to be comparable with the Earth's radius, leads one to the natural, though misleading, conclusion that when point P is more or less on the border line of the octant before the syzygy, where $\theta \sim 45°$, the direction of the tide-generating force is virtually tangent to the Earth's surface and normal to its radius. My argument is corroborated by the mathematical model I have been discussing, which proves that taking the Earth-Sun (or Earth-Moon) distance to be equal roughly to three or four times the Earth's radius makes the transition point fall virtually on the line $\theta \sim 46°$, as seen in the next diagram (Fig. A1.3).

In other words, it is quite possible that Newton confidently assumed the graphic representation of the three-body diagram to represent a good analogy with the real situation, given that in drawing a sketch like that of Fig. A1.1 the only linear proportion that must be respected is the ratio between the lengths of line SL and line SP, which represent the gravitational forces acting on P and Earth's centre T. Yet, he could have noted that the resulting tide-generating force LT is not independent of direction PS and that taking PS to be nearly parallel to ST – as it should be, given the enormous distance between Sun and Earth – would have rotated LT and, in consequence, cast some doubt on the location of the transition point.

Appendix 2. The math of warps

Part 1. A comparison between Galileo's formula and Huygens'

As we have seen in Sect. 4, a Galilean quasi-centrifugal force may be written in the following form:

$$\frac{Fg_A}{Fg_B} = \frac{-R_A + \sqrt{V^2 + R_A^2}}{-R_B + \sqrt{V^2 + R_B^2}} \tag{1}$$

whereas Huygens' formula (and ours) is

$$\frac{F_A}{F_B} = \frac{\frac{V_A^2}{R_A}}{\frac{V_B^2}{R_B}} \tag{2}$$

In order to compare relation (1) with relation (2), we have to abandon the proportion-like form in which they are expressed and assign numerical values to the parameters. First of all, let us consider the following ratio between F_g (Galilean quasi-centrifugal force) and F_h (Huygens formula):

$$\frac{F_g}{F_h} = \frac{-R + \sqrt{V^2 + R^2}}{\frac{V^2}{R}} \tag{3}$$

We can test F_g, for example, by assigning a value to V (Earth's tangential speed)[1] and calculating the function (3) for different values of radius R. Suppose $V = 462$ m/s (an average value) and R varies between $R = 1$ m and $R = 1000$ m. Fig. A2.1 gives relation (3) against R. As is clear, for R approaching $R = 1000$, $\frac{F_g}{F_h}$ tends to 1, namely, $F_g \to F_h$. Assuming a realistic range for R, for example $R = 1000$ m to $R = 6370$ km (an average value for the Earth's radius), we have virtually $\frac{F_g}{F_h} \cong 1$, namely, $F_g \cong F_h$.

If we now let $V = 1$, the approximation of Galileo's formula is even more striking, given that virtually $F_g \cong F_h$ when R approaches $R = 10$ (see Fig. A2.2). Obviously, the most significant difference that remains between Galileo's quasi-centrifugal force and Huygens' exact formula, is that Galileo's cannot describe centrifugal force as a function of two independent variables (radius and angular speed).

In conclusion, it must be noticed that Galileo's formula becomes exact (though it is not yet able to 'absorb' both the independent variables of circular motion), when

[1] Let us remember that in Galileo's formula V is actually a 'distance' travelled in a time equal to 1 unit, namely, the distance travelled in one second, where time is measured in seconds.

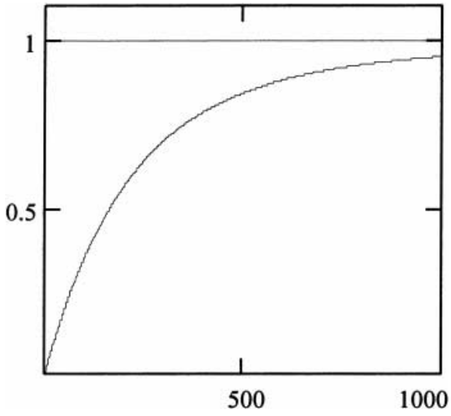

Figure A2.1

$\omega = \sqrt{\frac{g}{R}}$, that is, when it is applied to the 'weightless' condition. This result is consistent with the fact that the 'equilibrium solution' is a description of the 'weightless' condition, as we have seen in Sect. 4. To demonstrate this, let us write F_g under the 'weightless' hypothesis; when $V^2 = g \cdot R$,

$$F_g = -R + \sqrt{g \cdot R + R^2}.$$

If we now let $R \to \infty$, we obtain

$$\lim_{R\to\infty} F_g = \lim_{R\to\infty} -R + \sqrt{g \cdot R + R^2} = \frac{g}{2}$$

which is the distance travelled by a free-falling body in 1 second (g is the gravity constant).

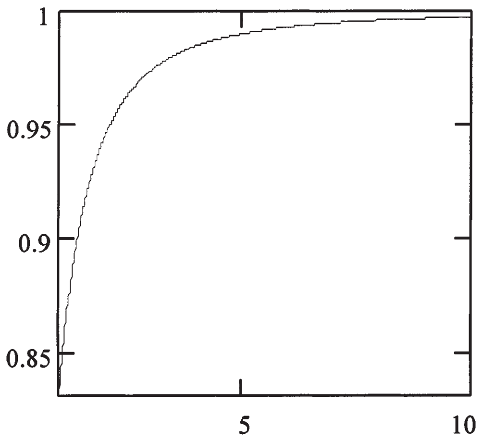

Figure A2.2

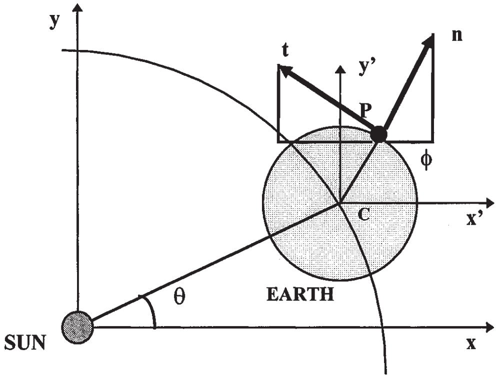

Figure A2.3

Part 2. The scalar products: $(\vec{F}_{PS} \cdot \vec{t})$ and $-\left(\frac{d^2}{dt^2}x_C \cdot \vec{i} + \frac{d^2}{dt^2}y_C \cdot \vec{j}\right) \cdot \vec{t}$.

In order to show that

$$(\vec{F}_{PS} \cdot \vec{t}) = F_{PS_t} = -G \cdot m_P \cdot m_S \cdot \frac{-\sin\phi \cdot x_C + y_C \cdot \cos\phi}{r_{PS}^3},$$

we need to express vector $\vec{t}$ as $\vec{t} = \vec{k} \times \vec{n}$ (where $\vec{k}$ is the vector normal to the plane of this sheet), because vector $\vec{n}$'s components are much simpler to 'see' in the drawing. Referring to Fig. A2.3, by virtue of elementary trigonometric relations it must hold true that in P vector $\vec{n}$ is given by:

$$\vec{n} = \cos\phi \cdot \vec{i} + \sin\phi \cdot \vec{j}$$

so that $\vec{t} = -\sin\phi \cdot \vec{i} + \cos\phi \cdot \vec{j}$. Let us recall Newton's gravitational force expression

$$\vec{F}_{PS} = -G \cdot \frac{m_S \cdot m_P}{r_{PS}^2} \cdot \frac{\vec{r}_{PS}}{\|\vec{r}_{PS}\|}. \tag{4}$$

Given that $F_{PS_t} = (\vec{F}_{PS} \cdot \vec{t})$, all we need do is calculate the product $(\vec{r}_{PS} \cdot \vec{t})$. But $\vec{r}_{PS} = (x_C + R \cdot \cos\phi) \cdot \vec{i} + (y_C + R \cdot \sin\phi) \cdot \vec{j}$, as Fig. A2.3 makes clear, and eventually

$$(\vec{r}_{PS} \cdot \vec{t}) = -x_C \cdot \sin\phi + y_C \cdot \cos\phi$$

so that

$$\vec{F}_{PS} \cdot \vec{t} = \vec{F}_{PS} = -G \cdot \frac{m_S \cdot m_P}{r_{PS}^2} \cdot \frac{1}{\|\vec{r}_{PS}\|} (\vec{r}_{PS} \cdot \vec{t}) = -G \cdot m_P \cdot m_S \cdot \frac{-\sin\phi \cdot x_C + y_C \cdot \cos\phi}{r_{PS}^3}$$

which is the relation we were looking for. In the same manner the reader can see by herself/himself that

$$-\left(\frac{d^2}{dt^2}x_C \cdot \vec{i} + \frac{d^2}{dt^2}y_C \cdot \vec{j}\right) \cdot \vec{t} = \frac{d^2}{dt^2}x_C \cdot \sin\phi - \frac{d^2}{dt^2}y_C \cdot \cos\phi$$

holds true.

Part 3. Newtons equations of motion of Earth's centre C

As we have seen in Sect. 4, the following relation represents Newton's expression for the gravitational force exerted upon the Earth's centre C by Sun S (where $\vec{r}_{CS}$ is the vector distance between Sun's centre and Earth's centre and m_S, m_T are the masses of Sun and Earth):

$$\vec{F} = -G \cdot \frac{m_S \cdot m_T}{r_{CS}^2} \cdot \frac{\vec{r}_{CS}}{\left\|\vec{r}_{CS}\right\|}$$

Given that, by definition, $\vec{r}_{CS} = x_C \cdot \vec{i} + y_C \cdot \vec{j}$ and that $\left\|\vec{r}_{CS}\right\| = r_{CS}$, then the previous relation's components simply become:

$$\begin{cases} \frac{d^2}{dt^2} x_C = -G \cdot \frac{m_s}{r_{CS}^3} \cdot x_C \\ \frac{d^2}{dt^2} y_C = -G \cdot \frac{m_s}{r_{CS}^3} \cdot y_C \end{cases}$$

which are the two equations of motion of Earth's centre C that we wanted to obtain, one in the x direction and the other in the y direction.

Appendix 3. The repetition of the spinning bucket experiments

Galileo has not left any sketch of the experimental apparatus he described in the *Discourse on Comets*, which was very simple, whereas Father Grassi in his *Libra* has had engraved four different drawings of the equipment he used.[1] These sketches are very clear and one of them is the same as that reported to Galileo by Giovanni Ciampoli in a postscript to his letter from Rome in August 1619. On the basis of Grassi's drawings, and of the verbal descriptions of both, I have built as similar a device as I could. I have chosen transparent materials like Plexiglas and glass in order to make it easier to photograph and to see through the walls. It should be noted that this choice does not affect the results, nor were the meterials ever a very controversial point in the discussion between the two scientists. It was only Galileo who ventured into an explanation of the boundary layer effect in alluding to the microscopic roughness of the surface of the vessel's walls. Indeed, what was really at stake in the dispute was the effect far from the walls.

The dimensions of the vessel and the glass sphere as well as the rotational speed are not mentioned by the authors, who refer to these parameters in qualitative language. I therefore felt justified in not bothering to reproduce exactly the mechanical device that sets the apparatus in motion. Neither did I need to measure the speed or any other effect in a precise numerical manner. Quantitative precision in measuring was never an issue; in any event, it must have been virtually beyond the practical ability of both the contenders.

The balance-like detectors are made of rigid cardboard and shaped after Grassi's drawings. They are suspended by a thin nylon thread and are also free to rotate on small hubs top and bottom, which I keep well lubricated so as to avoid friction. I have built

[1] See *Le Opere di Galileo Galilei*, VI, p. 156–159.

Figure A3.1

the sphere detector the same shape as the vessel – even though Grassi shows a different sort of circular detector in his drawing – simply because I could not understand how such an object could have been introduced into the sphere. Given that my detector has a smaller surface area than Grassi's, the effects in my apparatus can only be less evident than those he must have been observing, so that if some effect is produced – as indeed it is – the experiment remains valid. The wooden support for the candles is attached to a light aluminium tube which in turn is connected to a fixed wooden wall by means of a rigid L-shaped metal bracket.

By mounting small wooden winglets below the revolving plate I have achieved a simple turbine powered by compressed air capable of supplying the whole apparatus with the necessary energy to rotate, and, by slowly opening and closing the outlet of the air compressor I am able to make the vessel or the sphere rotate smoothly, avoiding any jolting that would otherwise interfere with the response of the detectors. Since neither Grassi nor Galileo described the means by which they operated their apparatuses, I felt free to choose the compressed air system as the most suitable and easy to operate. On the other hand, whatever the mechanical means they chose to set their equipment in motion, it must have been effective and simple enough not to cause them to raise any objections either way.

Figure A3.1 and A3.2 show the apparatus with the Plexiglas vessel and the glass sphere along with their balance-like detectors. They are sealed by means of cardboard lids. Figure A3.3 shows Galileo's Experiment 1, in which no bending of the flame is visible. Figures A3.4 and A3.5 show Grassi's Experiment 4 with the burning candle located in two different positions at different distances from the turning walls of the sphere. In both cases the flame is bent in the direction of rotation. Experiment 2 and 3 obviously cannot be documented by photographs.

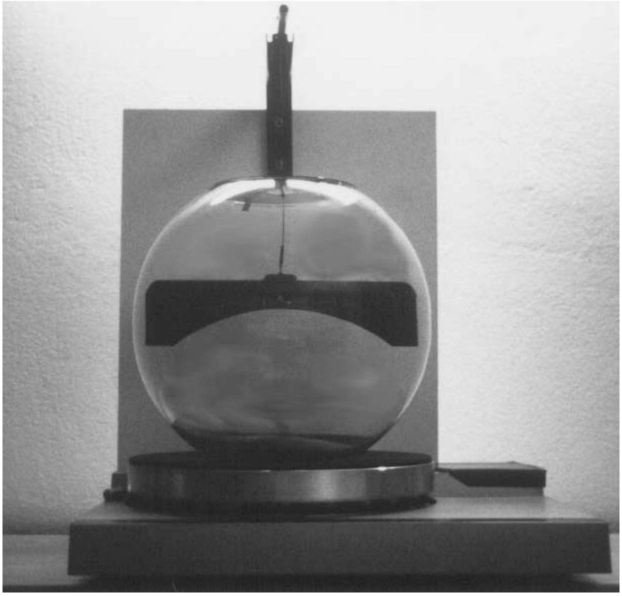

Figure A3.2

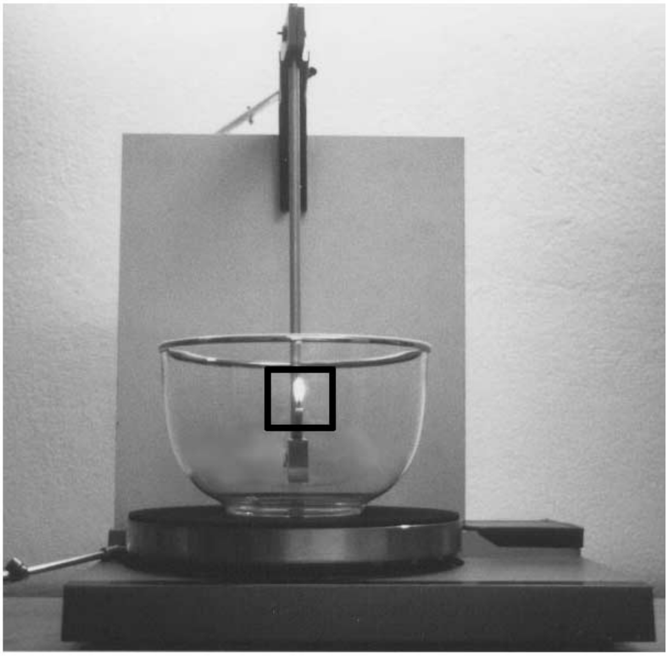

Figure A3.3

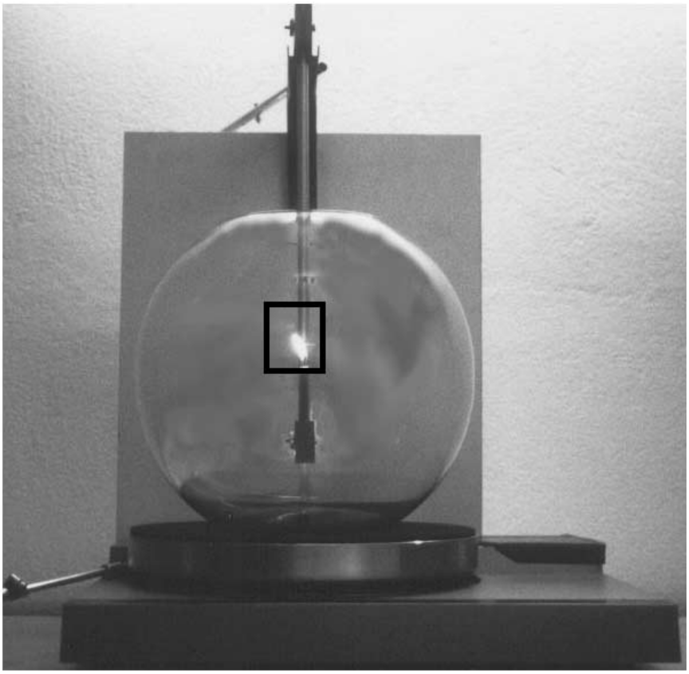

Figure A3.4

In conclusion, I would like to stress that the results have always turned out to be so very clear, the effects so easy to observe and so perfectly in accordance with the descriptions of their first authors as to credit both Galileo and Father Grassi with total transparency in their experiments. This also rules out the remote possibility that some of the tests – possibly the fourth, which is mentioned by Grassi only in the *Ratio Ponderum*, or even Galileo's original – may have been simply thought experiments that were not really carried out or only partly so, for the effects described are precisely what I have observed and could by no means have been predictable on the basis of pure intuition.

Appendix 4. The repetition of Galileo's experiments on tides

In order to repeat Galileo's experiments with 'small artificial vessels' I had a parallelepipedal glass tank built (70 cm long and 40 cm high) together with a robust wooden structure to support it. This structure is constructed in such a way as to guide the tank precisely along a rectilinear path, thereby avoiding any spurious effects due to unwanted transversal movement. By means of two simple handles at either end of the wooden structure I am able to move the glass tank to and fro and to impart to it acceleration and deceleration. By simply pushing the tank hard and immediately releasing it, so as to leave it at rest, I can simulate the so-called 'free-response' of the tidal wave, namely, the free motion subject only to gravity (physicists call this response more precisely the

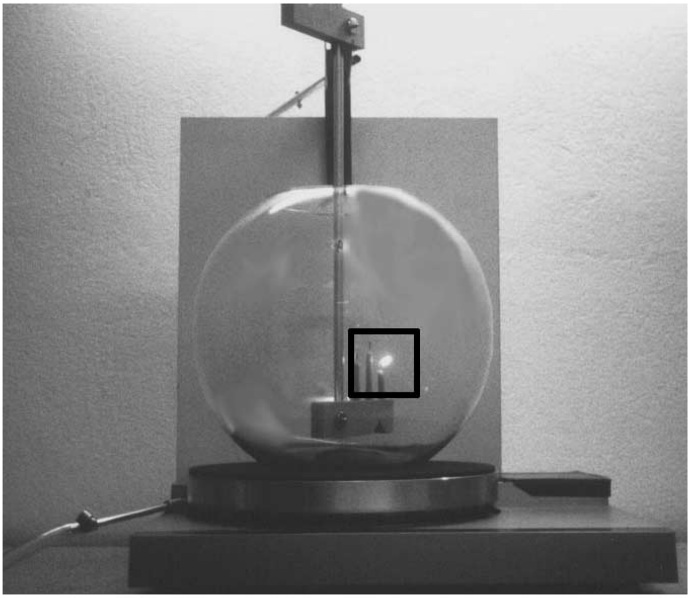

Figure A3.5

‘impulsive response’ because the initial conditions of the motion are dictated by a thrust acting for a very short time). Figure A4.1 shows the tank at rest and filled with water.

The procedure I follow in order to verify the law of depth (I have done the same for the law of width with a moveable glass wall located in different positions) is simple and does not involve the use of any measuring equipment that was not available to Galileo

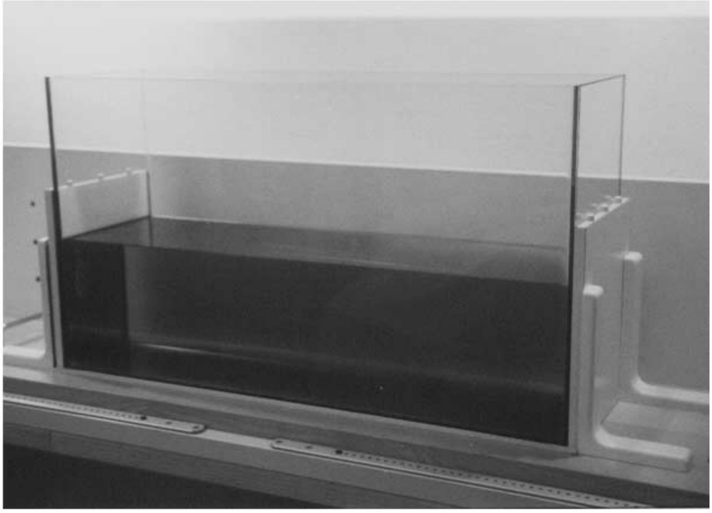

Figure A4.1

himself. I fill the tank with different quantities of water and then count the vibrations over one minute by simply observing the maximum peak of amplitude at one end of the tank (Galileo' could have chosen a different time, but it must be noted that what matters is not so much the precision in measuring short time intervals as the number of periods over a sufficient length of time). For such an approach to work, the motion must manifest a clearly recognisable maximum at the extreme. And for only one absolute maximum to be clearly detectable, one has to learn how to excite the so-called 'fundamental mode of oscillation', that is the sine wave with lowest frequency. This is because the wave profile is always the result of the mixing of a number of 'modes' of oscillation with different frequencies (actually the simple sine components of the resulting stationary wave).[1]

Before going on to present the quantitative results regarding the law of depth, a few elementary notions concerning the behaviour of waves in a rectangular tank need to be discussed.

By giving the tank a hard shove one should in principle be able to excite all the modes of oscillation. In practice, given that the shove can only last for a finite, though short period of time (on average, less than or equal to a second) and given that the higher frequency modes tend to disappear soon because of the energy dissipation caused by internal friction, by learning how to 'calibrate' one's shove one can easily learn how to excite mainly the fundamental mode of oscillation. The skill takes no more than a quarter of an hour to acquire. For a parallelepipedal tank like the one I have used, the fundamental mode is given by a wave whose 'wave-length' is half the length of the glass. Figure A4.2 shows a simulated wave consisting just of the fundamental mode of oscillation. Figures A4.3 and A4.4 show simulations of a combination of the first and second mode and of the first and third mode.

Figures A4.5, A4.6, A4.7, A4.8 are photographs that were taken with the tank at rest after a few seconds had elapsed since the initial shove. The delay is necessary in order to allow the motion to become as stationary as possible. As is clear, Fig. A4.5 displays exactly the same configuration as that given in the simulation of the above example in Fig. A4.4, while Fig. A4.6 represents the first mode, although in a less recognisable form with respect to the simulation of Fig. A4.2. This is because I had no way of synchronising the camera with the rapid motion in the tank (the basic frequency is about one second), so that, although I took no less than some forty shots, the best result is photograph A4.6.

Photographs A4.7 and A4.8 have been chosen because the former represents a combination of modes that give rise to a more complex wave profile with a long flat part, while the latter shows clearly that it is not difficult to obtain even a virtually totally flat profile. Such a profile may easily be interpreted as a swinging motion of the water about an ideal fixed point at the centre, a motion in which the surface of the water remains flat. This kind of behaviour is just what may have suggested to Galileo – and/or simply partially confirmed – the idea that the water motions inside rectangular basins can be studied as if the surface of the water were ideally 'rigid' and turning about a fixed hub, exactly in the way pendulums swing back and forth about the suspension point.

However, the first thing one learns on embarking on this sort of experiment is that, even though this pendulum-like behaviour certainly represents one of the ways in which

[1] See L.M. Milne-Thomson, *Theoretical Hydrodynamics, op. cit.*, pp. 438–440.

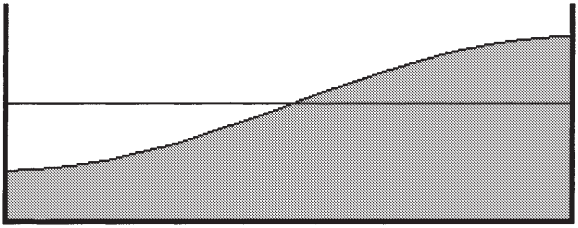

Figure A4.2

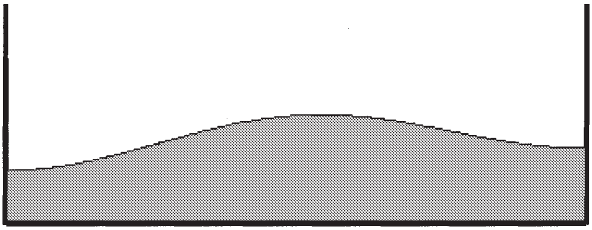

Figure A4.3

water will react, it is by no means the only way it will do so. There are in fact a host of different combinations as the above photographs demonstrate. It is therefore impossible to draw the conclusion that water inside rectangular 'artificial vessels' moves like a pendulum because it moves *both* like a pendulum *and* in many spectacularly different ways.

Nevertheless, after a short period of trial and error, one learns to apply the right amount of thrust so as to obtain a more or less pendulum-like motion that allows one to measure the vibrations by simply counting them and thereby verify the depth law. I carried out three tests, filling the tank with water to a depth of 12 cm, 15 cm and 18 cm, respectively. The following table gives the number of oscillations per minute corresponding to each test (I repeated each test three times and consistently obtained the same result):

WATER 12 CM OSCILLATION NUMBER 47
WATER 15 CM OSCILLATION NUMBER 51
WATER 18 CM OSCILLATION NUMBER 55

The table demonstrates that frequency increases with depth. Now, Galileo did not furnish a more precise mathematical relationship between depth and frequency, nonetheless it is interesting to note that the three numbers 47, 51, 55 are to each other precisely as the square roots of the depths, 12, 15, 18, respectively (see the next table).[2] In other words,

[2] On a first approximation, the period of the fundamental mode of oscillation of stationary water waves in rectangular canals is $T = \frac{2 \cdot L}{\sqrt{g/H}}$, where T is the period, L the width, H the depth

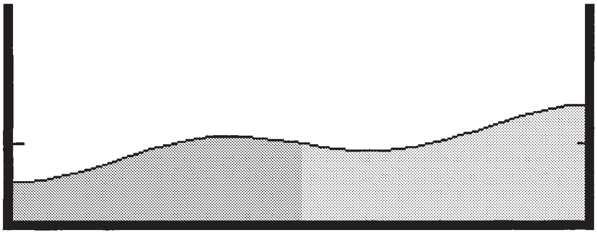

Figure A4.4

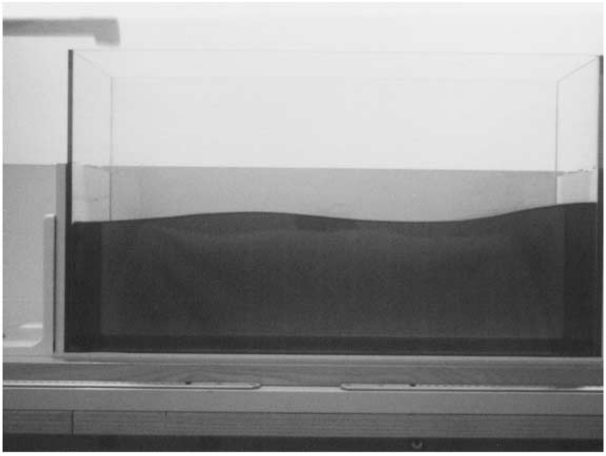

Figure A4.5

the simple count of vibrations occurring per minute is accurate enough to practically verify the law of depth. If Galileo had performed these simple calculations he would have obtained:

$\frac{55}{51} \cong 1.08$	$\sqrt{\frac{18}{15}} \cong 1.09$	Error = 0.9%
$\frac{51}{47} \cong 1.085$	$\sqrt{\frac{15}{12}} \cong 1.12$	Error = 3.1%
$\frac{55}{47} \cong 1.17$	$\sqrt{\frac{18}{12}} \cong 1.22$	Error = 4.1%

Here the ratios of oscillation $\left(\frac{55}{51}, \frac{51}{47}, \frac{55}{47}\right)$ correspond very closely to the square roots of the respective ratios of depth (right column in the previous table).

and g the gravity constant. See, for example, L.A. Pipes, L.R. Harvill, *Applied Mathematics for Engineers and Physiscists, op. cit.*, pp. 464–467.

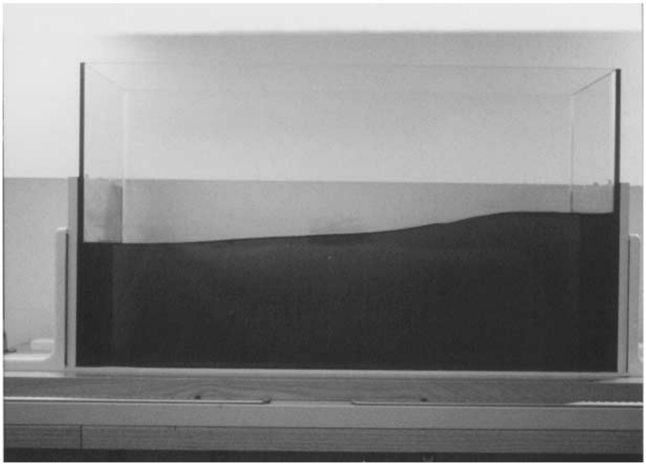

Figure A4.6

Whether Galileo even suspected such 'regularity' and performed these simple calculations, or simply accepted the more qualitative evidence stemming from such experiments without any further investigation, we do not know. But one thing is clear: the law of depth and the law of width (which may be verified by a practically identical procedure so that we can spare the reader the boredom of analogous data and computations) were well within the 'compass' of experiments the same or similar to these. Yet, it must have been the astonishing complexity of the many combinations of motions that waves display that in the end prevented Galileo from making public his results and furnishing the necessary evidence that such important discoveries would have deserved.

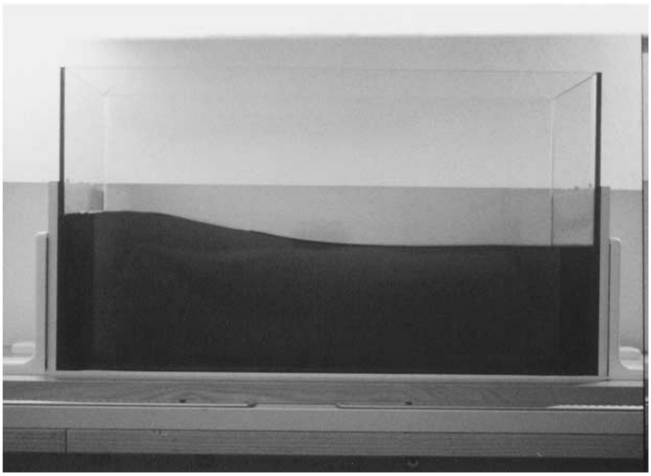

Figure A4.7

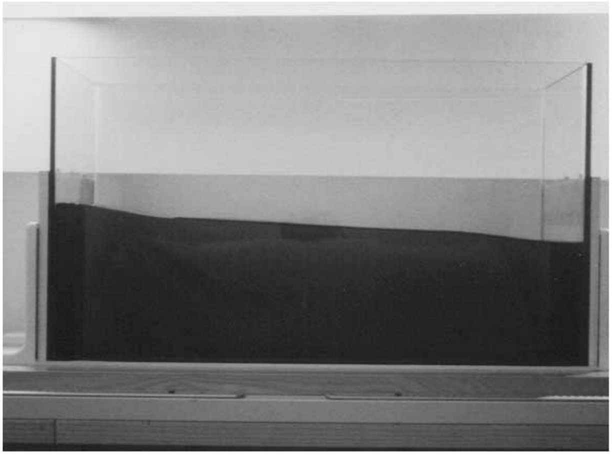

Figure A4.8

Appendix 5. The math of the wheel clock

Part 1. The equation of motion of the balance-stick and of the quasi-balance-stick

For the sake of simplicity let us reproduce in Fig. A5.1 the sketch given in Fig. 6.3 in Sect. 6. As we have seen, the resulting equation of motion for this system is:

$$\frac{d^2}{dt^2}\theta =$$

$$\frac{-\rho^2\cdot\left[-N\cdot\sin(N\cdot\theta)\cdot\cos(N\cdot\theta)+N^3\cdot\sin(N\cdot\theta)\cdot\cos(N\cdot\theta)\right]+R\cdot\rho\cdot N\cdot\sin(N\cdot\theta)}{\frac{m}{\mu}\cdot R^2+R^2+\rho^2\cdot\left[\cos^2(N\cdot\theta)+N^2\cdot\sin^2(N\cdot\theta)\right]+2\cdot R\cdot\rho\cdot\cos(N\cdot\theta)}\cdot\left(\frac{d}{dt}\theta\right)^2$$

To derive this equation we first write the expression of the kinetic energy of the Earth-Moon system. Let T be the total kinetic energy and V_x, V_y the Cartesian components along the x-axis and y-axis of M's speed.

$$T=\frac{m}{2}\cdot R^2\cdot\left(\frac{d^2}{dt^2}\theta\right)^2+\mu\cdot\left(V_x^2+V_y^2\right) \qquad (1)$$

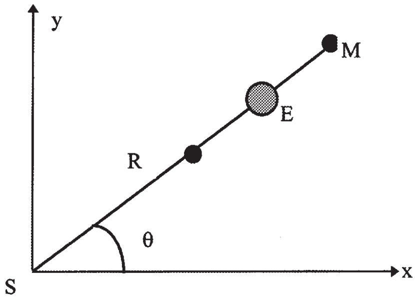

Figure A5.1

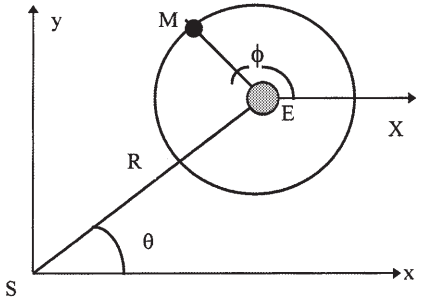

Figure A5.2

Following the Lagrange procedure, the equation of motion of Earth E will be given by

$$\frac{d}{dt}\frac{\partial}{\partial\theta'}\mathrm{T} = \frac{\partial}{\partial\theta}\mathrm{T} \tag{2}$$

where the partial derivative symbol $\frac{\partial}{\partial\theta'}$ means partial derivative with respect to θ' and $\theta' = \frac{d}{dt}\theta$. Now, by simply subsituting relation (1) into relation (2), one obtains the following relation (let it be relation (3)):

$$\frac{d}{dt}\left\{\frac{\partial}{\partial\theta'}\left[\frac{\mathrm{m}}{2}\cdot\mathrm{R}^2\cdot\left(\frac{d^2}{dt^2}\theta\right)^2+\mu\cdot\left(\mathrm{V}_\mathrm{x}^2+\mathrm{V}_\mathrm{y}^2\right)\right]\right\}$$
$$=\frac{\partial}{\partial\theta}\left[\frac{\mathrm{m}}{2}\cdot\mathrm{R}^2\cdot\left(\frac{d^2}{dt^2}\theta\right)^2+\mu\cdot\left(\mathrm{V}_\mathrm{x}^2+\mathrm{V}_\mathrm{y}^2\right)\right]$$

The next step is to remember that, according to elementary trigonometric relations and taking into account the so-called ‘constraint equation’ $\mathrm{R}(\theta) = \rho\cdot\cos(\mathrm{N}\cdot\theta)$, the

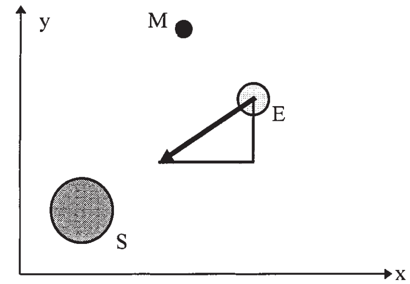

Figure A5.3

Cartesian components of M's speed may be written as:

$$V_x = \frac{d}{dt}(R \cdot \cos\theta + \rho \cdot \cos\theta \cdot \cos N\theta)$$

$$V_y = \frac{d}{dt}(R \cdot \sin\theta + \rho \cdot \sin\theta \cdot \cos N\theta)$$

Thus, by including V_x, V_y in relation (3) and by performing the differentiations one obtains the equation of motion.

As regards the quasi-balance-stick model, the procedure is quite similar, but we have to introduce a different constraint equation instead of $R(\theta) = \rho \cdot \cos(N \cdot \theta)$. Let us refer to Fig. A5.2 and draw a second horizontal axis X with respect to which the new angular variable ϕ measures the angle formed by the line joining the Earth and the Moon and the X-axis. The new constraint equation will be $\phi(\theta) = (N \cdot \theta)$.
The Cartesian components of the Moon's speed are now

$$V_x = \frac{d}{dt}(R \cdot \cos\theta + \rho \cdot \cos N\theta)$$

$$V_y = \frac{d}{dt}(R \cdot \sin\theta + \rho \cdot \sin N\theta)$$

and by exactly repeating all the steps needed to reach the equation of motion of the sliding Moon, one finds

$$\frac{d^2}{dt^2}\theta = \frac{m \cdot R \cdot \rho \cdot N \cdot (N-1) \cdot \sin((N-1)\cdot\theta)}{m \cdot R^2 + \mu \cdot \left\{\left(R^2 + \rho^2 \cdot N^2\right) + 2 \cdot R \cdot \rho \cdot N \cdot \cos((N-1)\cdot\theta)\right\}} \cdot \left(\frac{d}{dt}\theta\right)^2 .$$

Part 2. Newton's equations of motion of Sun-Earth-Moon system

Referring now to Fig. A5.3, all the bodies are free to move in space without any constraint and the only forces acting reciprocally on S, E and M are the gravitational forces. First of all, although the bodies have been drawn with different diameters they are regarded as points. The general expression of the gravitational force that the Sun exerts on a generic body P is $\vec{F}_{PS} = -G \cdot \frac{m_S \cdot m_P}{r_{PS}^2} \cdot \frac{\vec{r}_{PS}}{\|\vec{r}_{PS}\|}$, where m_S, m_P are masses, G is the constant of universal gravitation and $\vec{r}_{PS}$ is vector distance between the Sun and body P. For simplicity, we need to break the previous relation down into its two Cartesian components, namely, the horizontal component and the vertical one.

To do so, we can take the two sides of the so-called triangle of forces. Such a triangle is depicted in Figure A5.4 to illustrate the force of the Sun on Earth E. Now, if we assume SE = 1 and recall Pythagoras' theorem, the horizontal and vertical components of triangle SZE prove to be, respectively,

$$SZ = \frac{x_E - x_S}{\sqrt{(x_E - x_S)^2 + \left(y_E - y_S\right)^2}}$$

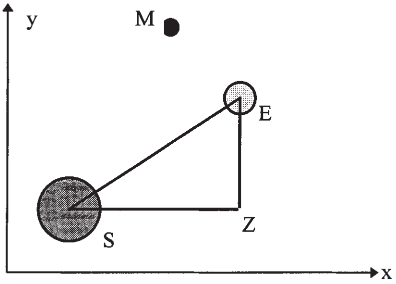

Figure A5.4

and

$$EZ = \frac{y_E - y_S}{\sqrt{(x_E - x_S)^2 + (y_E - y_S)^2}}$$

so that, in order to get the Cartesian components of force $\vec{F}_{PS} = -G \cdot \frac{m_S \cdot m_P}{r_{PS}^2} \cdot \frac{\vec{r}_{PS}}{\|\vec{r}_{PS}\|}$, which acts along the line SE, we take the modulus of $\vec{F}_{PS}$ and multiply it by SZ and EZ. We can now write the x-axis equation of motion of Earth E as:

$$\frac{d^2}{dt^2}x_E = -G \cdot \frac{m_S}{\left[(x_E - x_S)^2 + (y_E - y_S)^2\right]} \cdot \frac{x_E - x_S}{\sqrt{(x_E - x_S)^2 + (y_E - y_S)^2}} + \\ -G \cdot \frac{m_M}{\left[(x_E - x_M)^2 + (y_E - y_M)^2\right]} \cdot \frac{x_E - x_M}{\sqrt{(x_E - x_M)^2 + (y_E - y_M)^2}} \qquad (3)$$

The y-equation is now obtained by substituting y_E for x_E on the left-hand side of relation (4) and in the numerators of the two terms on the right. In the same way, one can build the x-equation and the y-equation of Sun S and Moon M. The final system is made up of six scalar Cartesian equations (two equations for each body). Here are the two final equations of motion of Earth E:

$$\begin{cases} \frac{d^2}{dt^2}x_E = -G \cdot \frac{m_S}{\left[(x_E-x_S)^2+(y_E-y_S)^2\right]} \cdot \frac{x_E-x_S}{\sqrt{(x_E-x_S)^2+(y_E-y_S)^2}} + \\ -G \cdot \frac{m_M}{\left[(x_E-x_M)^2+(y_E-y_M)^2\right]} \cdot \frac{x_E-x_M}{\sqrt{(x_E-x_M)^2+(y_E-y_M)^2}} \\ \frac{d^2}{dt^2}y_E = -G \cdot \frac{m_S}{\left[(x_E-x_S)^2+(y_E-y_S)^2\right]} \cdot \frac{y_E-y_S}{\sqrt{(x_E-x_S)^2+(y_E-y_S)^2}} + \\ -G \cdot \frac{m_M}{\left[(x_E-x_M)^2+(y_E-y_M)^2\right]} \cdot \frac{y_E-y_M}{\sqrt{(x_E-x_M)^2+(y_E-y_M)^2}} \end{cases} \qquad (4)$$

The other two couples of equations can be constructed by simply 'rotating' the indexes.

These equations of motion are second-order differential equations and are calculated by simulation, employing what mathematicians call 'numerical analysis techniques', namely, numerical methods that 'solve' the equations in each single case (i.e. these techniques do not yield the analytical or closed-form solution to the previous equation, which as such remains unknown, but simply assign a value to each parameter and, *step-by-step*, build the so-called integral curve of the differential equation in this specific instance). These procedures for solving differential equations have for years been widely known to physicists, mathematicians and engineers and well-documented in the literature. Nowadays, they are readily available to the 'layman historian' as user-friendly software packages that run on ordinary personal computers.[1]

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[1] I have carried out all the simulations using the software package *Mathcad 5.0 Plus* by Mathsoft ©1994 with a personal computer *HP Vectra Series 4 5/133*. It is not possible to give here a bibliographical summary of the vast scientific literature regarding these techniques. A text that I have drawn upon extensively is the classical L.W. Johnson, R.D. Riess, *Numerical Analysis*, 2nd ed., Reading-Massachussetts, Addison-Wesley, 1982. See also A.F. D'Souza, V.K. Garg, *Advanced Dynamics, op. cit*, in which many case studies are discussed.