GALILEO'S PHYSICS FOR A ROTATING EARTH

Ron Naylor

1 Introduction

We are dealing here with Galileo's most ambitious project. An audacious and even paradoxical attempt to create a radically new kind of universal physics by means of a reconfiguration of traditional concepts. It was founded on the ancient belief that circular motion was a fundamental and irreducible characteristic of the cosmos. In Galileo's view, Copernicus had shown that the perfect, eternal, circular motion of the heavens was shared by the planet Earth and all things on her. This unending inertial motion was supposedly shared by all terrestrial objects and was truly natural and intrinsically beyond analysis. Galileo's conservatism regarding the fundamental nature of circular motion blocked any prospect of his explaining it in terms of more basic concepts. Indeed he seems in his various analyses to have had the means of doing so within his grasp. But he did not, and remained locked into the classical view that circular motion was given, natural and beyond explanation. For that reason he could never create the universal physics he sought. Nevertheless his attempt to construct a quite new kind of physics to encompass the sun, moon and earth can be seen to be one of the first stumbling moves towards the new physics required by Copernicanism. He did not advance it initially in the belief that it was fully prepared for the consideration of the learned world, but only when confronted with an unanticipated demand to produce physical evidence for the Copernican system. The view presented to him in Rome in December 1615 was that as Copernicanism was an astronomical hypothesis without any physical evidence in it's favour, works claiming that it was physically true were likely soon to be banned.

It can hardly be wondered therefore that without further delay he threw into the fray the only argument he had to hand in the attempt to prevent the immanent ban on any further discussion of it as a physical theory. After that the die was cast. As a point of honour Galileo would never admit defeat on this question. Not surprisingly he did not succeed in this ambitious quest, not in preventing the ban on realist interpretations of Copernicanism in 1616, nor in devising a viable case for terrestrial motion. This bold project was inevitably doomed as he lacked both the conceptual and mathematical means necessary to accomplish such a daunting task.

It has been said that Galileo brought the heavens down to earth on the inclined plane. In practice he never succeeded in uniting his highly successful linear two dimensional terrestrial physics with the three dimensional spherical physics of the cosmos. His quest to construct such a link raised the inclined plane to heaven on a circle. For the "natural" linear motion of free fall and it's dilution on the inclined plane was claimed not to be natural at all, but an illusion produced by two unseen, truly natural, but circular motions. If the earth was after all just another planet, then, turning Aristotelianism on it's head, it's motion too would be ruled by the circle. Galileo claims that the "real" natural motion of a falling body is actually always on a circular arc ending at the centre of the earth . Galileo even jests that consequently, they could readily base their entire physics on the circle. For Segredo exclaims "according to this straight line motion goes straight out of the window and nature never makes use of it at all". In fact the theory was a brilliantly conceived attack on the most revered belief of Aristotelian and Christian cosmology: the sacrosanct distinction between the transitory limited linear motion of the earth and the perfect unending circular motion of the heavens.

2 Galileo's rotational theory of the tides

The earliest evidence of a serious attempt to use terrestrial rotation as a means of explaining the tides dates from the 1590's and is found in a copy of a series of notes originally in Paolo Sarpi's notebook. Whether the ideas concerned, which appeared four decades later in the *Dialogue*, were Sarpi's or Galileo's, or the result of mutual discussion, we cannot be sure.[1]

When in the *Dialogue* Galileo considers the effects of terrestrial rotation he treats free fall in the Second Day[2] and the tidal theory in the Fourth

[1] Drake, S., *Galileo Studies,* Ann Arbor, University of Michigan Press, (1970), 201-2.

[2] Galileo, *Le Opere,* ed. A. Favaro, 20 vols., Florence, Barbèra, (1899-1909) 7, 191-3. Galileo, *Dialogue on the Two Great World Systems,* trans Stillman Drake, University of California Press, Berkeley, 2nd Ed, 1967, 165-7.

Day[3], and though the treatments are very different they do reveal important conceptual similarities. They are both kinematic theories based on the concept of circular inertia and demonstrate his conviction not only that the phenomena of nature should have a simple mathematical explanation but that ideally physics is geometry. In each theory, two uniform circular motions are said to give rise to acceleration and thence to forces. Both accounts appear at first sight to be capable of providing identifiable predictions. Unfortunately this promise is promptly removed by Galileo in the case of the daily tides and never restored, as secondary effects are claimed to play a major role in determining the frequency of the diurnal tides, but other predictions, such as the times of the year when the maximum and minimum tides occur, are quite specific as they are not effected by secondary phenomena.

Galileo's aim of explaining the tides kinematically did not surface for many years after Sarpi's notes were written in 1595. Only in 1616 when aware of an impending ban on Copernicanism did he hurriedly draft an outline of what he had of his tidal theory for Cardinal Alessandro Orsini.[4]

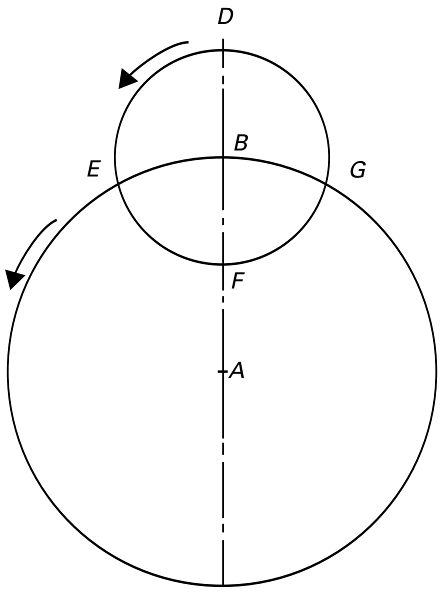

Figure 1 Galileo's rotational theory of the tides.

[3] Op. cit. (n. 2), Favaro, 7, 443-63, Drake, 416-62.

[4] Galileo, *Discorso del flusso e reflusso del mare, Le Opere,* 5, 377-395, trans. in Finochiaro, M.A., *The Galileo Affair,* London, University of California Press, (1989), 119-33. See also Shea, W.R., *Galileo's Intellectual Revolution,* London, MacMillan, (1972) for a discussion of these issues and the context. Shea identifies Galileo's adoption of the lunar theory of the tides in 1637.

The basic idea is that if the earth has two rotary motions, one on it's axis B, the other in it's orbit about the sun A, then the speed of the earth's surface relative to the sun will be continually changing. The absolute velocity of the earth's surface in space is regarded as it's speed relative to the sun. At D that is the addition of the earth's orbital velocity and it's rotational velocity. At F it is the orbital velocity less the rotational velocity. Galileo, contrary to Copernicus, claimed that the seas do not share directly in the motion of the earth but moved independently, being as he put it "a law unto themselves". For this reason when the earth's surface moved gradually faster between G and E it tended to leave the seas behind. However the sea basin acts as a container and gradually imparts it's motion to the sea. After E the earth's surface is moving more slowly than the earth's orbital velocity, and as the rotational inertia of the sea about the earths axis is conserved this means, according to Galileo, that it will move slightly faster than the earths surface which is slowing down in real space, and will therefore run ahead of it, this tendency continues and steadily increases till F after which it will gradually slacken off. The consequence of this, Galileo argues, is that at any point in a large ocean like the atlantic there should be one low tide and one high tide during each complete rotation of the earth. As the tides are determined entirely by the geometry of the physical situation they will occur at the same time each day when the earth and sun are in the same spatial relationship. In seas like the Mediterranean where the sea basin though large is not of oceanic dimensions Galileo claims that secondary counter flows can occur. The topography of the sea basin sets up these counter flows which alter the tidal frequency in a characteristic and determinate manner. In the mediterranean this means that there are two high and two low tides every day, not one. In addition there exists a second monthly periodicity in the tides which as Salviati states:

> seems to originate from the motion of the moon; it does not introduce other movements, but merely alters the magnitude of those already mentioned with a striking difference according as the moon is full, new, or at quadrature with the sun. The third period is annual, and appears to depend upon the sun; it also merely alters the daily movements by rendering them of different sizes at the solstices from those occurring at the equinoxes.[5]

3 The theory of circular fall

The interaction of inertial circular motions was also used by Galileo to explain how the observed acceleration of falling bodies in the vertical could occur on a rotating earth. Onto the Copernican idea that all bodies on the

[5] Op. cit. (n. 2), Drake, 418.

earth inevitably retain their rotational inertia Galileo grafts the assumption that on its release from the top of a tower a heavy object continues it's circular motion but from the instant of release it moves with a natural motion on a semi circular path terminating at the centre of the earth.

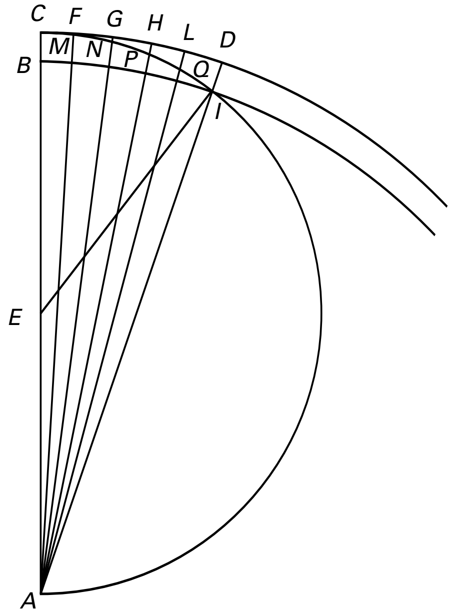

Figure 2 Circular fall on a rotating earth.

The fall from the tower BC, here shown situated on the equator, is said to result from the natural motion of the body on the circular path CIA terminating at A the centre of the earth. The speed of the body on this path is the same speed it had while at rest on top of the tower, which is the rotational speed of the tower. The uniform motion of the earth carries the tower to BF, BG, BL, and BD in successive equal intervals of time, while the falling body is successively at M, N, P, Q and I at the foot of the tower. An observer at the foot of the tower only sees the body successively at M, N, P, Q, and I, and is unaware of the circular motions of the tower or the body. As with the tides Galileo only considers the case on the equator. But both explanations are offered as accounts of phenomena actually seen by Simplicio, Sagredo and Salviati in Venice. It is understood of both rotational theories that to be valid explanations they have to be of general application. But this brings us to one of a number of very serious hidden paradoxes in the *Dialogue*.

In the case of free fall once the tower is north of the equator, as in Venice, the motion along the circular path to the center of the earth will be in a plane that contains the tower only at the point of release, the plane being tangential to the circle of latitude on which the tower lies. The motion on the circle to the centre of the earth is as before of the same magnitude as the rotational speed at the point of release. That is the rotational speed of the tower at the new latitude, which is considerably less than the rotational speed at the equator. There are two consequences of this. Firstly though the falling body will move at the same speed as the tower in the horizontal it will fall in a plane after release that does not contain the tower. As a result it's trajectory takes it away from the tower. In this case as the tower is north of the equator it will fall to the south. If the tower was south of the equator the body would fall to the north. Moreover bodies released at different heights from points in the same vertical line would not fall to the same point. All these effects would be detectable. But the second and much more serious consequence is that the acceleration in the vertical would change with latitude very markedly, as would the time of fall from the top of the tower.

The falling body will not reach the foot of the tower until it has completed it's motion along the arc CI. But as the rotational speed of the body and the tower at Venice is smaller than the speed of rotation on the equator it will take longer to complete it's motion along CI. Thus the time of fall will increase with latitude and the natural acceleration in the vertical will decrease. At the poles the acceleration will be zero and the body will not fall, being effectively weightless. If the speed of the body in it's circular path had remained at the equatorial value it would move faster in the west east direction than the tower on a curved trajectory to land some way from the tower.

4 The variation of rotational tide with latitude

Rotation of a planet, or any sphere, on it's axis, cannot produce the same physical effects over its entire surface. It follows as a consequence that the tidal theory also has unforseen consequences. Galileo only considered the case of fall on the equator in the *Dialogue* so no opportunity arose in which variations with latitude could come to light. That is not so with the tidal theory as he had to take the three dimensional character of the theory into account when he came to deduce the annual variation of the tides. This brought him the related issue of the variation of the tides with latitude.

Though Sarpi's notes refer to the annual changes in the tides it was over three decades later when Galileo completed his treatment of this subject for the *Dialogue*. Interestingly enough Galileo had made no reference to the annual changes in the tides when writing the *Discourse on the Tides* in 1616 though it had originally been claimed as one of it's virtues in 1595. It is true that the Sarpi note is little more than an indication that the annual variation is related in some unspecified manner to changes in the orientation of the ocean shores relative to the earths movement in it's orbit. It reads:

> Finally, it is manifest how the motion of the seasons, carrying the shores now to one site and now to another, make an annual variation of the augmentations and decrements.[6]

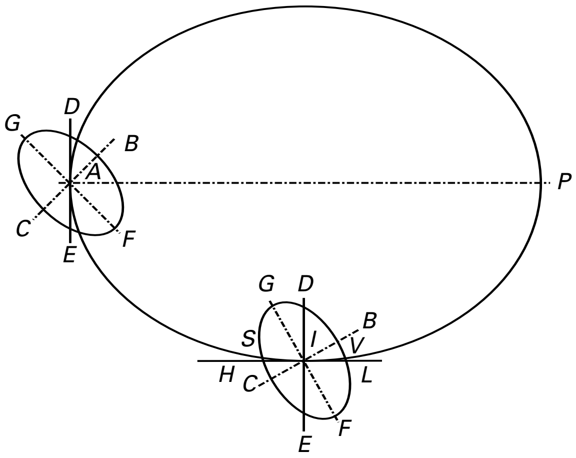

Figure 3 Annual variations in the magnitude of the tide of rotation.

In figure 3 BC is the earth's axis, AP the diameter of the earth's orbit, A being the centre of the earth at the summer solstice, when BC lies in the same plane as AP. I is the centre of the earth at the equinox when BC is perpendicular to the radius of the earth's orbit. Galileo deduces that the maximum addition of rotational velocity occurs at A as the diameter of the earth DE lies in the plane of the earth's orbit, whereas the minimum addition occurs at I as the diameter GF perpendicular to the earth's axis BC is inclined to the earth's orbit. At A the change in velocity at the earth's equator is the whole of the earth's rotational velocity whereas at I it is only the component of the earth's rotational velocity in the plane of the earth's orbit, and that is proportional to the projection of GF onto that plane. Galileo's conclusion is as follows:

> Then as to how much the least additions differ from the greatest, this is easy to determine: between these there is the same variation as between the whole axis or diameter of the globe and that part of it which lies between the polar circles. This is less than the whole diameter by one twelfth, approximately, assuming that the additions and subtractions are made at the equator; in other latitudes they are less in proportion as their diameters are diminished.[7]

[6] Op. cit. (n. 1), 202.
[7] Op. cit. (n. 2), Drake, 460.

Galileo is simply pointing out that the reductions retain the same proportion at every latitude. Thus the reduction gets less in absolute terms of velocity as the latitude increases. But that can only mean that as the rotational velocity reduces in any case with increasing latitude so too must the total tide it produces.

His statement makes it perfectly clear that as the component of the earths rotational velocity in the plane of the earth's orbit changes so does the tide. This does indeed follow directly from his theory. As there is no rotational velocity at the poles there would be no tide, while at a latitude of 60 degrees the tide would be only half the equatorial tide.

Galileo does not refer to this consequence of his tidal theory directly in the *Dialogue* but he does clearly refer to the equivalent effect in relation to equatorial winds. As a tidal prediction it would be possible to check. If equatorial tides were greater than tides elsewhere on the globe that would be quite noticable, as would be the fact that they decrease markedly with latitude. In reality they do not. Indeed tides are quite frequently found to be larger at higher latitudes than at lower. Such knowledge, which is not esoteric, was known to experienced ocean mariners. Such facts, if their relevance was perceived, would naturally be disastrous for Galileo's theory. For in the rotational theory there is no conceivable mechanism whereby a larger than equatorial tide could be generated at a higher latitude, it would be an impossibility. In any case the careful examination of Galileo's own words show that his theory predicted the opposite.

In practice a far greater headache would have been Galileo's prediction of the maximum annual tide, made in the passage quoted above, which claimed it would occur at the solstices, with the smallest tide at the equinoxes. This clashed completely with the perfectly well established and widely accepted facts of the matter known since classical times. Galileo could not call on secondary effects to remove this conflict, nor indeed to account for the fact that the tidal variation with latitude to be expected according to his theory was not observable. For he maintains throughout his discussion that the primary tidal movements are east to west for rising tides and west to east for falling. This idea is at the core of his theory. There are no mechanical means whereby rotational tides at one latitude might effect tides at a different latitude according to Galileo, as he was quite insistent that there simply are no significant north-south tidal flows.

5 Preparation and publication of the rotational theories in the Dialogue

By 1629 Galileo had evidently searched far and wide for evidence for his theory. As the *Dialogue* reveals he knew a great deal about prevailing oceanic winds and had obviously gleaned an impressive amount of information from those with direct knowledge of the world oceans. What is so remarkable about Galileo is his readiness to turn accepted Copernican arguments on their head. Though in the Second Day of the *Dialogue* he demonstrated that it is impossible to detect the shared motion of the earth and the bodies upon it, which is the strict Copernican position, he thereafter revises his position as we have seen, reverting to a quasi Aristotelian view. The air, like the sea, he argues, being fluid, does not share in the motion of the solid earth either. And indeed it would therefore follow logically that the air should be affected by the earths rotation in a similar way as the sea, and so it does as he points out:

> Hence, while the earth is revolving toward the east, a beating wind blowing from east to west ought to be continually felt in such places (large open planes), and this blowing should be the most perceptible where the earth whirls most rapidly; this would be in the places most distant from the poles and closest to the great circle of diurnal rotation. Now the fact is that actual experience strongly confirms this philosophical argument. For between the tropics, in the open seas... a perpetual breeze is felt moving from the east , thanks to this ships prosper in their voyages to the West Indies. Similarly, departing from the Mexican coast, they plow the waves of the Pacific Ocean with the same ease towards the East Indies...[8]

If winds of rotation are greatest at the equator so too inevitably must be the tides of rotation. Bearing in mind Galileo's evident grasp of the mechanics of the process that inevitably makes it very hard to believe that he could have overlooked it's relevance to the tides. And again if terrestrial rotation caused a continual steady easterly breeze at the tropics why does it not cause a continual steady current in the sea? The reason Galileo gives, as we have seen, is that the earths surface at the equator first speeds up by 2000 mph in 12 hours and then retards by 2000 mph in 12 hours, which causes the tides. But logically these large alterations in the real speed of the earth's surface, if they cause tides, should inevitably cause very noticable air movements also, giving rise to notable winds. It is the rapid motion of the earth underneath the sea and the air that would give rise to these winds, and in such a case there would be little frictional drag on the air to reduce and so moderate the strong winds that the theory implies. The same mechanical laws will necessarily govern the process in each case. Galileo however seems to switch backwards and forwards between two alternative conceptual systems in his discussion of winds and tides, whereas, to be consistent, he ought to use the same mode of explanation for both.

Though he refers to winds unique to the tropics, and to wind patterns in the mediterreanean which confirm his Copernican beliefs, there is no direct reference to any news of tides and whether they were the same or different

[8] Op. cit. (n. 2), Drake, 439.

around the world, whether in particular there were one or more tides each day in these great oceans of which Galileo had obviously learnt so much. The mariners he had consulted had surely told him whether there were one or two tides each day in the atlantic and pacific oceans, and whether the oceans of the north had similar tides to the equatorial oceans and the oceans of the south. What may seem surprising however is that there is no reference of any kind to the frequency or size of the oceanic tides, or indeed to any single piece of specific information concerning them at all. The reason cannot be because no information had reached Galileo, or that it was not reliable, on the contrary some very reliable information had quite evidently reached him, the problem was none of the tidal information would have agreed with his theory. Galileo was nevertheless determined to continue to the bitter end. The pressure to complete the book added to the elation of at last being able to prepare the long delayed work for publication: so he simply pressed on undaunted by these difficulties and steeled himself to disregard the problems his theory had to face.

Nevertheless as Annibale Fantoli has pointed out there are signs that Galileo did not display total confidence in all aspects of his tidal theory in the period just before it's publication. In his letter to Giovanfrancesco Buonamici in October 1629 he wrote:

> Your Lordship should know that I am about to publish some dialogues in which I treat of the constitution of the universe, and amoung the principle problems I write of the ebb and flow of the sea, I believe I have found the true reason for it, very far from those to which up to now that effect has been attributed. I estimate it to be true and so do all those with whom I have conferred about it.[9]

As Fantoli comments the use of the expressions "*I believe I have found*" and "*I estimate it to be true*" does suggest that despite everything that there remained a degree of uncertainty deep down. Again perhaps unexpectedly there exists a surprising gulf between the strength of his claims for the theory and the evidence for it. His concern that he is still in need of such evidence can be seen in this letter where he speaks of hope that further evidence of the "experimental" character of his theory will be forthcoming. But as this had not materialised over the forty years of it's existence Galileo must have realised that now at the eleventh hour he was extremely unlikely to find it.

[9] Fantoli, A., *Galileo for Copernicanism and for the Church,* trans. G.V. Coyne, Rome, Vatican Observatory Foundation, 2nd English Ed., (1996), 333-4.

6 Reception of the rotational theories and Galileo's reaction

Like the *Starry Messenger* the *Dialogue* provoked very mixed reactions. But whereas the major claims of the *Starry Messenger* within a short time received open recognition from the most notable of Galileo's intellectual peers the same could not really be said of the *Dialogue*. Though much of what he said concerning Copernicanism was welcomed by a significant group of his readers the physical proof from the tides received a rather mixed reception.

As Fantoli put it: "The book reception by Galileo's friends and admirers was as usual, enthusiastic, although frank reservations on the argument from the tides were not lacking". Giovanni Baliani was one amongst those with reservations.[10] Many found his theoretical arguments implausible. A further difficulty was seen in the fact that there was so little evidence in support of the theory. In 1595 Galileo must have expected to gather evidence to support it. In 1616 he believed he had obtained evidence from Lisbon that the daily tide in the atlantic ocean was twelve hourly.[11] He had since discovered that this was not so, which must have been a severe blow for him as it left him with no direct evidence for that fundamental feature of his theory. But no amount of adverse evidence seemed capable of shaking his resolve.

Having committed himself to his theory in 1616 and then having it rejected as a valid proof of the earths motion he was thereafter determined to justify himself and see his adversaries proved wrong. From then on he was too personally involved in the project to be capable of seeing the issue of the tidal theory objectively. There seemed to be no way in which he could envisage that his theory might after all be seriously flawed.

His opportunities to discuss the theory had been somewhat constrained in that it was with his ex-pupils and friends. They knew only too well the story of the events of 1616 and the difficulties under which Galileo laboured. They would not want to offer any adverse criticism of his cherished theory. Thus those he had "conferred with" could not have prepared him for the reception the theory was to receive from more informed and critical individuals. For it was certainly not the case that all those sympathetic to Galileo's general position were prepared to accept his rotational theories at face value. If Galileo had ever dreamed that the level of the objections raised would be similar to those of the bumbling Simplicio then he was rudely awakened.

Galileo naturally expected objections from convinced Aristotelians, but even they were not easily disregarded. As Antonio Rocco observed in respect of the tidal theory "all your controversial conclusions go against our sense knowledge, as any one can see by himself". Nor could Galileo deny it. And

[10] Ibid., 339.
[11] Op. cit. (n. 4), Finocchiaro, 128.

this as Rocco had pointed out, was after Galileo had promised to provide an explanation that would have convinced Aristotle himself. Who indeed apart from uncritical supporters and those completely ignorant both of the facts and earlier tradition could accept Galileo's account of the tides as having a serious claim to credibility? Thus though Galileo maintained he had devised a new mathematically and mechanically based theory quite superior in all respects to those of any of his predecessors he had found it quite impossible to justify this claim in practice. Indeed Rocco, though a die hard Aristotelean, was not simply dismissed by Galileo, for he evidently caught Galileo on a nerve if we are to judge by the lengthy and detailed response he provided to his critique.[12] But this was a straw in the wind. Galileo proved in reality to be over defensive, for deep down he knew he had much to be concerned about. Even had he consoled himself that scepticism and incomprehension from such quarters was only to be expected what he had not anticipated was the open scepticism of the *cogniscenti*. Apart from Baliani's requests for clarification within the year Jean-Jacques Bouchard wrote to inform Galileo of the doubts of a group of French physicists:

> They draw attention to a difficulty raised by several members about the proposition you make that the tides are caused by the uneveness of the motion of different parts of the earth. They admit that that these part move with greater speed when they descend along the line of direction of the annual motion than when they move in the opposite direction. But this acceleration is only relative to the annual motion; relative to the body of the earth as well as to the water, the parts always move with the same speed. They say, therefore, that it is hard to understand how the parts of the earth, which always move in the same way relative to themselves and to the water, can impress varying motions on the water. They entreat me to obtain from you some solution to their difficulty.[13]

In effect, Bouchard was indicating to Galileo, with all due respect of course, that the central argument of his theory looked openly inconsistent. Apart from repeating his line of reasoning there was really little else Galileo could do. There is no record of a response.

Though undoubtedly concerned by the adverse comments the *Dialogue* had provoked there was little he could do about it and he had in any case now to busy himself with the urgent and long delayed task of completing his *Discourse on The Two New Sciences*. This occupied him till March 1637. In the meantime he would have been reflecting on the *Dialogue*. Though throughout his life very independent in outlook and disinclined to accept

---

[12] Rocco, A., *Esercitazioni filosofiche,* Venice, (1633), *Le Opere,* 7, 712, see Shea, op. cit.183-4 .

[13] Letter from Bouchard to Galileo, 5 September 1633, *Le Opere,* 14, 252-2, translation is Shea's op. cit.,176.

criticism as anything other than wrongheaded by 1637 his attitude to criticism had changed . So that he appeared unexpectedly conciliatory and perhaps slightly apologetic when justified doubts about an aspect of his theory of circular fall reached him from one of France's most distinguished mathematicians. This was not a matter to be shrugged off easily. Pierre Carcavy, a staunch supporter of Galileo, had written to him passing on Fermat's observation that the path of a vertically falling body on a rotating earth would be a spiral. Galileo's response has two distinct parts. In the first he maintains that the actual path as far as the earths surface is parabolic, as the path of a projectile is parabolic. As he only wished to treat motions close to the earths surface this he believed was justified. In doing this he intended to display his precise knowledge of the physical situation and reveal himself in a better light, but left the puzzle why had he not said this in the *Dialogue*? That he knew that the trajectory was parabolic in 1604 is evident from his surviving manuscript notes on motion.[14] He does in any event refer to one of the characteristics of the parabolic trajectory in the *Dialogue* when he points out that all horizontally projected missiles complete there trajectories in the same time.[15] But he had apparently decided for purely tactical reasons to use the theory of circular fall. It functioned as a purely mathematical demonstration aimed to undermine the Aristotelian distinction between terrestrial linear motion and celestial circular motion. A thought experiment which he fully appreciated was only an approximation. Having indicated to Carcavy that he did after all know better, that still left him with the task of explaining why it was that he had ever said the path was circular:

> And though it was said in the *Dialogue* that it might be that, mixing the straight motion of fall with the diurnal motion, there would be compose a semi circle that would go to end at the centre of the earth, this was said in jest, as quite manifestly appears, since it was there called bizzaria, that is, a rather daring jocularity. So for this part I wish to be forgiven , and especially having drawn from this poetic fiction, as I shall call it, those three unexpected consequences that the motion would be always circular, and second, always uniform, and third, that in this apparent motion downward nothing would be moved more than it would had it remained at rest.[16]

But that hardly explains his action. The so called consequences were actually the assumptions on which he evidently quite intentionally advanced the explanation. Galileo's presentation of the case for circular fall

[14] See Naylor, R.H., "Galileo: The search for the Parabolic Trajectory", *Annals of Science,* 33, 153-174, and Drake, S., "Galileo's Confirmation of Horizontal Inertia", *Isis,* 1973, 64, 291-305, and Naylor, R.H., "Galileo's Theory of Motion: Processes of Conceptual Change in the Period 1604-1610", *Annals of Science,* 1977, 34, 365-392.

[15] Op. cit. (n. 2), Drake, 155.

[16] Letter to Pierre Carcavy, 5 June 1637, *Le Opere,* 17, 89, translation in Drake. S., *Galileo at Work,* London , University of Chicago, (1978) 376-7.

and his response to Carcavy indicates he knew quite well what he was about in the *Dialogue*. That he was aware that his explanation was not precisely correct is even implied by Salviati's words:

> But that the descent of heavy bodies does take place in exactly this way, I will only say that if the line described by a falling body is not exactly this, it is very near to it.[17]

But such difficulties could not have troubled Galileo unduly as they did not really threaten his overall project, and he had taken premptive action to shield himself from possible criticism by way of his comment that it was presented as a diverting curiosity.

If Galileo by 1637 was prepared to admit his rotational theory of fall did not match the phenomena quite as well as it should what assessment would he have made of his beleaguered tidal theory? The predictions were hopelessly out of line with the facts. The daily tides did not occur at the same time, as they ought according to his theory, but moved steadily round the clock fifty minutes later each day in a monthly cycle, a fact well known by european mariners for two thousand years. Not only that but the well known annual variations in the tides were completely out of step with his theory and the changes it predicted were far too small. As we have seen the rotational theory predicted an overall difference between the largest and the smallest tides in any year to be 1/12. But the actual changes observed in the oceans, which as Galileo insisted were the true tides, were very considerably larger: frequently between 6/12 and 10/12 and at times even greater, so he had no prospect of accounting for that. He was unquestionably aware of all these difficulties. And having since returned to his pursuit of pure science the great gulf that lay between the rigour and cogency of his mechanics and his work on the tidal theory could not but have struck him with renewed force. After a break of thirty years from serious work on motion this certainly would have been like reading the work of another author, indeed he even said he found some of his conclusions surprising and his earlier reasoning not always easy to follow. This would have given him pause for thought, and in time to fully digest the gulf that lay between that work and the *Dialogue*. For he could not but see that whereas he had unquestionably surpassed the ancients in his *Two New Sciences* he had not done so in his tidal theory.

7 Galileo's final rejection of the rotational theory of the tides

Looking back over his theory of the tides, and reflecting deeply on all the many comments he had received, as Galileo clearly did in the years following it's pub-

[17] Op. cit. (n. 2) Drake, 167.

lication, inevitably made the full extent of it's empirical failure clear to him. Now in a more balanced and critical frame of mind he had time to reconsider the important features of the theory, which, in conjuction with the criticisms he had received, made him aware of it's several serious and irremedial theoretical weaknesses. This accounts for the marked change of view concerning the tidal theory in 1637. For he decided to part company with his theory and that could not have been a sudden impulse but only a decision contemplated after much thought, for to finally relinquish his early hopes for the physical proof of Copernicanism, nurtured in adversity for so many years, was the most significant, and most salutary decision of his mature scientific career.

Early in that year, having apparently completed his life's work by finally publishing the *Discourse,* with failing strength and sight, Galileo had returned once again to detailed observation of the moon, and in November he wrote to Fra Fulgenzio Micanzio with some quite remarkable news: he had discovered there were three lunar librations. Quite a surprising acheivement for a physically frail man in his seventy fourth year. Indeed one might wonder why no other astronomer had detected any of these apparently simple phenomena? What had prompted this sudden late interest in lunar observation on Galileo's part, was it as it might appear to be, simply a belated surge of pure empiricism? As simple discoveries there was certainly no reason for him not to merely record them as such. But evidently that was not his only intent, for he wrote:

> I 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 that the same face was always seen to be the same to our eyes.
>
> What I find to be true is this; 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 a bit to the left, or else raising and lowering his face, or finally, tilting his left shoulder to right and left. All these variations are seen in the face of the moon, and the large and ancient spots perceived in it make manifest and sensible what I say. Add moreover another marvel, which is that these three variations have three different periods; for one of them changes from day to day, and comes to have the diurnal period; the second alters from month to month, and has it's period monthly; the third has it's annual period in which it completes its variations.
>
> Now what will you say on confronting these lunar periods with the three periods, diurnal, monthly and annual of the movement of the sea, which by common agreement of everyone, the moon is arbiter and superintendant.[18]

Rather than dwelling any longer on the failure of his rotational theory by 1637 he had evidently decided to abandon it for the only viable alternative. Recognising that it was ultimately undeniable that the moon alone

[18] Op. cit. (n. 16) Drake, 385. This is Drake's translation with slight modifications.

ruled the tides he nevertheless lacked grounds that would justify his personal adoption of the lunar theory. He had claimed the case for that theory to be inadequate in 1632. Was there another? He already knew of the visual diurnal libration, the result of parallax, as he referred to it in the *Dialogue*. If the idea of a relationship to the daily tide occurred to him late in 1636, it could have caused him to wonder whether there might be other librations that had a monthly or yearly period, also like the tides. That possibility could have provided motivation strong enough, despite his failing sight and health, to undertake what for him would have been the arduous task of regular observational measurements of the moon that led to his remarkable discovery of the monthly libration in longitude, which is almost 8 degrees compared to the diurnal parallax of 1 degree. His sketches of the moon in the *Starry Messenger* actually reveal he had already, unwittingly, detected the effects of the libration in latitude in 1610. However it was of no potential significance in 1610 or in 1637, and went undetected on both occasions. In 1637 he sought a cyclic variation with an annual period, and that was just what he found. Reference to the third libration is cryptic, but it seems he must have been referring to the cyclic annual shift in the position of the boundary dividing the illuminated and dark part of the moon which results from the earth's motion in it's orbit.[19]

Galileo had always believed that the moon determined the monthly changes in the magnitude of the tides, but to refer the diurnal tides to the moon was to opt for a completely different tidal frequency from that of his original rotational theory. He is telling Micanzio that it is the movement of the moon that everyone sees to be related to the tides and which accounts for their movement around the clock. And while it is clear that the tides follow the moon and change their times daily that leaves no place for a rotational tide. There are no secondary tides, and certainly none at the same time each day as Galileo was now clearly prepared to recognise. Thus Galileo had concluded finally that the actual daily tides are lunar tides and not rotational tides at all.

There is nonetheless an almost haunting degree of hesitancy about Galileo's posing of the question to Michanzio. It is as though Michanzio could answer where Galileo dare not. It did after all mean that he had finally admitted to himself after all those years and tribulations that he had all along deluded himself in believing he had found a proof of the earths motion. Naturally Galileo could only confide diffidently and in confidence to a trusted old friend, and there could be no mention of the fact that he was putting his old warworn theory behind him. His final observational discoveries, like a late echo of his earlier triumphs, now provided an appropriate means of at last bringing him safely back into the fold of an obser-

[19] See Righini, G., "New Light on Galileo's Lunar Observations", in Righini Bonelli M.L., and Shea, W.R., *Reason, Experiment and Mysticism in the Scientific Revolution,* New York, Science History Publications, (1975) 59-76.

vationally sound tidal theory. But of course he could never even hint publicly that his prized proof of the earth's motion, the proof indeed that had been the cause of his downfall, and which "had caused the greatest scandal in christendom" was after all no proof at all. This was Galileo's last word on the tides and his final farewell to observational astronomy. Before the year was out he had lost his sight forever.

8 Galileo's understanding of the rotational theory of the tides: and the aftermath

Though Galileo's ultimate rejection of his tidal theory in 1637 is clear his view of the rotational theory at the time of the publication of the *Dialogue* is not. Did he recognise it's flaws? Or could he have possibly overlooked them? They are not obvious or they would have been detected by his contemporaries or by later physicists. In reality his worst problems in 1632 were with the diurnal tide and the annual tide cycle, they were far more serious and quite obvious to anyone who knew the facts: and he had decided he had to ignore them. It is therefore not only conceivable but quite believable that he had recognised the faults in the rotational theory but regarded them as less acute and much less obvious than the two cited breakdowns of the theory, so he could have decide that he had little to loose by disregarding them also. As he perceived the overall evidence for Copernicanism to be overwhelming he could have decided to forge ahead regardless. In the short term he might carry his audience, in the long term he would be vindicated. That would fit the picture of Galileo the man who took risks. He took many in writing the *Dialogue*.

But unless Galileo was oblivious of the mathematical flaws in his theories or was convinced they would never be detected it would naturally be hard to reconcile such a course of action with his portrayal of the new sciences. Nevertheless one gains the sense in the Fourth Day of the *Dialogue* that Galileo felt compelled to complete his case for Copernicanism irrespective of the difficulties. It is also possible that only when he had at long last completed the Fourth Day, late in 1629, that he finally recognised the flaw in the rotational theory of the tides. Whichever of these scenarios is correct each in it's way reveals that he had ultimately got out of his depth.

His letters reveal that he was quite concerned by the serious empirical failure of the theory: more than he is prepared to admit in the *Dialogue*. We know he disregarded these difficulties as he had no option. But his anxiety about this is reflected by his disproportionate and ill concealed hostility towards the well established and empirically successful rival –the lunar theory. His generally contemptuous attitude towards it is ostensibly prompted by it's lack of an intelligible mechanism linking tidal to lunar motion. But in reality this is primarily a tactical attack, and a sign of his unease. The rotational theory has only the appearance of an intelligible mechanism linking tides to terrestrial motion, rather than a real one. Galileo does not even attempt to spell it out in any detail. His surprisingly scathing attack on the lunar theory concludes that as it lacks a valid mechanism then not only all that Kepler and Seleucus have said but "everything previously conjectured by others" can be dispensed with as it is based on a theory that is "completely invalid". But looking first at his own tidal mechanism and then at the evidence, one can see his conclusions are not only unreasonable but actually unjustifiable. Worse still on investigation one discovers that in his analysis of the phenomena Seleucus was in reality far more successful than Galileo. Little wonder that Galileo was apprehensive, for the observations of Seleucus were systematic, accurate and contradicted Galileo's theory completely.

Galileo had been mulling over his tidal theory, on and off, for four decades. In that time he had come to realise that he had taken on a far more difficult task than he had initially anticipated. Some of the more critical difficulties of his theory are buried so deeply in the *Dialogue* that to this day they have still not been unearthed. Ultimately this daunting, and actually impossible task inevitably got the better of him, so overwhelmed was he by it's myriad problems and contradictions. With time running out Galileo had to do his best, try to cut his losses and hope his rhetorical abilities would in some measure make good the deficiencies.

In presenting his new sciences Galileo obviously did believe that a degree of licence was acceptable in pursuit of a just cause, both in an empirical and in a theoretical sense. And in this his simplification of circular fall and his tidal theory resembles his simplification of the Copernican theory. His concern, as he had long maintained, was with philosophical astronomy, and it was in his view justifiable to deploy simplifications and rhetorical devices in the quest to illuminate some essential truth, and he had regarded Copernicanism to be an essential truth since 1613.

The reality is that the *Dialogue* is so much a work of persuasion and propaganda that as such it is not a reliable indication of Galileo's mature scientific approach. Such a predominantly didactic work inevitably yields a distorted image of Galileo's science. The whole history of the tidal project reveals that. Galileo did not discover the theory of the tides any more than he discovered the theory of circular fall, he constructed each of them as arguments for the earths motion. From the germ of an idea he fabricated the tidal theory, using whatever means he could, always the intention of demonstrating his case in view. Thus we find Galileo is repeatedly attempting to force the phenomena into his schematic framework even when it seems obvious that they will not fit.

If what Galileo had attempted in the *Dialogue* was done in order to make the merits of Copernicanism *comprehensible* to a non expert audience it did succeed in the astronomical domain but not on the crucial issue of the physical proof of the earth's motion. The mathematically competent were generally unconvinced by the tidal theory. On the other hand others were carried along by Galileo's prestige, charisma and rhetorical skills and accepted his opinion even though they did not understand his argument. Galileo was after all seen as a great astronomical authority: how could a man of his legendary achievement be wrong in a field he had proved himself to be so completely the master? So he did in large measure achieve his main objective.