THE STUDY OF SATURN'S RINGS

1610-1675

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by

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ABSTRACT

Shortly after the publication of his Starry Messenger, Galileo observed the planet Saturn for the first time through a telescope. To his surprise he discovered that the planet does not exhibit a single disc, as all other planets do, but rather a central disc flanked by two smaller ones. In the following years, Galileo found that Saturn sometimes also appears without these lateral discs, and at other times with handle-like appendages istead of round discs. These appearances posed a great problem to scientists, and this problem was not solved until 1656, while the solution was not fully accepted until about 1670.

This thesis traces the problem of Saturn, from its initial formulation, through the period of gathering information, to the final stage in which theories were proposed, ending with the acceptance of one of these theories: the ring-theory of Christiaan Huygens. Although the improvement of the telescope had great bearing on the problem of Saturn, and is dealt with to some extent, many other factors were involved in the solution of the problem. It was as much a perceptual problem as a technical problem of telescopes, and the mental processes that led Huygens to its solution were symptomatic of the state of science in the 1650's and would have been out of place and perhaps impossible before Descartes.

But once the ring-hypothesis had been formulated, it was not immediately accepted. It was one of a number of hypotheses and a process of evaluation had to be gone through. The most interesting and enlightening aspect of this process of evaluation is the work done by the Accademia del Cimento which acted as referee between Huygens and Honoré Fabri in a controversy between two opposing theories. Not until several adjustments had been made to Huygens' theory was it completely accepted by all important astronomers in Europe.

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(Galileo, 1610)

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(Huygens, 1659)

INTRODUCTION

Seventeenth century observational astronomy has generally been considered as a convenient source of information and anecdotes bearing on the development of celestial mechanics and the synthesis of celestial and terrestrial mechanics. But one only has to open Newton's Principia to Book III to see how great a debt Newton owed to all the men who had spent countless nights looking through their telescopes and making painstaking measurements and estimates. The conception of the Universe which held sway until the beginning of the twentieth century is usually referred to as the Newtonian Universe, and quite rightly so, but Newton's Universe was constructed from the data made available by observers of the heavens, and their observations were not fortuitous: these observers were usually trying to answer the same questions with which Newton was occupied. This is of course amply illustrated by the fact that many of the observers who made important astronomical discoveries were not merely astronomers. One has only to think of Galileo, Huygens, Borelli, and Hooke to realise this, and it is true for the vast majority of the observers of the heavens with whom we shall deal in this thesis.

The first thirty years after the invention of the telescope, coinciding with the latter half of Galileo's life, naturally bear Galileo's stamp. Not only was he the discoverer or at least the co-discoverer of all the wonderful new phenomena revealed by the telescope, but his attitude towards this instrument was characteristic of the general attitude towards it. All Galileo's discoveries, with the possible exception of the phases of Venus, were unexpected and therefore fortuitous. He never made systematic series of observations for their own sake. As a matter of fact, the only sustained series of observations made by Galileo was of the satellites of Jupiter, and his purpose here was a practical one: finding longitude at sea. He was content, (as well he might be!) with, so-to-speak, skimming the cream off the top, and after his brilliant discoveries, the telescope remained for a number of years a novelty with which to show one's friend or patron the Medician stars or the strange appearances of Saturn.

This situation did not change until the 1640's. Only in that decade did the telescope become a bona fide part of the arsenal of the professional astronomer. At this time also, the so-called Keplerian telescope came into use. It is fruitless to argue about whether Fontana, Scheiner, Jansen, Rheita, or someone else first used the Keplerian configuration of lenses. The fact that the idea had been around since at least 1611, but did not find wide application until the 1640's is ample proof that until that decade the telescope was indeed a toy in which an erect image was essential in order to show the writing on a far off church to some dignitary.

The improvement of the Keplerian telescope between 1645 and 1675 was indeed very great. But it is a very serious over-simplification to ascribe all the great astronomical discoveries made between 1655 and 1685 solely to improvements in the instrument. The satellite discovered by Huygens had already been seen in England and Danzig, but had been considered a fixed star. Huygens did not discover or see a ring: Saturn's ring was invisible when he formulated his ring-hypothesis. Solar parallax was not found fortuitously; one does not send a man to Cayenne to make fortuitous observations! The fact is that what astronomers saw and discovered after Galileo's initial discoveries depended very much on what they were looking for. It is not a coincidence that the first published tables of Jupiter's satellites did not appear until 1652 (the first <u>reliable</u> tables did not appear until 1668), while mentions of Kepler's third law were exceedingly rare before 1650 and increased rapidly after that date. Observational astronomy, after about 1645, directed itself to the important questions of the age, and the discoveries made by men like Huygens and Cassini must not be viewed outside this context.

For a greater part of the seventeenth century, the planet Saturn presented a great puzzle to observational astronomers. While today Saturn is still the most fascinating planet to look at through a telescope, it was no different in the seventeenth century, even before it was decided that its strange appearances were caused by a ring. Saturn presents a convenient opportunity for the study of seventeenth century observational astronomy because the problem of its appearances runs through the subject as a continuous thread. Interest in this planet was always high and if, for some reason, that interest temporarily flagged somewhat, it was always renewed by one of the periodic disappearances of the mysterious anses. But besides this, the problem of Saturn sheds light on the approach to problems and their solutions as practised by physical scientists in the seventeenth century. For this purpose it is a very convenient problem, with a definite starting date, a definite solution, and without any philosophical biases imposed by an earlier age. It therefore offers a unique opportunity to examine the method of approach employed by physical scientists in that century and how that method changed over the years.

It is therefore with a dual purpose that I undertook the research for this thesis: an interest in observational astronomy during that vital period of exploration of the heavens -- the period in which our immediate corner of the Universe took on its modern shape and dimensions - and an interest in the working of science in the seventeenth century as illustrated by this problem and its solution.

SECTION I

STATING THE PROBLEM

CHAPTER 1

Galileo and Scheiner

Galileo ended his Sidereus Nuncius with the statement 'Time prevents my proceeding further, but the gentle reader may expect more soon.' (1) And he was true to his word. He went on to discover the strange appearance of Saturn, the phases of Venus, and, independently of David Fabricius and Christoph Scheiner, he discovered sunspots. Galileo first observed Saturn in July 1610. In his letter of 30 July to Belesario Vinta he wrote:

> I began on the 15th of this month again to observe Jupiter in the morning, in the East, with his formation of the Medician planets, and I discovered another very strange wonder, which I should like to make known to Their Highnesses and Your Lordship, keeping it secret however until that time when my work is published. But I wished to inform Their Serene Highnesses of it in order that, if others should discover it, They would know that no one observed it before me. I am quite sure that no one will see it before I have pointed it out. This is that the star of Saturn is not a single star, but is a composite of three, which almost touch each other, never change or move relative to each other, and are arranged in a row along the zodiac, the middle one being 3 times larger than the two lateral ones, and they are situated in this form
>
>  , ... (2)

Thus Galileo acquainted his patrons, the Medici brothers, with his discovery and then sent an anagram containing this discovery to a number of scientists. The anagram, s m a i s m r m i l m e p o e t a l e u m i b u n e n u g t t a u i r a s , was, according to Vincenzio Viviani, sent to, among others, Benedetto Castelli in Brescia, fathers Clavius and Grienberger at the Collegio Romano, Johann Kepler and Giuliano de' Medici in Prague. (3) Kepler tells us in the preface to his Dioptrice of 1611 that from the letters of the anagram he had managed, with some gymnastics, to put together the phrase 'Salve umbistineum geminatum Martia proles', or 'Hail twin companionship, children of Mars'. As Kepler himself states, 'But I was a very long way from the meaning of the letters.' (4) In view of the discovery of Jupiter's satellites, it was only reasonable for Kepler to think in terms of more satellites. However, Thomas Harriot in England tried to solve the anagram in terms of the Moon's shadow. His efforts are preserved in the Harriot papers in the British Museum. (5)

The solution to the anagram was announced by Galileo in the autumn of 1610. On November 13th he wrote as follows to Giuliano de' Medici in Prague:

> But going on to other things, now that Mr. Kepler has published the letters which I sent to Your Illustrious Lordship in his latest work, it has come to my attention that His Majesty [the Emperor] would like to know the meaning of it. I now send it to Your Illustrious Lordship, to share it with His Majesty, with Mr. Kepler, and with whomsoever it pleases Your Lordship, wishing myself that everyone should know it. The letters then, combined in their true sense, say thus:
>
> <u>Altissimum planetam tergeminum observavi</u>
>
> [I have observed the highest planet to be tri-form.] This is to say that Saturn, to my very great amazement, was observed by me not to be a single star, but three together, which almost touch each other. They are completely immobile and are situated in this manner oOo ; the one in the middle rather larger than the lateral ones. These are situated one to the East and the other to the West, in the same straight line to a hair. They are not precisely according to the line of the zodiac, but rather the one to the West rises somewhat to the North. They are however parallel to the Equinoctial. (6)

Thus Galileo qualified his opinion on the direction of the little globes from his previous opinion, expressed to Belesario Vinto.

Those who did not have telescopes as good as those of Galileo might not be able to see Saturn in this form. Galileo, who had obviously also observed the planet with an inferior telescope, anticipated this in the same letter:

> If one looks at them with a perspective which is not of very great multiplication, they will not be seen as three distinct stars, but it will appear that Saturn is a long star in the shape of an olive, like this  . But when a perspective which multiplies one thousand times in surface is used, three globes will be seen distinctly, and [it will be seen] that they almost touch, no greater separation appearing between them than a thin dark line. Therefore I have found the court of Jupiter and two servants for this old man, who help him walk but never leave his side. (7)

Kepler's reaction to this last statement is equally metaphorical, if somewhat more enthusiastic:

> So says Galileo; but if I may do so, I will not make an old dotard out of Saturn, and two servants for him out of his companion orbs, but rather, out of those three united bodies I will make a triple Geryon, out of Galileo Hercules and out of the tube his club, armed with which, Galileo has conquered that most distant of planets, drawn it out of the furthest recesses of nature, dragged it down to earth, and exposed it to the gaze of us all ... (8)

Galileo was quite correct in his belief that the observers who had inferior telescopes would not be able to see Saturn in the form in which he had described it. Christophorus Clavius of the Collegio Romano wrote to him as follows on 17 December 1610:

> Antonio Santini has recently written to me that you have discovered that Saturn is composed of three stars, that is, two small stars stand next to him on either side. This we have not yet been able to observe: we have only noted with the instrument that it appears that Saturn is oblong in this fashion . (9)

And although Galileo assured Clavius in a letter of 30 December 1610 that if seen with a sufficiently large telescope Saturn indeed appeared triple-bodied, the mathematicians of the Collegio Romano could not clearly see Saturn as three separate globes:

> ... we have observed that Saturn is not round as Jupiter and Mars are to be seen, but of an ovate and oblong figure, in this fashion  ; we have not seen the two little stars on the sides clearly enough separated from the one in the middle to be able to say that they are separate stars. (10)

Furthermore, Kepler wrote to Galileo in his letter of 28 March 1611 that he had seen all four satellites of Jupiter with an instrument that 'multiplied' the diameter six times and showed the largest spot on the Moon the same size as the whole Moon seen with the naked eye. But Kepler says 'this instrument, I think, will not suffice to diagnose the figures of Saturn and Venus.' (11)

Obviously therefore, Galileo had one of the few telescopes capable of showing at least the separation between the ring and the body of Saturn. Perhaps the best illustration of the comparative merit of Galileo's telescopes is a contemporary description of Jupiter and Saturn by Jacob Christmann (1554-1613), professor of logic at the University of Heidelberg, in a little work entitled Nodus Gordius (1612):

> On the 22nd of October of the last year [that is, 1610], at half past seven p.m., Saturn crossed the meridian. His body showed itself in three distinct scintillations through the

> smaller rod [radius]. Through the larger rod, on the other hand, he was perceived split into four fiery balls. Through the uncovered rod [two lenses not in a tube], Saturn was seen to be one long and single star. Therefore it is shown to be the case that two, three, or four companions stars have been detected about Saturn with the aid of the new sight. But from this it does not follow that two or three servants have been assigned to Saturn by nature, who like bodyguards watch him and march around with him constantly. How this fantasy can arise is evident from the above ...
> At the beginning of the month December, by means of either radius cylinder, the body of Jupiter appeared in three distinct scintillations and exhibited two shimmering diameters, or rather, the body of Jupiter was seen completely on fire, so that it appeared separated into three or four fiery balls, from which were spread thinner hairs in a downward direction, like the tail~~s~~ of a comet. (12)

Although this is not the only example of incredibly bad telescopes, it will suffice to show how good Galileo's telescopes were by comparison.

In 1611, Christoph Scheiner wrote three letters on sunspots to Marc Welser, and in the third letter (26 December 1611) Scheiner states that Saturn is seen sometimes in an oblong form, and at other times with 'two lateral tangent companions'. (13) This statement was answered by Galileo in his first letter on sunspots to Welser (4 May 1612):

> But as to the supposition by Apelles [i.e. Scheiner] that Saturn is sometimes oblong and sometimes accompanied by two stars on his flanks, Your Excellency may rest assured that this results either from the imperfection of the instrument or of the eye of the observer, for the shape of Saturn is thus: ○◯○ , as shown by perfect vision and perfect instruments, but appears thus ⬭ where perfection is lacking - the shape and distinction of the three stars being imperfectly perceived. I who observed him a thousand times at different periods, with an excellent instrument, can assure you that no change whatever is to be seen in him. And reason based on experience of all other stellar motions renders us certain that none ever will be seen, for if these stars had any motion similar to the motions of other stars, they would long since have been separated from, or conjoined with the body of Saturn, even if that movement were a thousand times slower than that of any other star which goes wandering through the heavens. (14)

Thus, after having observed Saturn over a period of two years (during which he could hardly have made a thousand observations), Galileo was convinced that the triple appearance was the permanent shape of Saturn because any relative motion, no matter how slow, would have led to an observable change over that period of time.

However, in 1612 the picture suddenly changed. Since Galileo had not noticed any change whatsoever, he lost interest in Saturn, and after not having observed it for several months, when he observed the planet again the ring was edge-on and Saturn appeared as a single round globe:

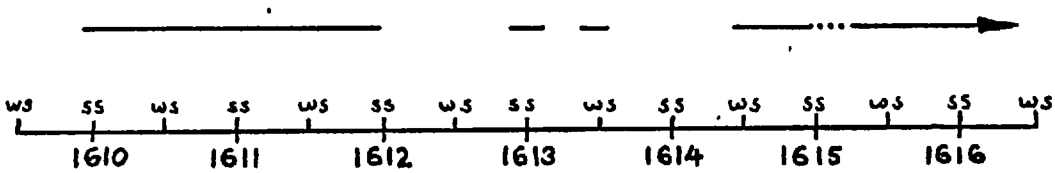
> About three years ago I wrote that to my surprise I had discovered Saturn to be triple bodied, that is, that he was an aggregate of three stars arranged in a straight line parallel to the ecliptic, the central star being much larger than the others. I believed them to be mutually motionless ... for when I first saw them, they seemed almost to touch, and they remained so for almost two years without the least change. It was reasonable to believe them to be fixed with respect to each other, since a single second of arc (a movement incomparably smaller than any other, in even the largest orb) would have become sensible in that time, either by separating or uniting these stars. I also saw Saturn triple bodied this year, at about the time of the summer solstice; and having then ceased to observe him for more than two months, as one who does not doubt his own constancy, finally observing him again these past few days, I found him solitary without the assistance of the supporting stars, and, in sum, perfectly round and clearly defined as Jupiter. Now what can be said of this strange metamorphosis? That the two lesser stars have been consumed in the manner of sunspots? Has Saturn devoured his children? Or was it indeed an illusion and a fraud with which the crystal deceived me for so long - and not only me, but many others who have observed him with me? Perhaps the day has arrived when languishing hope may be revived in those who, led by the most profound reflections, once plumbed the fallacies of all my new observations and found them to be incapable of existing. (15)

But Galileo quickly recovered from his surprise, for this third letter on sunspots continues:

> I need not say anything definite upon so strange and unexpected an event; it is too recent, too unparalleled, and I am restrained by my own inadequacy and fear of error. But permit me to use a little temerity; may this be excused by Your Excellency, since I confess it to be rash and I protest that I do not mean to register a prediction based on certain principles and secure conclusions, but only on probably conjectures, which I shall make obvious when they are necessary, either to show the excusable probability of the opinion which I favour now, or to assure the certitude of the assumed conclusion, whenever my thought encounters the truth. The propositions are these: the two minor stars of Saturn, which for the present are hidden, will however show themselves a bit for two months around the summer solstice of the next year, 1613, and then will conceal themselves, remaining hidden until near the winter solstice of 1614, around which time it could happen that again for a few months they will show themselves

> somewhat, then again concealing themselves until near the following winter, at which time I believe with great resolution that they will reappear, not hiding again until the following summer solstice, which will be in 1615. They will incline somewhat to wishing to hide themselves; however, I cannot believe that they will hide themselves entirely, but rather, manifesting themselves a little later, we will see them distinctly and more lucid and larger than ever. And I should almost dare to say resolutely that we shall see them for many years without interruption whatsoever. As for their return then, I have no doubt; as for the other particulars, I speak of them with reservations, since they are based for the moment only on probable conjectures. But whether it happens in this way or in another, I say indeed to Your Excellency that this star again, and no less than the appearance of horned Venus, coincides admirably with the harmonies of the great Copernican system, ... (16)

Thus, Galileo makes one certain prediction: Saturn will regain its companions; and he engages in a good deal of conjecture as to the sequence of appearances and disappearances. It is difficult to ascertain what Galileo's model for these 'predictions' was. Graphically they look as follows:



As can be see from this, there is no obvious pattern to the appearances and disappearances. What is more, the fact that after the last partial disappearance the companions would be seen for a long time without interruptions rules out any simple periodic model. Therefore Galileo's model for these predictions can only be guessed at. It appears most likely that he had a model in mind in which the lateral globes moved around Saturn, or in which the whole formation of the three globes turned about an axis. We shall return to this below.

The first part of the prediction, that the lateral globes would return around the summer solstice of 1613, came true, and Galileo received praise for this. On 13 July 1613, Giovanni Battista Agucchi wrote to him as follows:

> ... I thank you very much for informing me, and express my appreciation for the news you gave me of Saturn, that, according to your prediction, he has returned to being triple bodied this past solstice. I have seen him at other times, sometimes as an oval, that is, when I did not have a good enough instru-

> ment, sometimes as three distinct bodies. But I did not look at him when he was alone and perfectly circular in form. Now I have seen him clearly as you wrote me, with his two little globes flanking him; and I was indeed pleased that your opinion was proven correct, for your reputation, even as I write this account, is increasing. (17)

Note again that there is little question in Agucchi's mind as to what he should see with a perfect instrument. The tri-spherical appearance was clearly the primary model of Saturn, the model in terms of which every appearance had to be explained. It remained the primary model for the next thirty years or so and the mono-spherical and the later bona fide oval appearances remained secondary: appearances that were to be derived from this primary appearance.

It is therefore not surprising that the first effort to explain the changing appearances of Saturn was in terms of a 'satellite theory'. In 1614, a dissertation entitled Disquisitiones Mathematicae de Controversiis et Novitatibus Astronomicis was published in Ingolstadt. The author was Georg Locher and the examiner was his teacher, Christoph Scheiner. Galileo always ascribed these disquisitions to Scheiner, a supposition which is reasonable in view of the fact that Locher never published anything else. However, Alexandre Koyré disagrees with this and rather believes that it was Locher whose ideas were represented in the dissertation (although probably inspired by Scheiner). (18) It is clear that, on the subject of Saturn, these Disquisitiones represent the ideas of Scheiner, as will become evident below.

In proposition 44 of the Disquisitiones, the problem of Saturn is treated. It starts as follows:

> Hitherto Saturn deceives or really mocks the astronomers out of hatred or malice. For he has projected various appearances. Sometimes he is seen single and sometimes triple; at one time elongated and at other times round. (19)

After describing the different appearances and where and when and by whom they were observed, Scheiner (or Locher) asks whence this inconsistent appearance arises.

> Either therefore this is to be ascribed to local motion or it is to be assumed that those companions of Saturn are consumed. But the [latter] has not been claimed by any one thus far. Therefore it is to be ascribed altogether to local motion. But what is this motion? Here the astronomers are at a loss. For are these stars to be thought to revolve around Saturn with their own motion, or are they drawn along with his motion?

> If the first is true, it is necessary that they approach Saturn and recede from him and that, at length, they are occulted, etc., which thus far has not been observed, although some pretty definite hints exist, because Saturn may be seen now solitary, that is, in conjunction, now like an egg, when they are approaching or receding, and now tri-form, with ... [the lateral bodies] ... situated near station. If the second is the case, it is necessary that one or the other of the following is true: either Saturn revolves about his own axis and thus the stars under consideration revolve with him and are occulted in conjunction, which mode is possible but not very pleasing because it is seen to depend on conjecture, or it is necessary that it comes from this, that Saturn leads the attendants with him in the turning of his annual epicycle, while otherwise they remain besides him. But it remains to be seen whether and how this can be. It is also to be judged carefully whether the width of Saturn, now greater, now smaller, has anything to do with this. But in all these things we suspend judgment as yet, leaving them for further trials and phenomena; this alone being decided: these changes arise above all from local motion, be it of the little stars or of Saturn himself. And therefore it comes about that it can be ascribed to the revolution of the Ptolemaic epicycle, and, similarly, that it seems to go badly with the great orb of Copernicus. (20)

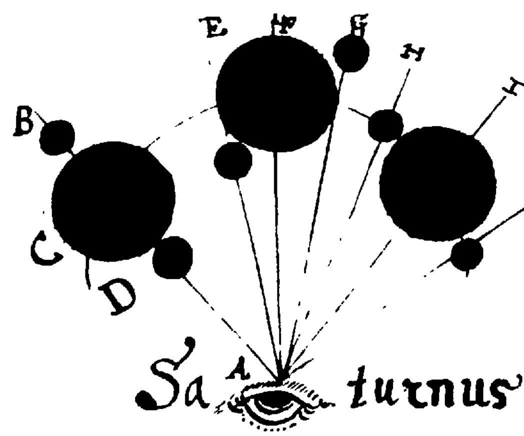
Note that, whereas in his third letter on sunspots Galileo had rather vaguely claimed that '... this star, no less than the horned Venus, fits admirably with the harmonies of the great Copernican system ...' (21), Scheiner here makes a case for Saturn's appearances supporting the Ptolemaic system. In fact, the different appearances of Saturn were no clear support for any system of the world. Galileo did try once more to use Saturn in more than a general way to support the Copernican system. This is in the <u>Dialogo</u>, where, on the subject of the diurnal rotation of the Earth, he cites Saturn as follows:

> SALVIATI: He asks me what the principles are by which the terrestrial globe makes its annual motion through the zodiac, and its diurnal motion around the equator upon itself. I say to him that they are similar to those by which Saturn moves through the zodiac in thirty years and about his own center in the equinoctial plane in a much shorter time, as the disclosure and hiding of his collateral globes shows us. (22)

In a manuscript entitled 'Tractatus de tubo optico', written by Scheiner in 1616 according to Ernst Zinner (23), Scheiner makes it clear that he shares the opinions on Saturn expressed in the <u>Disquisitiones</u>:

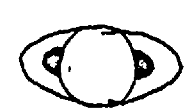
> However, for the source of this triple appearance no more suitable cause can be suggested at the present time, it seems, than that suggested in the <u>Disquisitions</u>, namely motion. (24)

He explains the different appearances by the same reasoning, with the help of the following figure,



This 'satellite theory' had a long life; we shall have occasion to return to it.

But Saturn had not yet exhausted his bag of tricks. As time passed, the inclination of the ring with respect to the Earth increased and in 1616, Galileo, who had again interrupted his observations of this planet, was treated to yet a third surprise. The following sketch is found in his manuscripts, but it has no date and no reference is made to it. (25) This sketch can easily be interpreted by the modern reader as representing Saturn and its ring. But Galileo had no idea as to what he was supposed to see and therefore made no such interpretation - in fact, he made no interpretation whatever. Although the sketch itself tells us nothing of Galileo's ideas (there is not even evidence in that manuscript that Galileo made the sketch), through the tireless researches of Antonio Favaro, we know Galileo's thoughts concerning the figure of Saturn presented to him in August 1616. Favaro found a letter from Giovanni Faber in Rome to Cardinal Frederico Borromeo in Milan, written on 3 September 1616. In this letter, Faber includes an extract from a letter from Galileo to Frederico Cesi, describing the appearance of Saturn:



> I don't want to keep from telling Your Excellency of a new and strange phenomenon I observed several days ago, which is that

> in the star of Saturn, whose two companions are no longer two small perfectly round globes as they were before, but are at present much larger bodies, and no longer round, as seen in the adjoined figure  that is, two half eclipses [sic] with two little dark triangles in the middle of the figures, and contiguous to the middle globe of Saturn, which is seen, as always, perfectly round. (26)

Faber's copy is much less reminiscent of a ring than is the sketch in Galileo's notebook. But the description in the letter fits the latter much better than the former.

It is interesting to speculate what might have happened had the ring been in an open position rather than nearly closed in 1610. Might Galileo or some other observer have guessed the true cause of the strange appearance? This is highly unlikely in view of later developments. Even when astronomers started to consider the 'handled' shape of Saturn as the primary appearance, in terms of which the other appearances had to be explained, they did not immediately formulate a ring-hypothesis. What is more, if Galileo or one of his contemporaries had taken the oval appearance of 1616 as the primary appearance, they would have had an equally difficult task of explaining the tri-spherical and round appearances in terms of this oval shape.

Galileo continued to observe Saturn in this oval appearance for a number of years, as evidenced by his letter of 11 January 1620 to Fortunio Liceti and by his sketch in Il Saggiatore of 1623,

Figure shown in the letter to Liceti, 11 January 1620 (27)

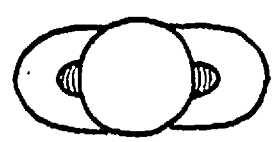


Figure in Il Saggiatore, 1623 (28)

He was aware that Saturn appeared single again in 1625 (29), but references to the planet in the Galileo correspondence are rare during the latter part of his life. In the above mentioned letter to Liceti Galileo predicts '... that Saturn's lateral globes will not be occulted [again] until about the year 1626.' Did Galileo realise that the 'occultations' occurred twice during each period of Saturn? If he did, he subsequently rejected that notion, for in the Dialogo he states:

> ... Saturn moves through the zodiac in thirty years and about his own center in the equinoctial plane in a much shorter time as the disclosure and hiding of his collateral globes show us. (30)

One thing is clear: Galileo definitely thought in terms of Scheiner's model: that is, the motion of the lateral globes about Saturn, or the turning of all three globes jointly, caused all the different appearances of the planet.

But in summing up his experiences with Saturn in a letter to Benedetto Castelli in 1640, Galileo makes no mention of such a model:

> The first view I had of Saturn was of three round stars placed in a straight line from west to east, the one in the middle much larger than the lateral ones. I continued to observe them thus for several months, and having interrupted the observations of him for several other months, I started to look at him again and found him solitary. Amazed by this, I reflected on how such a change could have come about, and imagining in my own special way, I took the courage to say that in 5 or 6 months time, when the time of the summer solstice was to come, the two little lateral stars would have returned. And so it happened, and they were seen then for a long time. Afterwards, having again interrupted the observations while they were in the rays of the Sun, I turned anew to watch him, and saw him with two mitres in place of the round stars, which caused him to appear like an olive. However, the ball in the middle was seen quite distinctly and surrounded by two dark spots positioned in the middle of the junctions of the mitres, or, that is to say, ears. Thus I observed him for many years. An now, as Your Rev. Father writes, the mitres are seen to be transformed into round little globes, as my friends still relate it to me; and it could be that for the last three years, during which I have not been able to see him, he has perhaps again been solitary and then returned to the first state, the state in which I first saw him. Contact in the future the others making observations, recording the time of the changes, whose periods will certainly be found when there are people who have the curiosity to do what I have done for so long, if not better. (31)

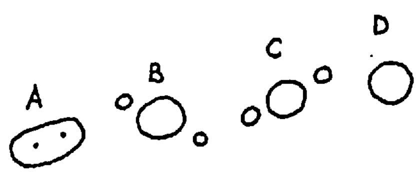
From the above, several things are clear: Galileo never made a sufficiently continuous series of observations to enable him to ascertain the progression of the 'phases'; he probably adhered to some 'satellite model', but never stated this explicitly and probably changed his mind several times. The only conclusion he arrived at was that more data were needed to solve this complex problem.

Galileo's contemporaries present a similar picture. References to Saturn in the literature between 1610 and 1642 are rare. Scheiner never again referred to it in his works after 1616, although his student and co-worker, the Jesuit Johann Baptist Cysat, alludes to Saturn as having two planets moving around it, in his work on comets, *Mathematica astronomica de loco, motui, magnitudine et causis cometae* (Ingolstadt, 1619). (32) That Saturn's strange shape was still a very new phenomenon in 1617 is evidence by the skeptical attitude of Giovanni Antonio Baranzani (1590-1622), a professor of philosophy at a religious college at Annecy (S.E. France). In his *Uranoscopia* of 1617, after quoting from the preface of Kepler's *Dioptrice*, he states:

> ... but what if two stars of the firmament behind Saturn, appearing in conjunction with Saturn, were seen by Galileo? What if his eye was hallucinated in the same way as those of the Hebrews? What if he had an inept eye? Therefore I certainly do not dare to call the planet simply a triple Geryon. You can try to detect the appearance and after that proclaim your judgment. (33)

But except for such skepticism, the astronomers of Europe generally accepted the 'satellite model'. Giuseppe Biancani (1566-1624), a Jesuit professor of mathematics at Parma, wrote in his *Sphaera Mundi seu Cosmographia* (Modena, 1620)

> It is also extraordinary what the astronomers of our time have revealed about Saturn, and all are equally surprised, namely that Saturn is surrounded by two small companions, just as Jupiter is surrounded by four, which are positioned with Saturn in a line parallel to the equator, just as the companions of Jupiter are positioned with him in a line parallel to the ecliptic. And they are in fact sometimes very close to Saturn so that they cannot be distinguished from him, but cause him to appear oval, as in fig. B, sometimes they are clearly separated from him, as



> in fig. C. But it is altogether strange that Galileo and others have seen them to stand near Saturn continuously for two whole years and longer, after which they vanished, which unexpected spectacle keeps observers astounded and astonished, nor do I know which of them is true. I myself, at the end of October of the year 1616 saw him oval and with two round spots on either side of the centre, as the first figure, A, shows, in which figure he still remains in the month of November 1619, when this was printed. See Galileo near the end of his book on [sun]spots and also the Mahtematical Disquisitions of Christoph Scheiner of our society. (34)

In his *Collecta Astronomica* of 1631, Christophoro Borri, a Jesuit professor at the University of Coimbra, affirms that although the little stars may appear attached to the middle globe, in reality they are separate and distinct. (35) Kepler, who was never really involved in the problem of Saturn beyond his comments in the *Dioptrice*, similarly believed that the lateral bodies of Saturn were satellites, or rather secondary planets, that is, planets that go around primary planets, and not, as the primary planets do, around the Sun:

> ... it is believed that Saturn has two of these and leads them around with him, which now and then become visible with the help of the telescope. (36)

This opinion was also held in England, where John Wilkins wrote in *A Discourse concerning a New World and another Planet* (London, 1640), that '... the lesser Planets lately discovered about *Jupiter* and *Saturn*, for which the Astronomers had not yet framed any Orbs', were additional evidence that solid orbs do not exist. (37)

Clearly then, the prevalent opinion was that Saturn was flanked by two lateral globes, whose movement explained the various appearances of this fickle planet. But it is equally clear that this model was only a very qualitative one, which caused astronomers concern, as in the case of Kepler and Baranzani. This primitive model was to prove unsatisfactory.

CHAPTER 2

The Telescope

One of the most important ingredients in the solution of the scientific problem posed by Saturn was the improvement of the telescope, and no study of Saturn in the seventeenth century can be complete without some considerations of telescopes and the role they played. Although it was not simply a matter of improving the telescope until observers could 'see' the ring, it is unlikely that the problem could have been solved in the decade of the 1650's if telescopes had not been greatly improved over the first instruments used to observe the heavens around 1610.

The invention of the telescope has been treated thoroughly by historians of science, but its subsequent development has been left to astronomers, with disappointing results. There is therefore no good historical account of the development of telescopes in the seventeenth century. For this reason, a brief account will be given in this chapter (an extended discussion is clearly beyond the scope of this thesis), and in the following chapters this account will be brought to bear on the problem of Saturn, as astronomers faced it.

Sources for a study of seventeenth century telescopes fall into three categories:

1) The telescopes themselves: this approach is limited on one hand by the small number of surviving telescopes from the period up to about 1650, and on the other hand by the obvious difficulties involved in testing the very long telescopes of the later period.

2) Seventeenth century literature: these sources are abundant, although they usually are not very enlightening on data that are today considered relevant, and even essential to a discussion of telescopes. Moreover, the accounts given by telescope makers were usually confusing and vague - often deliberately so - and, more often than not, exaggerated in order to enhance the author's reputation.

3) Observations made by astronomers: if approached with care, these sources can yield useful information about the instruments used. This is especially true in the case of Lunar observations.

The very early history of the telescope, connected with its invention, has been adequately treated (1) and need not concern us here since our study starts with Galileo and since almost nothing is known of the quality of perspective glasses before Galileo. Galileo did not invent the telescope, but he did improve it tremendously. It is fair to say that he combined all the qualities necessary to adapt the early perspective glass to astronomical use: great skill with his hands, great perceptive ability and the uncanny ability to sift out real appearances from illusory ones, and, finally, his great talent for polemic and popularisation, which was decisive in making the telescope popular. No other observer in Europe combined these qualities. Thomas Harriot in England, a very early observer, did not make his own telescopes; Johann Kepler had neither skill with his hands nor ability as an observer; David Fabricius' tract on sunspots was hardly noticed, and Simon Marius' tract on Jupiter's satellites was published too late to have any impact.

Galileo's skill in making telescopes was far superior to that of his contemporaries, especially during the early stages. Perhaps only Christoph Scheiner had telescopes that approached those of Galileo in quality. Certainly the example of Jacob Christmann (see pp. 13-14) gives some indication of the state of the art. The telescopes of Galileo remained the best in Europe until the 1640's. Indeed, it may be said that until his death, the rest of Europe was catching up with his expertise in grinding lenses. Peiresc asked Galileo for one of his famous telescopes in 1635 because neither he nor Gassendi had ever '... chanced to see Jupiter, or Saturn, or Venus, quite denuded of their rays, because of the weakness of our telescopes ...'. (2) Gassendi used the telescope, obligingly sent by Galileo, until his death in 1655. (3)

Unfortunately, Galileo was not in the habit of signing his name on lenses or tubes as later telescope makers did. As a result, only three examples of his work are known to survive: one complete telescope, one tube with objective lens but no eye piece, and the celebrated broken objective lens mounted later in the seventeenth century in an ornate stand. Although it is possible that other telescopes made by Galileo still survive (he made a great number of them for friends and patrons), most likely these have been lost. At any rate, the three described above are the only ones ascribed to Galileo. (4)

These telescopes, all dating from the period around 1610, were tested in 1923 by Giorgio Abetti, an astronomer, and Vasco Ronchi, an expert in optics. Abetti made observations with them, while Ronchi tested them in a laboratory. The findings for the objective lenses were as follows: (5)

lens diameter	aperture	focal length	magnification of telescope	resolution
51 mm.	26 mm.	1.33 m.	14	20"
37 mm.	16 mm.	0.96 m.	?	?
58 mm.	38 mm.	1.69 m.	±30	10"

The theoretical resolutions, given by the formula $\theta = \frac{1.22\lambda}{A}$, where λ is the mean wavelength of white light and A is the aperture, are 5.5", 8.8", and 3.7", respectively. Thus, in his best telescope of the period around 1610, the one of which the broken objective survives (aperture 38 mm.), Galileo achieved and actual resolution of only about one third the theoretical resolution predicted for this aperture. The significance of this will become clear from the discussion below. Note also the amount by which the apertures are stopped down, in order to avoid the aberration caused by differences in curvature between the outer and inner portions of the lens surfaces.

It does not appear that the magnification of Galileo's telescopes ever exceeded by very much the 30 diameters mentioned in Sidereus Nuncius. Benedetto Castelli wrote to Galileo from Rome in 1637 that he had made observations with a telescope which '... makes the object very large, to such a degree that I believe it enlarges the diameter of the object more than 44 times ...' (6), indicating that 44 was a very high magnification for that date.

The quality of Galilean telescopes up to about 1645 can be assessed, to some extent, by comparing the Moon-maps drawn by various observers. Although Galileo had drawn a likeness of the Moon for his Sidereus Nuncius, it was not his purpose to represent the features as faithfully as possible, but rather to argue for the similarity in nature between the Earth and the Moon. The only features that can be readily identified are the mares (see fig. 1). Abetti states that with the broken objective he could easily see features such as the crater Herschel, which has a diameter of about 28 miles, and that the smallest features visible were about 10 - 15". Galileo certainly showed no such detail in Sidereus Nuncius. Scheiner, whose purpose was much the same as Galileo's, did show some more detail in his likeness of the waxing Moon in his Disquisitiones Mathematicae of 1614 (see fig. 2). Although we can say with some certainty that he could see craters of the order of 25 - 30 miles in diameter, there are 'gemmae innumerae' around the south-pole, which he does not show at all. (7)

For the first serious attempt to represent the face of the Moon we must turn to Gassendi. In 1634 and 1635 Gassendi made a series of observations of the Moon, and on the basis of his sketches Claude Mellan, the famous engraver, made three engravings. Unfortunately, the subtle engravings of Mellan, using continuous burin cuts, varying their depth to bring out contrast, are difficult to reproduce and cannot be shown here. Although Gassendi generally only showed details of at least 30 miles in diameter, he showed some smaller details in Mare Imbrium, near the crater Plato, indicating that with proper contrast he could see features as small as 12 miles long, which is a little more than 10". This is no great improvement over Galileo's telescopes of 1610.

Another early Moon-map is one by Michael-Florent van Langren, or Langrenus, the court astronomer and geographer of Philip IV of Spain. Langrenus was the first to assign names to lunar features, naming craters after monarchs and philosophers, e.g. Copernicus was designated by him as Philippi and Eratosthenes was called Gassendi. The only name of this system which remains in our modern nomenclature is that of the crater he named after himself - perhaps an indireot acknowledgment on the part of Riccioli of the debt he owed to Langrenus. Langrenus' purpose was both to prepare Moon-maps for recording the progress of eclipses and to show the Moon's face as accurately as possible, purposes which converge in his dual occupation as astronomer and geographer. Lunar eclipses were used to find terrestrial longitudes, one of Langrenus' chief preoccupations. Details shown by Langrenus are comparable with those shown by Gassendi; although Gassendi showed more detail in promontories and mountains, Langrenus showed more and smaller craters, e.g. Mädler, Arago, Herschel, Harpatus, Sharp, Mairan, Pytheas, Lambert, and Euler, down to about 18 - 20 miles in diameter (see fig. 3) He did show some of the mountains near Plato, shown by Gassendi, indicating that the resolving power of his telescopes was also about 10". Langrenus presented a Moon-map to a member of the Spanish Royal House in 1628, and he was undoubtedly the first to make a model of the Moon. The map shown here was probably published around 1645. (8) These examples bear out the contention that Galileo's telescopes of 1610 were not surpassed for at least 25 years, and probably longer.

There is no evidence to suggest that Galileo ever tried his hand at making telescopes with convex eye pieces, the so-called Keplerian or astronomical telescopes. This might be due to the fact this type of telescope gives an inverted image, or it could be due to the fact that it was connected with the name of Scheiner - no great friend of Galileo. At any rate, it is likely that the Keplerian telescope did not come into use until after Galileo's death because Galileo did not endorse it. Significantly, this innovation was pioneered (after Scheiner) in Naples, Rome, and Augsburg, not in Florence, the undisputed capital of telescope making during Galileo's days. The erect image of the Galilean telescope was much less important in celestial observations than in terrestrial ones, and moreover, the concave eye piece gave a very limited field of view: in the case of the above-mentioned telescopes this was about 15', or one quarter of the area of the full Moon. Any increase in magnification resulted, as in any telescope, in a concomitant decrease in field. This put a practical limit on the magnification of the Galilean telescope, especially for terrestrial purposes. This practical limitation appears to have been somewhere near 30 diameters.

The Keplerian telescope on the other hand had a much larger field of view for comparable magnifications. Claims of telescope makers, after 1645, were as high as 30 times as great. (9) This is plausible, but probably somewhat exaggerated. A further advantage of this configuration is that objects inserted in the focal plane will give a sharp image superimposed on the image of the object under consideration. Thus, measuring devices could be introduced directly into the telescope. This advantage was recognised in England by William Gascoigne in the early 1640's, but measuring devices did not come into general use until the 1660's.

Although Francesco Fontana of Naples claimed to have invented a telescope with a convex eye piece as early as 1608 - that is, even before Kepler had introduced the idea in his Dioptrice of 1611 - this dubious claim has never been substantiated, and the complete destruction of the archives in Naples during the last war makes it almost certain that it never will be. In his Rosa Ursina (Rome, 1630), Scheiner mentioned that he had used a telescope with a convex eye piece for observations of sunspots as early as 1617.(10) But even after Scheiner's description of the Keplerian telescope, it was not put into practise by anyone, except perhaps by Fontana. It was not until after 1645 that the Keplerian telescope began to catch on, mainly due to two publications: Anton-Maria Schyrle de Rheita's *Oculus Enoch et Eliae* (Antwerp, 1645), and Fontana's *Novae Coelestium Terrestriumque Rerum Observationes* (Naples, 1646).

Although Fontana's publication came after Rheita's, his use of the Keplerian telescope goes back at least into the 1630's, as shown by the observations in the book. Besides making the claim that he had invented this type of telescope in 1608, Fontana discusses the making of telescopes, mentions an erector lens, and shows a great number of observations of the Moon, Jupiter, and Saturn, as well as a few or Mars, Venus, and Mercury. Fontana's reputation was greatly enhanced by these observations because published sketches of the planets were exceedingly scarce up to that time. These drawings, showing the bands of Jupiter for the first time, the anses of Saturn, and even a phase of Mars, were reproduced by other writers, e.g. Riccioli in his *Almagestum Novum* of 1651, and thus had a very wide circulation.

Fontana gives no useful information about his telescopes at all, except for an example of how to make a telescope of 50 palms (about 33 feet). (11) Matthias Hirzgarter's mention of a telescope of 'sechs schuhe' (6 ft.) made by a 'Neapolitan nobleman' (12) is, if somewhat short, probably much closer to the actual size of Fontana's telescopes.

Rheita was a wandering Capucin monk, born in Bohemia in 1597. He took his name from the town of Rheit, where his monastery was located. At the time of the publication of *Oculus Enoch et Eliae*, he had been in Antwerp for some time, but it seems that he had picked up his idea on the Keplerian telescope on his travels in Germany. He states that telescopes described by him could be obtained from Johann Wiesel, an optician, and from Gervasius Mattmüller, the Imperial Optician, both from the town of Augsburg in Bavaria. Rheita called Mattmüller '... a man very practised in practical as well as in speculative optics ...'. (13) Could it be that Rheita, Wiesel, and Mattmüller worked out the idea together in Augsburg, agreeing that Rheita should have the scholarly credit while the two opticians would share the business? This certainly would explain the references to these two telescope makers in Rheita's book, for it was not the habit of inventors not to try to exploit their inventions. Even men like Galileo and Huygens wanted to reap the profits, in money or patronage, from their inventions or improvements of instruments.

Rheita describes the Keplerian telescope in detail, including all its advantages, and gives tables of apertures and relationships between objective- and eye piece focal lengths. His recommendations work out to an aperture ratio of 72 and a magnification of 40 diameters for all sizes. (14) He also introduced the terms 'objective' and 'ocular'. At the end of the treatise, which is only a small part of a much larger work on astronomy, Rheita gives a cryptogram containing his <u>secreta</u>, which is an eye piece consisting of three lenses: in our terms, the ocular itself, an erector lens, and a field lens. But Rheita's information was very vague, just a statement that the eye piece consists of three lenses. (15) It was easy to understand that a lens had been added to erect the image, but what was the purpose of the third lens and where exactly should it be positioned? It is tempting to conclude that this obscurity was deliberate. People who wanted to know more would have to come to him, or buy one of these telescopes from Wiesel and Mattmüller. And even if it was not Rheita's purpose, people did precisely that. Sir Charles Cavendish was very interested in buying one of these telescopes, even before the publication of Rheita's book.(16), and Balthasar de Monconys insisted that Rheita make him a telescope. (17) Wiesel's fame seems to have spread rapidly, for a price list of his instruments, dated 1647, was sent to England from Hamburg. This list includes three types of telescopes, Galilean, simple Keplerian, and the new, so-called terrestrial telescope. (18) Although the simple Keplerian type was better suitedfor celestial purposes (Rheita used it in its simple form), the terrestrial type was well suited for military and naval purposes, giving a much larger field of view than did the Galilean type. It reigned supreme as the terrestrial telescope for 250 years, until it was replaced by Abbe's form of binocular field glasses with erecting Porro prisms. (19) The popularity of these terrestrial telescopes probably had a great deal to do with the spread of the simple Keplerian telescope. It may be that Galileo never liked Keplerian telescopes because he made telescopes mostly for patrons, who were probably more interested in their terrestrial uses. Not until the erector lens was introduced could the telescope with the convex eye piece compete in this field.

Rheita made his observations with a telescope of 15 feet, and Wiesel's price list includes telescopes of up to 14 feet, giving a good indication of the maximum lengths of telescopes in the late 1640's. But neither Rheita nor Fontana were very good observers. Fontana's

Moon-maps show some prominent features and for the rest a more or less random collection of spots, which are impossible to identify (see fig. 4). Rheita's representation of the Moon is somewhat better in this respect. Although he shows little detail, it is clear from the few details that he does show that he could see more than Gassendi and Langrenus could. He shows details inside Theophilus and Cyrillus, the small crater Rosse (8 miles diameter) in Mare Nectaris, and the craters Picard and Peirce in Mare Crisium (see fig. 5). The identity of other small craters can be suspected, but the shapes of the main features are so distorted that certainty is ruled out. However, based on the small craters and the details inside Theophilus and Cyrillus, it can be concluded that Rheita's telescope enabled him to see features as small as 8 miles in diameter, giving a resolving power for his telescopes of about 7". It must be stated, though, that the maps of Gassendi and Langrenus remained far superior in overall accuracy.

But Galilean telescopes were not replaced immediately. This was a process that took perhaps ten years, from 1645 to about 1655. In 1647 Johannes Hevelius published his Selenographia, a book entirely devoted to the Moon. Hevelius used Galilean telescopes for his observations. He describes as an '*egregium tubum*' one of about 5½ feet long, with a double convex objective of about 6 feet focal length and a double concave eye piece of 5½ inches focal length (magnification about 14). (20) He also mentions one with a plano-convex objective of 12 feet focal length and with the same eye piece, combined in a tube of 11 feet (magnification about 28). (21) But this last telescope cannot have had a very large field of view, especially with regard to lunar observations. It appears that Hevelius observed the main outlines with the shorter telescope and after drawing these, used the longer telescope to observe the smaller features, noting their position with respect to some convenient reference point, such as a major crater. The plate showing Hevelius in the process of making observations depicts the smaller telescope. (22)

In Hevelius' observations we see the best results that could be obtained with Galilean telescopes. There are many features which are rather difficult to identify due to some distortion introduced by the method of mapping described above. (see fig. 6) The smallest features shown by Hevelius are about the same size as those shown by Rheita, that is, about 8 miles in diameter, such as Rosse, and Cauchy in Mare Tranquilitatis, and a resolving power of about 7" is indicated.

If Hevelius' work showed the best results that could be obtained with a Galilean telescope, the work of Francesco Maria Grimaldi and Giambaptista Riccioli showed what the new telescopes could do. Riccioli's attitude toward telescopes was eclectic. He states in his Almagestum Novum (Bologna, 1651) that Grimaldi and he had used telescopes by Galileo, Torricelli, and Manzini, either given or lent to him, and another telescope, sold to him by a'Bavarian artificer' (almost certainly Wiesel), which served best of all,

> ... not so much because of its length, although this was 15 feet, as because of the combination of lenses, both convex, so favourably associated, that although they show at least the whole Lunar disc at Apogee at once, they nevertheless amplify it and its individual parts, so that in these parts the smallest particles are disclosed to sight, which with the others we had either not been able to see or had neglected.'(23)

Riccioli's and Grimaldi's likeness of the Moon is indeed far superior to Hevelius', although not so splendid in its engraving. Grimaldi (for he made the lunar observations) shows many easily identfiable small craters within larger ones. Thus, he shows no fewer than four craters inside Clavius, and two in Sacrobosco, the smallest of which is about 7 miles in diameter (see fig. 7). Therefore the resolving power of the best telescope used by Grimaldi, the Bavarian one, was 6" or better.

Thus, by about 1650, the advantages of the Keplerian telescope had become obvious, and the days of the Galilean telescope (for celestial purposes) were numbered. Gassendi was perhaps the last great observational astronomer to use Galilean telescopes seriously. His death in 1655 surely marks the end of an era in telescopes. An entirely new crop of telescope makers had entered on the stage. In Italy, the fame of Eustachio Divini was rapidly spreading during the late 1640's and early 1650's; in England some of the young astronomers under the guidance of Seth Ward were turning their attention to the making of telescopes; in Holland, Christiaan Huygens started to make his own telescopes (together with his brother Constantijn) because he was dissatisfied with the telescopes that were available from instrument makers; and finally, the greatest of all seventeenth century telescope makers, Giuseppe Campani of Rome, started his career some time in the late 1650's. Although professional instrument makers turned their attention to telescope making at a fairly early stage, before Wiesel commercially available telescopes were inferior to the best efforts of men like Galileo and Hevelius. But during the late

1640's this started to change, and by about 1660 very good instruments could be bought from craftsmen such as Divini, Campani, Reeves, Cock, and others. Men like Huygens and Sir Paul Neile rapidly became the exception rather than the rule, and even Huygens could not compete with Campani by 1664.

These men all preferred the Keplerian configurations of lenses. There is no evidence to suggest that Huygens ever made telescopes of the Galilean type. His first major effort (quite possibly his very first effort) was a 12 foot telescope, completed in 1655, with which he discovered the largest satellite of Saturn, Titan. (24) The objective of this telescope still survives, along with at least a dozen other Huygens objectives, and it was tested by Nijland in Utrecht, in 1898, along with a 10 foot Campani objective, dating from perhaps 1660. Both these glasses were mounted on a Steinheil refractor at Utrecht, and the following data were obtained: (25)

	aperture	focal length	resolution
Huygens	52 mm.	3.37 m.	3.8"
Campani	42 mm.	3.17 m.	3.7"

The resolution data were obtained with modern eye pieces, so that they may be somewhat better than they would have been with the original ones. The theoretical resolutions were 2.3" and 2.8" respectively. Comparing these data with the data obtained from the early efforts of Galileo, we see that between 1610 and 1655 the resolution had improved from 10" or more to about 4", but this improvement was not so much due to increase in aperture, because they are roughly the same, as to a much closer approach of the actual resolution to the theoretical one, i.e. better glass and, more importantly, better grinding and polishing techniques. It was remarked of the Campani objective that it would hardly be possible today (1898) to make a better uncorrected objective of the same size. (26) Thus, within the limitations of quality of glass, spherical aberration, chromatic aberrations (not theoretically known until Newton's papers of 1672) and other factors, seventeenth century telescope making had come very close to the theoretical limitations, and this had occurred before telescopes had become very long. Now it must not be thought that these telescopes were free of defects; the Huygens lens was greenish, with many bubbles and lines, although the Campani lens was relatively free from these defects; both suffered badly from aberration, which was particularly severe in the case of stars. Procyon, for example, was seen as a bright yellow disc of about 4.2" diameter, surrounded, in turn, by a dark ring, a blue-green corona which constantly changed, and parts of 3 to 7 further refraction rings; focussing was very difficult. (27) Clearly, considerable talent was needed on the part of the observer to use such a telescope effectively.

The length of seventeenth century telescopes has been a subject of some confusion. It seems reasonable that after the thorough treatment of the geometry of lenses by Descartes, in his Dioptrique of 1637, a rapid increase in the lengths of telescopes should occur, in order to minimise spherical aberration. This is certainly the impression given by H. C. King in The History of the Telescope (London, 1955). (28) King's account is further confused by an error which attributed a 123 foot telescope to Huygens in 1656. (29) But the fact is that telescopes did not rapidly increase in length after 1637; their lengths increased gradually until about 1675, and only then did a rapid increase from perhaps 50 feet to over 200 feet take place.

The reason for all this confusion is that the effect of science on the development of the telescope has been overrated. This is understandable since improvements in the telescope were mostly made by scientists, such as Galileo and Huygens. However, the main factors in the overall improvement of the telescope, the improvement of glass quality and the growth of better techniques and greater expertise in lens grinding, were practical factors. Perhaps the only scientific contribution of practical value during the period under consideration was the Huygenian eye piece. Even Galileo, who claimed to have found the configuration of lenses that would give the effect claimed for the Flemish tube he had heard about, admitted that this 'pure reasoning' consisted of nothing more than trying various combinations of lenses. (30)

Furthermore, telescope makers certainly did not have to wait until Descartes to know about aberration. Simple experience taught that the greater the curvature the more vague and coloured the image became, and that therefore the only way to increase magnification was to keep curvature the same, or decrease it, which meant increasing the focal length of the objective. The only effect of Descartes' work was to introduce the concept of lens surfaces in the shape of conic sections, which resulted in much fruitless labour. But this is, of course, hindsight. Descartes' influence, tempered with some practical considerations, is exemplified by Rheita's advice to readers to try to make hyperbolic lenses of large radius of curvature in the hope that the error would be so small, or advantageous, as to make these lenses superior to any lens with spherical curvature, worked with the greatest care. But Rheita continues: '... this art requires a great deal of perfection and a range of requisites penetrated by few, up to now.' (31)

In discussing long telescopes, care must be taken to distinguish between good telescopes and bad ones, not to mention ones that were planned or 'under construction'. If a telescope maker turned his hand to making a telescope which for that particular time was very long, success was by no means guaranteed. In general the maximum lengths of good telescopes increased from perhaps 8 feet in Galileo's time to about 15 feet by 1650. Although Fontana and Divini speak of telescopes of up to 35 feet, Wiesel's price list of 1647 only mentions telescopes of up to 14 feet, Divini himself made observations of the Moon with a 16 foot telescope, and Grimaldi used a Wiesel telescope of 15 feet for his observations of the Moon. Torricelli's telescope of 18 feet was perhaps the only good telescope exceeding this length before 1650. (32) By 1655, this length had not gone up appreciably. Hevelius still worked with tubes of up to 12 feet, and Huygens' first good telescope, made in 1655, was also 12 feet long. (34) The following year, Huygens made a telescope of double that length, which satisfied him for quite some time. Telescopes of about 25 feet were by and large the longest good telescopes up to about 1660. Reports from England informed Huygens that there were telescopes of up to 56 feet in England in 1656 (35), but the more modest effort of 35 feet, made by Sir Paule Neile in 1657, was probably the only very long telescope (for those days) with which good work was done during the 1650's. (36) Borelli mentioned in 1658 that he was going to search for Saturn's moon with an excellent Divini telescope of 24 feet (37), and even during his controversy with Huygens, in 1660 and 1661, Divini did not mention telescopes of over 27 feet. (38)

During the 1660's Huygens attempted to make longer telescopes; by 1669 he had managed to make glasses of 45 and 48 feet, only a few of which he judged to be of acceptable quality. (39) His major difficulty was the poor quality of glass. Auzout, whose name is usually connected with very long telescopes, could only boast telescopes of 12 and 21 feet in 1665 (40), and in 1668 he compared his new 35 foot telescope with telescopes by Divini and Campani, finding it equal to

Divini's tube, while the Campani telescope, in the possession of Cassini, was, by Auzout's own admission, definitely superior to both his and Divini's. (42) This Campani telescope was the 17 foot one that Cassini brought with him to Paris in 1669. Clearly, even in the late 1660's more could be gained from obtaining better glass (Campani used Venetian glass) and improving the grinding and polishing process (Campani turned his lenses on a lathe, without the use of a form) than from increasing the length of the telescope. Another great improvement came with the introduction in the 1660's of the Huygenian eye piece, which minimises chromatic aberration. Hevelius ordered a telescope of '40, 50, or 60 feet' in England in 1668 (42), and after a year, and one failure, he was sent lenses for a tube of 50 feet (43), with which he was apparently somewhat disappointed. (44) Thus, by 1670, telescopes had not really exceeded the length of Neile's telescope of 1657, and, as Campani had shown, they were by no means necessarily superior to some of the shorter ones.

Perhaps the best overall telescope of the seventeenth century was the 34 foot one made by Campani for the Royal Observatory in Paris, installed in 1672. The objective is still preserved in the Observatoire, and it was examined by Danjon and Couder. Its useful aperture is 108 mm. and its focal distance is 10.85 m. Its curvature is extremely regular, the glass itself contains many parallel lines of the same prominence as the lines in good ordinary glasses of today, and stellar images are round without astigmatism, although certainly affected by chromatic aberration. (45) It was probably coupled with a Huygenian eye piece, giving perhaps magnifications of up to 150. With this telescope, Cassine, almost certainly the best observer in the seventeenth century, made many important discoveries. His observations of the Moon led to the publication of the best Moon-map of the seventeenth century. (46) On this map, Cassini shows details as small as 4 miles in diameter, such as Lyot, a small crater inside Ptolemaeus: quite an improvement over the 7 miles of Grimald.

During the last 1670's and in the 1680's, telescopes grew to very great lengths indeed. Hevelius built one of 150 feet, which could only be erected on the beach near Danzig, and proved to be quite useless. (47) A conventional telescope of such a length was, of course, much too cumbersome and difficult to keep in adjustment. The solution to this problem was supplied by Huygens, who, in 1683, developed the aerial telescope, which was less cumbersome. After this, the Huygens brothers went on to build telescopes of over 200 feet in length. But they never managed to see the fourth and fifth satellites of Saturn (in order of discovery), detected by Cassini in 1684 with Campani telescopes of 70, 90, 100, and 136 feet (48), even when they made some shorter telescopes with larger apertures. (49) It appears that the 136 foot Campani glass mentioned here may identical with the objective preserved in the Istituto di Fisica of the University of Bologna. (50)

Because of the cumbersome nature of these very long telescopes and also because of a shift to positional astronomy, the long telescopes of the seventeenth century went out of use, although they were occasionally pressed into service in the eighteenth century for specific observations. It was not until the reflecting telescope had been greatly improved that astronomers could see some of the very small objects that had been seen by observers such as Cassini. Not until Herschel were new astronomical discoveries made.

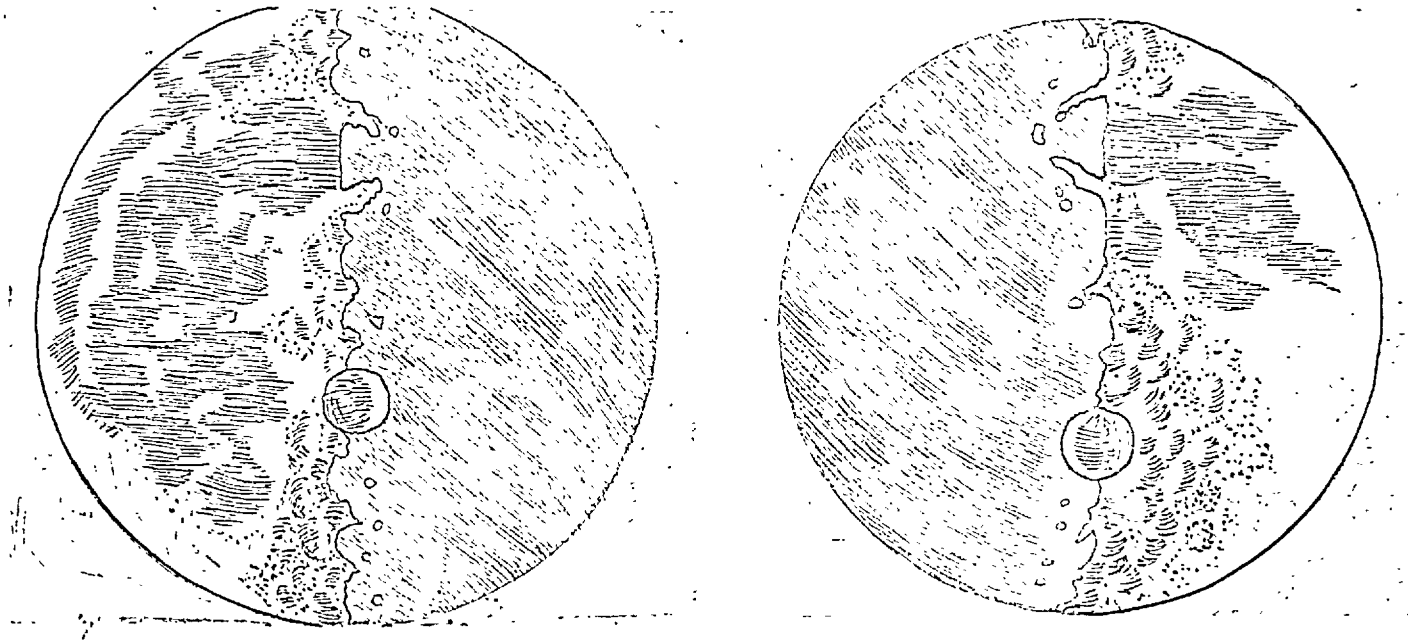


Fig. 1

Galileo's likenesses of the Moon
shown in Sidereus Nuncius

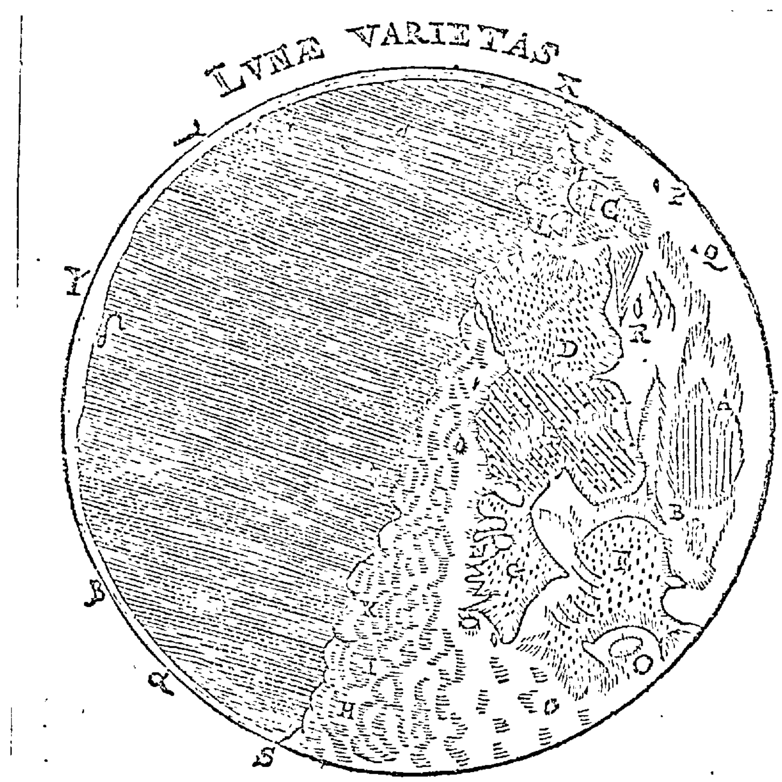


Fig. 2

Scheiner; from Disquisitiones Mathematicae

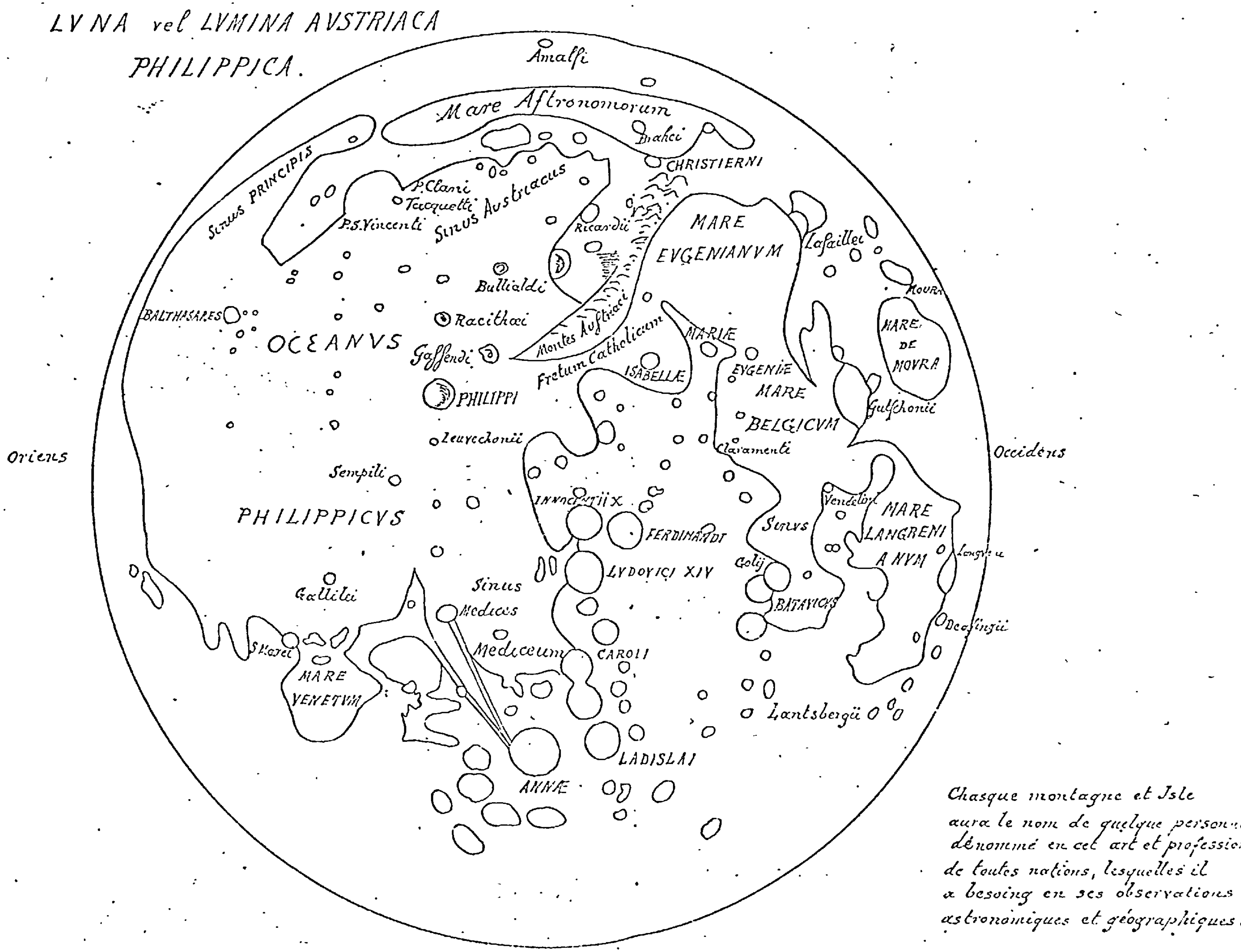


Fig. 3
Langrenus, 1645?

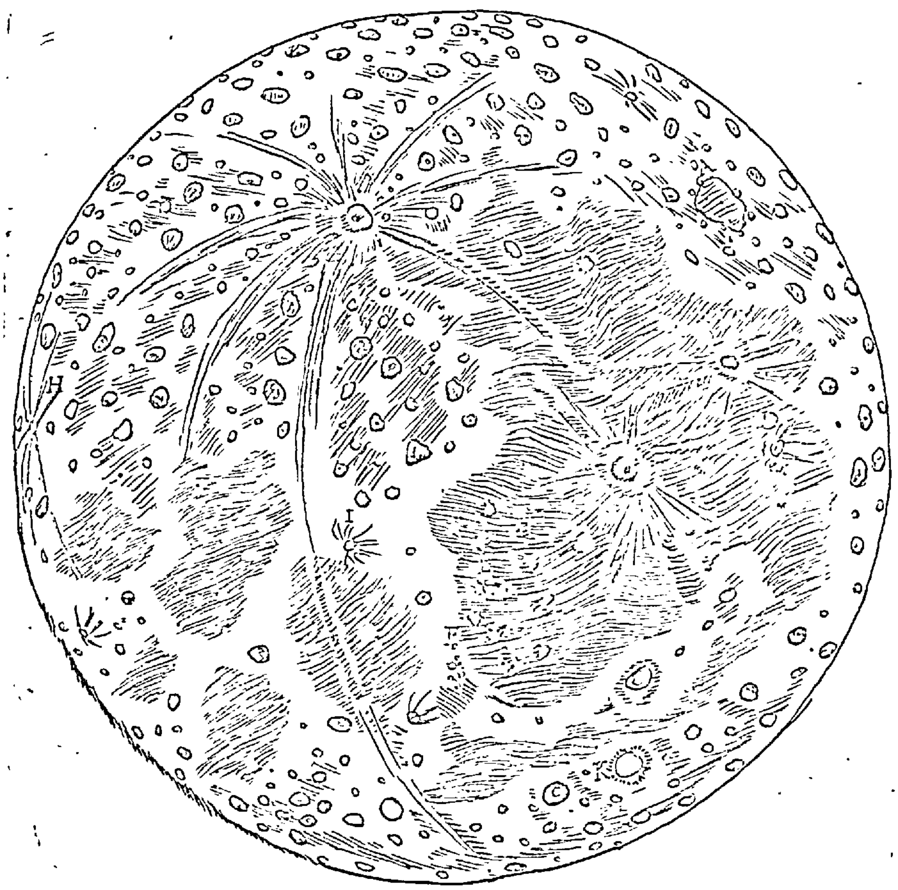


Fig. 4

Fontana; Novae Coelestium Terrestriumque Rerum Observationes, 1646

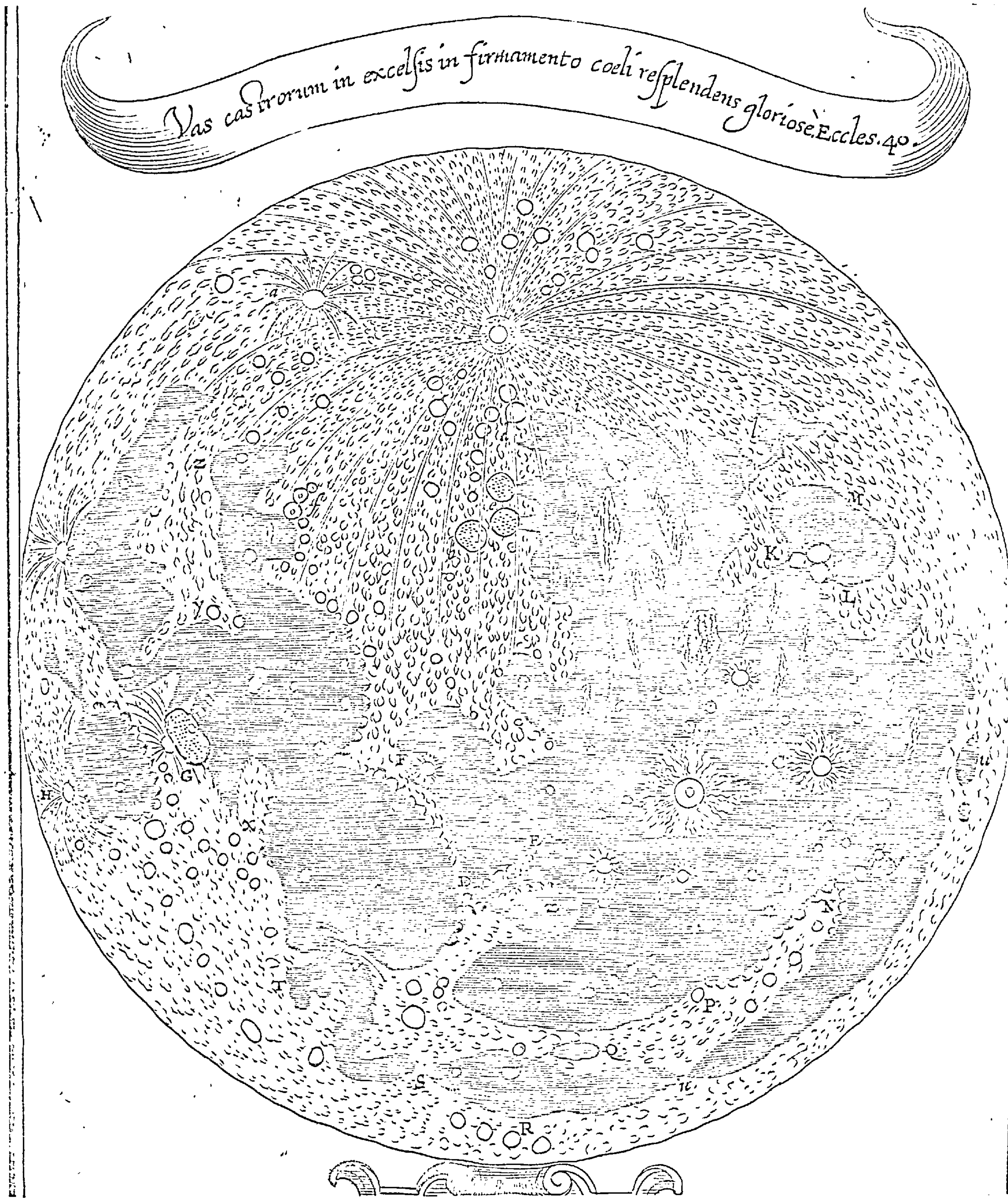


Fig. 5

Rheita; <u>Oculus Enoch et Eliae</u>, 1645

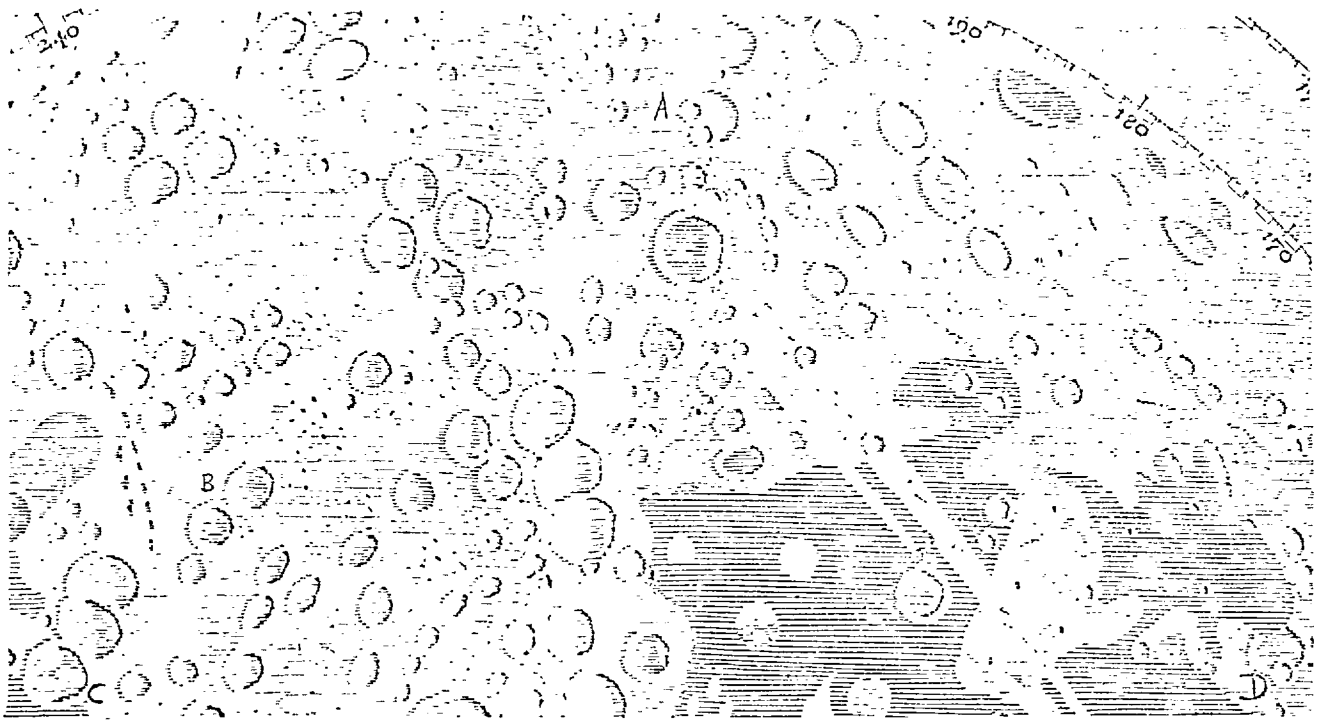


Fig. 6

Hevelius; Selenographia, 1647 (South Pole area)

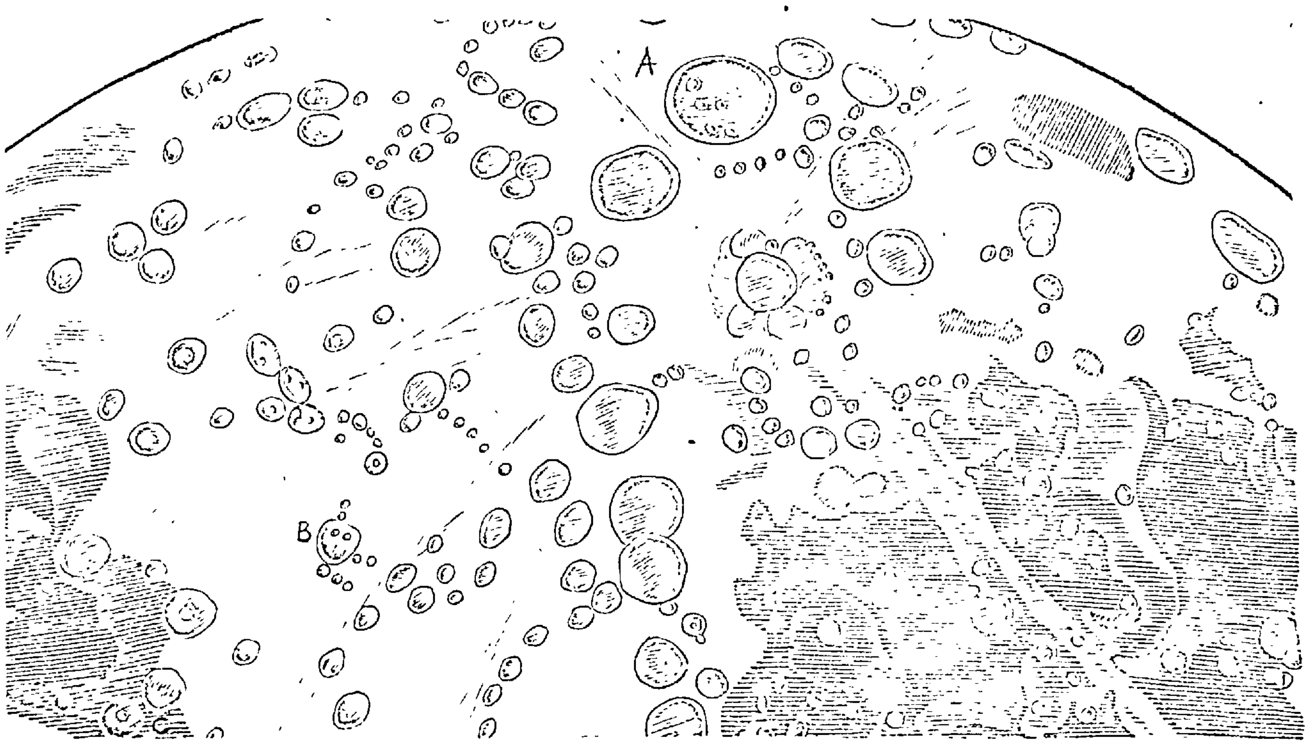


Fig. 7

Riccioli; Almagestum Novum, 1651 (South Pole area)

Note the difference in detail shown in Clavius (A), Sacrobosco (B), Theophilus and Cyrillus (C), and Gassendi (D).

CHAPTER 3

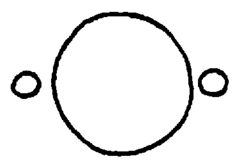
Gathering Information, 1642-1655

As shown in the first chapter, very little progress had been made on the problem of Saturn by 1642. Until that year, the only thing known about Saturn was that sometimes he was seen as a single round body, sometimes as three round bodies, and sometimes oval with dark spots. But at what point of the zodiac these various appearances occurred was only vaguely known. Moreover, not many of the astronomers were aware of the handled appearance, showing dark spots within an oval body. Besides Galileo's mention of it in Il Saggiatore of 1623, which was not widely read even by astronomers in Italy and was entirely unknown abroad, the only reference to this appearance in printed books before 1643 was in Biancani's Sphaera Mundi. (1) It appears that only Hortensius, Gassendi, and Fontana observed this phenomenon during the 1630's, and of these, only Hortensius' observation was in print before 1643. But Hortensius showed no figure and described the appearance in a very confused manner (see below).

Besides this, until 1642, Saturn's appearances were noted as completely separate and discontinuous entities. No idea of gradual changes from one appearance to the next seems to have entered the minds of astronomers in their observations and discussions of Saturn, after Galileo's initial statement as to the constancy of Saturn's figure. This failure to look for and detect gradual changes may have been partly due to the poor quality of telescopes which precluded such observations, in the case of most astronomers. But this was certainly not the case with Galileo. His failure to observe gradual changes was due on one hand to the character of observational astronomy (as will be shown later in this chapter) and on the other hand to the belief in the satellite nature of Saturn's companions.

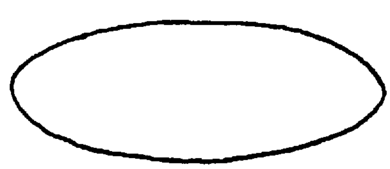
Until well into the 1640's, the analogy between Saturn's companions and those of Jupiter remained the only mode of explanation available. It was also, to some extent, a mode of perception; that is, it actually tended to determine what astronomers saw. Thus, Martinus Hortensius (1605-1639), professor of mathematics in Amsterdam, who was one of the few astronomers to observe Saturn's figure during the 1630's, saw Saturn single and round in 1625, and in 1632, when he saw it handled, he called the anses '... partially invisible companions.' (2) In the next year, 1633, Pierre Gassendi, who had been making telescopic observations since 1618, made his first entry on the shape of Saturn in his notebook:

> At about ten o'clock, when I observed Saturn with the tube through some gaps in the clouds, he was rounded off like a silk-egg, or that from which the silk thread is drawn [i.e. a cocoon]. The longer diameter (directed roughly along the zodiac) appeared hardly smaller than the diameter of Venus; to be sure, either diameter repeated eight or ten times, appeared about equal to the diameter of the opening of the tube. And indeed, on the preceding side of Saturn an ansa or little appendage was seen rather confusedly, but on the side following Saturn the ansa was displayed entirely distinctly; and the whole was seen in this shape and magnitude:



> At some times the body of Saturn appeared round and not with rays embracing the anses on all sides, and at other times it appeared rather confusedly, when the same anses had a symmetrical effusion all around them. (3)

Although this account is rather confusing, it is obvious that the figure shown by Gassendi does not resemble the egg-shape of a cocoon in the least. The ring was approaching its most open position with respect to the Earth, and like Hortensius Gassendi could have seen and drawn an oval figure, but he clearly wanted to see the tri-spherical figure. Indeed, the next year, when he had probably become somewhat used to the oval shape of Saturn, Gassendi drew it just like a 'silk-egg', with no markings whatsoever: (4)



The only other person making observations of Saturn in the 1630's was Francesco Fontana of Naples, whose observations were published in 1646. But the year 1642 saw another edgewise appearance of the ring, and Saturn was therefore again seen solitary. Gassendi, who had made only seven observations of Saturn before that year, made the following entry on 10 August 1642:

> When I had directed my telescope to Saturn, I observed something unexpected, namely Saturn without his anses, which I had not yet seen. I cannot fix the time since when he could have been like that and therefore I regret very much that I did not observe more often. In fact, it has been 32 months since I made any observations. I remember also that the great Mersenne, when he asked for my telescope ten months ago, said, upon returning it, that he could not discern any handles, but I was sure that it was due to the weakness of his vision. So I did not observe Saturn to see if he had anses. I have immediately advised my friends Bouillau, Vaselius, and others of this matter, to observe for themselves this noteworthy thing ... (5)

It is from this event that the serious study of Saturn and its anses started. There are no fewer than a dozen entries in Gassendi's notebook for the year 1642 and a further 32 entries from 1643 until his death in 1655. Bouillau, who had been making astronomical observations since at least 1622 (6), recorded his first observation of the shape of Saturn in 1642 (7), and Hevelius' study of the planet also dates from that year. (8) Riccioli and Grimaldi made their first observations of Saturn's shape in 1643 (9), Fontana carried on his observations, and numerous other observations, made by obscure observers and communicated to astronomers by letter, found their way into the printed material beginning in the 1640's.

Looking back to the period between Galileo's initial discoveries and the decade of the 1640's, it should be noted that neglect of Saturn was not an isolated case. Examination of other planets did not start until the 1630's. With the exception of the sketches in Il Saggiatore, no sketches of the planets are found in printed books until the 1640's. Clearly therefore, we are dealing with a general phenomenon. Examination of the planets and even the Moon was not a matter of great interest to astronomers before about 1640.

To say that this was due to poor quality of telescopes is perhaps partially true, but it misses the most important point. As shown in the previous chapter, Galileo and Scheiner depicted only a few of the lunar details out of the many that were visible to them. They were not interested in making Moon-maps, because Moon-maps had no bearing on the Great Debate. Moon-maps were made, starting with Langrenus and Gassendi for the purpose of more accurate determination of the times, progress, and durations of lunar eclipses, which would in turn make possible a more accurate determination of terrestrial longitudes. The periods of the satellites of Jupiter were of interest for similar reasons: occultation tables could be an aid to determining longitude at sea. But the mere observation of planets as such had no immediate use or significance, nor was it likely to answer any interesting questions. Why should anyone therefore be interested in recording the shapes of Saturn? Only during the 1640's did regular observations of the planets, and the stars, become a respectable occupation for astronomers, and a general growing interest in the appearances of the planets is reflected in the astronomical literature. Indeed, no book on astronomy was complete henceforth without some figures and discussion of the appearances of planets. Observations of the appearances of Saturn are a part of this more general movement.

But the mere gathering of information on Saturn did not immediately shed light on the problem. Indeed, it rather created more confusion. This was because telescopes were still very bad, and also because not all observers were as talented as Galileo in extracting the appearance of a celestial object from the confused image presented to the eye by these telescopes.. In the entry of 10 August 1642, Gassendi, who was sure that the central body of Saturn should be round, nevertheless stated:

> When I directed my attention to the circumference, I saw it not perfectly round as with Jupiter, since indeed it appeared rather sometimes of a pentagonal form, but with curved sides and blunt angles, so that it closely approached a circle. (10)

Furthermore, Gassendi continued to see the western globe of Saturn somewhat larger than the eastern one. It is difficult to determine whether this was due to Gassendi himself, or to his telescope. At any rate, the telescope he used until his death had been given to him by Galileo in 1636 or 1637 (11) and was therefore one of the better telescopes in the hands of an astronomer until well into the 1640's.

Gassendi's observations were not published until after his death, in his Opera Omnia (Lyons, 1658) (see figs. 20-40). However, astronomers were well aware of them during his lifetime, partly because of his close contact with Bouillau and partly because he mentioned and discussed them in several of his publications during the 1640's. He first wrote on Saturn in the curious work by Rheita, entitled Novem Stellae circa Jovem circa Saturnum sex, circa Martem nonnullae (Louvain, 1643). As indicated in the title, Rheita claimed to have discovered at least nine new satellites of Jupiter, Mars, and Saturn. Gassendi, whose judgment was included in the book (an indication of

Gassendi's stature as an astronomer in the eyes of his contemporaries, which is so often ignored by historians of science (12)) was of the opinion that Rheita had mistaken fixed stars for satellites. (13) Johannes Caramuel Lobkowitz, whose judgment was also recorded, supported Rheita's opinion. (14) In his essay, Gassendi discusses his observations of Saturn of 1642, thus bringing these important observations to the attention of astronomers in general.

The same year also saw the publication of Matthias Hirzgarter's Detectio Dioptrica (Frankfurt, 1643), a book, written in German, devoted solely to observational astronomy, or, as the title announces, to the '... wonderful, but actual, real, and natural figure and bodily shape and appearance of the seven planets and some fixed stars'. In this book, Hirzgarter brings the astronomical observations of Fontana to the attention of the German-reading scientific community, although Fontana is merely referred to as an 'Neapolitan nobleman'. (15) The handled appearance, shown here for the first time in detail (see fig. 1) (16), was a landmark in the study of Saturn. It was the first such figure since the figure in Il Saggiatore of 1623, which was still unknown.

In 1645 Rheita published another book, his Oculus Enoch et Eliae, which was generally referred to by his contemporaries by its subtitle Radius sidereo-mystico. The description of the new form of telescope has been dealt with in the previous chapter. It was because of this description that the book was widely read. But it also contained a wide-ranging discussion of astronomical topics, including that of Saturn. Although Rheita did not discuss or show his observations of this planet, if he made any, he did put forward a 'theory' of its structure and appearance. (17) This theory will be discussed below.

The amount of information available was greatly increased in the following year by the publication of Fontana's Novae Coelestium Terrestriumque Rerum Observationes. Fontana showed no fewer than seven different appearances of Saturn, observed between 1630 and 1645. (18) As can be seen from these observations, (figs. 2-8) they raised as many problems as they solved, but they supplied valuable information about a period during which few observations were made, and therefore Fontana was always referred to in subsequent discussions of Saturn.

Further information was supplied in Athanasius Kircher's Ars Magna Lucis et Umbrae (Rome, 1646) (fig. 9) (19); Hevelius' Selenographia (Danzig, 1647) (figs. 10-12) (20); Divini's single sheet of observations published in 1649 to advertise his telescopes (fig. 16)(21);

and Gassendi's *Epicuri Philosophia* (Lyons, 1649).. This last source contained no figures, but gave a detailed description of all his observations up to 1649. (22) The last important publication containing information on the shape of Saturn was Riccioli's *Almagestum Novum* of 1651 (figs. 13-15). Riccioli gave a complete review of all the observations of Saturn known to him, going back to 1610. This included several observations made by himself and his co-worker Grimaldi, as well as observations made by several other Italian observers. (23) There was also, of course, an exchange of observations in letters, some of which have been preserved (figs. 17-19).

Comparing all these figures, it can be seen that it was not an easy task to formulate a theory that would account for all of them. There were some palpable errors, either due to poor telescopes or bad observation. For instance, Gassendi consistently observed the right globe of the tri-spherical appearance to be smaller and closer to the middle globe than the left one. Although he had reason to believe that the irregular appearance of the solitary middle body, as he saw it in 1642, was not the true appearance, he had no way of knowing that the asymmetry of the configuration of the lateral globes was illusory. He marked down what he saw, or thought he saw, to the best of his ability. Hevelius' observations, showing the central body of Saturn elliptical in the handled appearance was an optical illusion, but again, Hevelius could not be sure that other observations were more correct than his. The bright points at the extremities of the handles, seen by Riccioli and Fontana, were due to the concentration of light in these areas, aggravated by the aberration of the lenses. Since no one knew what he was supposed to see, many such phenomena found their way into the literature. Not all astronomers by any means had that almost instinctive quality that allows one to search out the 'true' phenomena, in the absence of the knowledge of what causes the phenomena.

It is therefore not surprising that astronomers did not immediately put forward theories based on these observations. In some way, they all continued to adhere to the satellite model, as the best way of explaining the phenomena. Gassendi ventured no opinion at all in Rheita's *Novem Stellae* of 1643 (24) and neither did Hirzgarter in his *Detectio Dioptrica* of the same year.(25) Rheita ignored recent observations in his *Oculus Enoch et Eliae* and explained Saturn's appearances in terms of satellites. He believed that all planets revolved about their own axes, analogously with the Earth, and carried this analogy to extremes by supposing that since the Earth does so 365 times during one revolution about the Sun, the other planets must do likewise. Therefore Saturn rotates on its own axis once in every 29 days, 10 hours, and 1 minute. Furthermore, there is a definite relationship between the sizes of the planets. Thus, the central body of Saturn is equal in size to Jupiter, while the lateral globes are each equal to Mars. This, he states, he determined by very frequent observations. (26) Since Saturn is so far removed from the Sun, it cannot possibly receive all the light we see emanating from it from the Sun. Therefore Rheita supposes that Saturn receives most of its light from the lateral globes, which each have one light and one dark hemisphere, and move around Saturn without turning on their own axes (with respect to the fixed stars). This causes seasons on Saturn. This theory, fanciful as it was, did little to explain the appearances of Saturn: it posed more problems than it solved, including that of the period of the satellites, while it completely ignored the handled appearance. But it does illustrate the attraction of the satellite model, which was at that time still the only model available.

Fontana keeps referring to the handles as collateral stars (27), and in his later observations he states the opinion that the lateral stars are attached to the central body by these handles. (28) He identifies the appearances seen by him in 1644 and 1645 (figs. 6-8) with the little balls seen by him in 1630 (fig. 2), but the mechanism whereby these lateral bodies change their shape is left for future observers to decide. (29)

In his Selenographia, Hevelius first declares that he is '... entirely of the opinion that Saturn does not always appear oblong and made pointed by two small globes, but that occasionally these globes hide behind Saturn, like two stars that go around Saturn ...' (30), but after discussing the handled appearance observed by himself and Fontana in the preceding years, he states:

> What the bodies of these two arms of Saturn may in fact be: whether they are always visible in a shape like the waxing and waning Moon, and at the same distance from Saturn and with the same width, or whether they are partly round bodies, which at definite times are increased and decreased and vary their motion and sometimes approach Saturn more closely and at other times are even completely occulted, I have not yet been able to determine at this time. This is because the planet completes his period very slowly, and moreover needs diligent observation for many years through the above mentioned longer tubes, equipped with ingeniously polished lenses. Therefore I withhold my judgment and reserve this matter for another time. (31)

By 1649, Gassendi, who had also stated in his Institutio Astronomica of 1645 that more observations were needed, was still not ready to venture an opinion:

> Something could be added about the occultation of the handles of Saturn. But in fact, with what periods and in what sequence of transformation those occur, is a matter to be determined by posterity when they will have been more fully investigated. (32)

Riccioli's views are no different. Like Hevelius, he is inclined to a satellite model, but he deems that not very satisfactory. The dark interstices could be explained by Saturn's shadows, if the lateral globes move in a path behind Saturn, but this would necessitate Saturn's being much larger than the Sun, and Riccioli knew this to be not the case. He then mentions some other possibilities: perhaps Saturn illuminates the lateral bodies from an elevated mountainous source on its rear surface, which would cause incomplete illumination of the lateral bodies; perhaps there are dark caverns on Saturn, which by the various librations of his body cause the different appearances; perhaps Saturn is itself illuminated by the lateral bodies; or perhaps there are spots on its surface, as on the surface of the Moon, that are less suitable for the reflection of light than is the rest of the surface. Riccioli continues:

> ... or it is some other phenomenon that will be revealed to us or to our successors after much experience. In the meantime, nothing occurs to me that satisfies entirely, especially since not only Fontana, in 1644 and 1645, but some of the brothers of our society [i.e. the Jesuit society], and especially Father Mattheus Taverna, have testified that through the telescope Saturn appeared to them as shown at the end of this scholium [see fig. 15] ... (33)

From this it is clear that the handled appearance that had become common knowledge in the 1640's was still treated separately from the basic model of occultation of satellites. Obviously, the handled appearance could not be, and was not ignored, but the tri-spherical appearance was still the primary appearance while the handled appearance was still an anomaly in the satellite model.

Perhaps the most interesting speculation came from the Dutch, or rather Frisian astronomer Johannes Phocylides Holwarda (1618-1651), professor at the University of Franeker. In his Philosophia Naturalis seu Physica Vetus-nova (Franeker, 1651), a virulent anti-Aristotelian tract, he suggested that the main body of Saturn was oblong and oval, that is, egg-shaped, and that it is seen round when the points are turned towards the Earth. This egg-shaped central body came, of course, from Selenographia. The central body was accompanied on either side by the 'arms' seen by Hevelius and Fontana, which could be either handle-shaped bodies, or round bodies. The arms are 'increased and decreased at definite times' and are sometimes occulted.' (34) This last supposition was taken more or less directly from Selenographia. (35) Holwarda further suggested that since the appearance of the handles is so different from the appearance of the little globes, perhaps they are different bodies. Finally, he stated that Saturn has a compound motion, that is, it not only moves in its orbit around the Sun, but it also turns on its own axis. Except for the obvious difficulty in reconciling the handles or arms with the round globes, Holwarda presents here the germ of a theory. It was turned into a full-fledged theory by Hevelius, a few years later.

Thus, by 1651 astronomers still subscribed to a satellite model, and only in Holwarda's work is there the suggestion that the handled appearance was perhaps the primary appearance. The major difficulty remained the impossibility of fixing the period of the satellites, since the collateral globes remained motionless for a long time and then were suddenly occulted. The handled appearance, which had been observed for six years by 1651, presented an insurmountable obstacle to the satellite model, and in fact caused its abandonment by the middle 1650's. But if the handled appearance was an anomaly in the satellite model, then equally the tri-spherical appearance was an anomaly in any model which took the handled appearance as the primary appearance.

The continuous observations after 1642 had produced an extensive and rather sophisticated body of knowledge about Saturn's appearances by the middle 1650's. Whereas at the beginning of the 1640's discussions had been of a very qualitative nature because of the lack of information, presenting the appearances as it were separate, discontinuous and as almost mutually exclusive entities (e.g. in 1612 Saturn appeared round, in 1630 tri-spherical, and in 1636 handled), by 1650 the appearances were thought of rather in terms of a continuous progression, each appearance being connected with the one preceding it. Detailed observations had shown that the faithful observer could detect changes from one year to the next. Contrast the following passage from Gassendi's Epicuri Philosophia of 1649 with Galileo's letter to Castelli of 1640 (p. 21) and with Gassendi's own entry in his notebook in 1642 (p. 45). After discussing the round appearance of 1642, Gassendi continues:

> Near the end of the following May 1643 when he had risen from the Sun, he had indeed become triple-bodied [fig. 27], that is, to the middle or larger globe were attached two little globes of the same brightness. This shape persisted for the rest of the year and right up to March of the following year, 1644, when Saturn again set in the Sun, except that the little globes appeared gradually separate. In the following June, the separation had not only become somewhat more sensible, but also the roundness of the little globes adhering to the larger globe seemed somewhat flattened, and indeed turned into slight cavities [figs. 28-30]. In fact, at their opposing ends they seemed to be raised into dull points. From this, in turn, the cavities similarly brought forth points, so that near the end of the year 1645 these points touched the middle globe on either side with a dark crescented interstice between them [figs. 31, 32]. The little globes had thus been transformed into nearly hyperbolic crescents and because of their cohesion to the middle globe they represented the appearance of an egg [fig. 33]; and because of the handled condition, those interstices appeared like openings. In the year 1646 those dark interstices began to be blotted out near the points, and the brightness of the handles started to mingle with the brightness of the middle globe, which therefore began to appear somewhat sensibly distinct from the same. In the year 1647, the same form persisted, except that the brightness seemed more confused and the interstices became not only more round on account of the bluntness of the points, but also even somewhat more enlarged on either side toward the tips [of the handles] [fig. 34]. In the year 1648, furthermore, in the month of April, near the heliacal setting, the same form still remained ... (36)

This almost continuous record of minute changes is far superior to Galileo's description of 1640 and Gassendi's own description of 1642.

Hevelius wrote to Bouillau in 1650, remarking in a similar manner on the changes in the appearance of Saturn, and he concluded:

> I am almost convinced that Saturn as it were changes his appearance partly with the progress of time. However, by how much and in what period of time this is done, will become more and more evident to the vigilant, with time. (37)

There is some contrast between this statement and Hevelius' statement in Selenographia (p. 49). Whereas in Selenographia Hevelius had conveyed the feeling that the task had just been started, in this letter, three years later, he clearly indicates that it is at least partially finished. This is justified in view of the great increase in available information.

In the early 1650's Saturn's handles started to shrink and the sequence of the changes between 1642 and 1649 was reversed. By late

1653 Saturn was again triple bodied and by 1654 both lateral globes were again spherical. Finally, late in 1655, Saturn was again observed to be single and round. Now astronomers had detailed and complete information on a whole cycle of the phases, upon which theories could be built. The information still presented great difficulties, but these difficulties did not prevent theories from being formulated.

Before proceeding to these theories, some remarks are in order on the interaotion between the observer and his telescope. If it is maintained that progress in observational astronomy in general, and on the problem of Saturn in particular, was solely due to the improvement of the telescope, serious difficulties arise. One could easily say that, judging by his sketch of 1616 (p. 19), Galileo must have discovered the ring. Indeed, Bianchi was of the opinion that Galileo had already divined the true nature of Saturn's appendages in 1612, when he wrote about the solitary appearance in his third letter on sunspots. (38) But Antonio Favaro's evaluation of Galileo's observation of 1616 is a much better case in point. Favaro wrote:

> This figure ... both by itself and by virtue of the statement that accompanies it therefore represents [porge] an appearance close to the one revealed [rivelate] by Huygens forty years later, which those appearances seen [vedute] and described by Scheiner, Riccioli, Hevelius, Gassendi, Divini, Bouillau, Fontana, and Biancano were not, because in none of these is the ring, to which the name of Huygens is deservedly linked, as faithfully drawn as it is in the figure drawn by Galileo in August of the year 1616. (39)

The difficulties raised by the words 'seen', 'revealed' and 'represents' are obvious. Clearly, Huygens' 'revealing' was somehow different from Galileo's 'representing' and the appearances 'seen' by Riccioli and others were not those 'seen' by Huygens. To say that since everyone accurately represented what the telescope presented to their eyes, and that since Riccioli and others did not draw a ring their telescopes did not show a ring and were therefore inferior (which is not what Favaro says here!), is to miss the point. Seeing a ring around Saturn is achieved by a spontaneous process of mental patterning of certain light inputs which impinge on the retina, after passing through the combined optical system formed by the lenses of the telescope and the lens of the eye. On the subject of the well known picture that can be seen either as an old woman or as a pretty girl, N. R. Hanson writes:

> All normal retinas 'take' the same picture; and our sense-datum pictures must be the same, for even if you see an old lady and I a young lady, the pictures we draw of what we see may turn out to be indistinguishable. (40)

Similarly, the image that could be seen either as a central body with two appendages or as a central body surrounded by a ring was 'seen' by Galileo, Gassendi, and Divini as the former and by Huygens as the latter. But in many cases their representations were almost indistinguishable.

But it must not be forgotten that the figures discussed here were only a few among the widely varying figures of Saturn that had been observed. How were astronomers to know that this particular figure was somehow more 'true', or representative of the 'real' appearance of Saturn? These words only have meaning within the framework of a theory or model, and since the satellite model continued to supply the framework within which these words had any meaning, the handled appearance, no matter how closely contemporary representations of it may resemble a ring to us, continued to occupy an anomalous position until the satellite model had been rejected.

A further argument against the notion that the improvement of the telescope was solely responsible for the progress on this particular problem can be derived from a comparison of the observations of Gassendi and Galileo. Between 1610 and 1616, in the period of his greatest interest in Saturn, Galileo had detected no gradual changes in the planet's appearance at all. One period of Saturn later, between 1642 and 1646, Gassendi did detect gradual changes, and he detected these gradual changes with a telescope given to him by Galileo. Although it can be argued that the telescopes made by Galileo in the 1630's were better than those he made before 1616, which is probably true to some extent, Galileo <u>never</u> detected gradual changes, as evidenced by his letter to Castelli. Furthermore, he saw Saturn perfectly round and solitary in 1612, whereas one period later, presumably with a better telescope, Gassendi remarked that the solitary central globe appeared of an irregular shape to him. If observations were merely a matter of telescopes, the reverse would more likely have been true. Clearly, Galileo and Gassendi were looking for different things. The type of changes that Galileo looked for in the initial stages were positional changes of the lateral globes, like the changes in position of Jupiter's satellites. These changes in position of Jupiter's satellites occur daily. Thus, after a relatively short time, Galileo could conclude that there were no such changes in the case of Saturn's lateral globes and thereafter he observed Saturn only occasionally until he suddenly noticed that the lateral globes had disappeared, and later he also suddenly noticed the handled appearance. But Gassendi's observations took on a different form after 1642 from merely verifying a mental image occasionally. As he stated in 1642, his program was to find out the changes and how and when they occurred, by means of more frequent observations. Therefore Gassendi was approaching the problem with a different attitude and consequently he was able to see the changes he was looking for.

It can be argued that a good observer cannot fail to see changes in appearance which his telescope presents to him, even if he is not looking for them. This is perhaps true for twentieth century telescopes and observers, although we can only judge by our successes, as our failures are unknown to us. But it is emphatically not true for seventeenth century observers and their telescopes. From the previous chapter it should be clear that telescopes left a great deal to be desired. It took great effort on the part of the observer to suppress all the errors and known illusions caused by his telescope, not to mention unknown illusions, and one can readily see the role played by selective interpretation on the part of the observer.

Thus, what the observer sees depends very much on what he is looking for. There are numerous examples of this in the history of astronomy, as in the history of science in general. They include the discoveries of phenomena that were verified by others, such as Huygens' discovery of Titan and Cassini's discovery of the rotation of Mars and Jupiter, as well as phenomena which have not subsequently been verified, such as Hevelius' observation of the elliptical shape of Saturn's central body and Schiaparelli's observations of double canals on Mars. Clearly therefore, if astronomers had not been particularly interested in the problem of Saturn, the telescope would have had to be improved a great deal more before anyone could have discovered accidentally that Saturn is surrounded by a ring. As it was, the problem of Saturn was solved not directly by observation, but rather by reasoning based on certain important observations.

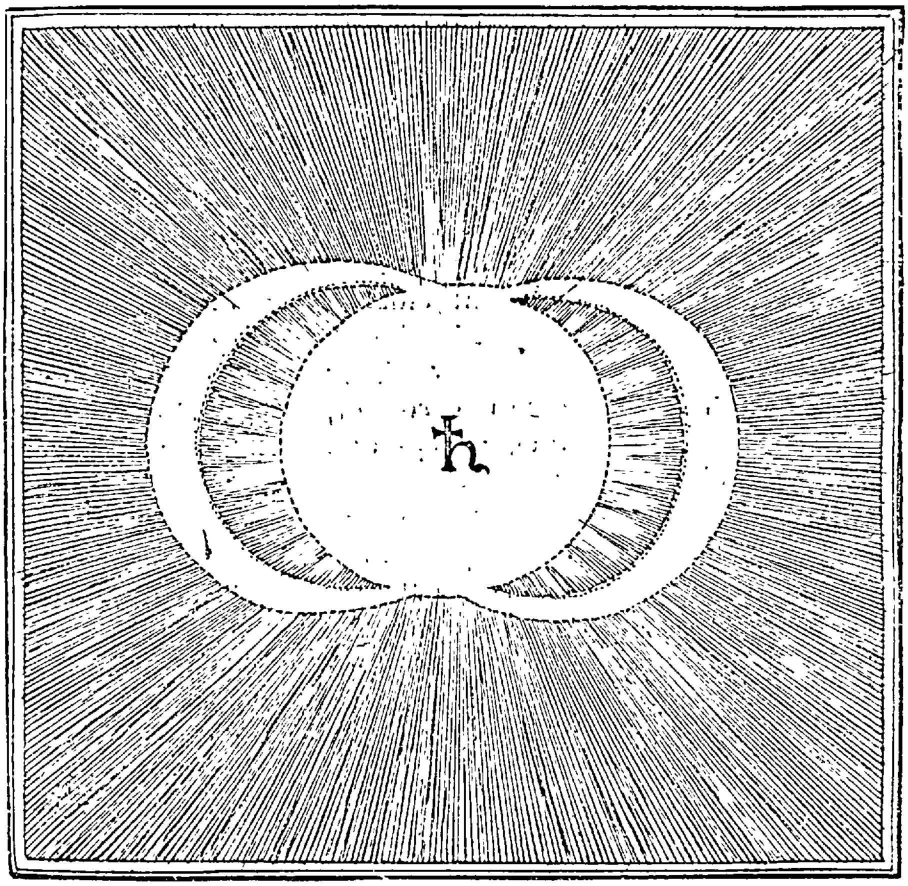


Fig. 1

Hirzgarter; Detectio Dioptrica, 1643.
Based on an observation by Fontana

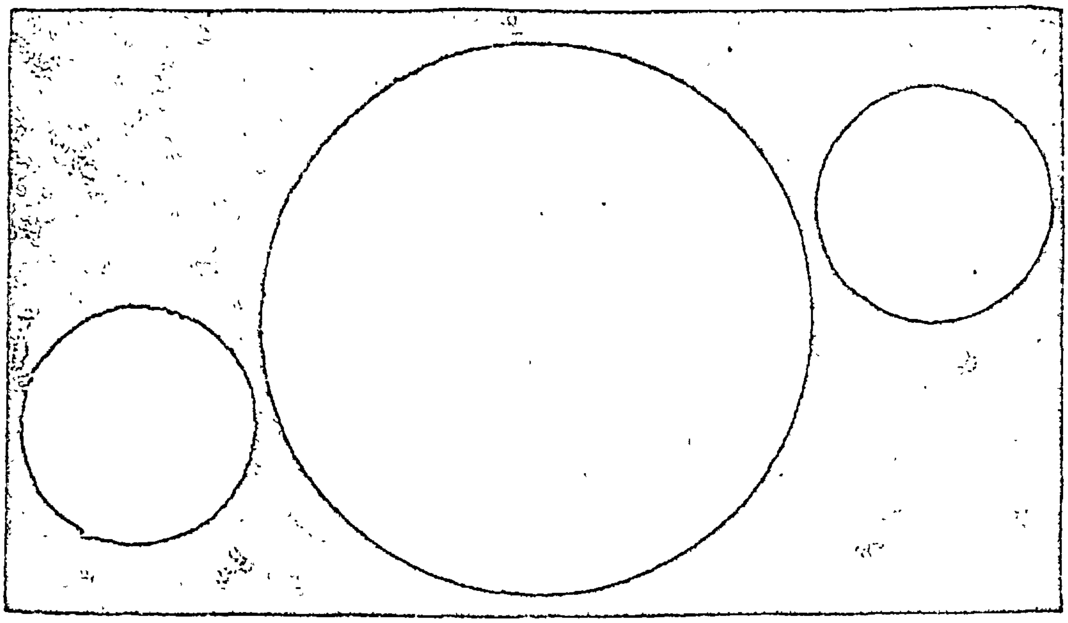


Fig. 2

Fontana; Novae Coelestium Terrestriumque Rerum Observationes, 1646.
Observation of 1630

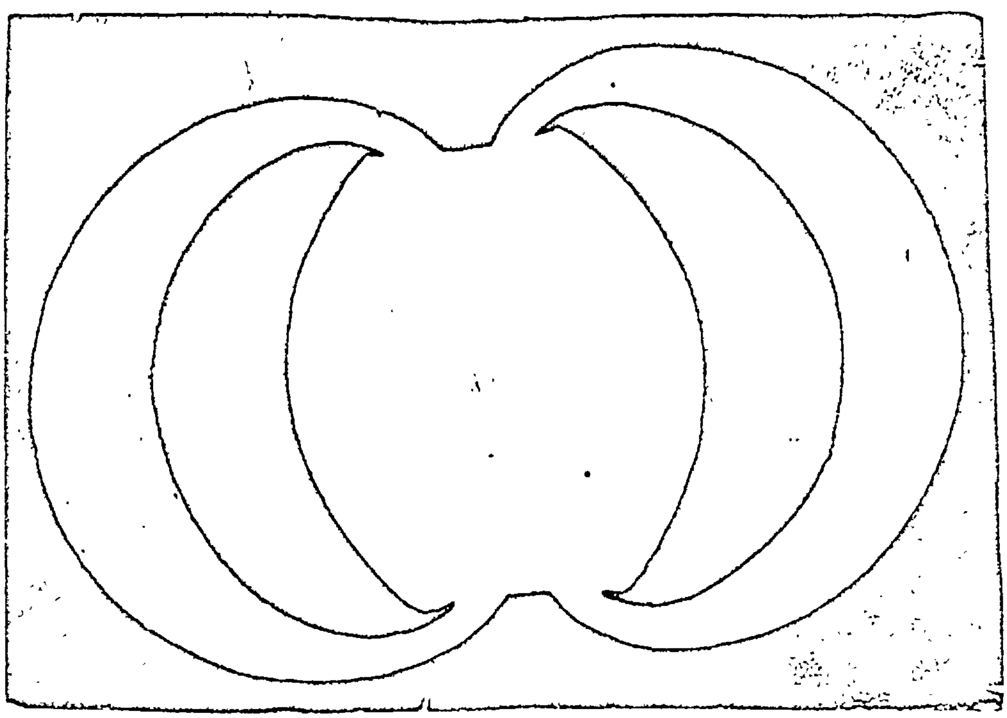


Fig. 3

Ibid., Observation of 1633

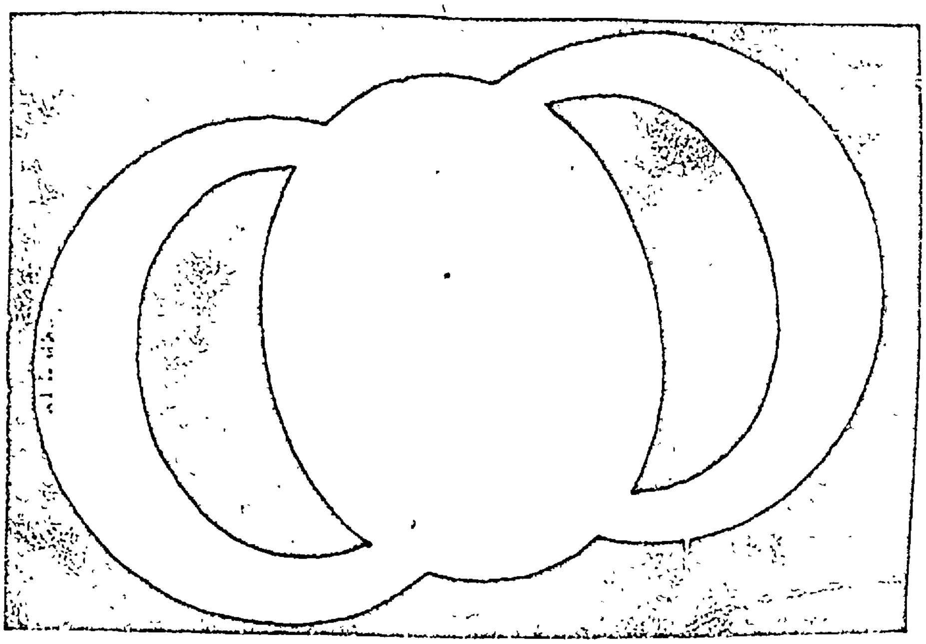


Fig. 4
Fontana; observation of 1634

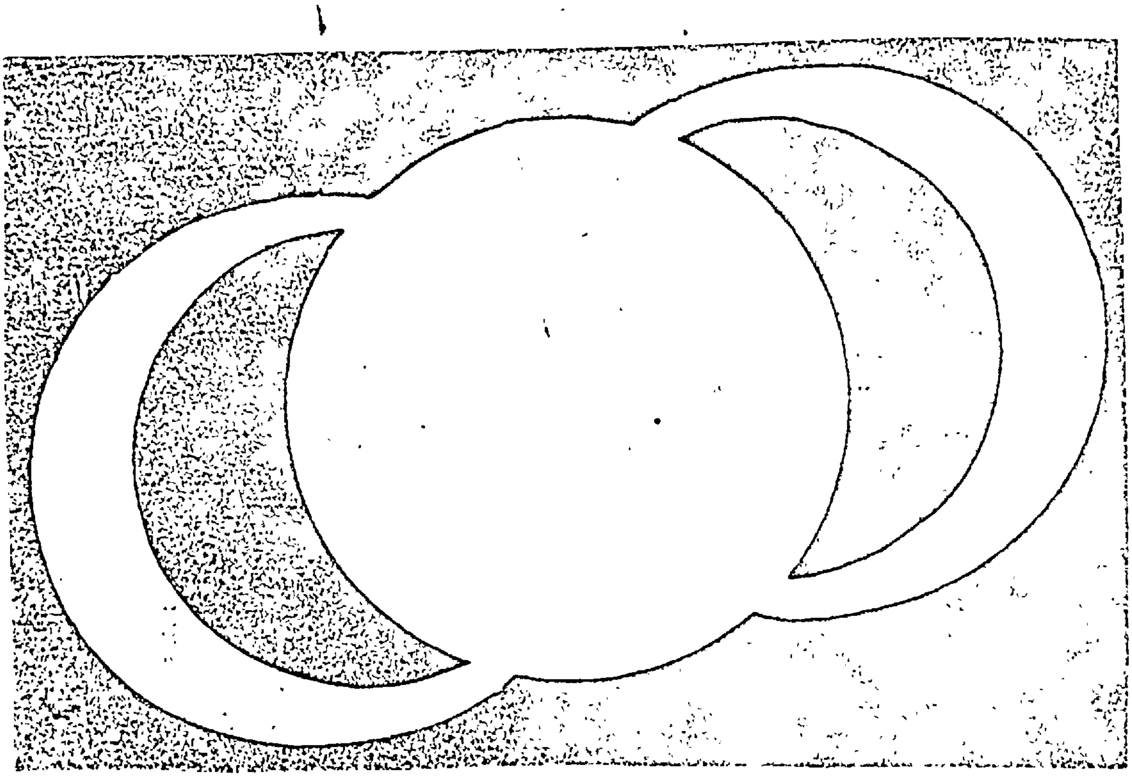


Fig. 5
Fontana; observation of 1636

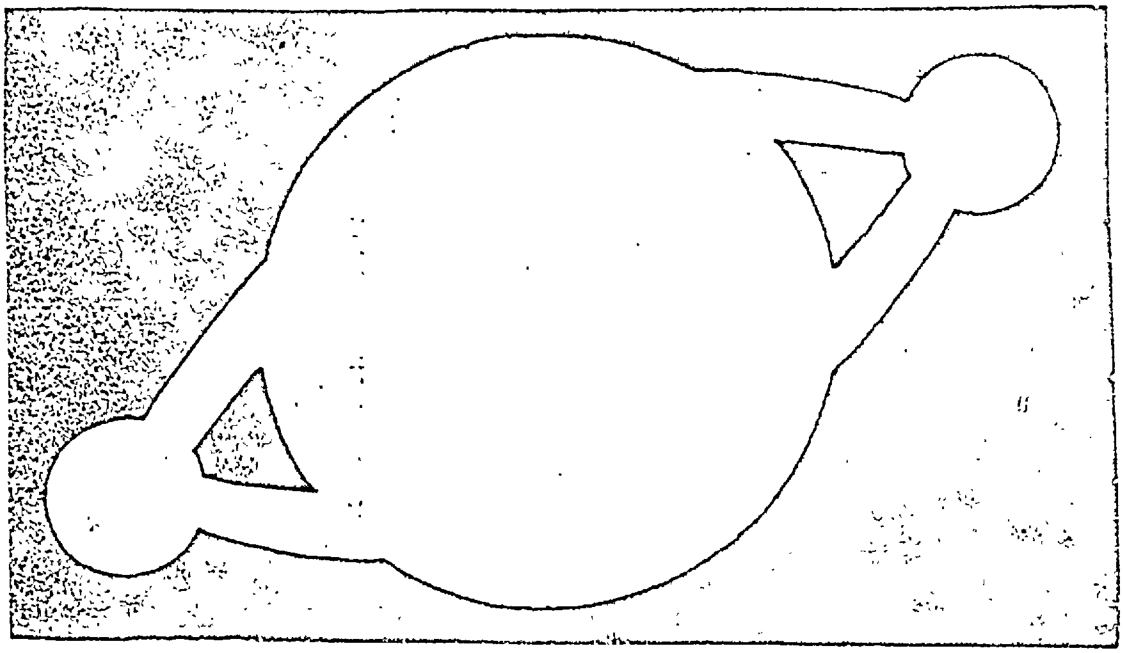


Fig. 6

Fontana; observation of 1644

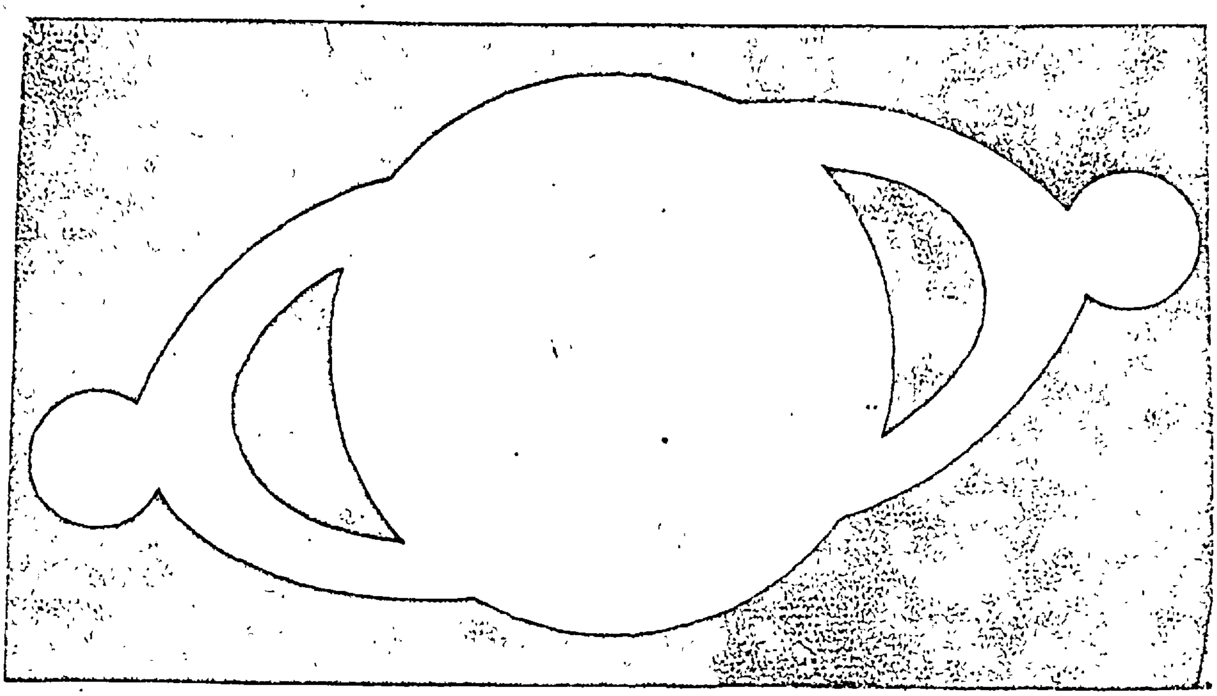


Fig. 7

Fontana; observation of 1645

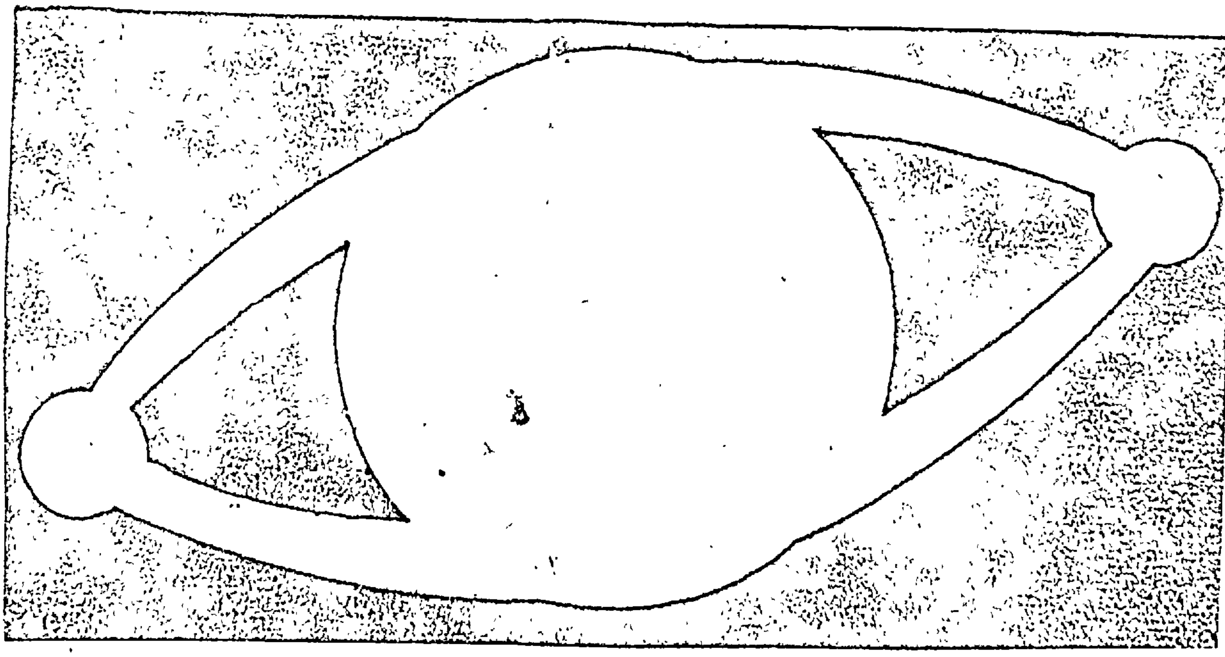


Fig. 8

Fontana; observation of 1645

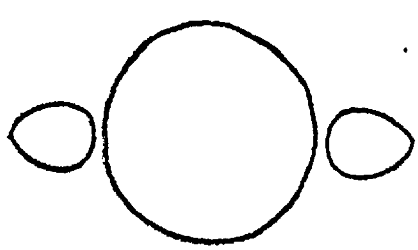


Fig. 9

Kircher; *De Arte Magna Lucis et Umbrae*, 1646

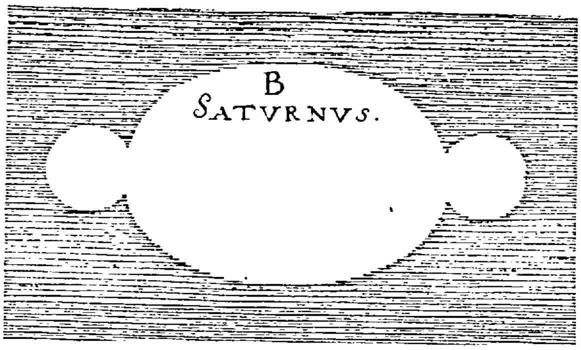


Fig. 10

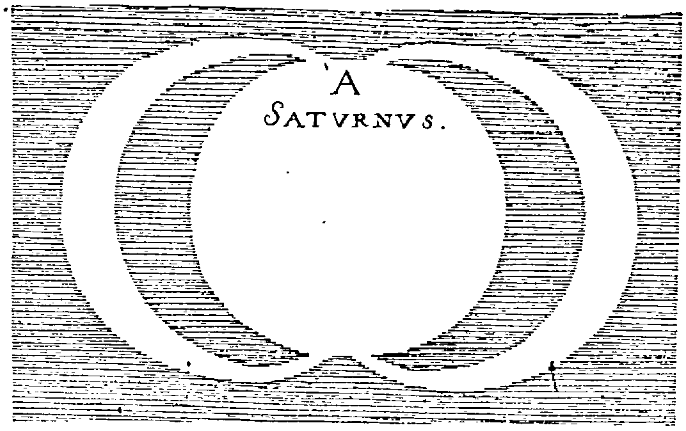


Fig. 11

Fig. 12

Hevelius; <u>Selenographia</u>, 1647

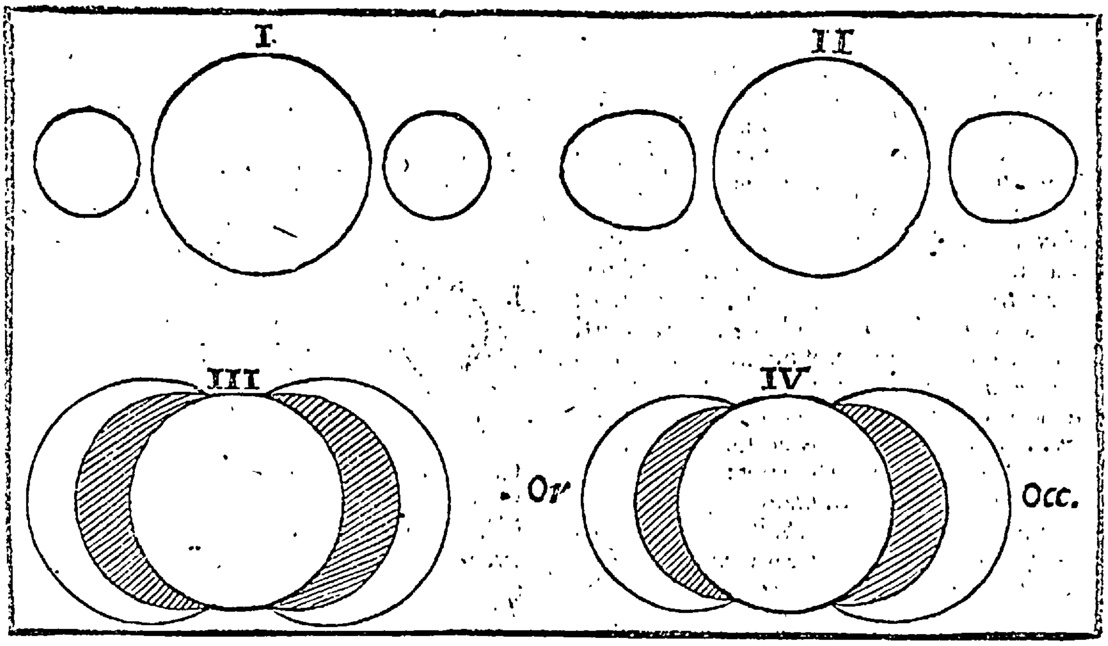


Fig. 13

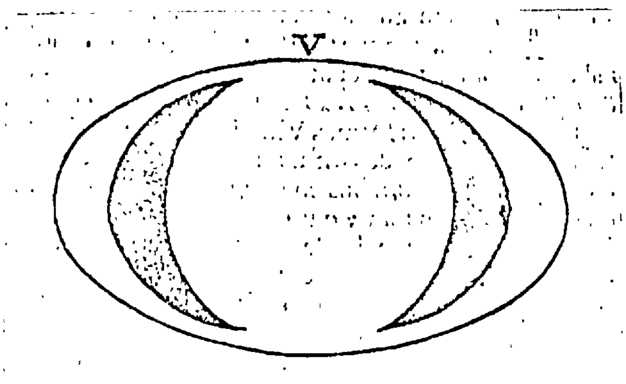


Fig. 14

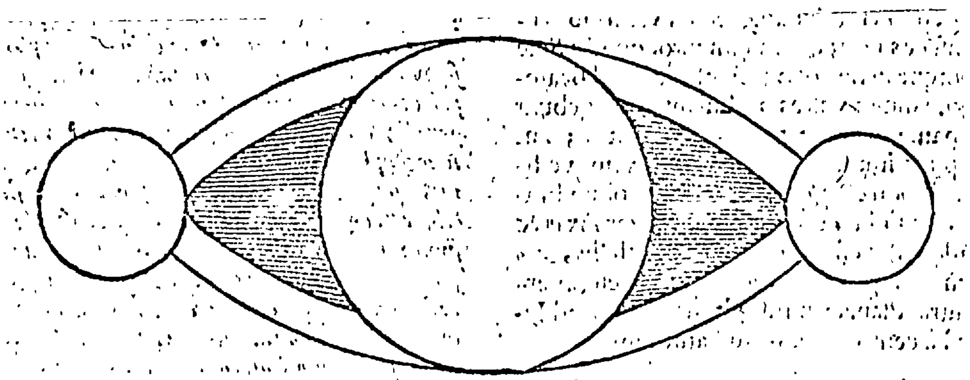


Fig. 15

Riccioli; Almagestum Novum, 1651

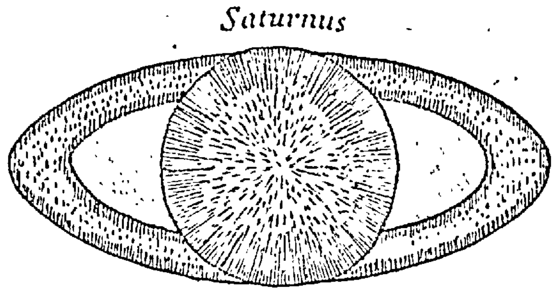


Fig. 16

Divini; observations from 1646 to 1648 (see n. 21)

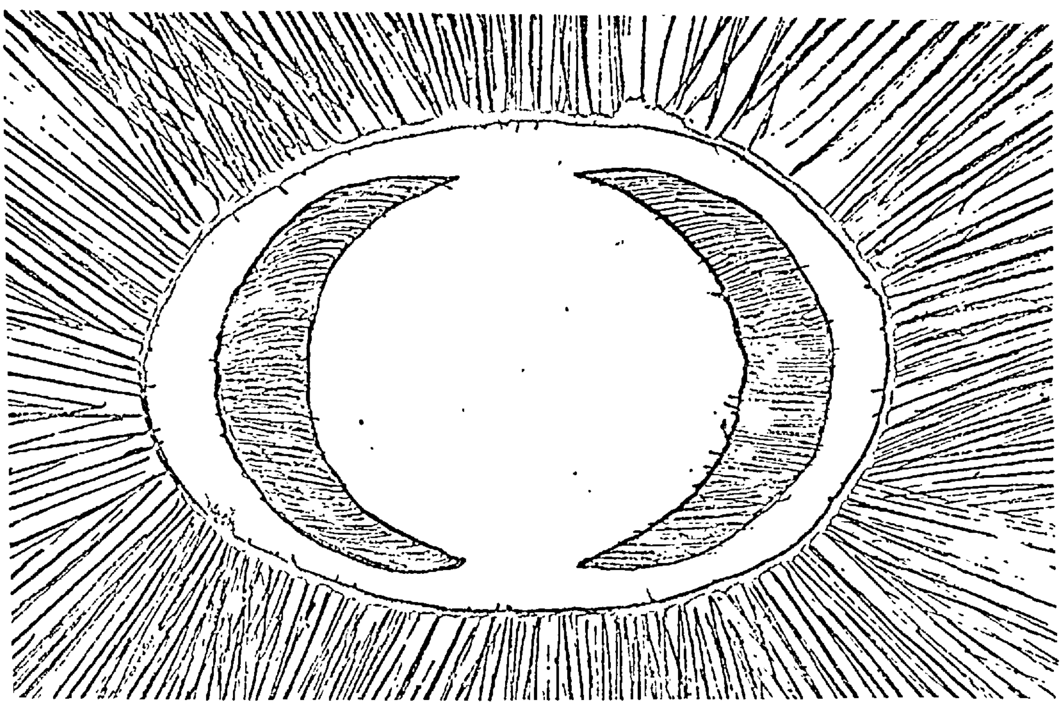


Fig. 17

Wiesel; observation of 1649, communicated to Hevelius
Observatoire de Paris MSS. 'Epistolae Hevelium' II, no. 171

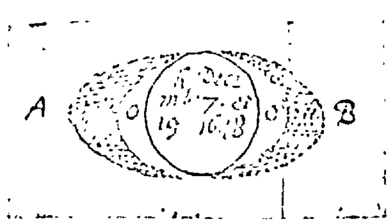


Fig. 18

Bouillau; observation of 1648; Biblioteca Nazionale (Florence) MSS. Gal. 275, f. 21r

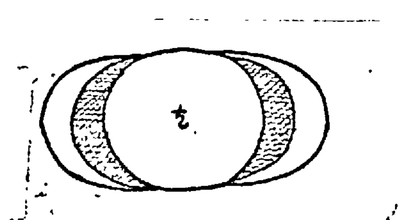


Fig. 19

Hevelius; observations of 1647-1648; Bibliothèque Nationale MSS. FF 13043. f. 20v

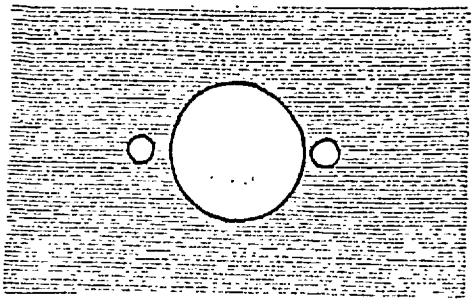


Fig. 20
1633

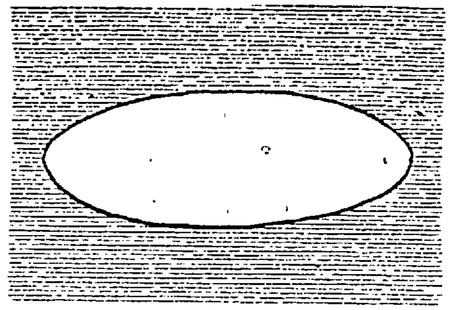


Fig. 21
1634

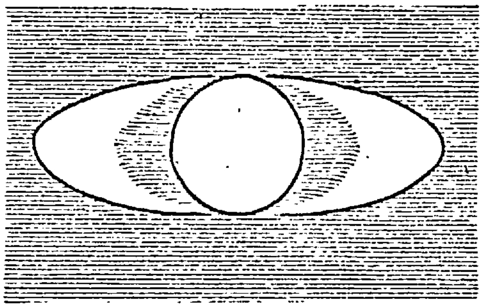


Fig. 22
1636

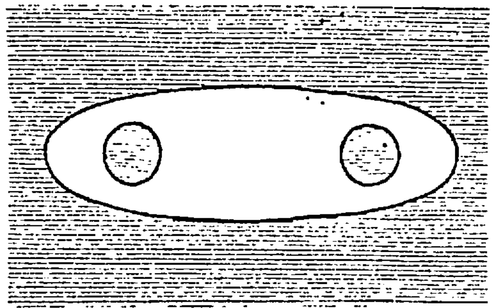


Fig. 23
1638

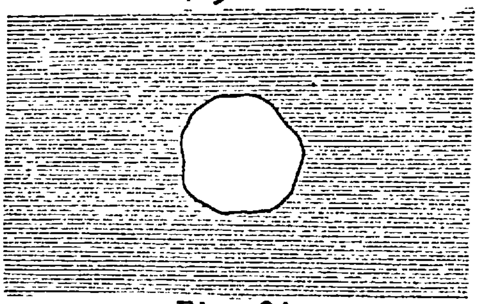


Fig. 24
1642

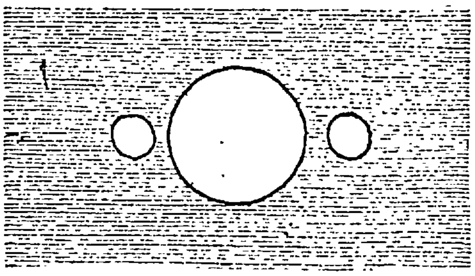


Fig. 25
30 May 1643

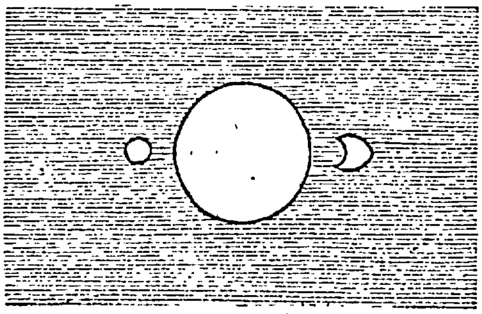


Fig. 26
12 December 1643

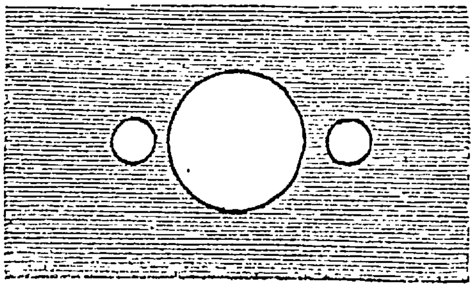


Fig. 27
29 December 1643

Gassendi, Opera Omnia, vol. IV, 1658

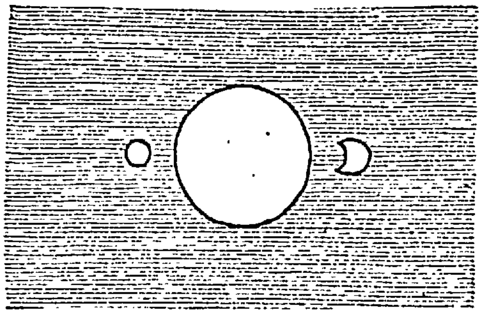


Fig. 28
22 June 1644

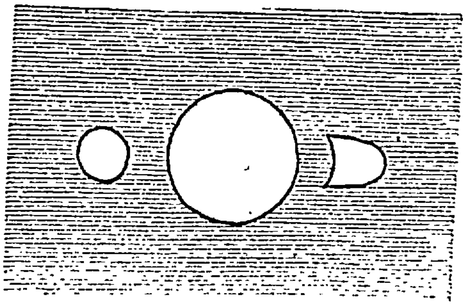


Fig. 29
24 August 1644

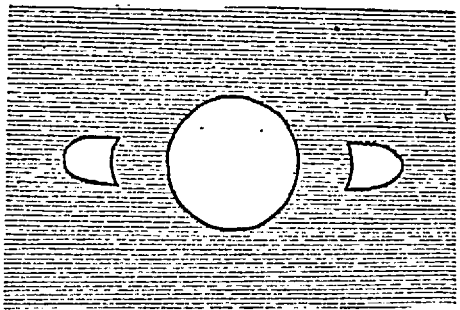


Fig. 30
9 November, 11 December 1644

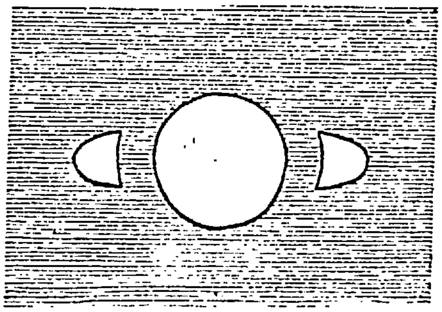


Fig. 31
12 January 1645

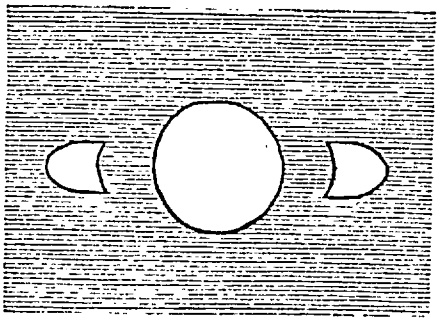


Fig. 32
11 March 1645

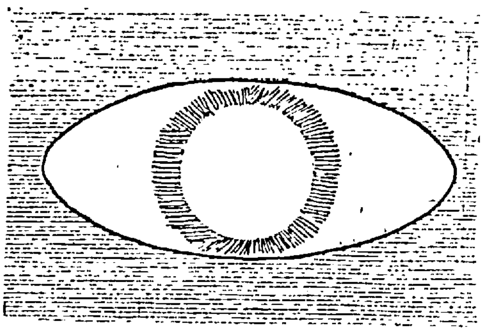


Fig. 33
26 December 1645

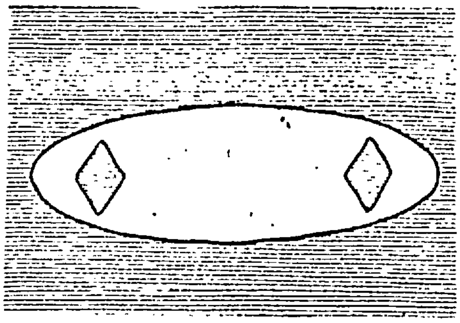


Fig. 34
1647

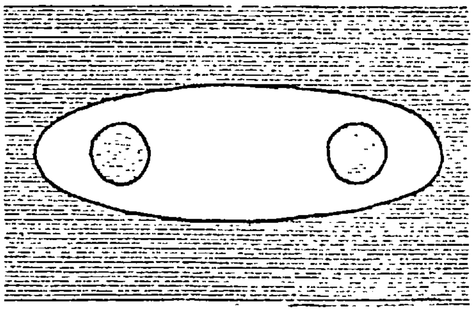


Fig. 35
1649

Gassendi; Opera Omnia, vol IV, 1658

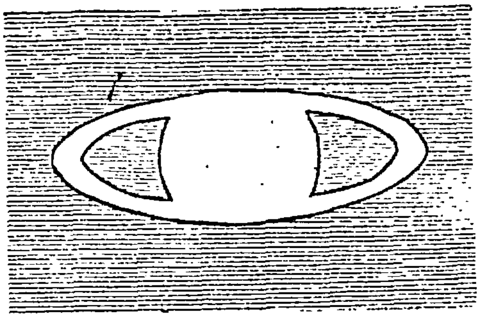


Fig. 36
1650

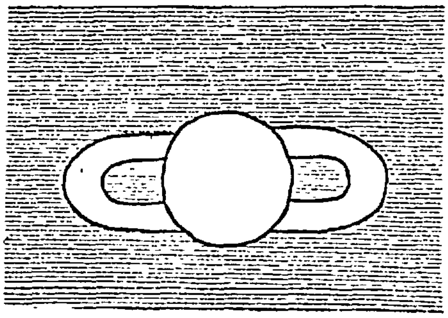


Fig. 37
1651, 1652

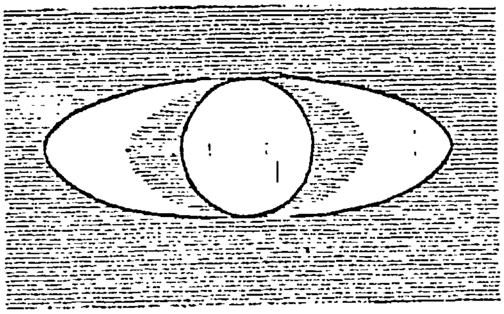


Fig. 38
6 April 1653

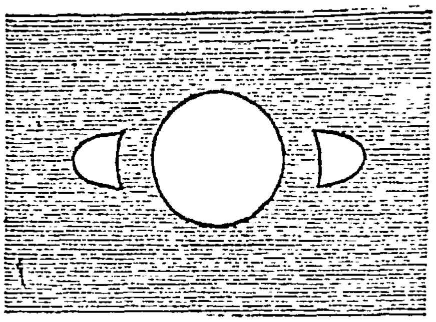


Fig. 39
29 September 1653

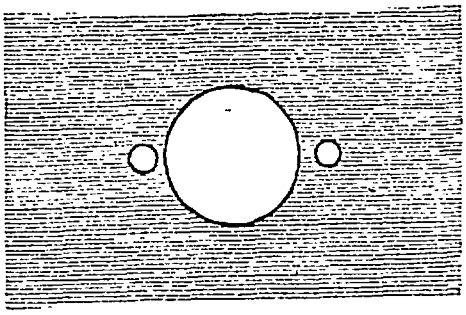


Fig. 40
1655

Gassendi; Opera Omnia, vol IV, 1658

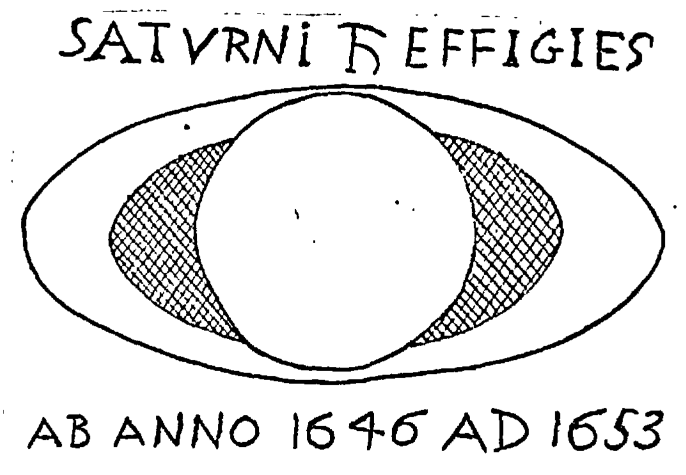


Fig. 41

Hodierna; Protei Coelestis Vertigines, 1657

SECTION II

THEORIES

Introduction

In the next four chapters the theories of Hevelius, Roberval, Hodierna, Wren, and Huygens will be discussed. The exact times at which these theories were conceived is, in most cases, difficult to determine and therefore they will be discussed in order of their publication, or (as in the case of Roberval and Wren) their completion in manuscript or letter form. This arrangement is more convenient in view of the subsequent histories of these theories.

But the events between 1655 and 1659 are, of course, interrelated, and because of the division by theory, the overall chronology may be rather difficult to ascertain from the individual accounts. It seems proper therefore to present a brief summary of the events, from the discovery of Saturn's satellite by Huygens, in March 1655, to the publication of Systema Saturnium, in July 1659.

25 March 1655	Huygens discovers a satellite of Saturn
13 June 1655	Huygens sends Wallis an anagram containing the discovery of the satellite
1 July 1655	Wallis sends back a bogus anagram
August 1655	Huygens informs scientists in Paris of his discovery of the satellite
January 1656	Wren and Neile make wax models of Saturn in an effort to arrive at a theory
Winter 1655/6	Huygens formulates his ring-hypothesis
8-15 March 1656	Huygens circulates copies of De Saturni Luna, giving the solution of his anagram and presenting another anagram, containing the ring-hypothesis
1 April 1656	Wallis sends the solution of his bogus anagram, contrived so as to claim that the satellite was discovered independently in England
6 May 1656	Hevelius sends Huygens his own anagram, containing his theory of Saturn
22 June 1656	Hevelius sends out copies of De Nativa Saturni Facie
4 August 1656	Roberval sends his hypothesis to Huygens by letter. This hypothesis was conceived a few months earlier

January 1657	Hodierna sends out copies of *Protei Caelestis Vertigines*, containing his hypothesis of Saturn. This was not received by Huygens until February 1658
Summer 1657	Bouillau is the first to learn of Huygens' ring-hypothesis, during a visit to The Hague
13 December 1657	Wren and Neile make the observations which shows that Saturn's anses maintain uniform length
Spring 1658	Huygens sends the substance of his hypothesis to Chapelain, who reveals it at a scientific gathering in Paris
31 January 1659	Huygens sends his hypothesis to Wallis
July 1659	Huygens sends out copies of *Systema Saturnium*

The detailed chronology applicable to each theory will be discussed in the appropriate chapter.

CHAPTER 4

Hevelius

Gassendi, the man who had made more observations of Saturn than any other astronomer, was never able to solve the problem presented by this strange planet. He had made continuous observations since the solitary appearance of 1642 and was looking forward to seeing Saturn solitary again in 1655 or 1656, but death came before he could witness this event. Had he lived a few years longer, he would not only have seen this event, but he would also have witnessed a number of theories put forward, perhaps even proposed one of his own. For the solitary appearance that occurred in 1656 was the occasion of renewed interest in Saturn and for the formulation of a number of theories. The first of these theories was put forward in 1656 by Gassendi's friend and correspondent Johannes Hevelius.

Hevelius had met Gassendi in Paris in 1631, when he was on his grand tour of Europe, after having finished his studies at the University of Leyden. He was the son of a rich brewer in Danzig and took over the running of the family business after he was recalled from his tour because all his brothers had died. He never left his native town again, but he kept in contact with a large number of scientists all over Europe through a vast correspondence. (1) Urged on by his childhood mathematics and astronomy teacher, Peter Cruger (1580-1639), Hevelius seriously took up observational astronomy, a subject that had interested him since his youth. (2) Although he ran his brewery and was active in the affairs of the city of Danzig as a member of the 'Rathscollegium', of which he was president ten times, and as a judge, it would be a mistake to think of Hevelius as just another 'talented amateur'. He set about his work in astronomy in a most professional way: he made his own telescopes when he could not buy good ones and he built a magnificent observatory on a large platform built on top of three adjoining houses. And he put his equipment to excellent use. It is fair to say that in the twenty years between the publication of his <u>Selenographia</u>, in 1647, and the rise of the completely professional astronomers such as Cassini, Picard, and Flamsteed, Hevelius was the best observational astronomer in Europe. (3)

Hevelius' astronomical career started around 1640 and his first major effort was to map the Moon. For five years he worked by night making observations and by day engraving these in copper. The result of this effort was one of the most lavish scientific books published in the seventeenth century, *Selenographia* (1647), which established Hevelius' reputation as an astronomer. It contained figures of no fewer than forty phases of the Moon and proposed a system of nomenclature based on geographical names. Although *Selenographia* was widely read, Hevelius' nomenclature was not generally accepted and today we use the system proposed by Riccioli in his *Almagestum Novum* of 1651.

During the next forty years, Hevelius published books and tracts on every conceivable aspect of observational astronomy. But his major concern was the improvement of star catalogues, a project on the importance of which he constantly insisted in his letters and on which he worked from 1641 until his death. His *Prodromus Astronomiae* containing the positions of 1553 stars was published posthumously in 1690. Although telescopic sights on astronomical instruments came into use during the 1660's, Hevelius persisted in his naked eye observations, claiming that telescopic sights would introduce inaccuracies. His controversy with Hooke on this subject is well known (4), and it is true that the observations made by Flamsteed, using telescopic sights, were much more accurate than those of Hevelius. But it must be remembered that Flamsteed's star catalogue in *Historia Coelestis Britannica* was not published until 1725. Therefore, for 35 years Hevelius' catalogue was the best star catalogue available.

His other major works were *Cometographia* of 1668, in which he gave a complete review of all observations of comets going back to Antiquity, and suggested that comets could possibly move in parabolic paths, and *Machina Coelestis*, published in two parts, in 1673 and 1679, in which he gave a complete description of his instruments (just before his observatory was completely destroyed by fire in 1679), and which also presented a large number of observations. But he wrote numerous smaller tracts, on eclipses, transits, new stars, variable stars, the motion of the Moon (he discovered the longitudinal libration of the Moon), and his observations of the planets were published in a number of books. To his credit it should be mentioned that Lambert's discovery, in 1773, that the mean motions of Jupiter and Saturn are not always continuously accelerated and retarded respectively, but are accelerated and retarded alternatively, a discovery which allowed Laplace finally to solve the riddle of the irregularities in the mean motions of these planets in 1784, was based on a comparison of Hevelius' observations with observations made a hundred years later. (5)

During his lifetime Hevelius held a pension from Louis XIV, awarded in 1663, a pension from the Polish king John III, awarded in 1667, as well as an exemption from taxes, the right to sell his beer in other towns, and the right to operate his own printing press. (6) In 1664 he was elected a Fellow of the Royal Society and many of his observations were published in the _Philosophical Transactions_.

One of the problems that held a great interest for Hevelius was that of Saturn. He had made regular observations of this planet since 1642 and in his _Selenographia_ he showed several different aspects of the planet (ch. 3, figs. 10-12), but he was unable, at this time, to draw any conclusions as to the cause of these appearances. His interest in the planet did not diminish as is evidenced by his references to it in his correspondence, and finally, in 1656, after having observed one full period of the 'phases' of Saturn, he formulated a theory on the cause of these 'phases'.

When Christiaan Huygens' younger brother, Philips, attached to a diplomatic mission to Poland, visited Hevelius on 4 May 1656 and delivered a copy of Huygens' _De Saturni Luna_, he found Hevelius (whom he described as such a nice little man that he regretted not having visited him immediately upon his arrival in Danzig) hard at work on a book on Saturn. Hevelius even showed him the copper plates for the figures in the book. (7) Thus, Hevelius' _De Nativa Saturni Facie_ was not written as a response to the challenge of Huygens in _De Saturni Luna_ as has been sometimes suggested. With his letter, Philips sent an anagram by Hevelius containing the most important aspects of his thesis. This anagram was designed to safeguard the claim of Hevelius in case his theory should turn out to be identical to that of Huygens. (8)

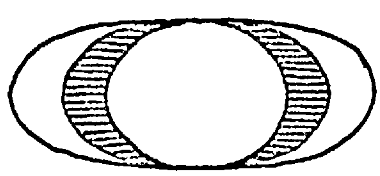
Dissertatio de Nativa Saturni Facie, ejusque varijs Phasibus certa Periodo redeuntibus ... was published in June 1656. The dedication was dated 11 June, and copies were sent off on the 22nd. (9) In a letter of that date to Bouillau, Hevelius explains that he had had the idea of a work on the phases of Saturn in mind for several years, but he had waited until he had again observed the mono-spherical phase (10), which occurred early in 1656. Thus, Hevelius had not wasted any time in publishing his work. In this he was unlike many other seventeenth century scientists, including Huygens, who waited years, and sometimes decades, before publishing their findings. The dissertation includes a description of the solar eclipse of 1656, as well as a measurement of the diameter of the Sun. Although it is rather short (40 pages), it is executed in the distinctive, lavish Hevelian style that sets his books apart from other scientific books. The dedication to Louis XIV praises the great French nation that had produced so many geniuses, among them the incomparable Gassendi, recently carried away by death, to the grief of all learned men, and Bouillau. This dedication was apparently not wasted on the future Sun King, for when Colbert started on his scheme of patronising the sciences, Hevelius was given a pension by the French Crown, although he never visited France again after 1634.

Hevelius' introductory remarks deal with the different shapes of Saturn and the opinions, or rather lack of opinions, of Gassendi, Riccioli, and Holwarda. Hevelius had thus read Holwarda's *Philosophia Naturalis* of 1651 (see pp. 50-51) and it is very possible that he got at least the central idea of his theory from Holwarda's reflections. Hevelius does not put forward his theory in a very positive manner. He states that he is *almost* persuaded (*pene persuadeor*) that he has discovered the causes and period of this phenomenon, and that future generations will probably add to his theory. He then states the main points of his thesis:

1) Saturn is illuminated by the Sun

2) Saturn does not always exhibit the same shape, but six or seven primary shapes and numerous secondary shapes derived from these. The primary phases occur in a recurring cycle.

3) The actual shape of Saturn is triple-bodied, as shown in this figure:



The middle body is not round, but elliptical, and the lateral bodies are not spherical and moveable about Saturn, but are firmly adhering bodies with almost hyperbolic outlines. They are affixed to the middle body near the top and bottom, and revolve together with the middle body about a single axis in a fixed period.

The various appearances are then explained by the different aspects presented to the Earth at different times. As for the middle body, '... it is obvious from optics that an oblong and cylindrical body appears shorter and rounder the more obliquely it is seen, and gradually becomes a circle ...' (p. 4) Hevelius will not add new and more complicated circles to explain this motion, but rather he will apply the bare hypothesis of Copernicus, although the motion can equally well be explained by the hypotheses of Tycho and Kepler. The six major phases are shown in figs. 4-9.

Before reaching his explanation however, Hevelius deals with the period of the phases. One period of the phases is equal to half the time it takes Saturn to traverse all the signs of the zodiac, i.e. about 15 years. This is clearly different from the period of the phases of the Moon, Venus, and Mercury, which are equal to their periods of revolution about the Sun. The reason for this difference is that the phases of the Moon, Venus, and Mercury are caused by their varying positions relative to the Sun and the Earth, whereas the phases of Saturn are caused by the varying aspects of the planet itself with regard to the Earth. The variations in the aspect of the planet arises from its revolution on its axis. One complete revolution takes about 30 years, but because of two-fold symmetry, Saturn goes through the complete progression of phases twice during this period.

Hevelius then fixes precisely at what point in the zodiac each phase is seen. The 'elliptico-ansatus' phase, that is, the most open phase, is seen at Saturn's apogee and perigee, which occur in Sagittarius and Gemini respectively, while the 'mono-spherical' appearance is always seen at mean longitude, i.e. in Pisces and Virgo. Between apogee and mean longitude,

> ... Saturn narrows his appearance gradually and appears shorter, so that initially his middle body becomes somewhat round, and then changes his arms into pointed little globes and then into smaller round one, until the time when at mean longitude the disc of Saturn turns into a perfect circle ... (p. 6)

But lest the reader think that Hevelius is too rash in stating all this so definitely, he is assured that it is based on Hevelius' own observations and on as many observations of others as he had been able to gather. All these observations are classified and presented in one convenient table (fig. 1). In this table, 48 observations, from 1612 right up to 1656, are presented with their date, Saturn's position in the zodiac, its position relative to the Earth and Sun (i.e. opposition, conjunction, quadrature, etc.), the observed shape, and, in some cases, the direction of the line of the anses. (11)

However, the matter is not quite as simple as it has been presented thus far, for

> ... a certain difference and diversity of aspect can sometimes arise ... so that occasionally Saturn appears handled, with globes, or round a bit sooner and [remains so] a bit later than he actually should according to this motion. (p. 9)

This, Hevelius explains, is due to the motion of the Earth, which is superimposed on the motion of Saturn. It is a concomitant of retrogression and therefore retrogression of the phases always goes together with retrograde motion of the planet. Based on these considerations and on the data presented, Hevelius predicts that Saturn, which at that time (1656) was still solitary, would regain its globes at the earliest near the end of 1656, but if they should be seen then, they would disappear again in April or May 1657 because of the Earth's motion, not to return until September or October of 1657. Hevelius stresses that in order to contemplate these transformations an excellent and very long tube is needed, at least 10, 12, or 15 feet long. (p. 11) Moreover, observations must not merely be made every year; in order to render the observer more certain of the appearances, observations should be made at least every month.

The explanation of Saturn's motion according to Copernicus' theory is done with the help of the figure shown here in fig. 2, showing the Sun (A), the Earth (F) on its great circle (FFFF), the excentric point (B) of Saturn, and Saturn itself (E) moving on its epicycle whose centre (C) moves on the deferent. Saturn always keeps its 'elliptico-ansatus' face turned toward the centre of its epicycle, just as the Moon always keeps the same face turned toward the Earth, and thus, as Saturn moves on its epicycle in consequence, this causes the planet to be seen at various angles from the Earth. The Earth's motion of course causes the planet and the progression of the phases to vary their speeds. The relative dimensions of the figure have been adjusted to show things more clearly. In reality they are as follows: if BC is 10,000, then AB, the excentric, is 900, CE, the radius of the epicycle, is 300, and AF, the radius of the Earth's annual circle, is 1090. These proportions are taken directly from Copernicus. (12)

Hevelius is now ready to make long-term predictions and he gives a table relating all Saturn's appearances to their places in the zodiac. Thus, anyone who can read ephemerides can, with this table, predict the appearance of Saturn at any given time. But for those who cannot read ephemerides he gives the principle phases for the next 50 years. The next solitary appearance would occur from March 1672 to April 1673. We shall return to this prediction in chapter 11.

Hevelius considered the difference in size of the lateral globes observed by Gassendi, Riccioli, and himslef as phenomena caused by the aspect of Saturn relative to the Earth. In some signs, e.g. Capricorn, Aquarius, and Aries, the

> ... phases meet obliquely with the eye and therefore one of the arms or globes should appear then toward the rear and more elongated, somewhat narrower and more compressed, and consequently also smaller. (p. 19)

Furthermore, this globe should disappear somewhat sooner than the other one, resulting in a round outline on one side, while on the other side, the side closest to us, a small portion of the globe still juts out. But this is seldom perceived easily, except by the most sharpsighted and lynx-eyed observers. Clearly, Hevelius is here stretching the usefulness of his theory. (pp. 19-20)

The ticklish question as to how round globes can be produced by arm-like appendages is dealt with next. They are, in fact, not round but concave on the inside and convex on the outside (fig. 8). To the question of why such shapes should appear as perfectly round globes, he answers:

> ... that appearance arises only from hallucinated vision, and ... the said bodies, because of the immenseness of the distance and the weakness of the emanation, or because of distortion of the senses, may be perceived by us less articulated and those interior parts cannot be seen distinctly concave. We have in fact also learned from opticians that irregular bodies, seen from a distance in a certain way, appear spherical, especially when positioned obliquely, when the natural body is always seen deformed. Because the more remote and the smaller the objects are, the less perfectly they are seen. We grant therefore that Saturn's adhering globes appear in fact entirely round to us, but, all the same, that they are not at all exactly spherical, but, in short, of a shape that is shown clearly in the fifth figure in plate F (fig. 8). (p. 21)

This was of course a touchy point and Hevelius managed to avoid the central issue, which was how the figure of the detached globes could arise.

Hevelius now advances an interesting argument. Since Antiquity it had been supposed that the varying brightness of Saturn was directly related to the variations in its distance from the Earth. But the fact is that Saturn appears very bright at apogee, when it is furthest from the Earth, and much more dim at mean longitudes, when it is closer to us. This is explained by the phases. At apogee, Saturn is 'elliptico-ansatus' and subtends a greater angle than at mean longitudes, when it is 'mono-sphaericus'.

Another problem is that of the direction of the anses. It was not an easy matter to determine this direction accurately. Galileo had changed his mind on it in 1610, and subsequent observers were all a bit confused on this subject. In fact, this direction was at the time roughly 28° inclined to the ecliptic. Hevelius supposed that the line of the anses was parallel to the orbital plane of Saturn, rendering it sometimes parallel to the ecliptic and at other times parallel to the equator. Hevelius' method of measuring this direction was quite simple. He looked in the vicinity of Saturn until he found a known star on the extension of the line of the anses. Then, knowing the position of this star and the position of Saturn, the direction of the anses could be determined by a simple calculation. But over such small angular distances, a small error in the position of either body will result in a sizable error in the calculated direction. For such observations Hevelius recommended ' ... a tube of ample capacity, that is, one that reveals an ample expanse of sky and which consists of several convex glasses ...' (p. 29) It is quite possible that around 1655 Hevelius may have seen Saturn's satellite, which at that time was on the extension of the line of the anses.

Finally, Hevelius also explains the progression of the phases on the basis of the Keplerian hypothesis of elliptical orbits. He uses the magnetic fibres of Kepler, aligning them with the line of the anses, to keep Saturn's major axis parallel to itself (fig. 3). Therefore the planet in effect rotates on its axis (with respect to the Earth and Sun) once during every revolution about the Sun, and thus presents different aspects to the Earth from different points in its orbit. Hevelius feels that the very simplicity of this explanation is a potent argument for Kepler's hypothesis of elliptical orbits and magnetic fibres. Other astronomers disagreed with this.

Hevelius had not moved as far away from the satellite model as would appear at first glance. He essentially reversed the priority of appearances. Rather than saying that the tri-spherical appearance is primary or real, and having to explain the anomalous handled appearance in terms of it, he opted for the primacy of the handled appearance and had to account for the tri-spherical appearance in terms of it. This does not make the problem any easier and Hevelius did not successfully dispose of the incompatibility of the tri-spherical and handled appearances. By stating that the round globes are really not round, but rather crescent-shaped, he merely put the problem back one step, because the question now became: how can the rotation of the body and arm-like appendages change the appearance of the appendages from that in fig. 4 to that in fig. 8? This metamorphosis could not be explained in terms of optics. What Hevelius' model predicts and what Hevelius says it predicts are quite clearly two different things (see fig. 10) The progression from dimension 'a' to dimension 'b' was not at all obvious from his theory or from his discussion. It was noticed by Hevelius' contemporaries and duly commented on. A Jesuit teacher at the college of the order at Clermont, Antonius Vatier, wrote to Hevelius on 3 April 1657 that he found one or two difficulties in Hevelius' theory. First, he thought that it was

> ... not sufficiently clear ... how the anses of Saturn, which equal the total width or diameter of his disc when they are unfolded, can contract into two globes which reach only the third part of Saturn's diameter, such as they appear now ... (13)

Furthermore, Vatier believed that when the globes appear round they are indeed round. Huygens did not object to Hevelius' optical explanation of why crescents should appear as globes, but he did object that '... it is not obvious how these arms can contract into such small orbs ...' (14), no matter from what angle they are regarded. Christopher Wren expressed the same opinion. (15) This was then the major objection to Hevelius' theory. But Hevelius did not think that his theory stood or fell on this issue. After all, he had not declared himself to be totally certain of the causes of the appearances, and in his reply to Huygens (7 September 1656) he agreed that it was entirely possible that better telescopes would show a different appearance and that his theory might therefore have to be altered:

> It is not the case that anyone is convinced that I have investigated every last aspect of this business and that nothing remains [to be done] - no, certainly not! Indeed, nothing is perfect when it begins, nor did the most learned of all, the Ancients as well as the Moderns, publish anything that was not gradually revealed more clearly and refined with time. Why therefore is not this also to be hoped for in due time in this most abstruse subject matter, which I have just now tackled for the first time? (16)

But he did attempt to explain the variations in the latitudinal dimension of the anses by stating that the decrease in this dimension is proportional to Saturn's (latitudinal) variation from the ecliptic. The globes or anses are narrowest when Saturn is furthest removed from the ecliptic. (17) But since Saturn's maximum deviation from the ecliptic is only about $2\frac{1}{2}^{\circ}$, it is difficult to see how this could account for the large variation in the latitudinal dimension of the lateral bodies.

However, Hevelius definitely did not think that this was the essence of his theory. The solution to his anagram, transmitted to Huygens was: *Integra phasium Saturni revolutio absolvitur quindecim circiter annis*: The complete revolution of Saturn's phases is completed in about fifteen years. (18) He considered *this* the cardinal point of his theory. In the letter to Huygens accompanying *De Nativa Saturni Facie* Hevelius described his dissertation only as follows:

> I have in fact attended to the changes in the shapes and phases of Saturn very diligently for many years now. I have, in my opinion, penetrated his definite period. What observations I have obtained and what I think of this phenomenon you will see from this little dissertation. (19)

Later, in 1659, he wrote to Bouillau

> ... it does not matter so much by what hypothesis that phenomenon [the solitary appearance] may be demonstrated, as how the period of all the phases and the intervals of the changes may be correctly found and defined for the first time. (20)

Hevelius was of course entirely correct in maintaining that he had been the first to publish the period of the phases of Saturn. But his publication came at a time when this period had become pretty well self-evident to anyone who had made observations for the last few years and had read e.g. Riccioli's *Almagestum Novum*. Huygens and Wren surely thought that the period of the phases was common knowledge and neither bothered to give Hevelius credit for being the first to establish it. In the case of Huygens, Hevelius was made very angry because Huygens did not give him the credit he thought he deserved. We shall discuss this in chapter 8.

In view of all the statements as to authors' ignorance about the period of the phases, from Galileo in 1640 until well into the 1650's, Hevelius' claim was quite justified. He had finally done what every astronomer had agreed was of crucial importance for the solution of the problem of Saturn: he had finally quantified the available information and put it into an orderly system, which even allowed prediction. Perhaps Bouillau, who was not given to needless flattery, did Hevelius more justice when he wrote:

> I have read your whole book; and while reading it, the excellence of your genius occurred to me very often, and I admired your tireless diligence. With the greatest pleasure I saw Saturn's various phases, as they have always been seen by me and by others, very

correctly described, and the period of that phenomenon detected. (21)

If perhaps the 'excellence of Hevelius' genius' was not as great as that of a Huygens or Newton, it is certainly true that in his 'tireless diligence' he was unsurpassed in his time.

Obſervationes Phaſium Saturni, à nonnullis Siderum Scrutatoribus, ope Teleſcopiorum peractæ.

A quibus, & quo loco fuerint obſervata.	*Anno.*	*Menſ.*	*D.*	*Longitudo Saturni.* °	'	*Si.*	*Sub quo Solis & Saturni adſpectu.*	*Quâ facie Saturnus apparuerit.*	*Quam plagam verſùs anſularum ſeu glob. extiterit directio.*
Gallilæus, Flor.	1612	*Decemb.*	1.	11	27	♓	□ ⊙ ♄	*Rotundus*	
Hortenſ. Amſt.	1625	*Febr.*	27.	29	5	♌	☍ ⊙ ♄	*Cum globulis*	
Fontana, Neap.	1630	*Junii*	20.	29	4	♎	△ ⊙ ♄	*Globulis ſtipatus*	
Hortenſ. Amſt	1632	*Julii*	16.	22	27	♏	△ ⊙ ♄	*Anſatus*	
Fontana, Neap.	1633			5	0	♐		*Anſatus*	
Gaſſendus, Lut.	1633	*Jun.*		5	0	♐	*poſt* ☍ ⊙ ♄	*Anſatus*	*Æquinoctialis*
Fontana, Neap.	1634			20	0	♐		*Anſatus*	
Gaſſendus, Lut.	1634	*April.*		22	0	♐	△ ⊙ ♄	*Anſatus*	*Æquinoctialis*
Gaſſendus, Lut.	1636	*Novemb.*		10	0	♑	⚹ ⊙ ♄	*Anſatus*	*Æquinoctialis*
Fontana, Neap.	1636							*Anſatus*	
Gaſſendus, Lut.	1637	*Febr, Jul. Nov.*		20	0	♑	⚹. □ ⊙ ♄	*Anſatus*	*Æquinoctialis*
Gaſſendus, Lut.	1638	*Decemb.*		4	0	♒	⚹ ⊙ ♄	*Minimè rotundus*	*Æquinoctialis*
Zucchius, Rom.	1640	*Maji*	23.	0	21	♓	□ ⊙ ♄	*Cū glob. acumin.*	
Gaſſendus, Lut.	1642	*Auguſt.*		23	0	♓	*ante* ☍ ⊙ ♄	*Rotundus*	
Bullialdus, Lut.	1642	*Auguſt.*		23	0	♓		*Rotundus*	
Hevelius, Ged.	1642	*Septemb. Octob.*		19	0	♓	*poſt* ☍ ⊙ ♄	*Rotundus*	
Gaſſendus, Lut	1643	*Maj.*	30.	5	56	♈	⚹ ⊙ ♄	*Tricorpor.*	
Ricciolus, Bon.	1643	*Octob.*		5	0	♈	*poſt* ☍ ⊙ ♄	*Cum globulis*	
Gaſſendus, Lut.	1644	*Mart.*		9	0	♈	*ante* ☌ ⊙ ♄	*Cum globulis*	
Gaſſendus, Lut.	1644	*Jun.*		19	0	♈	⚹ ⊙ ♄	*Cum globulis*	
Fontana, Neap.	1644	*Decemb.*		18	0	♈		*Cum globulis*	
Hevelius, Ged.	1645	*Sept. Octob. Nov.*		2	0	♉	☍ ⊙ ♄	*Anſatus*	*Eclipticalis circ.*
Gaſſendus, Lut.	1645	*Octob.*		2	0	♉	☍ ⊙ ♄	*Anſatus*	
Hevelius, Ged.	1646	*Novemb.*		15	0	♉	☍ ⊙ ♄	*Anſatus*	*Eclipticalis circ.*
DeDivinis, Rom.	1646			16	0	♉		*Anſatus*	
Ricciolus Bon.	1646	*Octob.*	10.	16	55	♉	△ ⊙ ♄	*Anſatus*	*Æquinoctialis*
Hevelius, Ged.	1647	*Sept.*	7.	2	16	♊	□ ⊙ ♄	*Elliptico-Anſatus*	*Æquinoct. circ.*
L. Eichſtad. Ged.	1647	*Sept.*	7.	2	16	♊	□ ⊙ ♄	*Elliptico-anſatus.*	
DeDivinis, Rom.	1647			2	0	♊		*Anſatus*	
Hevelius, Ged.	1648	*Decemb.*		13	0	♊	☍ ⊙ ♄	*Elliptico-Anſatus*	*Æquinoct. circ.*
DeDivinis, Rom.	1648			12	0	♊		*Anſatus*	
Bullialdus, Lut.	1648	*Decemb.*		13	0	♊		*Anſatus*	
Ricciolus, Bon.	1649	*Mart.*		12	0	♊	*ante* ☍ ⊙ ♄	*Anſatus*	*Æquinoctialis*
Hevelius, Ged.	1649	*Decemb.*	23.	26	43	♊	☍ ⊙ ♄	*Elliptico-anſatus 1*	*Æquinoctialis*
Hevelius, Ged.	1650	*Octob.*		14	0	♋	□ ⊙ ♄	*Elliptico-anſatus 2*	*Æquinoctialis*
Hevelius, Ged.	1651	*Novemb.*		29	0	♋	△ ⊙ ♄	*Sphæro-anſat. ferè*	*Æquinoctialis*
Hevelius, Ged.	1652	*Octob. Novemb.*		12	0	♋	□ ⊙ ♄	*Elliptico-anſatus 2*	*Æquinoctialis.*
Ricciolus, Bon.	1655	*April.*		4	0	♍	△ ⊙ ♄	*Triſphæricus*	
Hevelius, Ged.	1655	*Maj.*	30.	4	0	♍	□ ⊙ ♄	*Triſphæricus*	
Bullialdus, Lut.	1656	*Jan.*		23	0	♍	△ ⊙ ♄	*Rotundus*	
Hevelius, Ged.	1656	*Febr.*	25.	20	54	♍	*ante* ☍ ⊙ ♄	*Monoſphæricus*	
Hevelius, Ged.	1656	*Mart.*	11.	19	44	♍	☍ ⊙ ♄	*Monoſphæricus*	
Hevelius, Ged.	1656	*Mart.*	24.	18	44	♍	*poſt* ☍ ⊙ ♄	*Monoſphæricus*	
Hevelius, Ged.	1656	*Mart.*	28.	18	27	♍	*poſt* ☍ ⊙ ♄	*Monoſphæricus*	
Hevelius, Ged.	1656	*April.*	11.	17	35	♍	*poſt* ☍ ⊙ ♄	*Monoſphæricus*	
Hevelius, Ged.	1656	*April.*	21.	17	5	♍	△ ⊙ ♄	*Monoſphæricus*	
Hevelius, Ged.	1656	*April.*	29.	16	48	♍	△ ⊙ ♄	*Monoſphæricus*	
Hevelius, Ged.	1656	*Maj.*	2.	16	44	♍	△ ⊙ ♄	*Monoſphæricus.*	

Fig. 1

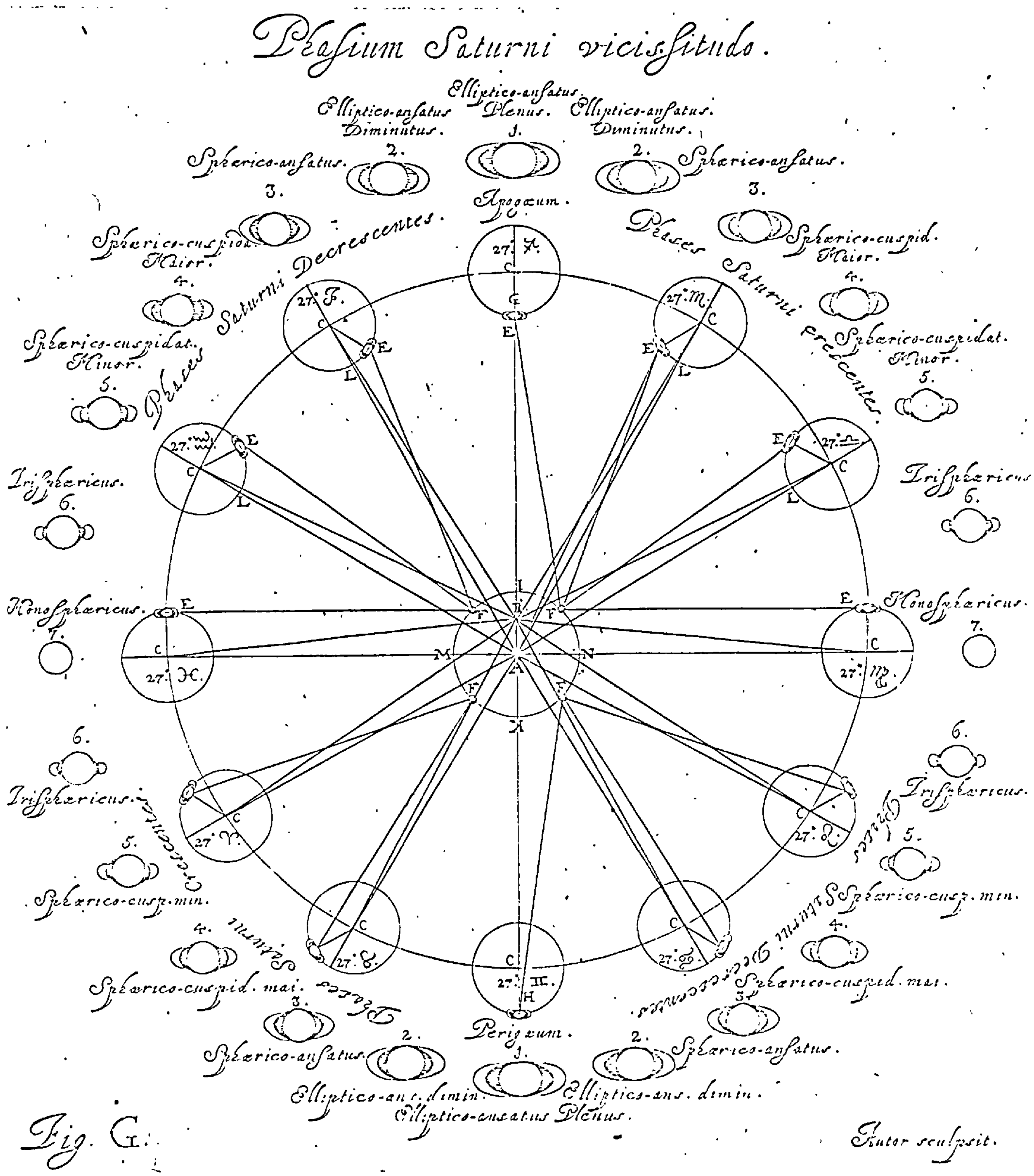

Phasium Saturni vicissitudo.
Elliptico-ansatus Plenus. 1.
Elliptico-ansatus Diminutus. 2.
Elliptico-ansatus Diminutus. 2.
Spherico-ansatus. 3.
Spherico-ansatus. 3.
Spherico-cuspid. Maior. 4.
Spherico-cuspid. Maior. 4.
Spherico-cuspidat. Minor. 5.
Spherico-cuspidat. Minor. 5.
Trisphæricus. 6.
Trisphæricus. 6.
Monosphæricus. 7.
Monosphæricus. 7.
Apogæum.
Perigæum.
Phases Saturni Decrescentes.
Phases Saturni Crescentes.
Trisphæricus. 6.
Trisphæricus. 6.
Sphærico-cusp. min. 5.
Sphærico-cusp. min. 5.
Sphærico-cuspid. mai. 4.
Sphærico-cuspid. mai. 4.
Sphærico-ansatus. 3.
Sphærico-ansatus. 3.
Elliptico-ans. dimin. 2.
Elliptico-ans. dimin. 2.
Elliptico-ansatus Plenus. 1.
Fig. G.
Autor sculpsit.

Fig. 2

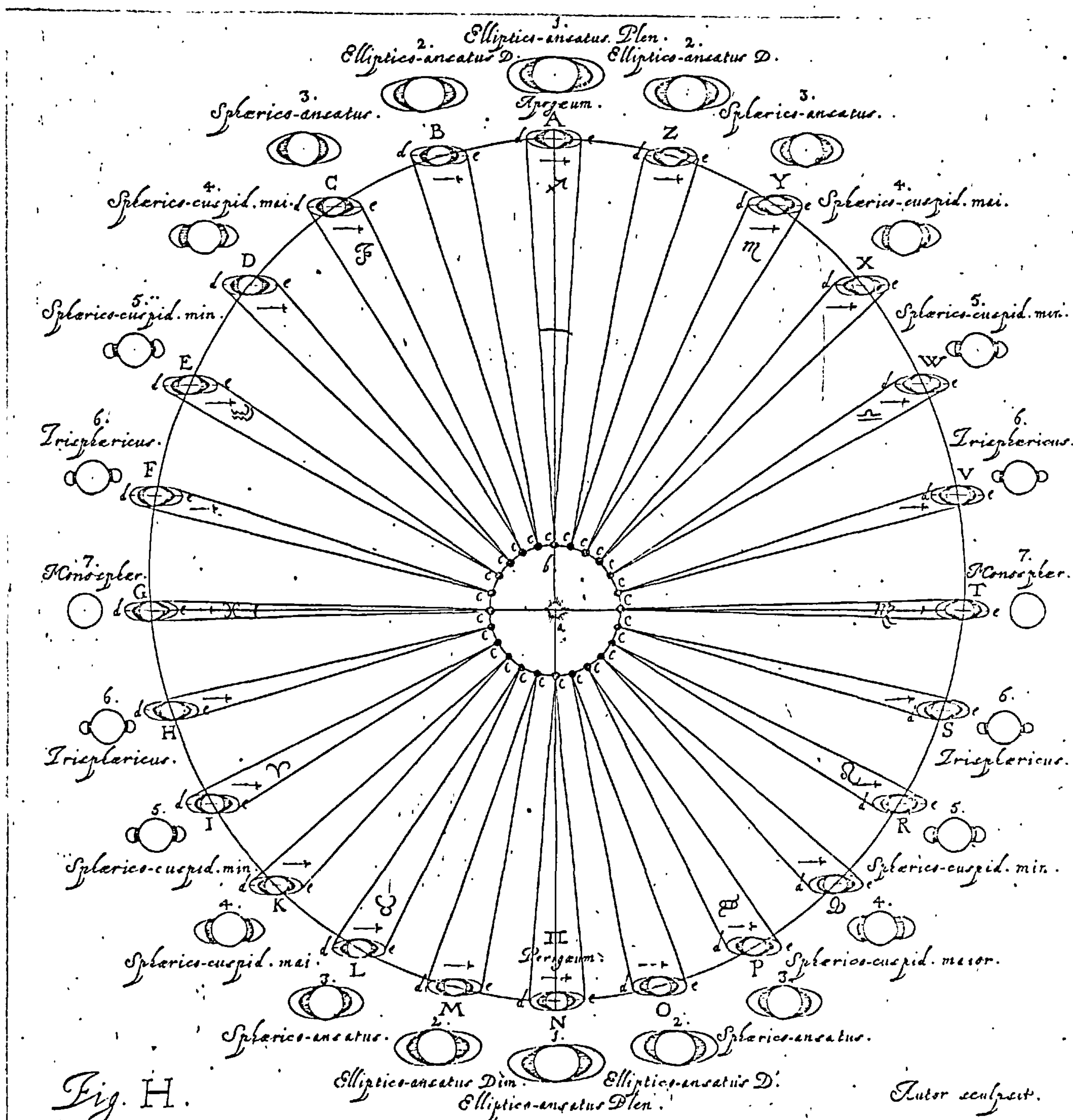
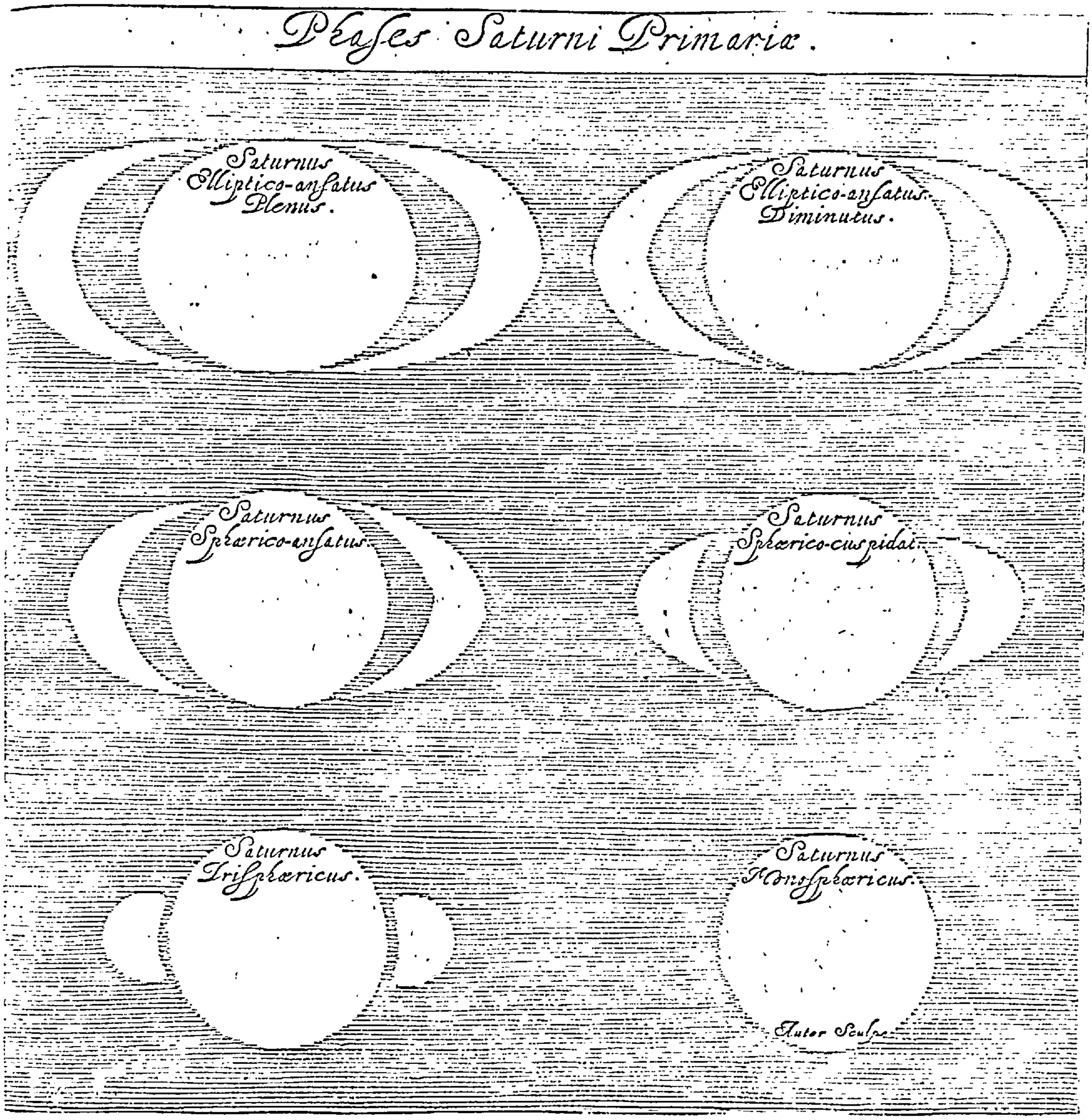

Elliptico-ansatus Plen.
Elliptico-ansatus D.
Elliptico-ansatus D.
Spherico-ansatus.
Apogeum.
Spherico-cuspid. mai.
Spherico-cuspid. min.
Trisphericus.
Monospher.
Périgeum.
Elliptico-ansatus Dim.
Elliptico-ansatus D.
Elliptico-ansatus Plen.
Fig. H.
Autor sculpsit.

Fig. 3


Phases Saturni Primariæ.
Saturnus Elliptico-ansatus Plenus.
Saturnus Elliptico-ansatus Diminutus.
Saturnus Sphærico-ansatus.
Saturnus Spærico-cuspidat.
Saturnus Trisphæricus.
Saturnus Monosphæricus.
Autor Sculpsit

Figs. 4-9

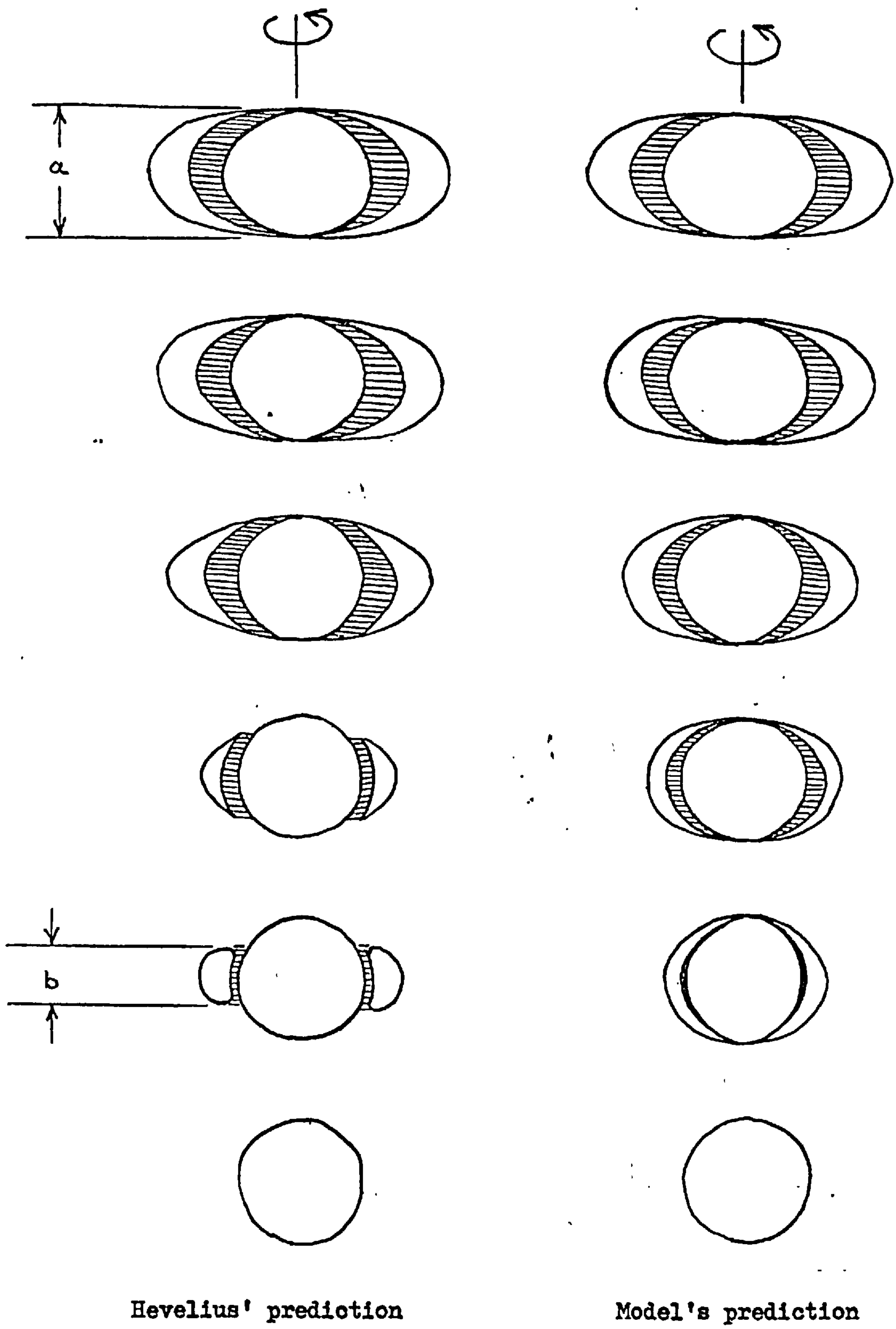


Fig. 10

CHAPTER 5

Roberval and Hodierna

Huygens' public announcement of the discovery of Saturn's moon was sent to a number of scientists in March 1656. Copies were sent to Paris, where Huygens had made many scientific acquaintances during his visit of the previous summer. One of these acquaintances was Gilles Personne de Roberval (1602-1675), the well-known mathematician who had held Ramus' chair of mathematics at the Collège Royale de France in Paris since 1634. He held this chair until his death, although he had to defend it against all comers every third year. He also held the chair of mathematics vacated by Gassendi at his death in 1655 at the same college, as well as the chair of philosophy at the Collège de Maistre Gervais. Roberval is of course best known as a mathematician; only his failure to publish his Traité des Indivisibles gave Cavalieri priority in his treatment of infinitesimals. It has been suggested that Roberval's reluctance to publish his work was due to the fact that he wanted, so to speak, to keep the opposition in ignorance in the debates for his chair. (1)

Although he was a mathematician first and foremost, Roberval had a great interest in other branches of the sciences as well. He was closely associated with Pascal and performed with him the Torricellian experiments in 1648; he even initiated some of these experiments. During the regular scientific meetings held in Paris during the 1650's and early 1660's, and after that at the meetings of the Académie Royale des Sciences (of which he was one of the seven original members), he took an active part in the discussions on a variety of topics. Because of his combative attitude he was involved in a number of quarrels, resulting from a disagreement on some issue.

Huygens sent Roberval a copy of De Saturni Luna in March 1656, accompanied by a letter in which he expressed his confidence in his new hypothesis:

> J'y ay mis dans ce billet [i.e. De Saturni Luna] une prediction, que peut estre vous trouverez asse hardie touchant le retour des bras de Saturne; toute foy ne croyez pas que j'aye rien hazardè trop legerement, qui me pourroit faire passer pour faux prophete. (2)

The prediction in question was that Saturn would regain its 'arms' toward the end of April, if not sooner. (3) Perhaps Huygens' apparent confidence caused Roberval to think about the problem of Saturn and the challenge contained in the tract. At any rate, over the next few months he developed an hypothesis of his own and on 4 July he wrote to Huygens:

> J'ay aussi pensé une hypothese qui me satisfait fort bien, touchant les diverses faces du meme Saturne, quoy qu'en consequence de vostre lune, il doive se mouvoir sur son centre en moins de 24 de nos heures; mais, comme Je ne fais point de secret, Je l'ay communiquee publiquement dans la meme chaire; Je vous le manderay si vous le desirez. (4)

Huygens was of course very eager to know Roberval's theory, and he wrote to Roberval on 20 July:

> Je seray ravy de veoir vostre hypothese pour ce qui est des anses de cette planete, laquelle je suis bien assurè qu'il ne resemblera pas à la miene, puis qu'elle ne souffre pas que Saturne fasse le tour sur son centre en si peu d'heures que la miene semble requerir. (5)

Although Huygens repeated the request in a letter a week later (6), it was not necessary. As soon as Roberval received the first request, he sent off a letter containing his hypothesis - 4 August 1656. (7) Since he never wrote a treatise on his hypothesis, only the description presented in this letter, a brief outline in a letter from Chapelain to Huygens(8), and Huygens' description of the hypothesis in his *Systema Saturnium* (9) exist.

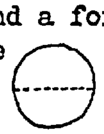
Roberval starts by affirming his belief that Saturn turns on its axis in much less time than the 16 days that it takes the newly discovered satellite to complete its circuit around Saturn. But in answer to Huygens' statement that his theory did not depend on Saturn's rotating, Roberval points out that his hypothesis too is quite independent of this rotation. Obviously, the reason why Roberval mentions it is that he had arrived at his theory by considering the rotation of Saturn on its axis - just as Huygens had done a few months earlier (see ch. 7).

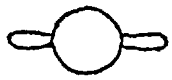
According to Roberval's theory, Saturn has a torrid zone in the form of an equatorial band, just like the zone on the Sun in which all sunspots are generated. At certain times vapours are 'exhaled' from this torrid zone, and these vapours rise and collect above the surface of the planet, forming a band all around the planet, parallel to the equator. These vapours, which are much less dense than the terrestrial clouds or fogs, can be quite far removed from the surface of the planet. At certain times these exhalations completely fill the space above the torrid zone, like a terrestrial fog, while at other times a space of free transparent air is left between the surface and the collected exhalations, as is the case with terrestrial clouds. But since the exhalations are not as dense as earthly clouds or fogs, they remain transparent unless they are very thick.

With these suppositions, regardless of whether Saturn turns on its axis rapidly, slowly, or not at all, the different appearances are explained by Roberval as follows: Saturn appears solidly oval when all the space above the torrid zone is filled with exhalation, and the elongation is, of course, East-West because the torrid zone is an equatorial zone. When there is some space left between the exhalations and the surface of the planet, Saturn will appear flanked by two stars, because only where a double thickness of exhalation is presented to us will the exhalation be opaque. At all other places the exhalations are thin enough to be transparent. When there is a great distance between the surface and the exhalation, and the band of vapour is narrow, Saturn appears handled. The solitary appearance will occur when there are no exhalations. (10)

This is only a brief outline of a theory and it obviously leaves many questions unanswered. Roberval realises this, and his statement that the complete details would require too much space for a letter was probably an excuse for not supplying details which he did not have. But he stated that he believed it to be faultless and that by means of it he had been able to answer all the objections of the 'sçavans'. Optics would show Huygens the way, and a figure, which Huygens could easily draw himself, would be of help. (11)

Huygens' response, framed in very polite language, goes right to the heart of the matter:

> Vostre hypothese pour Saturne est certainement tres bien imaginée, et n'ayant point d'autres phaenomenes a consilier ny d'observations plus exactes, vous ne pouviez pas peut estre mieux rencontrer. Je m'estonne toutefois que vous ne faites aucune reflexion sur le temps periodique de toutes les diverses apparitions de Saturne, qui reviennent tousjours successivement et deux fois en 30 ans. Si les anses estoyent produites d'une exhalaison, il n'y a pas beaucoup d'apparence qu'elles renaistroient si precisement a des certain temps, et le quiteroient de mesme. la forme ovale que du commencement quelques uns ont observée a esté causee de l'imperfection des lunettes dont ils se sont servy. autrement le corps du milieu de ce planete paroit tousjours rond a fort peu pres. Cette année je l'ay veu tousjours de cette forme  l'année precedente il me paroissoit tel

lors  que tous les autres observateurs la voyent
ainsi  : mais avec des lunettes qui ne leur decouvroit
pas le nouveau satellite, d'ou il s'ensuit que les mienes estoient meilleures. (12)

Thus, Huygens thought that Roberval's theory was a likely story, but since it was based on bad observations, it could not be right. Furthermore, the theory did not explain why certain appearances should occur at certain points in Saturn's orbit, as they do.

These criticisms, devastating as they were, did not change Roberval's mind. In April 1658 he still advocated this theory at the weekly scientific assembly held at the house of Habert de Montmor. Chapelain, who at this point already knew Huygens' ring-hypothesis, made some objections to Roberval and showed him some observations that Huygens had sent. Roberval, still ignorant of the ring-hypothesis, then expressed the opinion that Huygens had either borrowed or copied his hypothesis from his ideas. (13) When in a subsequent meeting (the attendance of which has so often been quoted from Chapelain's letter) Chapelain revealed Huygens' ring-hypothesis, with Huygens' permission, Roberval was convinced that Huygens had borrowed nothing from him, but he was not convinced of the superiority of the ring-hypothesis, and clung to his own. His reason for this was that his contrivance was completely natural, whereas Huygens' ring was ' ... une machine toute d'art et dont il n'y avoit aucune image dans la Constitution du monde' (14), a criticism which was to be repeated by others.

Huygens again raised the same objections but went so far as to say that had Roberval had better observations at his disposal, he would certainly have arrived at the same hypothesis as he had done, except that Roberval's ring would have been of a light material that is dissipated at various times, whereas his ring was solid and permanent. (15) It seems that even after the publication of Systema Saturnium in which Huygens again raises the same serious objections to Roberval's theory (16), Roberval still did not immediately reject his own hypothesis.

But in all his objections Huygens did not come to grips with the strong points of Roberval's theory. For Roberval's statement that Huygens' ring was 'une machine toute d'art', while there were plenty of precedents for his own device in the known Universe, was a point that could not be ignored, as will be shown in chapters 8 - 11. Roberval's geometrical, or astronomical solution was indeed hardly worth the name of a theory: any shape could be explained by such a device! But the strength of his ideas lay in the structural approach to the problem. The idea that the formation about Saturn, whatever its shape, that gives rise to all the appearances, was made up of some sort of vapourous emanation was a very attractive one. Roberval was not the only scientist to arrive at this idea: Wren arrived at it independently, and Borelli and Magalotti of the Accademia del Cimento thought it was the most logical solution to the problem of the generation and stability of the ring. Huygens' reasons for rejecting such a solution will be discussed in chapter 7. Obviously, Roberval had added an important idea to the study of Saturn's appearances, an idea which was by seventeenth century standards a very reasonable one.

*　　*　　*

Another response to the challenge of Huygens' De Saturni Luna came from Sicily. Upon receiving a copy of the tract (probably early in the autumn of 1656), Johannes Caramuel Lobkowitz (1608-1682), the Vicar General of England in absentia, sent a copy of it from Rome to Giovanni Battista Hodierna (or Odierna) (1597-1660), a priest and the mathematician of the Duke of Palma. (18) Hodierna was a man of many talents. He wrote on the microscopic appearance of the eye of a fly (19), the colours seen through a prism (20), on comets (21), and he prepared the first published ephemerides of the satellites of Jupiter. (22) He was also the first person to appreciate the role in reproduction of the queen bee.

When Hodierna read De Saturni Luna, he was, by his own testimony, so aroused that he rushed into print his ideas about Saturn which he had had for some time, so as not to be defrauded of his priority. (23) He must have written and published his thoughts rather quickly, because communications between Northern Europe and Italy were bad, and Huygens knew no one in Italy to whom he could send his tract. We find Erasmus Bartholin (or Rasmus Berthelsen) (1625-1689), the Danish mathematician and physicist, writing to Viviani from Paris on 26 July 1656 that Huygens had discovered a moon of Saturn and had published a tract on it. (24) It seems therefore fair to say that the tract itself, or rather a handwritten copy of it, probably did not reach Italy until the autumn of that year. The tract of Hodierna, Protei Caelestis Vertigines seu Saturni Systema, was dated 20 December 1656 and bears the publication date 1657. It is divided into five chapters or parts. Chapter one is in the form of a letter to Caramuel, to whom the tract is dedicated;

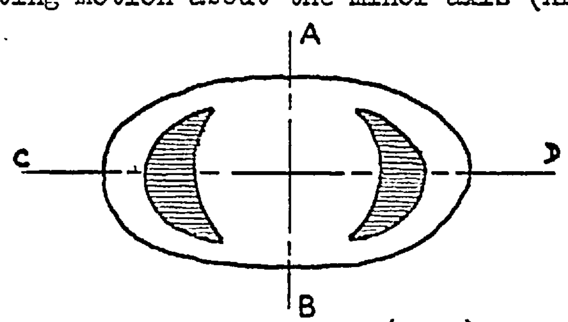
chapter two describes Saturn's shape in detail and gives a number of descriptions of observations made by Hodierna; chapter three develops a 'shorthand' nomenclature for the different phases of Saturn; chapter four gives a table of the phases and where in the zodiac they occur; and chapter five is a letter to Huygens.

Hodierna begins by discussing Huygens' discovery of the new moon. He laments the fact that his telescope only 'expands' the diameter of an object twenty times, and this is why he has not yet been able to see this new moon. But it is indeed curious that this moon has not yet been seen in Rome with those marvellous telescopes of 30 or 40 palms, made by that very great artificer Eustachio Divini. (25) Perhaps the moon is one of those which Fontana claimed to have seen. (26)

After a brief paragraph about the marvels of the Saeculum Christallinum (the age of the telescope) and the progress that has been made with the telescope, Hodierna states Huygens' anagram on the system of Saturn and then, quite unnecessarily, posits an anagram of his own, containing the gist of his hypothesis, the solution of which follows immediately: Mole unica: Figura elliptica: binis maculis obducta, Aequatoris Planum respicit: He regards the plane of the equator, with one mass, and an elliptical figure covered with two spots. (p. 5) Hodierna explains what he means by this:

> Although Saturn appears triple like a great celestial Proteus, or like the Cerberus of the heavens, and fashions himself in different shapes, he is nevertheless simple, and his great single mass persists constantly in its entirety, but does not, like the rest of the heavenly bodies, rejoice in a roundness of body, but rather presents an elliptical or oviform figure, by virtue of which he represents a chicken's egg, or portrays the likeness of the fruit of an olive, plum, or palmtree. The surface is spotted like the Moon, where it shows raging oceans enclosed by land. (p. 5)

Furthermore, Saturn has a rotating motion about the minor axis (AB) of the ovoid body, making one complete revolution in every period. Thus, twice during Saturn's journey around the Sun, the major axis (CD) is pointed towards the Earth,



and the body, seen from the small end, appears round. (p. 5)

On the subject of the direction of the anses, Hodierna defines a 'planum libramentum', which is the plane perpendicular to the axis of rotation AB at the moment when the longitudinal axis CD passes through the Earth. Since Saturn's orbital plane is inclined $2\frac{1}{2}^{\circ}$ to the ecliptic, Saturn roughly follows the ecliptic in its journey around the Sun, and therefore moves away from this 'plane of libration'. But the axis of Rotation AB remains perpendicular to this plane. However, although Hodierna states in his anagram Aequatoris planum respicit, the plane of libration is not exactly the same as the plane of the equator. The two are slightly inclined and the nodes of the plane of libration occur at 21° Pisces and Virgo, slightly removed from the nodes of the equator. At these nodes, Saturn appears round, while at the greatest recession from the plane of libration, at 21° Gemini and Sagittarius, Saturn appears full-faced to us. (pp. 5-6) All these conclusions are drawn from data presented by Hodierna, which comes mainly from Riccioli's Almagestum Novum. Throughout this discussion, Hodierna does not mention Ptolemy, Copernicus, or Tycho, and states no preference for any system. He does not even state what system he uses.

A possible difficulty is foreseen in that when either end of the body is presented to the Earth, the spots on the hemisphere turned toward the Earth should still be partially visible. But the reason why we do not see these spots is perhaps that these areas are depressed or recessed, and we only see the lucid edges. (p. 8)

Next, Hodierna treats the apparent magnitude of Saturn, its colour, and the effects of the changes of the planet's shape on Earth-dwellers. The length of the anses was measured by Hodierna to be between 70" and 77", while the diameter of the central disc was between 28" and 30", and the width of the 'extreme lucid zones' 11". This was when Saturn was at perigee and near conjunction. In fact, at perigee and near conjunction, the apparent diameter of the body should be about 15". Hodierna's measurements were thus too large by a factor of two - no doubt due to the colour fringes caused by aberrations. But according to Hodierna, the central disc always appears brighter than the extreme zones. Furthermore, Saturn is so bright that, in view of its great distance from the Sun, its surface must be made of some substance like plaster of Paris, gypsum, or white lead, which can 'multiply the rays of the Sun in an astonishing manner'. (p. 9) The colour of the light is not leaden but rather like straw.

As to the effects of Saturn on the Earth and its inhabitants, they follow the aspects of the planet: when Saturn shows his full face, with the turbulent spots at their largest, obscure and turbulent effects follow. But when it is seen round with no spots, the effects are more clear and simple. Astrologers are welcome to use these principles in their work. It is difficult to determine whether Hodierna was serious about this, which is quite possible, or whether he just added it to please Caramuel, who, according to Hodierna, believed in astrology and also believed that Saturn's effects change as the planet changes its shape. This part of the tract ends with a plea for more and better observations. (p. 10)

The second chapter, dedicated to the Duke of Palma, is very curious indeed. By means of the figure shown in fig. 1, Hodierna demonstartes how to construct the full-faced figure of Saturn geometrically. B, C, and D divide AE into four equal parts and AG and EF are each equal to three of those parts, so that GF : AE = $2\frac{1}{2}$: 1. H and I divide AG and EF into halves; JHK is a third of a circle with A as centre, while PGQ is a third of a circle with H as centre. PTN and QVO are similar circular segments.

Hodierna explains rather vaguely how this figure varies as Saturn moves through its orbit. First, the regions PGQH and NFOI change from a parabolic form (which they are not, because he has just shown them to be circular segments) to a circular form, so that they resemble handles. Then these regions become compressed and more round, the connecting legs becoming thinner and the dark regions contracting, until the handles have taken on the shape of globules. The transition from the tri-spherical shape to the round shape is not dealt with at all. Hodierna leaves a fuller account to others who have more time and better telescopes. (p. 13) This chapter ends with descriptions of a number of observations of conjunctions of Saturn with various fixed stars, by means of which Hodierna has determined the direction of the line of the anses. (pp. 13-16)

Chapter three shows how symbols for all the phases of Saturn can be composed from the letters C, D, and O, of the Roman alphabet. These symbols are then used in the table shown in fig. 2, which makes up chapter four. The table shows the recession of the planet from the plane of libration, analogous to the annual sequence of deviations of the Sun from the equator (column 2); the phases (column 7) and the positions of Saturn in the zodiac where these phases occur (columns 3-6)

Finally, chapter five is a letter to Huygens, in which Hodierna explains how De Saturni Luna came into his possession, and that his tract is an answer to Huygens' challenge. Huygens' moon, which at that time described a straight line across the face of Saturn, would, if it retained its orientation with respect to the anses, in time describe an apparent ellipse about Saturn. Hodierna trusts Huygens to make careful observations with his excellent telescopes and asks that if Huygens should detect something new, he will let him know right away. (pp. 21-24)

Protei Coelestis Vertigines is a very strange tract. Because of grammatical and typographical errors and ambiguous wording, not to speak of Hodierna's obscure style of prose, it is rather difficult to follow. There are also many puzzling aspects. Why, for instance, should Hodierna state an anagram of his own, only to give the solution in the very next sentence? Perhaps he wanted to show that he too could make up anagrams, or perhaps he did not fully understand the purpose of a scientific anagram. Furthermore, what is the purpose of the curious notation to distinguish the different phases, and why should he insist on showing how to construct the full-faced figure of Saturn? This construction confuses the issue, since he uses circular sections, while in the rest of the traot he speaks of either elliptical or parabolic sections.

On the central issue of the tract, the first thing that comes to the attention of the modern reader is the shape of the open phase which Hodierna takes as his starting point. As stated on the title page, he observed this form from 1646 to 1653 (see fig. 3). It is as good an image of what could be interpreted as a ring as had been drawn before Huygens actually drew a ring. And it was seen with a telescope that magnified only twenty times. Surely this shows that as far as the telescope was concerned, there was nothing to prevent observers from recognising a ring when Saturn was in its most open position, even at a very early date. The telescope only presented a problem when Saturn was almost round, and, significantly, the problem was solved when Saturn was in just such a position.

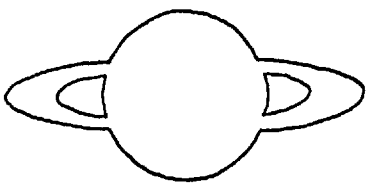
But Hodierna was in exactly the same dilemma as Hevelius was. The tri-spherical figure could not be explained in terms of the primary shape. But Hodierna does not really make an effort to deal with this problem, as Hevelius had done. He does not tell the reader why the anses should become narrower in a latitudinal direction, and he does not deal with the transformation from the tri-spherical to the round appearance at all. Furthermore, when one of the small ends of the body is presented to the Earth, the dark spots should still be visible as narrow streaks on the sides. The ad hoc device of recessing the dark spots in order to overcome this difficulty is not very satisfactory.

Hodierna's theory predicts that Saturn has a rocking motion of $23\frac{1}{2}^{\circ}$ each way, with respect to the Earth. This should result in an easily observable rocking back and forth of the spots, superimposed on the rotation of the body. This effect is completely ignored.

Although the plane of libration is defined by the longitudinal axis passing through the Earth, there is no reason inherent in the system why the major axis of the ellipse should pass through the Earth exactly at the time when Saturn passes through this plane. If this were not the case, there would be no reason why Saturn should ever appear round from the Earth. But yet, the realisation that the plane perpendicular to the axis of rotation remains parallel to itself and is roughly parallel to the plane of the Earth's equator, and not to the ecliptic was very important. If Hodierna had realised, as did Wren and Huygens that the anses keep their length, he might have used the rocking motion (with respect to the Earth) predicted by his theory to explain the changes in appearance, rather than ignoring this motion.

Hodierna sent his tract to Huygens early in 1657 and it reached Huygens a year later, in February 1658, by way of Michel Angelo Ricci (1619-1692) (later a member of the Accademia del Cimento) in Rome, who was in correspondence with Réné François Sluse (1622-1685), a canon at Liège. Huygens did not reply to the tract and letter until 14 September 1658, when he sent Hodierna (by way of Sluse and Ricci) a copy of his Horologium, which contained a full description of his newly invented pendulum clock. Huygens told Hodierna that he preferred his theory to that of Hevelius because he himself agreed with Hodierna and Riccioli that the anses were parallel to the equator and not to the ecliptic (or rather Saturn's orbital plane) as Hevelius maintained. Moreover, Hodierna's values for the longitudes at which the round phase occurs were better than those of Hevelius.

Huygens also sent Hodierna a sketch of how Saturn had appeared to him lately and told him that he would not be surprised if Hodierna could now guess his hypothesis. (27) In his Systema Saturnium Huygens also



dealt with Hodierna's theory. He pointed out that it did not agree with the appearances he had observed in 1655 and 1658, that the spots should still be partially visible when Saturn is round, and he asked Hodierna to paint two dark spots on an egg and discover for himself that his theory could not agree with the observed phenomena. (28) But Hodierna never saw Systema Saturnium; before it arrived in Sicily, he had died. (29)

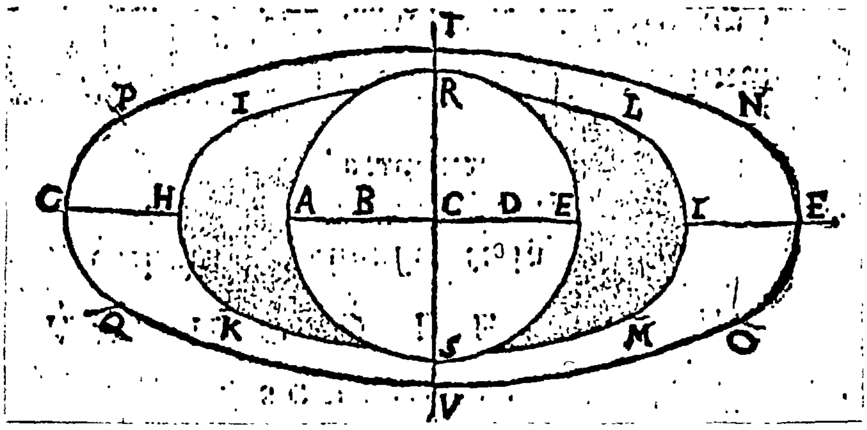


Fig. 1

INDEX RECESSVVM,
in quibus Saturni Phases transmutandæ veniunt,
signis Zodiaci perpetuò iure
congruentium.

Recessus numerati	Declinatio Par.	Min.	Zodiaci Signa	Gr.	M.	Eclypticæ Signa	Zodiaci Gr.	M.	Eclypticæ Phasiũ difformit.
o	o	o	♓, ♍	21	o	♓ ♍	21	o	O
I	3	22	♓, ♍	29	28	♓ ♍	12	32	°O°
II	6	43	♈ ♎	8	3	♓ ♍	3	57	oOo
III	10	5	♈ ♎	17	o	♒ ♌	25	o	OOO
IIII	13	26	♈ ♎	26	36	♒ ♌	15	24	ɑOD
V	16	48	♉ ♍	4	45	♒ ♌	4	35	DOD
VI	20	10	♉ ♍	20	46	♐ ♋	21	14	ɑOD
VII	23	31	♊ ♂	21	o	♊ ♂	21	o	COↃ

Fig. 2

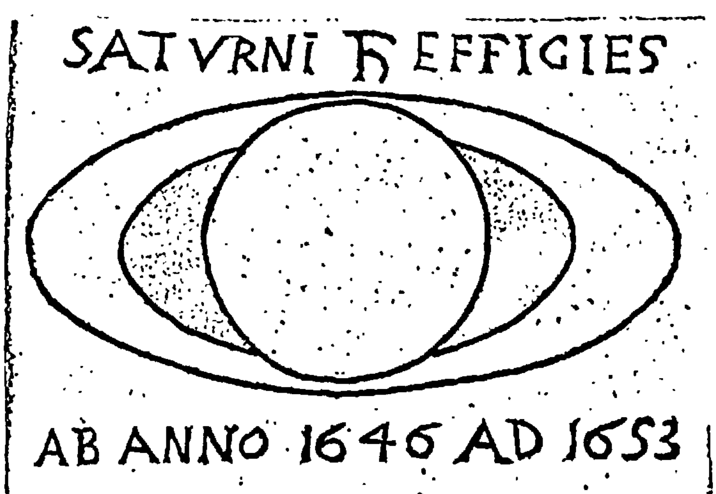


Fig. 3

CHAPTER 6

Wren

Among the many topics discussed by the men who assembled regularly in London during the 1640's for the purpose of scientific discussion, was the problem of the appearance of Saturn. (1) When the centre of gravity of this group shifted to Oxford, after 1648, the interest in this problem did not flag, and practical steps were taken toward its eventual solution. Among the men who gathered in Oxford, were a number with a keen interest in astronomy. John Wilkins (1614-1672), the warden of Wadham College since 1648, in whose quarters the scientific meetings were often held, had already established himself as a popular writer on astronomy and a champion of the Copernican system. (2) Seth Ward (1617-1689), Savilian professor of astronomy since 1649, gained prominence for his criticism of Bouillau's geometrical treatment of planetary orbits (3) and John Wallis (1616-1703), Savilian professor of mathematics since 1649, kept in close touch with events in astronomy. (4) From 1650 until he became Gresham professor of astronomy, Laurence Rooke (1623-1663), a bright young astronomer, who unfortunately died very young, was also at Oxford. (5) These men were aided in their practical astronomical work by the service of Dr. Jonathan Goddard (1617-1675) and Sir Paul Neile (1613-1686), who supplied them with telescopes made in their own workshops. (6) Christopher Wren, who entered Wadham College as an undergraduate in 1649, at the age of 16, rapidly assumed an important place in this group.

In view of his later career as an architect, the earlier scientific work of Wren has often been overlooked. (7) But until his work in architecture curtailed his other interests, Wren was a creative scientist of the highest calibre. He was Gresham professor of astronomy from 1657 (when he was only 25) until he accepted the Savilian professorship of astronomy at Oxford in 1660, a post which he did not give up until 1673. His work in astronomy earned him the respect of men like Flamsteed (8), who was not in the habit of paying unnecessary compliments, and his work on collision, communicated to the Royal Society in 1668, was an important contribution to the science of mechanics. (9) Wren was one of the few scientists to whom Newton acknowledged his debt. (10)

The work on Saturn in Oxford had its start in 1649. Both Wren and Wallis speak of a continuous series of observations of the planet going back to that year. (11) No doubt these observations were carried out in the observatory established by Ward in the tower of Wadham College. (12) But Wren's active interest in Saturn probably started in about 1654.(13) From the beginning there was close cooperation between Wren and Neile, and Wallis never mentions Wren in connection with Saturn without mentioning Neile in the same breath. (14) Besides Wallis, who apparently kept in close touch with Wren's progress, William Balle (1627-1690), who was an amateur observer, also kept the two men informed of his observations of Saturn. (15) It was Balle who first made an important new discovery. In 1655 he observed a dark band on the disc of Saturn, undoubtedly the shadow of the ring on the body. (16)

When Wallis received a letter from Huygens in 1655, in which Huygens sent the anagram containing his discovery of Saturn's satellite (17), Wallis sent Huygens an anagram of his own, which, he claimed, contained an astronomical discovery made in England. (18) But in fact, it was merely a judiciously selected collection of letter, which Wallis, upon being given the solution of Huygens' anagram, made to represent the same discovery. (19) When he later admitted his hoax to Huygens, he claimed that one of his reasons was to protect any claim that Wren and Neile might have to the discovery Huygens was hiding in his anagram. (20) Although he did not know, of course, just what Huygens had discovered, he had surmised from the wording of the anagram (21) that it had something to do with Saturn, and since Wren and Neile had been observing that planet for some time by then, he thought they might have made the same discovery. In fact, Wren and Neile had observed this satellite more than once, but without realising that it was a satellite. (22)

By January 1656, Wren was making models in an effort to solve the problem of Saturn's appearances (23), but, according the Wallis, little progress had been made by April 1656:

> Furthermore, as regards the phases of Saturn, which we have observed, we have searched all this time for a figure of Saturn and his anses and an hypothesis of his motion that will satisfy those phenomena. But after having observed all [appearances] one after another, we can still by no means boldly state an opinion. (24)

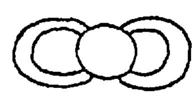
There is little doubt that by 'we' Wallis meant Wren and Neile as well as himself.

Four months later still no progress appears to have been made, as Wallis wrote:

> I have nothing new to say about Saturn. Until his handles are restored, we shall await rather than predict the event. I hope to communicate to you shortly, when the fog ceases, the phase of Saturn seen here. That promising young man, Christopher Wren, will, of course, see to it for me. (25)

But in an appendix to the same letter Wallis sets out some thoughts which he and Wren had concerning Saturn:

> We had imagined at least two forms of Saturn: a simple one, satisfied by one motion, which appears preferable. But if we admit this one, not a few of the observations left to us by others are to be rejected, or at least corrected ... The other, which corresponds to the phenomena, even those conveyed by others (for the most part), but which rendered the form of Saturn quite misshapen, ... and which was seen to require twin motion ... upon related axes ... But if some of them can be rejected or corrected (which every day appears more and more necessary) that simpler form and simple motion appears to satisfy. Indeed, that [theory] was such that to the body of Saturn two little handles are attached (with roughly that form which, at least according to us, appears as some form of attached arches, as if they were handles), namely thus:



> It is moreover moved about the longer axis, whence arises the opening and closing of the handles. And, in fact, supporting this theory fully, we had seen the handles long and open for several years, gradually closing, until arriving at a long arm on either side ... and indeed with only the edge turned toward our eyes, that edge can be of so little thinness that it disappears and only the spherical body of Saturn appears. Because we have not observed that transformation from the form seen with the long arms to the round form, our observations of whether a thinning out and disappearance occurs, or whether a shortening happens, are l$acking, which lack perhaps you can supply from your observations. (26)

There are three important points in this passage: the growing realisation that not all observations depicted in astronomical sources were reliable, the rotation about the major axis of the anses, and the extreme thinness of the anses. Obviously, Wren and Wallis had done quite a bit of thinking about the problem, but they were not yet able to decide whether they could reject some of the observations of others - perhaps even some of their own - and arrive at a simple hypothesis, which they preferred, or whether a shortening of the anses actually occurred, in which case they would have to add another rotation, probably about the minor axis, as in the hypotheses of Hevelius and Hodierna. This decision would have to wait until they had observations bearing on this aspect. Apparently Wren and Neile had not been able to make such observations when Saturn was about to lose its anses. The anses were visible in 1655 right up to Saturn's heliacal setting in June, but after its heliacal rising later that year they were invisible and Saturn appeared solitary. Perhaps the telescopes used by Wren and Neile in 1655 were not yet good enough to make the necessary observation. Saturn remained solitary throughout its period of visibility which ended in June 1656, and after its heliacal rising in October of that year, the anses were once again visible. But Wren and Neile were not able to make the crucial observation until December 1657, a full year after the anses had again become visible. According to Wren

> ... this kind of Saturne was ... hatched ... at Whitt-Waltham, uppon the observation of December. 3 1657 [o.s.] when first wee had [an] apprehension that the Armes of ♄ kept their length. wch produced [this] hypothesis ... (27)

It seems most likely that the reason for this delay was that Wren and Neile did not have sufficiently good telescopes before that date. This was the first time that they used Neile's famous 35 foot telescope, which was good enough to show the shape of the narrow anses more accurately than their other telescopes had been able to do. The fact that this observation was made at Neile's house, where his telescopes were made, and not at Oxford or London, supports this contention. Furthermore, this 35 foot telescope was erected at Gresham College in 1658 for Wren's (and presumably Rooke's) use. (28) Thus, it seems reasonable that it had just been finished when the observation of 13 December 1657 was made.

Once Wren knew that the anses keep their length, he quickly completed his theory and made several models, one of which, in metal, was put on top of the 'Obeliske' erected at Gresham College to accomodate Neile's 35 foot telescope. (29) Wren lectured on his hypothesis in his astronomy lectures at Gresham College, and he was planning to write a full and detailed treatise on the subject. He was however 'enjoyned to give that short & generall account of it' (30) and produced a short treatise in manuscript form, which circulated among a very few of his friends. It appears that Wren never went beyond this, perhaps because of the unsettled conditions at Gresham College in 1658, and when he read about Huygens' hypothesis, early in 1659, he promptly rejected his own. (31)

The tract is entitled <u>Christophori Wren Londini in Collegio Greshamensi Astronomiae professoris De Corpore Saturni ejusque Phasibus Hypothesis</u>. It is an outline of a theory rather than an exhaustive treatise, and gives no supporting data, or quantitative information as to where exactly the different phases occur. Although it was never published, it found its way into the Huygens correspondence and is therefore printed in the Oeuvres Complètes, in the original Latin. (32) A translation of the text can be found in the article in Appendix D.

The tract starts with the praise of Galileo and his 'crystal sceptre' with which he single-handedly revealed all the mysteries of the heavens. The only things that remained to be done by his successors was to describe the Moon's appearance more accurately and to show the different phases of Saturn. According to Wren, Saturn is the greatest test for the ever improving telescopes:

> This is the target upon which they aim their artfully strengthened vision and they strive to bind this most deceitful star with the laws of a particular hypothesis. For Saturn alone stands apart from the pattern of the remaining celestial bodies, and shows so many discrepant phases, that hitherto it has been doubted whether it is a globe connected to two smaller globes or whether it is a spheroid provided with two conspicuous cavities or, if you wish, spots, or whether it represents a kind of vessel with handles on both sides, or finally, whether it is some other shape. For without motion and some rotation of the body, even ten different forms of the body would not suffice, although a single body, diversely rotated, could very well account for the observations worthy of consideration. On the other hand, it has not been possible thus far to devise one shape so flexible as to be in sufficient agreement with all observations taken indiscriminately. And certainly, because observers did not often use very long tubes and absolutely perfect lenses (of which there is need) and did not take good enough care to remove completely all superfluous light fringes from the aperture in the customary manner, or because they were unaccustomed to depict graphically on the spot just what they saw distinctly, it came about that they left us very disparate figures, so that if any one chooses to construct an hypothesis which may agree accurately with all the sketches published lately by Galileo, Fontana, Gassendi, Riccioli, Hevelius, and others up till now, he wastes his time completely, for he impedes himself with so many contrary motions of the anses, that it is necessary either to give plastic wings or handles (according to taste) as attendants to a monstrous star, or to make it protean and animate. Indeed, at certain times and intervals nothing will come out right and nothing agreeable to the uniform and beautiful harmony of natural motions is portrayed. (33)

This is indeed a very eloquent statement of the problem. Wren again points out the dilemma between a simple hypothesis, necessitating the rejection of a large number of observations, and a very cumbersome and inelegant hypothesis, if all the data are to be used. But he has clearly made up his mind. A simple hypothesis is to be preferred. Yet, a problem remains: one cannot arbitrarily discard data, one must explain how and why they are erroneous, in this case, by the use of optics:

> But those sketches are not, therefore, to be rejected as being altogether deceptive, because it cannot be that the telescope represents things that have no existence at all in nature. No one will deny that indeed things can appear otherwise than they really are, for the telescope has all the treachery of the naked eye, and, in addition, those that generally arise from the imperfection of the instrument. But these are both things that cannot be concealed from the experienced observer and practised optician, so that he readily takes notice of them and substitutes genuine phenomena for erroneous ones, especially if he makes use of not one, but several telescopes at the same time. (34)

The statement about the 'experienced observer and practised optician' is, of course, just rhetoric. What differentiates this observer from the 'less experienced' observer and 'less practised' optician, is only the use of a very good telescope in this case. Wren had one excellent telescope, the 35 foot effort by Neile, and a number of lesser quality. After seeing the 'true' appearance through the best telescope, one checked how this appearance was portrayed by the inferior ones, and explained the differences as best one could by means of what was known of optics.

Wren then explains his hypothesis. With the good telescope, he found Saturn to be 'exactly spherical [obviously a reference to Hevelius] and variegated with spots'. (35) These telescopes were thus still incapable of showing the oblateness of Saturn's body, which is, in fact, quite pronounced. (36) The spots refer to the dark band across the globe, first noticed in England by William Balle, which Wren could only barely make out. Saturn is surrounded by a thin elliptical corona, which touches the central globe at two opposite points on the equatorial circumference (fig. 1). As to the thickness of this corona, it '... is not sufficient to be seen in any way by the inhabitants of the Earth, and for this reason the corona may be taken as a mere surface.' (37)

The body and corona rotate about the major axis of the corona once in every period of Saturn, such that near aphelion and perihelion the corona is at right angles to the plane of the orbit, and at mean longitude the corona is in the plane of the orbit, and presents its edge to the Earth and the Sun. It is therefore invisible at mean longitude, so that Saturn appears round. But since the axis of rotation coincides with the line of the anses, this means that this direction is always parallel to Saturn's orbital plane.

Wren then sets out the major phases as follows: the diameter of the body is divided into twelve digits; when the point of contact of the corona, as seen from the Earth, is at the centre of the disc, Saturn is 'unarmed (<u>Inermis</u>); when the point of contact is one digit removed from the centre of the disc, the phase is called 'cusped' (<u>cuspidatus</u>), as in fig. II; at a distance of two digits, Saturn appears 'dart-like' (<u>spiculatus</u>), as shown in fig. III; at four digits, the darts become more blunted and take on the appearance of handles and Saturn is called 'handled' (<u>ansulatus</u>), as in fig. IV; finally, when the point of contact is six digits removed from the centre of the disc, that is, on the outside periphery, the phase of Saturn is called 'full' (<u>plenus</u>), see fig. V. These are the theoretical figures predicted by Wren's theory.

But, in fact, they are not seen exactly like that, especially when the point of contact is near the centre. Thus, in the 'cusped' phase a different appearance is seen:

> ... the cusped shape is seen in the telescope not because it is really like that, but because of diffuse light and weakness of vision, as for instance the new Moon spreads its image beyond the actual limits of the disc, so that the luminescence is seen to thrust out beyond the circumference of the dark part (as also happens to every white object placed against a black one). So, in the case of Saturn, the apparent shape gains a little around all its real edges, and makes the shape broader. Whence it comes about that (in fig. II) the parts bc and bd come together more quickly than ought to happen at b, and the parts around b appear to be nearer the body, because the narrow spaces made by the extremely acute ellipse bcd are wholly filled up by the neighbouring light of the cusps; so also the parts c and d, although luminous, escape from sight because of their thinness. For this reason, instead of appearing in the true cusped shape, Saturn is seen with its arms detached from its body. [see fig. 2]. (38)

Wren likewise explains how the appearances shown in fig. 3 - 6 come about.

The belt on Saturn, which appeared to Wren in the form of four spots (see fig. I), was first seen by William Balle, who showed it to Wren. Although Wren cannot see the whole band, he is sure that it exists. This belt, according to him, would substantiate his hypothesis of the rotation of Saturn about the major axis of the corona, because the belt coincides with the colure to which the corona is attached, and thus, as the planet rotates, the dark band should move up, or up and down (in the case of reciprocation) the disc.

Wren criticises Hevelius' hypothesis because the length of the corona remains the same, which is not predicted by the Hevelian hypothesis. Furthermore, the cusps do not separate from the body gradually, as Hevelius would have it, but all at once. Wren then states that

Hevelius is no happier in his discussion of the inclination of the anses. Since Wren substantially agrees with Hevelius on this issue, it appears that this criticism is directed at the involved way of trying to reconcile the inclination with observations which show it to be parallel to the equator.

Perhaps Saturn in fact librates on the axis, rather than rotating; perhaps the maximum inclination of the corona is variable, and perhaps the corona even turns independently of the main body. All these things would become obvious during the next few years. Precise quantitative data and tables are left for a fuller treatment, which Wren obviously planned when he wrote this. A treatment of the newly discovered moon, which Wren states he has often observed with Neile, is left to Christiaan Huygens.

The nature of the corona is a difficult subject. The spots on the body of the planet show that the body itself is opaque and solid, but Wren believes that the anses cannot be solid:

> ... but to believe that the anses are made of solid matter, like vast arches built on the globe, exceeds credibility, especially since they have no thickness by which such a great mass, many times exceeding the Earth's diameter in height, could be sustained. What then? Is the corona merely an appearance like the halo or the rainbow? But this is ruled out by the varying appearances, which variation is nevertheless linked to the motions of the star. Lastly, is it a fluid? Nothing is more likely, and I hardly know if anything more suitable can easily present itself. For since the belt follows the motion of the anses, what is rather to be said than that only this spotted zone emits vapours, the rest of the globe being miserably barren? From which it follows that the globe is not totally surrounded by an atmosphere but only by a vaporous corona, which, like a cloud, drinks in the splendour of the Sun, and in turn gives back a visible glimmering brilliance. (39)

Thus, this dark band dovetails nicely with the vaporous nature of the corona.

Wren ends the tract by commenting on the sight presented to the inhabitants of Saturn by this wonderful corona, and by explaining his model by means of which all the appearances of Saturn can be simulated.

This theory shows at the same time the strength and the weakness of a theory that is arrived at by mental and physical model building. There can be little doubt that Wren had the appearances of Saturn right and that he could quite adequately explain the various 'erroneous' figures drawn by other observers. What is more, at the time when this theory was formulated, there was no obvious observational way of proving it wrong. It was borne out by good observations and there was no appearance of Saturn that it could not explain or did not predict, while there were no predictions in the theory that did not occur.

Starting with the correct appearances, the theory was arrived at quite naturally. Wren assumed that since in the most open phase Saturn appeared surrounded by an elliptical body, that body was indeed elliptical, and since in the most open position it was seen to touch the central body, it did indeed touch the central body. There is nothing unreasonable about this approach; it is in fact rather to be expected. Furthermore, Wren was certain that the anses kept their length. Therefore it followed that the narrowing and final disappearance was caused by a rotating or rocking motion about the major axis of the ellipse. Wren's ingenious mind shows itself in making the corona so thin that its edge is invisible from the Earth - the simple and elegant answer to the problem of its disappearance. But this thinness of the corona led Wren to the next logical consideration: structurally such a thin corona could not exist if it were solid. Therefore it had to be of a fluid nature, and what was more reasonable than to assume that, analogously to our clouds, it was made up of emanations from the planet? At this point, the dark band served nicely to explain why such emanations should only occur around a narrow band.

Until the 1660's, the shadow effects that could have made the problem somewhat different were not observed. Both Huygens and the English observers had seen a dark band on the face of Saturn in 1655 and this was the only shadow effect observed before 1660. But neither Huygens nor Wren believed this dark band to be a shadow! Thus, in 1658, when Wren wrote the tract, the appearances of Saturn were still entirely, so to speak, two-dimensional. Wren did not see Saturn <u>surrounded</u> by anything. He saw handle-like appendages attached to a round disc. Thus, he built a model which had all the two-dimensional attributes observed built into it. But it never occurred to Wren that, just as the central disc is (presumably) the two-dimensional projection of a sphere, the elliptical corona could be the projection of a circular ring.

One difficulty with Wren's hypothesis was the actual shape of the corona. Wren thought that the inside as well as the outside edge of the corona were elliptical, but in such a way that their minor axes coincide. This common minor axis was of course the diameter of the central globe. It was perhaps rather difficult to prove this supposition false in 1658, when the ring, or corona was not yet very much displaced from its edgewise aspect. But as the corona opened more and more, this deficiency would have become more and more obvious. An adjustment of the theory on this point might have been somewhat awkward, but it probably could have been made.

Wren was not the only one to arrive at this hypothesis. Bouillau and Riccioli also favoured it (the latter as late as 1665), both when they already knew Huygens' ring-hypothesis, as will be shown in chapter 11. This makes the case of Wren all the more curious, for when Huygens sent the gist of his hypothesis to Wallis by letter, early in 1659, and the latter transmitted it to Wren, Wren immediately became converted to Huygens' theory:

> ... but when in a shorte while after, the Hypothesis of Hugenius was sent over in writing, I confesse I was so fond of the neatnesse of it, & the Naturall Simplicity of the contrivance agreeing soe well with the physical causes of the heavenly bodies, that I loved the Invention beyond my owne & though this [i.e. his own hypothesis] be so much an equipollent with that of Hugenius, that I suppose future observations will never be able to determine which is the trewest, yet I would not proceed with my designe ... (40)

Thus, although Wren did not think (as late as 1661) that observations could show which of these two theories was correct, he rejected his own in favour of Huygens' because the latter was more elegant. This is an exceedingly rare occurrence among seventeenth century scientists and it stands in welcome contrast to the more usual distasteful squabbles among men like Huygens, Hooke, and Newton.

Wren's theory, which existed only in a single manuscript copy might have been lost or forgotten, had it not been for a coincidence. In August 1661, Bernard Frenicle de Bessy (1605-1675), a French councillor and mathematician, wrote a letter to Sir Kenelm Digby (1603-1665), in which he put forward a theory on Saturn which had some similarities to Wren's theory (see ch. 11). Digby read the letter at a meeting of the Royal Society, at which both Wren and Neile were present. Neile, who knew Wren's hypothesis, pointed out that Wren had had a similar hypothesis some years ago. When Wren hesitatingly confirmed this, he was asked to submit a copy of it to the ammanuensis, so that a copy could be made and forwarded to Frenicle. (41) Wren had his doubts about this because he did not like what he thought an imperfect theory revealed to the world, especially since Huygens' complete treatise on the subject had already been published. (42) But after some prodding by Neile, he obliged and sent the only existing copy of the tract to Neile, accompanied by the letter of 1 October 1661 (o.s.), which has been quoted several times in this chapter. Wren asked that Neile keep the tract in his hands and return it to him when he was finished with it, implying that he did not want it to circulate, or have more than the necessary copies made. (43) But a number of copies were in fact made of the tract before it was returned to Wren. (44)

Huygens who had seen the model of Wren's hypothesis on a visit to England in 1661 (45), made only one objection to Wren's theory in 1662. This was that the phase of Saturn visible in 1662 clearly showed that Wren had been wrong about the shape of the corona, because the anses did not become thinner toward the points of contact with the body, as Wren's theory predicted. (46)

Frenicle's objections to Wren's theory were basically two-fold: the shape of the corona did not agree with the observed shape, and the vaporous emanation, which he thought would probably have to be caused by the Sun, occurred only in the zone shown by the dark band according to Wren, regardless of whether the Sun's rays were incident on this zone perpendicularly, as when the corona is edgewise to the Sun, or whether they hit this region almost at a tangent, as when the corona is completely open, or even regardless of whether that zone is completely hidden from the Sun, when it is on the back face of Saturn. (47)

Wren of course never said that the vaporous emanations were caused by the Sun, but Frenicle does raise a valid point: if one wants to use emanations, analogous to the Earth's clouds, one must explain them properly, and Wren's hypothesis does not cover this point adequately.

But in comparison with the hypotheses of Hevelius and Hodierna, Wren's hypothesis is very sophisticated. Allowing for two modifications, the inclination of the anses and the shape of the corona, the hypothesis actually predicts the observed shapes, and explained the observations not agreeing with it. Neither Hevelius nor Hodierna had been able to do this. Wren had overcome the difficult problem of the diminution of the anses in two directions by showing that the diminution in length was in fact illusory, leaving only the narrowing of the anses to be accounted for. Moreover, Wren added an important ingredient to the eventual solution of the problem: the extreme thinness of the anses.

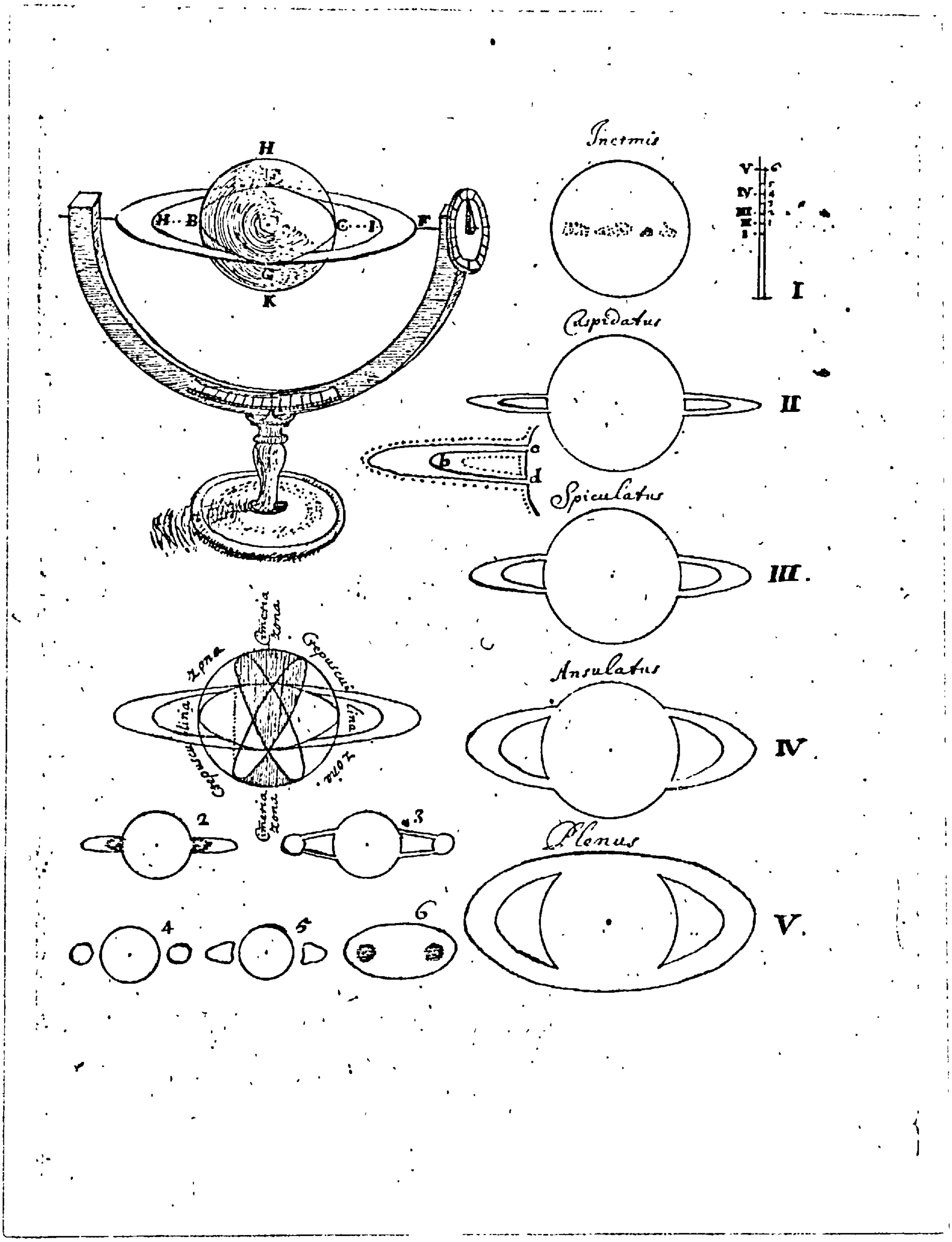


Fig. 1

Wren's sketches explaining the various appearances of Saturn.
From De Corpore Saturni

CHAPTER 7

Huygens

Christiaan Huygens (1629-1695) has been one of the most underrated scientists of the seventeenth century. This does not mean that during his lifetime he was not respected - far from it. But historians of science have to a large extent neglected him. There are a number of reasons for this. Huygens lived, historically, in the shadow of Descartes, and during his lifetime he was eclipsed by the rise of Newton. Furthermore, he spent most of his creative scientific life in France, preferring the intellectual (and financial) climate there to that of his more provincial native city of The Hague. Thus, during his lifetime he was perhaps less venerated in his native land, which, after all, was in an almost continuous state of war with France during the second half of the seventeenth century, while in France he remained at best a foreigner, and after the repeal of the Edict of Nantes, an undesirable heretic.

Although numerous literary monuments have been erected for men like Galileo, Descartes, Newton, etc., Huygens has been virtually ignored in the nineteenth and twentieth centuries, with some notable exceptions. First and foremost are, of course, the Oeuvres Complètes, collected and edited by the Hollandsche Maatschappij der Wetenschappen, containing all Huygens' works as well as his correspondence, but also including a series of splendid introductory articles by eminent Dutch historians of science such as Dijksterhuis and Volgraeff. Although Volgraeff wrote a lengthy running account of Huygens' life, in the last volume, this is rather a source for a biography than a biography itself, since it does nothing to interpret Huygens' achievements, or to put them in historical perspective. The only recent biographies are an inadequate effort by Bell (1) who is English, and a Russian work by Frankfourt and Fraink (2); the Dutch themselves have not produced any recent biographies.

The only endeavour of Huygens which has been dealt with fully is his optics. He is seen as the founder of the modern wave theory of light and several books and lengthy articles deal with this aspect. But Huygens was much more than an optician: he was a complete scientist. He did important work in mathematics, mechanics, astronomy, pneumatics, as well as optics, and made important contributions in areas that are more properly called technical than scientific, clock making, telescope making, and ballistics. Moreover, he can be regarded as at least the spiritual father of the modern heat engine. If Cartesianism before Huygens was a philosophical approach to Nature, Huygens was almost solely responsible for the establishment of Cartesian science, although in the process he drifted further and further away from Descartes' scientific ideas. Perhaps the only dimension, common to Galileo, Descartes, Newton, and Leibniz, which was lacking in Huygens was the philosophical one: Huygens never pretended to be a philosopher.

His scientific career started at an early age, and by the time he turned his attention to the problem of Saturn, at the age of 25, he already had several scientific publications to his name (3) and was entering one of the most fruitful periods of his scientific career. Between 1655 and 1660, when he was engaged on his work on Saturn, he was actively involved in solving mathematical problems, he did research in optics, which he put into practise in his telescopes, and made perhaps his most important overall contribution to science by constructing a practical pendulum clock (besides working out its theory). He also worked on the central problem of Cartesian mechanics, the problem of collision, as well as carrying on an ever increasing correspondence with notable scientists all over Europe.

When Huygens became interested in making his own telescopes, he rapidly progressed from the level of a novice to that of an expert. It is indicative of his great practical ability that although he only started to make his own telescopes late in 1654, by March 1655 he had already made an astronomical discovery. In this aspect, as in so many others, Huygens resembles Galileo, who progressed very quickly from his first experimental telescope to the best telescopes in Europe. As indicated in the article in Appendix ~~D~~ C, Huygens' reputation as a telescope maker has been overrated in all probability (not least by himself). He made several excellent telescopes, but his discoveries were much more the result of his talent as an observer and his powerful mind than of the high quality of his telescopes.

If Galileo rang in a new age when he directed his telescope to the Moon and Jupiter, Huygens revived the art of astronomical discovery when he turned his first good telescope to the planet Saturn. He had examined other planets to see if his telescopes could reveal anything new, carefully examining their vicinities for possible undetected satellites. This was a fairly common procedure among astronomers, whenever they made or obtained a new telescope, although apparently not all astronomers searched for satellites. Fontana and Rheita had claimed that they had discovered new satellites (4), but their claims had never been substantiated. But no satellites were seen by Huygens about the interior planets, nor Mars. Jupiter clearly showed its four Medician stars, but no new ones.

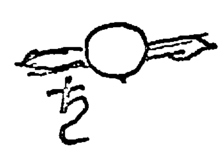
On March 25th 1655, Huygens noticed a small star roughly on the extension of Saturn's anses, not far removed from the planet. He suspected that it might be a satellite, and during the next few nights he proved his suspicion to be true, when the little star shared the motion of Saturn with respect to the fixed stars. Now Saturn's satellites all move roughly in the same plane as the ring, and since this plane is inclined to the ecliptic (unlike the orbital plane of Jupiter's satellites, which lies roughly in the ecliptic), the satellites of Saturn only describe a straight line across Saturn's disc when the ring is nearly edge-on. At all other times they describe apparent ellipses about the planet. In view of the motion of the Medicean planets it is therefore understandable that this satellite was discovered in 1655 when the ring was nearly edge-on, and not in say 1650, when its character was much less obvious. To this must be added the fact that when the ring is nearly edge-on the planet is much less bright, allowing a better view of the regions immediately surrounding it - especially in the seventeenth century when aberration was such a problem with bright objects. All five satellites of Saturn discovered in the seventeenth century were in fact discovered when Saturn was at or near its equinoxes.

Huygens did not immediately announce his discovery. He first determined the satellite's period, fixing it at 16 days, and only in June did he intimate to his fellow scientists that he had made a discovery by sending around the following anagram: (5) Admovere oculis distantia sidera nostris v v v v v v v v v v r r h n b q x. This line from a poem by Ovid (6) concealed the statement that Saturn had a satellite whose period was sixteen days. One immediate response came from John Wallis, who posited an anagram of his own, which, as it turned out later, had no solution at all (see ch. 6, p. 99). Huygens went on his first trip to France later that year and met most of the famous scientists who adorned the French capital at that time. Among others, he met Bouillau, Gassendi (who died in October), Roberval, Petit, Mylon, and made a very valuable acquaintance in the dilettante and poet Jean Chapelain (1595-1674), whom Louis XIV called at one time '... the greatest French poet who has ever lived.' (7) Huygens told the scientists about his discovery of the moon of Saturn, and Chapelain counselled him to publish his discovery. (8) Upon his return to The Hague, just before Christmas 1655, Huygens and his older brother Constantijn (1628-1697) worked on making longer telescopes, for the lenses of which they had to have the forms made (9), and between Christmas and the beginning of February 1656, Huygens arrived at his ring-hypothesis. (10) These dates are rather important. There is no evidence to suggest that Huygens formulated his hypothesis before his return to The Hague in December 1655. Saturn became visible to him, after its heliacal rising, on 16 January 1656, and on 8 February he wrote to Colvius:

> J'espere de vous montrer bien tost un bel effect de mes lunettes en vous envoyant le Systeme de Saturne, que j'ay dessein de mettre au jour, et qui enseignera la cause de toutes les differentes apparitions de cette planete. (11)

This indicates that Huygens had at that time only recently formulated his hypothesis, a contention which is supported by the fact that only after that date did Huygens approach his correspondents about their past observations of Saturn and any books that they might have, or know of, that contained observations of the planet. (12)

Since the new 23 foot telescope was used for the first time on 19 February 1656 (13), it is evident that any clue that Huygens may have got from his own observations was obtained with at best a 12 foot telescope (magnifying about 50 times) and not with the 23 foot tube as Bell states. (14) Furthermore, when Saturn became visible to Huygens on 16 January 1656, the anses had disappeared and he did not see them again until October of that year. Since his first recorded observation of Saturn dates from March 1655 (15), when the anses were very narrow as shown here, it is clear that Huygens never observed Saturn with anything that looked like a ring, before he formulated his hypothesis of the ring. (16)



How then did he arrive at the ring-hypothesis? Huygens himself tells us in his Systema Saturnium. The observation shown here gave him an important indication: even when the anses are very narrow, they have the same length as when they are at their widest. From this observation it was also clear that the tri-spherical appearance was an illusion, caused by inferior telescopes. It appeared that the anses became successively narrower until they disappeared. But Wren also knew this. Therefore, there has to be another factor. By considering the satellite of Saturn, in an analogy with our Moon, Huygens reasoned that if the Moon goes around the Earth in about 29 days while the Earth turns on its axis (roughly perpendicular to the orbit of the Moon) once in 24 hours, then perhaps Saturn turns on its axis, perpendicular to the orbital plane of the satellite in about half a day (as the period of its satellite is 16 days). Although Huygens does not state this, Cartesian vortices were probably a great help in this consideration. But perhaps all the matter between Saturn and the satellite turns about the same axis in some intermediate period of time. If that is true, then the only explanation for the fact that the anses do not change their shape over such a short period of time could be that the anses are of a shape which has rotational symmetry about Saturn's axis. At this point, Huygens realised that a ring would satisfy all these considerations. Whether this realisation came to him as a sudden revelation or only after arduous trial and error consideration, is not revealed by Huygens. (17)

Having thus arrived at the skeleton of the ring theory, Huygens' task was by no means complete. He realised that his theory would be judged for a large part on its quantitative aspects, i.e. its predictive value. But he only had observations dating back to March 1655. To fix the exact times of the various appearances he needed many observations, and besides, he wanted to test his hypothesis on all available observations in order to forestall criticism. Hence, in early February he started writing to various people with the purpose of obtaining as many observations as possible. On 11 February 1656 his old mathematics teacher at the University of Leyden, Frans van Schooten (1615-1661) wrote to him:

> I send you herewith Detectio Dioptrica [see p. 47], of which you spoke recently, and which I received yesterday ... (18)

And on 8 March Huygens wrote to Hevelius in the letter accompanying a copy of De Saturni Luna, delivered by his younger brother Philips (1633-1657):

> I beg that you will choose to commit to a letter those things for the understanding of which he is less suited [i.e. Philips], and especially if you happened to have noticed something new about Saturn. No doubt you have continued the observations from 1645 up to the present, which have not all shown the same figure to you. Should I find the consequences of my hypothesis have been observed in these variations, I should rejoice very much at that agreement. Only last year did I learn the art of telescope making, nor do I have anterior observations. Therefore, if I should be allowed

some observations of earlier date, recorded by your care and diligence, nothing could be more welcome to me. (19)

The formulation of the ring-hypothesis no doubt acted as an added incentive to publish the discovery of Saturn's moon, and the tract, *Christiani Hugenii de Saturni Luna Observatio Nova*, dated 5 March 1656, finally came off the press and was sent to Huygens' correspondents. It contained a description of the discovery as well as the determination of the moon's period. It also contained an anagram (this time merely a collection of letters rather than a line of poetry) with the promise that the 'system of Saturn' would be published as soon as it had been perfected. (20)

But this perfection took much longer than Huygens had expected. There are several reasons for this. Huygens had thought, based on his own observations and some calculations, that the anses would return before the end of April of 1656, and he had made this prediction in *De Saturni Luna*. (21) But in June, at Saturn's heliacal setting, they had still not appeared. Only when the planet became visible again in October of that year had the anses reappeared. In the meantime, Hevelius' dissertation had been published and Huygens was understandably anxious to finish his 'system'. But even after the anses had again become visible, the road was not smooth. On 8 December 1656 Huygens wrote Claude Mylon: 'Je travaille encore au Systeme de Saturne qui ne me donne pas peu de peine.' (22)

At this point, Huygens' energies were diverted to another, more important project. In December 1656 he invented his pendulum clock. Its construction, experimentation, and patent applications, all culminating in the publication of *Horologium* in September 1658, took precedence over his work on Saturn. Finally, on 19 September 1658, he returned to his work on Saturn (23), and finished his book in March 1659, after which the engraving of the figures and the printing took until the end of July. At long last, starting on 28 July 1659, the 'system of Saturn', promised three and a half years before, was sent out to his correspondents. (24)

But in the meantime Huygens' theory had not remained entirely unknown. Bouillau was the first to be informed of it by Huygens, when he spent some time in The Hague (on official business) in the summer of 1657. (25) Huygens made Bouillau promise to keep the hypothesis a secret and there is no evidence to suggest that Bouillau broke this promise. Under pressure from Chapelain, Huygens revealed his theory to him in the spring of 1658 (26), and after some further prodding, he allowed Chapelain to divulge it to the scientific gathering that was held regularly at the house of Habert de Montmor. (27) It is indicative of the greater importance Huygens attached to his pendulum clock that the secret of this device, contained in the same letter to Chapelain as the revelation of the ring-hypothesis, was to be kept an absolute secret. (28) Thus, by the spring of 1658, the ring theory was known in Paris to a fairly large number of people. (29) But it did not, apparently reach England from Paris. When Wallis admitted to Huygens, in his letter of 1 January 1659, that his anagram about the English co-discovery of Saturn's satellite had been a hoax, Huygens sent the solution to his second anagram and a brief description of his ring-theory to Wallis by return mail.(30), so that the English scientific community also knew the theory before the publication of *Systema Saturnium.*

It is unlikely that prior knowledge of the theory decreased the interest in *Systema Saturnium*: it rather raised curiosity. (31) And the readers were not disappointed by the book when it finally appeared. Whereas the tracts by Hevelius and Hodierna had been brief and left many questions unanswered, *Systema Saturnium* was a much more elaborate and carefully constructed tract, which greatly enhanced Huygens' reputation and did much to further his theory as well.

It starts with a dedication to Prince Leopold de' Medici, who had two years previously founded the Accademia del Cimento. Perhaps Huygens was hoping for patronage from this prince. Bouillau, who served as an intermediary between Huygens and Prince Leopold, probably had a hand in this. On 4 July 1659 he wrote to Huygens:

> ... vous pouvez juger que la dedicace que vous luy ferez de vostre escrit de Saturne sera receue de son Altesse avec tout le bon accueil & toute la faveur qui se puisse tesmoigner a une personne que l'on estime. (32)

But however much Prince Leopold may have admired Huygens, the kind of patronage that he extended to Nicholas Steno (1638-1686), a Catholic, was quite out of the question in the case of Huygens because he was a Protestant and he openly avowed his adherence to the Copernican hypothesis in the dedication. (33) After Galileo's trial even the powerful Medicis had to be very careful on these issues. (34)

Huygens states that the Copernican hypothesis is supported by his new discoveries about Saturn and then makes the curious declaration that he thinks that no new satellites will be found, because with the new satellite of Saturn, the number of primary planets equals the number of secondary ones, and together they add up to twelve which is a perfect number. Since this statement is quite uncharacteristic of Huygens, it seems most reasonable to ascribe it to a youthful overindulgence in rhetoric rather than to a secret belief in Pythagorian number mysticism. (35)

The introductory remarks of the treatise itself deal with the discoveries of Galileo on the subject of Saturn and, ignoring most of the intermediate work for the moment, Huygens goes right into his own work on that planet. After some general remarks, he starts the serious discussion with a full description of his own telescopes and their use, treating the different methods of measuring magnification: the focal length method, the method of comparing apparent magnitudes, and the method of angles. He adds that merely stating the size of an object as it appears in the telescope (e.g. Saturn appears as large as a ducat in my telescope) is not enough to tell the magnifying power of a telescope. (36)

To establish the quality of his telescopes and his skill as an observer, Huygens then discusses some of his observations of other celestial objects. He states that he had often checked to see if he could detect satellites about Venus, Mercury and Mars, but without success. About Jupiter he could easily see the four satellites discovered by Galileo, and he was even able to see them when they were in the process of being occulted by the planet. He had also often seen the bands of Jupiter, which did not always appear the same to him, and he presents two figures showing the bands. But whereas other observers had seen the bands dark, contrasted against the brighter remainder of the planet, Huygens saw light bands against a dark background. He had also seen a large dark band on Mars, of which he shows a figure, and remarks that he had sometimes also seen that part of the disc of this planet was lacking - i.e. the phases of Mars. He had of course also observed the phases of Venus. Furthermore, Huygens takes this opportunity to refute Hevelius' claim of having measured the diameters of fixed stars. (37) After stopping down the aperture of his telescope to the size recommended by Hevelius for this purpose, he did indeed see a disc, but Huygens attributes this quite rightly to the optical system rather than to the stars themselves. He also shows a figure of the nebula of Orion, which he describes as appearing like a hole in the dark sky, through which one can see a lighter region. (38)

After having thus laid the groundwork for his specific considerations about the planet Saturn, Huygens describes his discovery of the satellite, and the appearance of the anses at that time. Then follow a number of pages of observations of the satellites, with corresponding appearances of Saturn itself. These observations range from 25 March 1655 to 26 March 1659. On the basis of them Huygens then computes the sidereal period and the synodic period of the satellite (15d. 22h. 39m., and 15d. 23h. 13m., respectively). These values, based on three years of observation, that is, about 68 circuits of the satellite, were fairly accurate, but in 1683 Edmond Halley corrected the sidereal period to 15d. 22h. 41m. 6s. (39), which is only a few seconds different from the modern value. Huygens' calculations ignored the excentricity of the satellite's orbit, and all inequalities of its motion. His results are presented in a table by means of which Titan's (40) position could be found until the end of 1673. By this date, Huygens' values were in effect almost 14° too large, while in 1683, when Halley made the correction, this discrepancy had increased to almost 22°. (41) But the exhaustive treatment of the satellite made doubting its existence rather difficult, unless one wanted to maintain that Huygens had fabricated all the observations.

At this point Huygens enters upon the discussion of the planet itself and the anses. Like Wren, he first establishes the fact that not all observations of the previous 40 years can be taken at face value:

> ... il sera nécessaire d'examiner aussi les résultats d'observations faites à d'autres époques et décrites par plusieurs savants depuis 40 années et plus. Mais comme je parcours toutes les figures de Saturne qu'ils nous ont dessinées, je les trouve tellement nombreuses et prodigieuses que s'il fallait inventer une hypothèse capable de rendre compte de chacune d'elles, il n'y aurait personne je crois qui, en tâchant d'en forger une, ne perdît sa peine: attendu qu'aucune cause d'une transformation si multiple et si énorme ne serait concevable à moins qu'on ne voulût admettre que la masse du corps même de Saturne prît continuellement des formes différentes, ce qui est contraire à toute vraisemblance. Il faut donc trier leurs observations et examiner lesquelles méritent notre croyance, lesquelles doivent au contraire être rejetées comme suspectes. (42)

But the reader will immediately ask: What right does Huygens have to sit in judgment on the observations of others? Here the careful construction of the tract as a whole is felt for the first time:

> Dans cette investigation nous exigeons qu'on nous concède que, parce que nous avons avec nos télescopes découvert pour la premiere fois le satellite de Saturne et que nous le voyons distinctement quand cela nous plaît, pour cette raison nos télescopes doivent être préférés à ceux avec lesquels d'autres personnes, quoiqu'occupées journellement à observer Saturne, ont été incapables d'atteindre ce satellite: et que par conséquent aussi les résultats de nos observations touchant la forme de la planète doivent être jugés conformes à la vérité, toutes les fois que des figures différentes auront simultanément été aperçues par nous et par elles. (43)

Although the argument is convincing, Huygens might have phrased it in a somewhat more humble fashion. After all, he had only been making observations for three years and men like Hevelius, who had made observations of Saturn since 1642 (not to mention other observations going back to 1630) were sure to take offence. (44)

The examination that follows of all the figures observed up to that date, is extremely thorough. As shown in the case of Wren, the idea that not all these figures could be trusted was dawning on astronomers, but Huygens was not content to point out some examples of how such 'erroneous' figures could have come about and limit himself to some general comments as Wren had done. After all, the men who would judge his theory were, in many cases, the same men whose observations Huygens wished to 'correct'. That this is no idle consideration will become evident in chapter 8. Thus, Huygens faces up to the laborious task. All the published figures, from Galileo's earliest ones, right up to those shown by Hevelius in *De Saturni Facie* and Hodierna in *Protei Coelestis Vertigines*, are classified into thirteen general shapes and presented in a plate (see fig. 1). One by one the observations of Galileo and his successors are analysed and explained, with constant reference to the figure. The analysis is in terms of optics and Huygens quotes, wherever possible, his own observations, made with his 'superior telescopes'. Only the figure published by Galileo in *Il Saggiatore* of 1623 is missed. Huygens does not fail to point out, in the case where he himself has made observations, that when he used an inferior telescope he did indeed see the appearance depicted by other observers. Furthermore, he disposes of the differences in size of the anses and their distances from the central body as being due to imperfect vision.

Next Huygens discusses the theories put forward by others, up to that time. He deals with the theories of Hevelius, Roberval, and Hodierna, Wren's theory being at that time still unknown to him. (45) His comments on these three theories have been discussed in chapters 4 and 5. It should be added that he does not miss the opportunity to criticise Hevelius' predictions concerning the reappearance of the anses (they were invisible at the time of the publication of *De Saturni Facie*), a prediction in which Huygens himself had erred in *De Saturni Luna*! But he was now speaking from hindsight.

After disposing of the three theories, Huygens firmly declares that the cause of their faultiness lay in the inferiority of the telescopes used by those who formulated these theories:

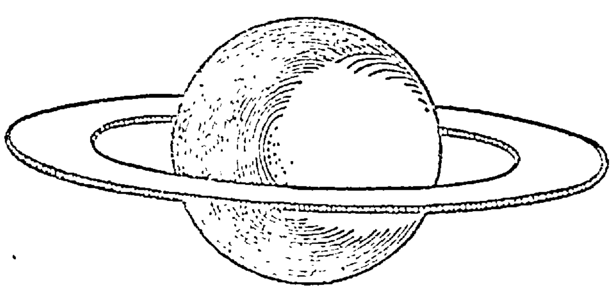
> Il n'est nullement suprenant, et il n'y a pas lieu de vous en vouloir, que ni vous [i.e. Hodierna] ni les autres hommes éminents dont j'ai critiqué auparavant les opinions, ne soyez parvenus à atteindre la vérité touchant le sujet qui nous occupe, attendu que beaucoup de phénomènes faux vous ont été rapportés comme vrais, et que d'autres qu'on peut observer chez Saturne de telle manière que l'imperfection de la vision ne nous empêche pas d'être certains de leur existence, ne sont pas venus à votre connaissance. S'il vous était échu de contempler ces phénomènes avec nous, il est croyable que vous en auriez tiré la même conclusion que nous sur la véritable forme de la Planète. (46)

Surely this statement would not endear Huygens to men such as Hevelius.

Huygens now enters upon the discussion of his own hypothesis, beginning with an account of how he arrived at it (see pp. 113-114). After having decided on a ring and having satisfied himself from the figures of others that it was not attached to the planet, and after having determined the inclination to the ecliptic, he had published his anagram, the solution of which is given here:

> Annulo cingitur, tenui, plano, nusquam cohaerente, ad eclipticam inclinato

Huygens states that the space between the ring and the planet is equal to or exceeds the width of the ring, and that the largest diameter of the ring is to the diameter of the body as about 9 to 4. All this is shown in the following figure:



The first reaction to such a ring, which, Huygens states, is solid, would be that it is unprecedented in the heavens and that it cannot exist unattached to the planet. How can such a ring maintain the same distance from the body of the planet and yet be transported with it through the zodiac? Huygens counters these objections by stating that this is not an hypothesis solely due to his imagination, as was the case with the epicycles of astronomers, which do not appear anywhere in the sky, but that he sees the ring very distinctly: 'hunc quoque annulum satis evidenter me percipere '. (47) Furthermore, there is no reason why such a form, if not spherical yet equally commodious for motion about a centre, should not exist in the heavens. It is certainly better than some of the other shapes that have been suggested! Saturn is at the centre of its vortex, and the ring weighs equally heavily toward the centre from all sides. Therefore it stays in equilibrium, just as, according to the speculation of some men, a symmetrical vault could be built around the Earth without any foundation, and it would not come into contact with the Earth. But at any rate, if one believes in the infinite power and majesty of Nature, why cannot one accept a new shape in the heavens?

In order to explain the phenomena according to the ring-hypothesis, it is important to determine the exact inclination of the ring to the ecliptic. Huygens had determined that at 3° Libra the line of the anses was inclined 21° to the ecliptic, by plotting Saturn's course along the ecliptic and measuring the angle between this course and the line of the anses. But since at 3° Libra the equator is at 21° to the ecliptic, the line of the anses is parallel to the equator. This was verified by letting Saturn cross the field of a telescope when it was held steady, and observing that the line traced by the planet across the field coincided with the line of the anses. Huygens mentions Galileo, Gassendi, Bouillau, and Riccioli, who all agree with him on this inclination. But Hevelius does not agree with him and insists that the anses are parallel to Saturn's orbital plane. However, Huygens is sure that Hevelius will change his mind.

With the direction of the anses (or rather the inclination of the ring) known, Huygens can explain the appearances. He does so with the help of a figure which is almost self-explanatory and is still today the best figure for explaining the different aspects of the ring (see fig. 2). The figure ignores, for the sake of simplicity, the $2\frac{1}{2}°$ inclination between Saturn's orbital plane and the plane of the ecliptic. Saturn revolves about an axis perpendicular to the plane of the ring and to the orbital plane of the satellite. This axis remains parallel to itself as the Earth's axis does. The locations in the zodiac of the different phases and their descriptions are shown and discussed, starting with the most open phase and ending with the round, solitary phase. This phase is the most difficult to explain and Huygens spends almost a fifth of the whole tract on this particular problem.

From the geometry of the figure, it is obvious that twice during each period of Saturn the plane of the ring passes through the Earth. When Saturn is at these points in its orbit, only the edge of the ring is presented to the Earth. But if this were the only condition for invisibility of the ring, the round or solitary phase would only last for a very short period of time. Yet, observations had shown that the ring remained invisible for a whole 'semester', between November 1655 and June 1656. Obviously Huygens speaks here of one of the major problems that delayed the publication of *Systema Saturnium*. The longer period of invisibility of the ring is due to the fact that it cannot be seen when the ring plane passes between the Earth and the Sun. When this is the case, the surface of the ring illuminated by the Sun is not seen from the Earth, so that no reflected light from the ring reaches the Earth.

But, Huygens continues, the reader probably wonders why, in both cases, the ring should be invisible at all, since sunlight reflected from the edge of the ring should still reach the Earth. One reason for this could be that the ring is so thin that light reflected from the edge is imperceptible to our telescopes. But this cannot be the case. The same fact that makes this impossible also furnishes us with the real cause. This fact is the band across the body, which is darker than the rest of the body. This band was seen by Huygens when Saturn was round as well as when the anses were visible. Presumably, this dark band was the shadow of the ring on the body, but the editors of the *Oeuvres Complètes* point out that in no fewer than four instances between 1655 and 1659, cited in *Systema Saturnium* Huygens shows a dark band on the planet adjacent to the *exterior* edge of the ring (see fig. 3), while on the dates on which these observations were made the relative positions of the Sun, the Earth, and Saturn were such that the shadow should be seen (if at all) adjacent to the *interior* edge of the ring. Therefore, what Huygens saw on those dates could not possibly have been a shadow. The only possible explanation, according to the editors, is that Huygens saw the contrast between the bright ring and one of the somewhat more obscure equatorial zones on the planet's body.

Thus, the dark band across the face of Saturn's body is never interpreted by Huygens as a shadow, but rather as the edge of the ring. This edge, Huygens states, either does not reflect light at all, or only slightly. He believes that the edge is covered with a material which is not equally suited for the reflection of light as the rest of the surface of the ring. After all, on the surface of the Moon we see the same phenomenon: some areas are much darker than other areas. On the Moon these darker areas are not entirely devoid of light, but if the Moon were as far away from the Sun as Saturn is, at which distance the Moon would only receive one hundreth of the light it now receives from the Sun, these obscure regions would be absolutely invisible, especially if they were fairly narrow like the edge of Saturn's ring. But it could also be said that the exterior edge of the ring is covered with a material like water, or at least endowed with a surface which is smooth (laevis) and brilliant (splendida), in which case reflection from this edge could be seen at one point only.

Having thus satisfied the projected objections as best he could, Huygens proceeds to explain, by means of the drawing shown in fig. 4, how to calculate (with the help of tables) exactly when the ring will be invisible. First it is demonstrated that the ring must be invisible under three condition: when the ring plane passes through the Earth, when it passes through the Sun, and when it passes between the two, that is, when Saturn is at its apparent equinoxes (as seen from the Earth), or at its real (solar) equinoxes, or anywhere in between. The true equinoxes are determined from Huygens' own observations of 1655-1656 and from an examination of extant data of 1612 and 1642. Huygens determines them to be at $20\frac{1}{2}^{\circ}$ Virgo and Pisces (i.e. 170° 30' and 350° 30'). But based on these data also, Huygens concludes that the ring is invisible within 6° on either side of the equinoxes, because in those regions the Sun's light falls too obliquely on the plane surface of the ring, and therefore not enough light is reflected to the Earth to make the ring perceptible. At a distance of 6° from the equinoxes, Huygens can, with his own telescopes, just about detect the vague appearance of the anses. But with telescopes comparable to those used by Galileo and Gassendi, Saturn has to be more than 6° removed from its equinoxes in order for the anses to be visible. But because of the motion of the Earth, it can happen that, at quadrature, the ring may be invisible, even though Saturn is up to 9° removed from its true equinoxes.

On the basis of these considerations, the prediction of the next solitary phase is not difficult. According to Huygens this should occur from August 1671 until July or August 1672. The succeeding solitary appearances would occur from the beginning of March 1685 to March 1686, and from late 1700 or early 1701 until the beginning of 1702. If astronomers find, upon observing the next round phase, that they agree either exactly or very closely with Huygens' prediction, they will also know that the natural and true causes of these phenomena have been explained to them. But the reverse is not true: if his predictions are wrong by a large amount, then certain circumstances concerning the round phase will have remained hidden to him, and perhaps no man in the world will ever gain an understanding of these circumstances. However, this does not mean that in this case the hypothesis of the ring will have to be rejected, as long as this hypothesis explains all the other phenomena about the anses that will have been observed.

At this point the discussion of the hypothesis is finished and Huygens ends the tract by giving an account of the relative sizes of Saturn and the ring as well as of all other bodies in our solar system. These sizes are arrived at by measuring the apparent magnitudes of planets and then adjusting them in terms of the relative distances given by Copernicus. Thus, Saturn's body is 5/37 of the diameter of the Sun, while the diameter of the ring is 11/37 of the Sun's diameter. But to express the sizes of the planets in terms of the size of the Earth is very difficult, since the distance between the Earth and the Sun is not agreed on by astronomers. Huygens therefore expresses all sizes in terms of the Sun's diameter, and for the Earth he picks a value intermediate between Venus and Mars. He arrives at the following values:

	Ratio of the diameter of the planet to the diameter of the Sun	Modern value
Mercury	none	
Venus	1:84	1:112
Earth	1:111	1:109
Mars	1:166	1:202
Jupiter	1:5½	1:9.8
Saturn	1:7.4	1:11.6

And from this, the distance between the Earth and the Sun is found to be 12,543 Earth diameters (slightly over 100 million miles)

Huygens explains how he measured the apparent diameters of the planets. First he introduced a stop into the focal plane of his telescope. This stop gave a sharply defined field, the angular dimension of which Huygens measured by timing the passage of a star across it (using his pendulum clock). This dimension was found to be 17¼'. He then introduced small rods or sticks (virgulae) into the focal plane until he found one that just covered the disc of the planet whose size he was measuring. The apparent diameter of this planet was then found by taking the rod out, measuring its thickness, obtaining the fraction that this thickness was of the actual diameter of the stop, and multiplying this fraction by the angular dimension of the field defined by the stop. The book ends with the advice to cover the eye piece with a thin layer of soot when observing Venus and Mercury.

Thus, Systema Saturnium is more than just a tract about Saturn; it is a complete and carefully constructed astronomical treatise. In it Huygens frankly revealed the specifications of his telescopes, set out all his data on the new satellite, refuted all the theories that were known to him, and put forward his own theory in a detailed exposition. Finally, his discussion of the measurements of the planets revealed a new way of determining small angular distances in the heavens. It is of course true that William Gascoigne had made a better micrometer in the early 1640's, but in 1659 this micrometer was still unknown, even in England. Effectively, the introduction of measuring devices into the telescope itself began with Huygens' publication.

But what about Huygens' hypothesis? If it was argued that he put an unprecedented shape in the heavens, that same argument would apply at least as much and possibly more to the other hypotheses put forward. Compared to the appendages in Hevelius' theory (not to mention the ovoid body), the awkward elongated body proposed by Hodierna, and the elliptical corona of Wren, Huygens' ring, if equally unprecedented, was at least simple and elegant. But Roberval and Wren did not have to explain why their vaporous emanations did not fall back onto the planet or were not left behind as the planet moved along the zodiac, because these emanations were analogous to the clouds on Earth. Likewise, Hevelius did not have to explain anything about his (presumably solid) appendages, because they were attached to the planet. But Huygens' ring was both solid and unattached, and this was indeed unprecedented. The problem of why such a ring should remain everywhere equidistant from the planet and move with it through the zodiac, could be solved by Cartesian vortices to some extent, but this only meant substituting another problem in its place. If the ring turns about the same axis as Saturn and its satellite do, it is true that the action of the vortex will keep it in a balanced position, but why does not the ring break up under the action of 'centrifugal force'? This question is particularly apt as Huygens was working on precisely this type of problem at the time of the publication of Systema Saturnium, and three months after the publication he had found out how to calculate the magnitude of vis centrifuga. (48) Moreover, Hevelius, who was by no means a great physicist, asked this question in his comments on Systema Saturnium. (49) The question of whether the ring could be a solid structure remained without a definite solution until Maxwell's theoretical proof of the impossibility of such a structure in 1857, and Keeler's experimental proof of the 'satellite nature' of the ring in 1895 (see Appendix A).

The one aspect of Huygens' theory that stood out as awkward and contrived was the reason given for the invisibility of the ring. A very thin ring would have been considerably more elegant, but Huygens states that he was forced to reject this solution by the dark band across the face of Saturn. As shown above, Huygens saw, on a number of occasions, a dark band which could not have been the shadow of the ring on the body. The solution offered by the editors of the Oeuvres Complètes, although possible, is not very satisfying. It is extremely unlikely that Huygens could have seen (unknowingly) the contrast between the bright ring and a darker equatorial zone. In all the numerous sketches of Saturn in Huygens' notebooks he never showed any such equatorial zone and it is unlikely that he ever saw one. Moreover, in Systema Saturnium Huygens insists that the body of Saturn is smooth and without markings. Must we conclude with the editors that Huygens really saw something? Other observers were subject to seeing things that did not exist (e.g. Hevelius' insistence on an egg-shaped body); why could not Huygens have fallen into the same error?

It must be remembered that Huygens did not like to publish his findings until he had been able to demonstrate them clearly and unambiguously. Thus, the demonstration of his formula for centripetal acceleration, found in 1659, was not published until after his death, in 1703; his thoughts on optics mostly formulated in the late 1670's, but in some cases going back to the early 1650's, were not published until 1692, and his work on collision, going back to the 1650's, was not published completely until 1673. The reason for this was most likely Huygens' aversion for non rigorous demonstrations. On the subject of Huygens' mathematics, Dijksterhuis has observed:

> The mathematician, seeking for new facts in the privacy of his study, is free to consider a plane area as the sum of an infinite number of line segments, or to replace a small part of a curve either by a tangent or by a secant, provided that in his publication not the least reminiscence of this procedure can be detected and that everything is demonstrated carefully and amply in that very trustworthy and completely rigorous style of Greek mathematicians ... (50)

Extending this to Huygens' treatment of Saturn, it can be argued that in this case too, Huygens' mental processes were not quite the same as his demonstrations. It could be that from the moment he conceived his hypothesis, he considered that dark band across the globe, which he saw in the spring of 1656 (see fig. 5), as the edge of the ring. Thus, in subsequent observations he naturally looked for the edge of the ring, and managed to see something that was not there. It should be pointed out that the observation of shadow effects was by no means an easy task. After all, Wren states that he saw the dark band in the form of four spots, but that he was nevertheless convinced that it was a continuous band.

To support this argument it can be said that if Huygens was forced to reject the more obvious and preferable solution of a very thin ring on the basis of his observations of this mysterious dark band, then when even better telescopes, used by an ever increasing number of highly qualified observers, failed to substantiate these observations, Huygens would have been happy to reject his thick ring and return to the more obvious thin ring. Yet, despite the fact that no evidence sustaining his observations was uncovered, as late as 1685 he could still state that a dark line on the face of Saturn had to be the exterior edge of the ring. (51) It appears therefore that Huygens' ring was conceived as a thick ring right from the beginning and that this conviction was the cause of his spurious observations.

But except for this aspect, there is little criticism that can be levelled against Systema Saturnium. Huygens' values for the position of Saturn's equinoxes are less than 2° from the modern values. The inclination of the ring-plane to the ecliptic had to be adjusted in subsequent years, and the prediction of the round phases of 1671-1672 and 1685-1686 were slightly in error of course. Perhaps it can be said that Huygens did not work out all the implications of his theory. With today's telescopes, the ring is only invisible when the ring-plane passes through the Earth, and therefore it often happens that the ring disappears and reappears more than once because of the motion of the Earth. In Huygens' time, the ring was invisible during a rather long period before and after Saturn passed its equinoxes. But even so, it might have been possible that if the ring had just disappeared when Saturn reached station, then it could reappear when Saturn became retrograde. This in fact precisely what happened in 1671 (see ch. 11) But Huygens does not touch on this, as Hevelius had done, except that it follows from his comments on the real and apparent longitudes of Saturn's equinoxes.

In spite of the problems that remained to be solved, Systema Saturnium is a very impressive piece of work. By a flash of genius

Huygens had shown that all the monstrous shapes that had been observed all arose from a very simple configuration - more simple in geometry than any solution proposed before - and that the contrived motions that had been proposed by others could be replaced by a complete absence of motion for which there was a powerful precedent: the stability of the Earth's equatorial plane with respect to the fixed stars. Yet the ring-hypothesis was not immediately accepted. For some time Huygens had to defend and explain his theory against - in some cases strenuous - opposition.

Note on Section II

The five theories which have been discussed in this section were not the only theories put forward, theorising about the appearances of Saturn did not stop in 1659. In subsequent years several further theories were advanced and maintained with varying degrees of success. But these theories have not been included in this section for the following reason: after the publication of Systema Saturnium all further comments on Saturn started from this book, regardless of whether one approved or disapproved of the ring-theory. The controversy between Huygens and Fabri and Divini is inextricably interwoven with the evaluation of the ring-theory, Frenicle's ideas on Saturn take parts of the ring-theory as their starting point, and Riccioli's whole discussion of Saturn in his Astronomiae Reformatae is a commentary on Systema Saturnium. Therefore, these ideas or theories are more properly placed in the following section, which deals with the evaluation of the theories.

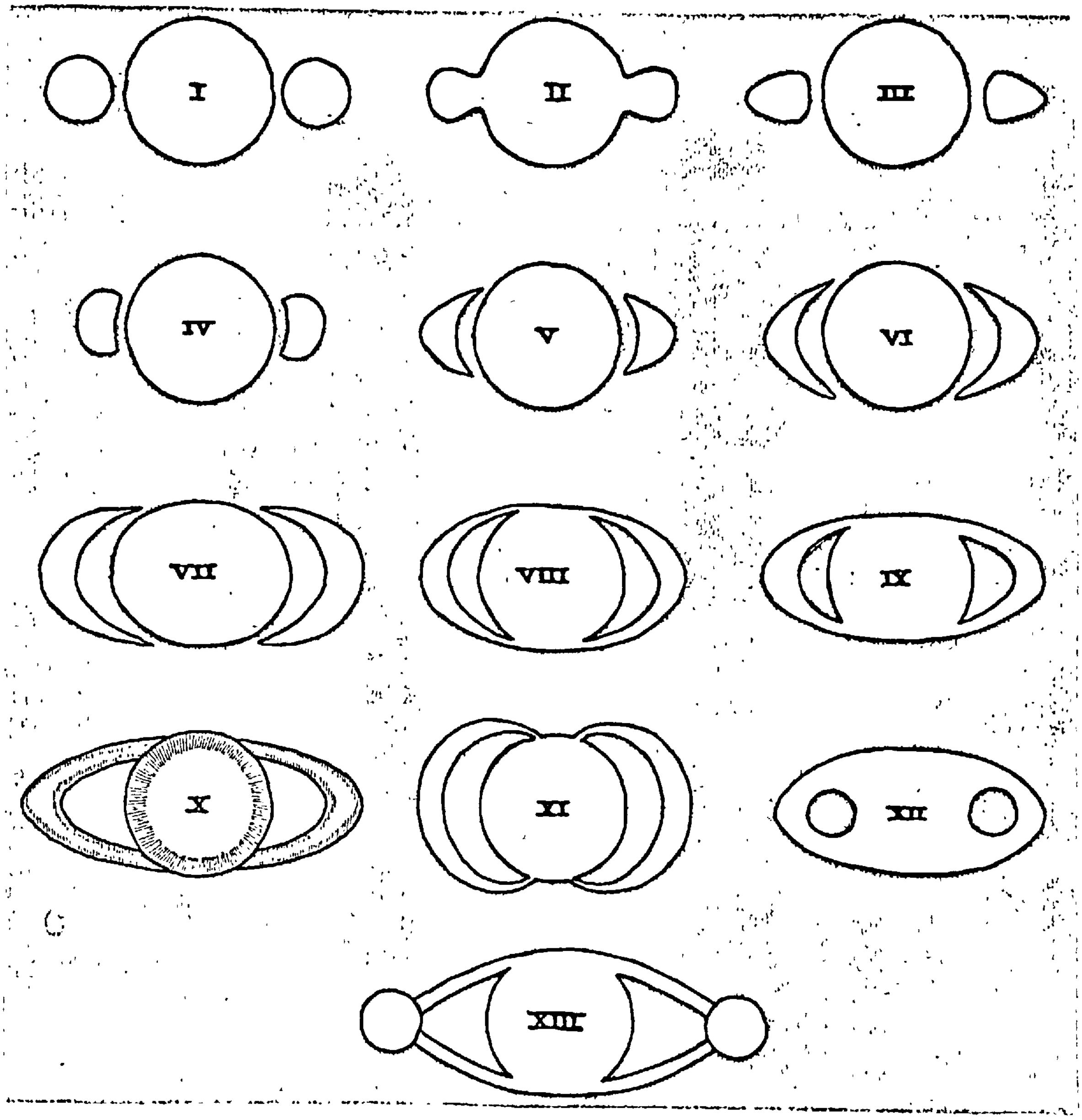


Fig. 1

[Fig. 77.]

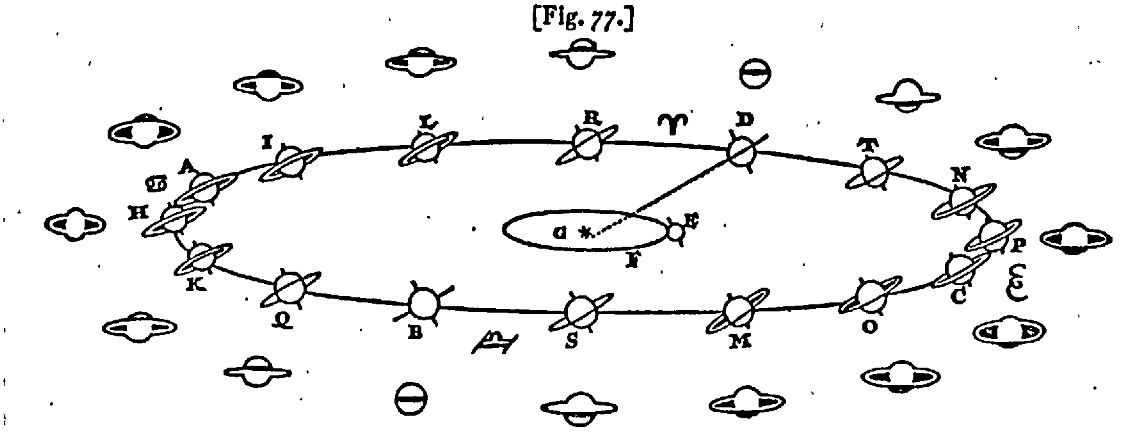


Fig. 2

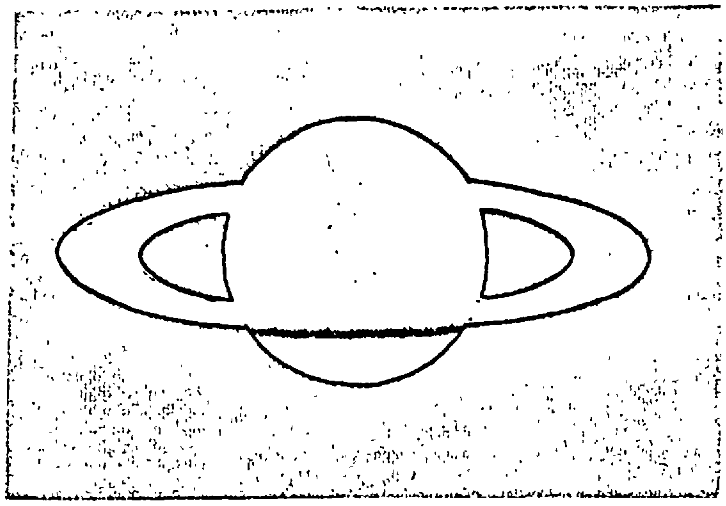


Fig. 3

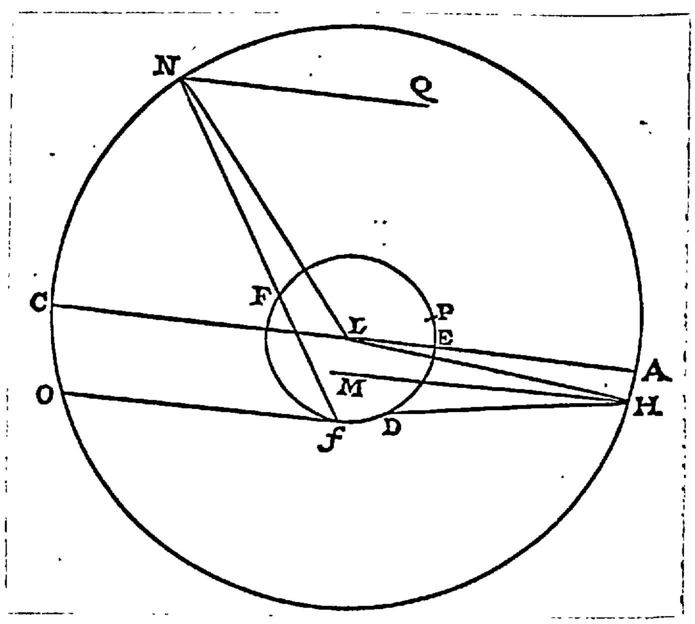


Fig. 4

In this figure, the Sun is at L, DEPFf represents the Earth's orbit, and ANCOH represents Saturn's orbit. The direction of the ring-plane at different times is represented by NQ, CL, Of, HM, and AL. A and C are Saturn's equinoctial points.

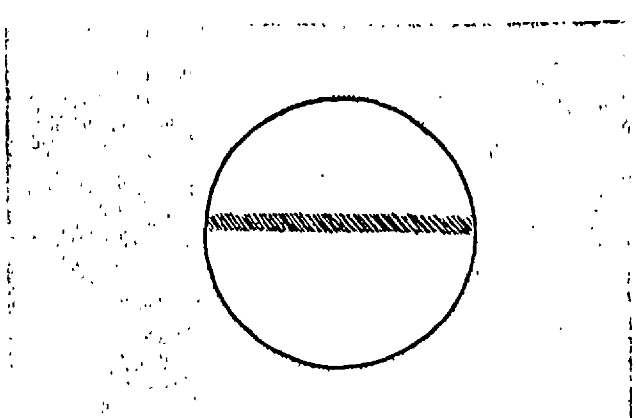


Fig. 5

SECTION III

EVALUATION

CHAPTER 8

The Immediate Reception of the Ring-Theory

Since Huygens revealed his theory to certain people well before Systema Saturnium was published, the immediate reception of the ring-theory covers quite a long period of time, that is from the beginning of 1658 to the summer of 1660, when the polemical tract against Systema Saturnium was published by Divini and Fabri. After this, comments from scientists and virtuosi can no longer be called initial, since they bear on the controversy.

Between 1658 and 1660 comments on the ring-theory were made by a number of men, ranging from astronomers actively engaged on the problem of Saturn, such as Hevelius, Bouillau, and Wallis, to men whose only reason for commenting was a general interest in science and a friendship and admiration for Huygens. It can be stated generally that although there were some difficulties with Huygens' theory, it was generally accepted and admired by the latter (although they did voice some objections to certain aspects of the theory), while the former were more inclined to question the entire theory.

The first reaction came from Bouillau who had been the first to learn the ring-hypothesis, in 1657. In January 1658 Bouillau wrote:

> J'ay pensé a votre hypothese & je trouve qu'elle peut subsister. Si je peux avoir du loisir je m'appliqueray un peu de cette Theorie. (1)

But this did not mean that Bouillau accepted the theory. He was a very good friend of Hevelius, whose theory he also held in esteem. Upon receiving a 22 foot telescope from Huygens, Bouillau wrote:

> Nous aurons moyen de confronter votre hypothese avec celle de Monsieur Hevelius; comme vos Lunettes sont meilleures que les siennes vous avez remarqué ce qu'il n'a pû voir. (2)

We shall take up Bouillau's reaction to Systema Saturnium itself later in this chapter.

With Huygens' permission, the poet and virtuoso Jean Chapelain, a devoted friend of Huygens, revealed the ring-theory, which Huygens had recently sent to him, at a scientific meeting at the house of Montmor - the so-called 'Montmor Academy'. After the meeting Chapelain wrote to Huygens:

> ... je leus hautement et distinctement vostre exposition du Systeme que ceux qui estoient a mes costés suyvoient de l'oeil sur le papier les plus eloignés ayant plus de peine a le oomprendre faute d'en pouvoir regarder les figures au mesme temps, horsmis Monsieur de Roberval qui m'avoua apres que selon que vous l'avies escrit a mesure que je le lisois il l'avoit aussi bien conceu que s'il eust eu les yeux sur la lettre mesme. Pour les autres les plus et ceux qui estoient le plus touchés de Speculations celestes prirent la lettre pour le voir à leur aise et verifier lhypothese sur les figures tracées aux lieux necessaires de vostre discours. Et je puis dire avec toute ma sincerité qu'encore que tout le monde ne donnast pas dans vostre sens comme a une chose toute certaine la plupart neantmoins l'estimerent tresprobable et louerent infiniment vostre sagacité et vostre jugement de la portée des sens ... (3)

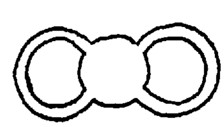
In this letter Chapelain also relates the opinion of Roberval:

> ... mais qu'encore qu'il estimast beaucoup vostre pensée comme fort ingenieuse et fort juste il croyoit pourtant la sienne expliquée dans ma precedente plus approchante de la verité, pour ce qu'il n'y avoit rien que de natural au lieu que la vostre estoit une machine toute d'art et dont il n'y avoit aucune image dans la Constitution du monde. (4)

Roberval's opinion of the ring-theory has been discussed in chapter 5 (see p. 90). He expresses here a vague feeling of uneasiness about such a ring. This was of course entirely to be expected since there was absolutely no precedent for such a structure in the heavens. But as the novelty of the ring wore off, this became much less of an obstacle.

One other opinion was registered before the publication of *Systema Saturnium*. In January 1659 Huygens, who thought that Wallis might have already been informed from Paris about the ring-theory, revealed it to Wallis in a letter. In his reply, Wallis mentioned that he had not known Huygens' hypothesis before receiving Huygens' letter. As one reason for this Wallis gave '*quod ... Galli non tam aliorum quam suis inventis praedicandis videantur dediti*.' (5) On Huygens' hypothesis Wallis made the following comment:

> I reject your hypothesis by no means; but I embrace it as being sufficiently probable (it is in fact supported by the turning of the companion). The reason why we, on the other hand, have been more inclined toward another [theory] is that the phases conveyed by some [observers] have shown the handles wider than the actual body of Saturn (roughly in this form),



> which appearance, if it is true, does not allow this hypothesis. However we do not consider that that [appearance] of which we have doubted [i.e. the above appearance] should be rejected rashly, until time returns him [Saturn] to the place where (if true) it is to be expected again. (6)

Wallis shows here that he was out of touch with Wren's ideas concerning Saturn. Wren had already decided that the figure shown here by Wallis was not a true appearance of Saturn, and in 1658 he had already written De Corpore Saturni, in which he explained his own hypothesis (see ch. 6). Wallis was apparently impressed by Huygens' theory, but he was not sure that it was true.

As soon as copies of Systema Saturnium had been distributed, Huygens began to receive comments, directly and indirectly. Of course, all his correspondents were well educated men, sufficiently versed in the basic principles of astronomy to understand Systema Saturnium and to make intelligent comments on the proposed hypothesis. But very few of them were qualified to pass judgment on the observations upon which Huygens based his hypothesis. Thus, Huygens' observations were generally taken at face value by those who had never made observations of Saturn or had done so only once or twice. For example, Réné Francois de Sluse tried to account for the unequal anses reported by some observers:

> For my part, if it appeared from observations that the planet was excentric, I would not disagree so much, for I hardly find anything in Nature that obeys the law of centre exactly. I add also therefore that perhaps this is the reason why the handles appear unequal, which very many assert to happen sometimes. But since your observations do not admit it, I rather agree with you. (7)

But, as mentioned above, their inability to judge Huygens' observations in no way prevented these men from making comments on the hypothesis. Thus, Gregory of St Vincent (1584-1667), a mathematician of Ghent, mentioned in his letter in which he praised Systema Saturnium that Godefried Wendelin (1580-1660), a canon of Rothenac and a well known mathematician and astronomer, approved of Huygens' theory, as he himself did. But he wondered how it was possible that the dark band across the face of Saturn could ever be not visible. This question arose because Huygens had shown the most open phase of Saturn, to be expected in 1663 and 1664, without such a dark band. (8) Huygens answered this by pointing out that the bigger the anses (i.e. the more open the ring) the more difficult it is to see the dark band because of the excessive brightness of the ring. (9) St Vincent touches here tangentially upon one of the major difficulties of the ring-theory: the thick ring with its curious outside edge that did not reflect light. Besides incidental criticism, such as Kinner von Löwenthurn's objection to the Copernican hypothesis on which Huygens based his ring-hypothesis (10), the thick ring with a surface that did not reflect light was a difficulty that was raised time and again, even by men who whole-heartedly favoured the ring-theory. Perhaps the general reaction to Systema Saturnium among Huygens' correspondents is best exemplified by Pierre Petit's letter:

> Je le leus sans le quitter, car vous ecrivez si bien que quand on ne chercheroit rien autre chose que la pureté et l'elegance il ne faut que lire vos ouvrages, ce qui me charma encor d'avantage, c'est l'histoire de vos observations, elles sont si bien faites et si bien suivies qu'il me semble qu'on ne peut rien dire contre l'hypothese de l'Anneau que vous en tirez, L'Orbite Elliptique de la petite Lune qui Elargit et Estrecit son Ellipse dans les mesmes temps que celle de l'Anneau, me semble un argument asseurè pour le Conclure, J'espere que vous continuerez d'observer et que dans la suitte des temps touts les phenomenes se trouveront conformes a vostre hypothese, J'ay pensé que l'umbre de l'Anneau pouvoit contribuer quelque chose a faire paroistre la petite bande noire sur le Corps de Saturne, pour respondre a ceux qui ont difficulté d'admettre que la superficie convexe de l'Anneau soit polie et d'une maniere a ne point reflechir de Lumiere que ses bras ne seroient pas visibles hors la planette quand nostre oeil est dans le plan de l'Anneau a cause de son peu d'espaisseur; ... (11)

It seems that perhaps Petit himself was one of 'ceux qui ont difficulté', but he obviously accepted the general hypothesis.

Jean Baptiste Duhamel (1624-1706), chaplain of Louis XIV and later perpetual secretary of the Academie Royale des Sciences, had just finished writing his Astronomia Physica, published in 1660, when Systema Saturnium was brought to his attention by Petit. Without altering the comments on Saturn already written in the main body of the book, Duhamel put down his thoughts on the ring-hypothesis in an appendix to it. Thus, Astronomia Physica gives Duhamel's thoughts both before and after he had read Systema Saturnium. In the main text he wrote:

> The last of the planets is Saturn, who is carried through his orbit in 30 years, and is also believed to be accompanied by two satellites. But nothing is more likely than that certain more eminent parts exist which adhere to the planet through certain cavities, and these valleys cannot be seen because they are more depressed. The argument for this is that they never leave the side of Saturn and do not revolve about Saturn as the satellites of Jupiter [revolve about Jupiter].(12)

Thus, before reading Systema Saturnium, Duhamel believed that the anses were attached to Saturn, much in the way proposed by Hodierna, but he was not very certain of this.

Systema Saturnium was, in Duhamel's eyes, full of erudition, and since the author had made use of such excellent telescopes, more faith should be put in his observations than in those of others. (13) He thinks it useful therefore to give a brief outline of the book, stressing the quantitative results, such as the inclination of the anses and the exact locations in the zodiac where the various phases occur. (14) Before examining Huygens' hypothesis itself, Duhamel reviews all other theories put forward, and finds none very satisfactory. But he does not find the ring-hypothesis entirely satisfactory either:

> But the ring is not like the mobile Moon, but remains immobile, although perhaps it turns about its own centre, together with the globe of Saturn, like a vortex [instar turbinis]. I am much inclined toward the opinion - although Huygens disagrees - that the ring is connected to the body of the planet, for the interposed fluid [i.e. interposed between body and ring] does perhaps not reflect the [light] rays at all. Therefore it does not appear to the eye, except as a blackish zone, not different from Moonspots. In fact, what else is that blackish ring that almost divides the middle of Saturn and is discerned best when he is spherical but the extreme fringe of the ring? Blackish spots are also observed on other planets, as on Mars. (15)

What Duhamel means here is that the ring is attached to the body much in the same way as he had the bright prominences attached to the main body, 'through certain cavities' in the main text of the book. The dark exterior edge of the ring and the dark spot on Mars are examples of how such a material can exist.

Although his comments are rather vague, Duhamel apparently accepts the idea of a ring, but it has to be attached to the globe of Saturn by means of some material which has all the optical properties of black sky. Obviously, form a geometrical point of view it matters little whether the dark interstice between the ring and the body is in fact black sky or black material. It must therefore be the case that Duhamel could not accept the unattached ring from a physical point of view, although he does not say so.

Although the generally favourable reactions from his correspondents must have been encouraging to Huygens (and certainly flattering), the real test of his hypothesis was how well it was received by the astronomers and specifically those astronomers who knew most about the problem of Saturn, Hevelius, Bouillau, Riccioli, and perhaps Wallis and Wren. Wallis, who had already written that he held the ring-hypothesis to be very probable before Systema Saturnium was published, wrote after he had read the tract that he approved of the theory. (16)

Likewise, Wren was an immediate convert:

> ... but when in a shorte while after, the Hypothesis of Hugenius was sent over in writing, I confesse I was so fond of the neatnesse of it, & the Naturall Simplicity of the contrivance agreeing soe well with the physicall causes of the heavely bodies, that I loved the Invention beyond my owne ... (17)

Yet Wren believed his theory to be 'equipollent' to that of Huygens, to the extent that even in 1661 (when he wrote these comments) he believed that '... future observations will never be able to determine which is the trewest ...' (18) It must therefore be concluded that Wren preferred Huygens' theory on the basis of elegance. Wren was a Cartesian at this time and the ring (except for the fact that Huygens had made it solid) was practically an embodiment of a Cartesian vortex, and therefore it agreed very well with the 'physicall causes of the heavenly bodies'. But this most favourable reception by a man whose mind Huygens greatly respected remained unknown to Huygens until the latter part of 1661. (19) In the meantime, the short statement of agreement by Wallis was the only unqualified approval that Huygens received from men who were closely involved with the problem of Saturn.

Riccioli's initial reaction is difficult to ascertain. On 25 March 1660, Pierre Guisony (fl. c. 1660), a doctor from Avignon, wrote to Huygens from Rome:

> Apres avoir entretenu à Bologne le Pere Riccioli et de vôtre vertu & de vôtre Systeme de Saturne, je luy donné de vôtre part un de vos livres qu'il receut aveq avidité, & je ne crois pas que les difficultés qu'il me proposa ladessus vaillent la poene de vous étre écrites ... (20)

Perhaps Guisony wrongly judged Riccioli's objections to be trivial, or perhaps Riccioli developed more serious objections upon closer examination of Systema Saturnium, after Guisony left Bologna. At any rate, Riccioli's objections were apparently serious enough to cause him to contemplate writing a tract on Saturn and against Systema Saturnium. Huygens was informed of this by St Vincent, who had received the intelligence from G. F. de Gottigniez (1630-1689), a fellow Jesuit and professor of mathematics in Rome. (21) But if Riccioli was indeed planning such a tract, he changed his mind, although he did not at all agree with Huygens' theory. Instead he published his thoughts on Saturn in his Astronomiae Reformatae of 1665. We shall discuss his comments in chapter 11.

A measure of the esteem in which Huygens held Bouillau is the fact that Bouillau was the first person (except for Huygens' father and brothers presumably) to whom Huygens revealed his hypothesis. Although it is stated nowhere explicitly, it seems clear that Bouillau was at this time the man whose words Huygens took most seriously in astronomical matters. Bouillau's credentials were indeed impressive. He had been making observations since the early 1620's and telescopic observations since the early 1640's. He had observed Saturn more or less continuously since 1642, although he had never published anything on this planet. But Bouillau was perhaps the most respected mathematical astronomer in Europe at that time, mainly because of his Astronomia Philolaioa of 1645, an influential Copernican tract.

Bouillau did not judge Systema Saturnium and the ring-hypothesis hastily. On 10 October 1659, a few days after he had finally received the copies sent to him by Huygensm he wrote to Huygens:

> J'ay parcouru vostre livre du Systeme ... Lorsque j'aura plus de temps que je n'ay pas, je liray tres exactement vostre livre, & je le conferay avec celluy de Monsieur Hevelius ... (22)

During the next month Bouillau did manage to find the time to study the book more carefully, and on 21 November he sent Huygens his considered opinion:

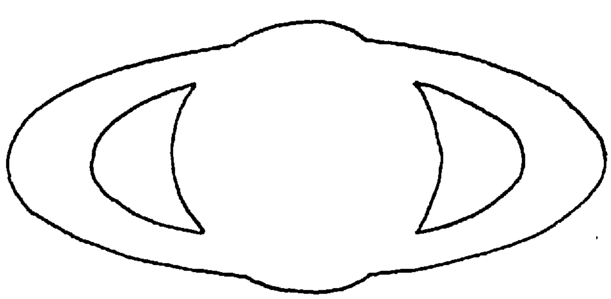
> J'ay leu par deux fois vostre Systeme de Saturne. Vous establissez fort bien vostre hypothese, & elle procede regulierement, pourveu que vous puissiez persuader que ce cercle puisse devenir invisible a cause du peu de consistance en espesseur qu'il a en soymesme. Je scay que la nature a pû faire un cercle autour de ce corps la, & que par la raison qui fait que la terre est suspendue in aëre libero, un anneau peut aussi y estre suspendu; neantmoins il vous faut encore quelques experiences pour demonstrer absolument ce que vous posez. Ce que j'ay veu par vos Lunettes en vostre compagnie, dont vous me faites l'honneur de faire mention, me peut induire a penser que ces appendices de Saturne ne sont pas absolument & entierement de forme sphaeroide, dont la base soit un cercle, quand vous les couperez vers le sommet dans ce qui est tousjours lumineux; & je croy que par les apparences l'on n'en peut juger autre chose, sinon que cette base est une Ellipse. (23)

In this passage Bouillau makes three important points. First, the theory is only consistent if Huygens can persuade himself that the periodic invisibility of the ring is due to its thinness. A thick ring makes the theory unsatisfactory. On this point Bouillau was not alone. Secondly, although he realises that the ring can be suspended in the aether, or aëre libero, Huygens is here by no means on solid physical ground. Thirdly, although Huygens' theory 'follows regularly', presumably from his observations, Bouillau rejects these observations, or more precisely, Huygens' interpretation of the images presented to him by his telescope. Coming from Bouillau, this was a serious attack indeed because Bouillau had actually made observations with Huygens' 23 foot telescope when he was in The Hague in 1657. Therefore Huygens could not discount his criticism on this subject on the basis of inferior telescopes.

Of course, the reason why Bouillau opposed Huygens' observations was that he had his own theory:

> Pour vous confirmer encores dans ce que vous avez posé de cet anneau qui ceint Saturne, il faut que vous attendiez quil vienne dans la fin du ♐ [Sagittarius] pour voir si ses bras se termineront selon l'angle de l'obliquité que vous luy donnez. & si les extremitez ne toucheront pas le disque de Saturne [i.e. are tangent to the central disc]; car s'il arriva qu'elles le touchent il faut que cet anneau soit elliptique & quil ayt un mouvement de conversion sur son grand axe. Si veritablement les extremitez de ces bras ne touchent pas, mais que continuees elles coupassent le disque de Saturne, cela sera encores pour vous, mais je doute que cela arrive. Jay veu Saturne a la fin des ♊ [Gemini] de sorte que les bouts de lanneau joignans Saturne le touchoient & faisoient avec la planete une parfaite ovale, & cela avec une Lunette de XI pieds de long que Monsieur le Grand Duc me donna il y a tantost 9. ans. En physique vous aurez peu de personnes qui tombent dans vostre sentiment. Jusques a ce qu'il me soit venu autre chose dans l'esprit je m'en tiens au doute dans lequel je suis. (24)

Bouillau was here referring to the figure of the most open phase predicted by Huygens in <u>Systema Saturnium</u>. Huygens had predicted the appearance of this phase as follows: (25)



In this figure the central disc protrudes on both sides from the apparent ellipse of the ring. Huygens based this prediction on his determination of the relative diameters of the ring and the body and on the measured inclination of the ring-plane to the ecliptic. Since he had determined the ratio of diameters at 9:4 and the inclination at $23\frac{1}{2}^{\circ}$, his prediction of the appearance to be expected when the ring was most open was somewhat in error. Bouillau, who had personally observed the open phase of the late 1640's (fig. 18, p. 63) (26), believed that in

1663 and 1664 the body of Saturn would appear completely enveloped by an ellipse. But rather than acknowledge that this error, if indeed it was an error, could be eliminated by merely adjusting the inclination of the ring-plane to the ecliptic, Bouillau chose to make this point a test of the whole theory. During the early 1660's the prediction by Huygens was the subject of considerable difficulty and it will be dealt with in chapter 11. For the present, it should be pointed out that this method of criticism indicates how deeply Bouillau was committed to his own theory, although he never published it. That this objection was indeed the major one, as far as Bouillau was concerned, is evidenced by an extract from a letter from Bouillau to Des Noyers at Danzig, the secretary of the French Queen, in which Bouillau forwards this as the only objection he had made to the ring-theory, (27) and by a letter to Prince Leopold de' Medici, who had asked Bouillau for his opinion of the theory (see ch. 9). (28) In this last letter Bouillau makes one other criticism. He writes:

> ... indeed the physicists hardly grant that that bright ring is of such thinness that it sometimes disappears, when, of course, the axis of vision is united with the plane of the ring. Nor have I been able, up till now, to convince myself that this is in fact so, although it is indeed so thin that its thickness does not exceed the diameter of stars of the sixth magnitude. It is however visible because of its continuation and size. (29)

Bouillau argues here not against Huygens' theory, which, after all, employed a thick ring, but against what he thought this theory <u>should</u> be. This illustrates that a very thin ring seemed very obvious and much preferable to readers of Huygens' book. This passage also, incidentally, illustrates that the issue of the size of stars was still by no means settled. Bouillau apparently still thought that stars had diameters that could be resolved with telescopes of sufficient power and quality.

Huygens of course defended his theory against Bouillau's objections. He pointed out that the dark band on Saturn's disc had to be the exterior edge of the ring according to his observations, and that it was not necessary to resort to an ellipse with a reciprocating motion if the ring completely surrounded Saturn in its most open position. All that would be necessary was a slight increase in the obliquity of the ring-plane. As to the physical difficulties, he asked Bouillau to make the objection to which he had alluded (see p. 138) and to suggest what experiments could be made to show that a ring could be suspended in the aether. Finally, Huygens writes:

> La plus forte preuve pour la veritè de mon hypothese sera quand on trouvera que mes predictions touchant la phase ronde s'accorderont avec les observations en l'annee 1671 et 72. pour faire les quelles j'espere que vous et moy aurons assez de vie. (30)

With this statement the issue of Saturn was not discussed further in the correspondence between Bouillau and Huygens.

But Huygens now knew the weaknesses of his theory. He discussed the objections to his theory with Chapelain who wrote to him:

> Je vous suis bien obligé de la peine que vous vous estes donnée de me rapporter les plus pressantes objections qu'on a fait contre et d'y joindre les solutions. La principale difficulte sans doute est cette invisibilité de l'anneau en certain temps. Mais je n'y vois rien d'impossible de la façon que vous l'avés pensé, non pas mince de biais que la pris nostre excellent Amy [Bouillau], mais sans reflexion de lumiere, ce qui pour espais quil fust, pourroit arriver si l'on supposoit que ce bord convexe eust une matiere analogue a nos mers ou à nos forests, lesquelles absorbent la lumiere sans la renvoyer. (31)

If Chapelain, a poet by profession, seems not very successful in his effort to help Huygens overcome the difficulty of the thick ring, it must not be thought that he was incapable of making some shrewd and even profound scientific observations. The letter continues:

> Pour l'autre qu'il est besoin de nouvelles experiences pour faire croire qu'un cercle tel que celuy la pust demeurer suspendu in <u>aere libero</u>, je la croy de nulle force, n'y ayant pas plus d'inconvenient pour un cercle en matiere de suspension que pour un globe et ayant tousjours consideré vostre cercle Saturnien comme celuy que descrit la Lune autour de la Terre, et qu'elle descriroit tout de mesme si au lieu d'un globe Lunaire il y en avoit coste a coste en rond un nombre aussi grand quil en faudroit pour remplir le cercle d'un terme à l'autre; et cela ne me paroist pas sujet a replique. (32)

Chapelain's suggestion of a complete ring of satellites is of course not compatible with Huygens' thick and solid ring. But it was indeed the best physical solution to the problem of the stability of the ring. It is interesting indeed that such a solution should come from, of all people, a poet! Huygens wrote in reply that he was above all impressed by this subtle observations and that he was sorry that he had not thought of this when he was writing his book. (33) However, this did not mean that he might in that case have advocated a ring made up of a very large number of satellites, but rather that he could have used this argument to justify in an indirect way the existence of a ring in the heavens.

Besides the reaction of Bouillau, Huygens was of course particularly interested in the reaction of Hevelius to his ring-theory. Huygens had sent copies of his book to Hevelius by two routes, but he had only included a letter in one of the packages. (34) As it happened, the package without the letter reached Hevelius first, probably early in December 1659. (35) But even when the other package arrived, Hevelius did not immediately send Huygens his opinion of Systema Saturnium because apparently he was very busy. However, this did not prevent him from giving his opinion on the subject to Bouillau. On 16 January 1660 Bouillau wrote to Huygens:

> J'ay receu une lettre de Monsieur Hevelius par cet ordinaire qui escrite le 9e Decembre. il ne fait point d'autres objections a vostre Systeme que celles que je vous ay faictes, aussi sont elles les seules. Il se plaint un peu de vous de ce que vous n'avez pas rapporté de luy dans vostre livre tout ce qu'il croid que vous deviez alleguer. (36)

This was a great understatement. The letter Bouillau was referring to is an eleven page letter with a one page appendix. Of the letter, five pages (over 2000 words) deal with Systema Saturnium, while the appendix consists of Hevelius' observations of Saturn's satellite. Furthermore, the letter is full of objections to the ring-hypothesis and the book, and to say that Hevelius 'se plaint un peu' was an outright lie (although a white lie): his attacks are quite personal and virulent. Although the editors of the Oeuvres Complètes were very thorough in their search for Huygens' correspondence and for other letters which have bearing on Huygens and his work, this letter has been overlooked (although it is in the Bouillau correspondence in the Bibliothèque Nationale). It seems therefore proper to present the complete letter in Appendix B.

Hevelius starts as follows:

> The basis of the whole hypothesis of Huygens consists of this, that it states that Saturn, as far as the middle body is concerned, is completely spherical. Ours [consists] of this, that the same body of Saturn is considered by me to be an elliptical body. And therefore, if this shape of ours is the real shape of Saturn (as will have to be proven), Huygens' entire splendid scheme of the ring collapses. But, impetuously, Huygens attaches more faith to his own observations than to all others (p. 35) [made] with his most splendid tubes with which, whenever he pleases, he clearly sees the new Saturnial [companion], which others are not able to see. For this reason he asserts that the rest of his observations are more correct than the rest of the observations of others. I reply that I likewise possess telescopes of various lengths and indeed of various types, not only with two lenses but also with three and five and more. These lenses were made partly by my work and partly by the work of others, especially by that most skillful Imperial Opt-

> ician Wiesel, who supplied me with a tube for 500 Polish or French florins. You know that I have accurately observed that companion of Saturn with the benefit of these various telescopes several years ago now, as my observations demonstrate [Hevelius refers here to the observations of the satellite contained in the appendix to the letter], yes indeed, I remember that I already noticed that companion near Saturn ten years or more ago. But being too careless at that time, I held it to be a fixed star. Thus, on this point I do not concede anything to him. And therefore I see no reason why our observations do not merit the same faith as those of Huygens. (38)

Thus, Hevelius attacks the very foundation of Huygens' whole argument. The careful construction of Systema Saturnium is to no avail. The claim that since he discovered the satellite his telescopes had to be better than those of any one else and that therefore his observations of Saturn also had to be better, is swept aside by Hevelius. Whether Hevelius' telescopes were as good as those of Huygens is difficult to determine. No direct comparison was ever made. Philips Huygens, Christiaan's younger brother, who met with an untimely death in Poland, did comment on a 12 foot telescope with six lenses in the possession of Hevelius, but he only observed in daylight, which is not a valid test for the suitability of a telescope for night use. He did however say that now he considered Christiaan and Constantijn (his brother) the best grinders of lenses in the world, because their telescopes of equal length, having only two lenses were superior to this telescope with six lenses. (39) Bouillau, who went to visit Hevelius in 1661, made no comparison at all, although he had a Huygens telescope. But, as is shown in Appendix C, Huygens' claims for the superiority of his telescopes, and the belief, on the part of others, in this superiority, were by no means beyond question.

Hevelius goes on to deal with Huygens' theory:

> Nevertheless, Huygens sharply insists that the middle body of Saturn was seen with his perspectives at no time otherwise than round, which of course I grant willingly. But he is asked at what time and on what day he observed Saturn. Certainly, if I am not mistaken, he made observations only for a period of four years, that is from the year 1655 right up to then [1659], and not at any time before that ... But at that time he could only observe Saturn either completely spherical or spherical-cusped. For it is clearly seen as also from our Ephemeride on p. 17 and from our table of the individual phases [in De Nativa Saturni Facie], that he never appeared in another form then. From which we correctly conclude that Huygens could never until then have observed Saturn elliptical-handled with his own eyes. Nevertheless, he argues as follows: 'Since I never saw Saturn elliptical-handled, therefore he only exists with a spherical body.' You easily perceive what kind of truth this leads to. I ask that he wait for the years 1663 and 1664. I do not doubt that he will plainly perceive Saturn to have an elliptical form, since in that shape I

> observed him not once or twice, but many times, and not alone, but with very distinguished men, very skilled in astronomical matters. For in fact I have indeed observed Saturn not for four years only, but actually for 17 years, to be sure from 1642 up till the present, and for three whole years, namely from 1647 to 1650, he continuously appeared in no other form than the mentioned elliptical one, as our observations show. Or does Huygens perhaps suppose that I and others are not able to discern what is elliptical or spherical, or that it was invented by my mind as he writes on p. 39, or rather that I dreamt it? No, by Hercules! (40)

Obviously Hevelius was not pleased with Huygens' statement as to which appearances were real and which were imaginary. Hevelius was fully convinced that in the years to come the globe of Saturn would appear elliptical again:

> Time surely will teach, and he himself with his own telescopes will, in due time, clearly see that now and then Saturn also shines forth with an elliptical shape. As this shape is only with difficulty included in the ring, so also all the phases of Saturn cannot well, in my opinion, ... be explained by the ring for that reason. (41)

Clearly then, Hevelius rejects the ring-hypothesis and maintains his own theory.

Next Hevelius aims his attention on the obvious weakness of the ring-theory as it was put forward by Huygens:

> Besides, even if the ring-hypothesis is granted, all the same, it cannot, evidently, explain the mono-spherical figure of Saturn well enough. At any rate, he toils away very hard on many points, foreseeing very well the perplexity of the thing, so that he can extricate himself from the traps and tricks which occur abundantly ... about that phenomenon, especially since he cannot deny that the supposed ring must have a certain thickness. If in fact it is such a huge device, it ought to have a very noticeable thickness or depth, in order that during that very rapid motion it does not completely collapse, which otherwise would happen, not without great detriment and confusion to the celestial bodies! ... He also attempts to persuade us that the light of the Sun can either be absolutely not reflected, or only very slightly from those lateral parts or edges of the ring, or that a material similar to water, or at least provided with a smooth and brilliant face, surrounds the extreme parts of the ring. ... But who does not see that these reasons are dull and that they cause the very contrary of the matter? Certainly, if the fringe, or border of that ring consists of a material such as Huygens wishes, I think that the other parts consist of a similar material, as far as the two surfaces which are situated on either side of the ring. But he himself has not at all seen such obscurities and inequalities in the surface plane. How, therefore, can we grant it to him in the said marginal parts? But, in fact, from my hypothesis any one can grasp without any trouble that at a certain time in ♍ [Virgo] and ♓ [Pisces], Saturn necessarily appears completely round, so that it is in no way necessary to have recourse to such little diversions. (42)

It is curious that Hevelius, who had made absolutely no mention of any physical or structural considerations in De Nativa Saturni Facie, should criticise Huygens' hypothesis on these grounds. By 'very rapid motion' he obviously means rotary motion, since otherwise his appendages would be subject to the same tendency to 'collapse'. In agreeing that such a large ring must have considerable thickness, he in effect cuts off Huygens from the obvious way out of the dilemma by making the ring very thin. He then rejects the anisotropic character of Huygens' ring: if one surface is smooth and shiny, the other surfaces ought to be equally smooth and shiny. All in all, Huygens' 'little diversions' are unsatisfactory to him. From Hevelius' point of view, a thick solid ring, with a very arbitrary set of surface properties, surrounding a body which he knew to be egg-shaped, was not a very good hypothesis.

But the difference in approaches to the problem is much deeper. Hevelius was first and foremost an observational astronomer in the tradition of Tycho Brahe, whereas Huygens was a physicist for whom Saturn presented a problem of form, and for whom the drudgery of nightly observation for its own sake had little appeal. Astronomy only interested Huygens in so far as it presented interesting problems or possibilities. Therefore he was primarily interested in the question: What causes the appearances of Saturn? For Hevelius on the other hand, the sequence of the appearances and their exact location was the primary point of interest. Thus, Hevelius wrote:

> ... it does not matter so much by what hypothesis that phenomenon may be demonstrated, as how the period of all the phases of Saturn and the intervals of the changes may first be found and derived. I first of all found this period ... and submitted it to public judgment: since indeed my dissertation about the shape of Saturn had been written and published [sic] before that page about Saturn's moon had been brought to me by Huygens' brother [Philips]. ... also, it is abundantly clear from that page ... that the same period of the phases was utterly unknown to him; ... from this cypher of mine [see p. 80]... he will also have understood very well that I was not at all ignorant of the sequence of the changes; nevertheless, not with one little word does he mention in his system of Saturn that I certainly am the first inventor of that truth. (43)

The fact is, of course, that only in the early 1650's had it become possible to define the progression of the phases, since serious observation of Saturn had only started with the round phase of 1642. The round phase of 1656 fixed the period of the phases as well. Now Hevelius was indeed the first one to state these facts explicitly, in De Nativa Saturni Facie, published in 1656, but all the men who had been making observations since 1642, e.g. Bouillau and Riccioli, certainly knew the period of the phases by then. Furthermore, Huygens did not need all this information. The progression of the phases was obvious from the ring-hypothesis and he only had to determine one equinox to pin down all the phases to their proper place in the zodiac. When he wrote De Saturni Luna in 1656, he stated: 'And it will not be difficult to determine for the future the epoch of these changes if we may apply ourselves for another two months to the observations ...' (44). This statement, quoted by Hevelius, was interpreted by him to mean that Huygens did not know the period and progression of the phases. But Huygens meant by it that in another two months (when he believed the anses would have returned), he would be able to calculate the exact location of Saturn's equinox more accurately from the times of disappearance and reappearance of the anses. Hevelius then, believing that the determination of the period and progression of the phases was the most important matter, naturally felt slighted by Huygens' failure to give him credit for his priority.

In the remainder of the letter Hevelius compares his own quantitative results with those of Huygens, showing at every instance that he had already published these results in 1656, concluding 'And indeed it is obvious that with regard to the period of the phases of Saturn ... he was absolutely unable to contribute anything new.'(45) Finally, after pointing out that Huygens had not faithfully represented the figures of Saturn attributed to Hevelius, Hevelius ends:

> In sum, I congratulate Huygens and myself: myself, at any rate, because I was the first who discovered the period of the phases of that world and showed an hypothesis of those things that is not very absurd; Huygens indeed because, treading in my footsteps, he devised, for the sake of truth, a new scheme of the phases to be explained. Let learned men therefore choose one and approve it. Meanwhile, I do not doubt at all that those changes can be demonstrated still otherwise, or rather, better, which, as tends to happen in astronomical matters, we adopt heartily in due time. (46)

Perhaps there is a hint here that Hevelius in fact considered the ring a better mechanism to explain the phenomena than his own appendages, but he certainly was not at all convinced of its correctness. And it is abundantly clear from the highly polemical tone of this letter that in convincing Hevelius Huygens would not only have to overcome the scientific objections, but also a personal barrier.

Of course, Huygens never read this letter. He only heard about it from Bouillau. In fact, he had to wait quite a while before he received a letter from Hevelius, written 13 July 1660, seven months after the letter to Bouillau was written. In this letter to Huygens, Hevelius uses an entirely different tone:

> Indeed, in the principal matter, which is the most important, that is, of the period of the phases, you clearly agree with me as I see it, of which I am very glad. But clearly, the other hypothesis with which this phenomenon is to be explained is contrived so that you differ entirely from me in this hypothesis. But I am not angry with you because of this, but rather I praise your efforts much more because, as becomes all cultivators of the truth and of the sciences, you refused to disclose your judgment to the world for the sake of truth, which I certainly think highly of. In the meantime, however, I also regret that I have published my hypothesis, on which I do not want to judge whether it is better or more true, but rather leave this to others, and especially to time. (47)

The only comment on the ring-hypothesis is the objection about the shape of the central body. Hevelius asserts that in 1663 and 1664 Huygens would see the central body in an elliptical shape and that would end their quarrel (in Hevelius' favour, of course!). A marginal comment by Huygens on this passage reads:

> If that ellipse departs so very little from a circle as you have expressed in your book on the system of Saturn, it cannot at all be decided whether he displays a circle or an ellipse, since some superior and inferior part is obstructed by the ring. (48)

In his reply, Huygens makes the same observation, and adds that therefore the issue cannot be decided until the solitary appearance of 1671-1672. At that time, it could be determined whether the anses projected as straight arms from the central body or whether appeared as detached globes. Of course, Huygens expresses his great confidence that his hypothesis will be confirmed. (49) After this, there is no further discussion of the relative merits of the two hypotheses in the correspondence between Huygens and Hevelius. But there was discussion in their correspondence about another controversy which was carried out in public and which had a great effect on the course of events as regards the problem of Saturn.

CHAPTER 9

Divini and Fabri versus Huygens and the Judgment of the Accademia del Cimento

Copies of Systema Saturnium had reached Italy by several routes. Huygens had sent one copy to Prince Leopold de' Medici, to whom the tract was dedicated, via the link between Nicholas Heinsius (1620-1681) in The Hague and Carlo Dati (1619-1679) in Florence. (1) Prince Leopold received that copy before 9 October 1659. (2) Pierre Guisony, who visited Huygens in The Hague in the autumn of 1659 (3), took several copies with him when he departed for Italy by way of Vienna. Riccioli and Grimaldi received a copy each from Guisony (4), while another copy was sent to Hodierna in Sicily. (5) But Hodierna had died before it arrived. (6) Guisony also spent some time in Rome, where he probably lent his copy to various people, among them Ricci, Divini, and Fabri. (7)

Prince Leopold was very slow in responding to Huygens. He let him know by way of Dati and Heinsius that he had received the book and that he would write as soon as he had read it.(8), but he apparently had doubts about Huygens' theory. When he asked Bouillau's opinion, this astronomer's response did nothing to dispel these doubts (9) and therefore the Prince waited further with his reply. Huygens was greatly distressed by this delay. His increasing frustration is clearly evident from successive letter to Bouillau. On 20 November 1659 he wrote:

> J'attends encore la response du Prince Leopold et je m'estonne qu'elle tarde si long temps puis quil a fait escrire a Monsieur Heinsius qui avoit envoyè mon livre, qu'il respondroit apres l'avoir leu, et qu'il l'avoit eu tres agreable. (10)

On 1 January 1660 he reminded Bouillau, who had assured him that the dedication to Prince Leopold would be received with '... tout le bon acceuil & toute la faveur qui se puisse tesmoigner a une personne que l'on estime'(see p. 116), (11)

> Je n'ay pas encore receu response de Monsieur Hevelius dont je suis un peu estonnè. Mais bien plus de ce que le Prince Leopold ne me fait rien mander. Il me semble que du moins par son Secretaire il me devroit faire dire quelque mot de civilitè, la siene estant si grande comme autrefois vous m'avez assurè. (12)

By 22 January Huygens was even more bitter:

> Monsieur Heinsius, qui a envoyè mon systeme au Seigneur Carlo Dati, me soustient par quelques raisons que le pacquet ou il avoit des lettres pour luy et pour moy doit avoir estè esgarè, et il a aussi escrit pour sçavoir ce qui en est. de sorte que j'espere d'en estre esclairè bientost, qui est tout ce que je pretens, car si l'on me repond ou non je ne m'en soucie pas beaucoup ... (13)

But his anger may have been somewhat lessened by Prince Leopold's request (again through Dati and Heinsius) for a statement by Huygens of the distance at which a person with good vision could see a certain letter or character through his telescopes. He asked Huygens to include an exact reproduction of the character as well as a specimen of the unit of length employed in the test. On this basis, Prince Leopold would be able to make a comparison between the telescopes of Fontana, Torricelli, Divini, and Huygens. (14) Huygens complied with the request, not failing to point out that telescopes should be compared in nocturnal use, because during the day the objects observed are covered as it were by a mist. (15)

Prince Leopold did not become aware of Huygens' disappointment at not having received a direct response, until the summer of 1660, when Dati wrote to Heinsius that Huygens' feelings had come to the Prince's attention. The Prince wanted to have a correspondence with Huygen, but as Huygens had not included a personal letter to the Prince when he sent the book, Prince Leopold had not written to him. If Huygens would write a letter to the Prince, Dati was sure that a lengthy and fruitful correspondence would ensue. (16) In fact, Huygens and Heinsius had decided against writing a personal letter to the Prince to be sent along with <u>Systema Saturnium</u> because '... il sembleroit que je demandasse autre chose que son approbation!' (17) Whether, in fact, this was Prince Leopold's real reason, or whether, more likely, he did not write because he had doubts about the ring-theory, the situation was now fully cleared up and Huygens did not miss his cue. On 13 August 1660 he wrote a letter which was the first in a correspondence which lasted, intermittently, until 1673. (18)

But by the summer of 1660 the situation was no longer simply a matter of the relationship between Prince Leopold and Huygens; another factor had been added. In his <u>Systema Saturnium</u> Huygens had commented on the observations of, among others, the Roman telescope maker Eustachio Divini (1610-1695) (19), who had been making telescopes since the 1640's and whose reputation as a maker of excellent telescopes was international.

Huygens' comments had been generous, but Divini nevertheless objected to them. Most likely Divini's opposition to Systema Saturnium stemmed from Huygens' claims for his telescopes. If everything Huygens said about his telescopes was true, then Divini was obviously not the best telescope maker in Europe.

Early in April 1660 Huygens received from St Vincent an extract of a letter from Rome by Gottigniez:

> Since I last wrote Your Reverence, the small book of Mr Huygens has passed through the hands of various people and it is still circulating ... and it is being studied among a number of opponents who translate it from the Latin language into Italian and prepare a defence in order to overthrow it. They make tubes, prepare their pens, and, oh wonder, they are thinking of naming a place and a time at which, the author of this work and the corrector being gathered with some excellent mathematicians, the tubes will be compared, and the matter will be examined more thoroughly and a judgment will be made of the phenomena of Saturn which have been published, on the condition that he who is convicted of errors leaves the arena and pays the expenses. With these conditions, Eustachio Divini proposes to challenge Mr Huygens, and already he has almost completed a tube of 36 palms, which is about 25 feet. The choice of the place or the city in which they would meet, he offers to Mr Huygens. (20)

It is interesting to speculate whether Huygens would have accepted such a challenge if it had indeed been made. But Divini seems to have thought better of this idea and the report of Guisony, who went to visit Divini late in March, does not mention such a challenge:

> Eustachio Divinj me soutint opiniátrement que pour avoir veu en angleterre et en hollande ce que vous écrivés de Saturne, il falloit que le Ciel de ces Paijs fut autre que celluy d'Italy; dautant qu'apres avoir imité icy la proportion de vos Lunetes, méme en avoir travaillé des plus longues, il n'a jamais rien pú decouvrir de semblable. il veut enfin avoir veu des anses terminées, sans qu'il fut possible aucune autre continuations de cercle Lumineux, & n'avoir point mis les ombres que vous imputés à son Scheme; aveq vótre permission nous accorderons l'un & l'autre à Sa Signorie, pour n'étre point fulminés du vatican. il me montra les plus beaux de ses Telescopes qui passent au delà de 30 piés, & nous les comparámes aveq un de Reeves de la methode du Chevalier Neal qu'on à envoiée au Cardinal Ghisy; il n'a garde de ne tirer l'avantage de son cóté, mais sans mantir il se trompe lourdement. (21)

Perhaps Guisony was a bit too worried about possible church action against Systema Saturnium, but Huygens' religion and his Copernican beliefs were definitely a factor in the subsequent controversy.

Divini did not face Huygens alone. After all, he was hardly equipped to do so. The challenge that came from Rome was the work of two men, Divini and Honoré Fabri (1606-1688), a French Jesuit, at this time grandpenitentiary of the Inquisition. On 25 May 1660, Dati wrote to Heinsius:

> That Eustachio Divini writes against, or rather on the work of Mr Huygens must be true. But because he is an ignorant man, he does so with the help of Father Honoré Fabri, a French Jesuit, who puts it into Latin ... (22)

The authorship of the two tracts published against Huygens under Divini's name has usually been attributed to Fabri. This supposition is well founded. On 10 July 1660 Divini sent a copy of Brevis Annotatio in Systema Saturnium Christiani Hugenii to Prince Leopold. In the letter accompanying the tract (dedicated to Prince Leopold) Divini wrote:

> This Easter, the little book of Mr Huygens, dedicated to Your Highness, came into my hands. I value the work both because it is addressed to a Personnage so sublime and celebrated and because of the skill of the Author. Yet, when reading it again, I found that in some things he has too much confidence both in himself and in his perspectives. Therefore, in my examination I noted some things which, when communicated to some friends, spurred me on to publish them all together in some way. While I was executing this, I realised that I was toiling in vain, as it was in our idiom, in which we would serve few people. However, I decided to ask Father Honoré Fabri, a French Jesuit, a man of singular worth in all scientific endeavours, and a personal friend of mine, to gratify my desire in some way. Out of kindness, he effected it, putting that rough design of mine in this form, as I have not made a profession out of Latin letters. (23)

Thus, by his own account, Divini was not versed in the Latin tongue. Furthermore, the letter quoted here is signed in a different hand from the one in which it is written, indicating that Divini did not write this letter himself. Therefore, it is safe to assume that he cannot have been a man of great literary merit, and that Fabri wrote the tracts, using information supplied by Divini when it came to discussing telescopes.

Brevis Annotatio is a fairly short tract (23 pages), written in the form of a letter to Prince Leopold. That it is a polemical tract is already evident in the second paragraph:

> Dans cet écrit j'accomplirai donc tres choses; d'abord je réfuterai l'accusation de fraude qui m'est faite; j'indiquerai ensuite quelques erreurs de l'auteur, en me servant toutefois d'un style modéré tel qu'il aurait convenue à un homme Chrétien et ingénu, pour ne pas dire 'Eugenius'; enfin, comme l'auteur écrit à la première page que la raison ou cause de l'apparence de Saturne n'a été comprise ni par Galilée ni par aucun autre astronome, mais qu'elle peut s'expliquer en attribuant au globe de Saturne une couronne formée d'un anneau resplendissant, j'exposerai brièvement ... certain principes du système de Fabri par lequel les phénomènes considerés de Saturne s'expliquent avec une facilité qui à mon avis ne peut guère

> être surpassée, et cela dans l'hypothèse d'une terre immobile que les astronomes catholiques ont pour devoir d'adopter et de défendre contre les Aristarques hétérodoxes. (24)

Clearly, Fabri was motivated by more than just pointing out some of Huygens' errors and proposing his own theory: he was in fact defending the faith.

Immediately following the above, Fabri writes:

> J'ajoute que je soumets volontiers tout ce que j'écrirai à Votre très juste censure, à Votre jugement éclairé; et de bon droit, à ce que j'estime, attendu que tous les gens des lettres Vous révèrent comme un Mécène et ceux qui cultivent les arts ou les sciences comme leur patron par excellence. (28)

Thus, since both Huygens and Divini had dedicated their tracts to Prince Leopold and asked him to judge their merits, the Prince was cast in the role of referee, which, in view of Huygens' Copernican conviction and protestant faith, was not an easy role.

Divini (because obviously this part of the tract is based on his experience) first establishes himself as a maker of good telescopes. He mentions that, as shown in his circular of 1649 (see p. 47), he already had telescopes of up to 36 palms (27 feet) at that time (29) and that a number of men had bought his telescopes. Sir Kenelm Digby had taken no fewer than six with him when he left Rome, and a certain Thomas Paggi (probably Thomas Paget), an English nobleman, had also taken one back with him to England. Besides these, there were Divini telescopes in Turin, Bologna, and of course a number in Rome. 'In fact', says Divini, 'this has been my sole occupation for many years.' (30) Furthermore, in a telescope of 36 palms (27 feet), or 4 feet longer than Huygens' 23 foot telescope, Divini had an objective lens with an aperture an inch larger than the $3\frac{1}{3}$ inch aperture in Huygens' telescope. Also, Divini's telescopes had ocular lenses with shorter focal lengths than the focal length of the combination of the two lenses which Huygens used as an eye piece. Thus, Divini's telescopes were superior on all these points. (31)

Next, Divini deals with Huygens' 'accusation of fraud'. In <u>Systema Saturnium</u> Huygens had written:

> C'est aussi sous cette dernière forme qu'Eustachio Divini les [i.e. the anses] a dessinées en 1646 1647 et 1648; ... Vu qu'il est considéré comme un très excellent fabricateur de télescopes [<u>perspicillorum artifex</u>], il est croyable que c'est lui qui nous a montré la forme de Saturn la plus rigoureusement vraie, à cela pres qu'il a ajouté de son cru, me semble-t-il, les ombres qui apparaissent dans la figure [see p. 64, fig. 16]. (32)

To this Divini replied:

> Mais si j'ai ajouté des ombres de mon cru, Huygens est coupable du même crime, lui qui pour donner plus de relief à son anneau imposé à Saturne a ajoute ça et là des ombres bien plus considérables. En vérité, ni l'un ni l'autre de nous n'a rien ajouté de son cru; pour répresenter une sphère, c'est à dire un corps solide, dans un plan, il faut y mettre un peu d'ombre afin que ce corps apparaisse solide et se montre en relief, si non on n'aura rien qu'une circonférence de cercle; quel débutant même dans l'art de dessiner l'ignore? (33)

Divini's basic argument is that he disagrees with Huygens' observations. First, he maintains that the dark interstices between the globe and the handles appear to him darker than the black sky and therefore they cannot be holes through which the sky is seen. (34) Furthermore, notwithstanding Huygens' testimony, in the years between 1653 and 1658 he saw the companions of Saturn round and completely separated from the main body, just as Galileo had seen them in 1610. He had made these observations with an excellent telescope of 27 feet 1657, and in 1658 he had seen the same with a telescope of 18 feet in the presence of men with impeccable credentials, among them Giovanni Alphonso Borelli. (35) Divini now uses the same argument as Galileo used against Scheiner (see p.14):

> ... en effet, avec un télescope plus court leur séparation ne pourrait être discernée quoiqu'elle existat en réalité; par consequent, comme je les ai vu séparés avec mon télescope, j'en déduis logiquement que ce dernier est plus parfait et préférable à l'autre. (37)

Divini suggests that Huygens ignores or denies observations that would be incompatible with his theory. In fact, the dark spaces between Saturn's body and its anses are darker than the rest of the sky, and the companions are not attached, but separated. Therefore the ring-theory must be false. Furthermore, the dark band seen by Huygens across the face of the body when it was solitary, and later above and below the so-called ring, is also a fiction contrived to explain away the problem of the periodic invisibility of this supposed ring.

> Tous ces propos se dissipent aisément; en effet je n'ai jamais pu voir cette bande noirâtre quoique je me sois servi d'un excellent télescope; personne hors lui ne l'a jamais observée; et je crois bien pourtant que mes télescopes ne sont pas inférieurs à ceux de Huygens; cette bande doit être réputée fictive ou tout au moins due à un défaut des verres. (38)

The contention that the edge of the ring was made of a material that does not reflect light is rejected because all the parts of the disc of Saturn appear equally clear and bright. Moreover, Divini asserts, the situation in which the side of the ring toward us is not illuminated by the Sun can never arise due to the large diameter of the Sun. (39)

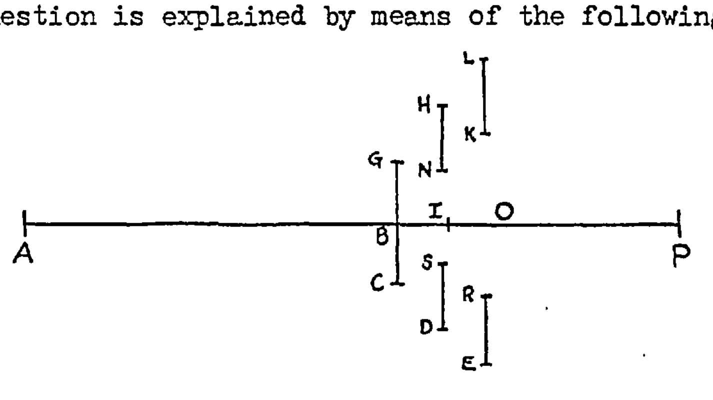
Huygens had only made observations over a period of three years, and not very regularly (according to Divini and Fabri). During that time, Saturn had travelled through 38°. Was this small segment of its total arc enough to base an hypothesis on? 'Obviously not!' say the Roman antagonists. (40)

But Divini and Fabri do not disagree with Huygens on every issue. They agree that the line of the anses is parallel to the equator and that both anses are the same size. Furthermore, they agree that a satellite indeed exists and praise Huygens for having discovered it. They have observed it since the summer of 1657 and give details of some of those observations. The reason why it had not been discovered before 1655 is not inferior telescopes, but inadvertance, or the fact that at the time of an observation it was either in conjunction with Saturn or too far removed from the planet. (41)

At this point, the theory of Fabri is set out in 23 propositions, the first of which states:

> ... que la terre est immobile au centre du monde et que les sphères célèstes tournent autour d'elle; c'est là une opinion qu'il [Fabri] défend avec ténacité, la jugeant conforme à la fois au décrets catholiques, aux saints livres, aux phénomènes observés et à la saine raison. (42)

The first proposition is in fact subdivided into six parts dealing with the motions of the heavenly bodies with respect to the fixed Earth, and explaining the basic configuration of Fabri's system. The planets have three motions, a circular diurnal motion, a rectilinear reciprocating motion from North to South, and a rectilinear reciprocating motion between apogee and perigee. Fabri places all satellites of Jupiter and Saturn 'above' these planets. Saturn has not one, but five satellites, two of which are dark and three of which are light. One of these latter is, of course, the satellite discovered by Huygens, which does not play a part in the explanation of Saturn's phases. The motion of the four satellites in question is explained by means of the following figure:



For the sake of simplicity Fabri only shows the diameters of the bodies. NH and SD, the dark satellites, revolve in the same orbit about point I, while LK and RE, the luminous satellites share an orbit whose centre is O. All four satellites are equally large. When they are all in conjunction with Saturn (CG), this planet appears solitary. When the luminous satellites are furthest removed from AP, and the dark satellites are in conjunction, the figure of a large disc flanked by two smaller ones is seen. When all four satellites are at their furthest points from AP, Saturn appears with two crescents. (43)

After explaining all the observed phenomena by means of this hypothesis, except for the observations of Huygens which are rejected, the tract ends with an appeal to Prince Leopold to judge the relative merits of the theories. Fabri's theory might be called an orthodox theory. It is indeed curious that a Jesuit, whose order had been most reluctant to accept the existence of the newly discovered satellites of Jupiter 50 years earlier, now forged a weapon out of satellites with which to combat yet another 'unorthodox' appearance in the heavens, tied in this case, as in the previous one, to the Copernican theory.

Before the controversy between Huygens and Fabri can be discussed further, a word should be said concerning the controversy between Huygens and Divini about the quality of their telescopes. Because of his alliance with Fabri, Divini explicitly denied observations which were later shown to be real, and therefore, not surprisingly, it has been concluded that Divini's telescopes were inferior to those of Huygens. Although an extended discussion of the relative merits of the telescopes of these two men is beyond the scope of this thesis, enough evidence was uncovered during my research to cast serious doubt on this assumption. This evidence is presented in the article entitled 'Eustachio Divini versus Christiaan Huygens: a Reappraisal', presented in Appendix C.

By the middle of July 1660, Prince Leopold thus found himself in the position of having to decide which theory, Huygens' or Fabri's, was better. Huygens, who had already been apprised of Divini's and Fabri's effort, wrote to Prince Leopold before having received Brevis Annotatio:

> I hear that by now a little work produced by the joint effort of Father Fabri and Eustachio, that Roman craftsman, against my system, has come into the hands of Your Highness, and the eminent Dati has promised that the same will arrive here shortly also. Wherefore, until it arrives, and until Your Highness will have seen my defence, I shall not fear that your equanimity will allow anything to be an-

> nounced against me. But, knowing their objections, it will perhaps not be difficult to ascertain what I shall answer to them, yes, as I hope, the truth of my Hypothesis will also shine forth more clearly after such an airing. (44)

Obviously Huygens did not want the Prince to decide before he had seen his reply to Divini and Fabri.

It is clear from the extensive correspondence on this subject in the Galilaeana Manuscripts in Florence that Prince Leopold and his Accademia del Cimento took their job of judging in this dispute quite seriously. Prince Leopold asked Fabri, by way of Ricci in Rome, to impart some of his curious observations to the gentlemen of the Accademia and in his reply Ricci wrote:

> When I spoke to him [Fabri] of his system of Saturn, he freely confessed to me that he does not defend it as being true, but holds it purely as one hypothesis of many that can save the appearances hitherto observed in this planet by the Astronomers. (45)

Ricci went on to point out that Fabri's hypothesis was an integral part of his defence of the Earth-centered system of astronomy.

Prince Leopold was completely fair to both parties. Dati wrote to Heinsius on 27 July 1660:

> The little book [Brevis Annotatio] has been read with some attention, and various reflections have been made on it, of which I cannot say anything except that when they are sent to Mr Divini and Father Fabri, they will also be sent to Mr Christiaan Huygens ... (46)

Fabri too was apprised of the secrecy of the investigation of the Accademia del Cimento. Under orders from Prince Leopold, the Accademia performed a systematic investigation of both theories and wrote reports on these investigations, which were forwarded to Fabri and Huygens. These reports are interesting for several reasons: besides describing the painstakingly impartial investigations, they give some insight into Borelli's astronomical thoughts at this time, and they contain significant contributions to the theory of Saturn's ring.

The reports to Fabri were sent by Magalotti, via Ricci, on 17 August 1660. The package contained letters to Fabri by Prince Leopold and by Magalotti, and reports on Brevis Annotatio by Borelli and Dati. Prince Leopold's letter was merely a complimentary cover letter which indicated that there were some comments and questions on Fabri's most eloquent writing on Saturn. (47) A short note by Borelli showing how it was possible that the illuminated part of the ring postulated by Huygens was turned away from the Earth was included in the package, but

Magalotti makes it clear that it is not intended for Fabri, but only for Ricci, since the men of the Accademia do not wish to embarrass Fabri about his silly statement. (48) It is left to Ricci's discretion whether or not to mention Borelli's demonstration on this subject to Fabri. Magalotti's letter to Fabri apologises for the frank style of the enclosed reports and asks for his comments on them. (49)

The reports by Borelli and Dati are straightforward critiques. Borelli rejects Divini's reasoning regarding the validity (or lack of validity) of Huygens' observations. Just because Divini and many others had seen the anses in the form of separated globes around 1655, this did not mean that Huygens' different observations for this period had to be rejected along with the claim for the superiority of his telescopes. This was especially true since Huygens had also seen the anses as round and detached bodies when he used inferior telescopes. Obviously Eustachio resents the idea that his telescopes '... tanto pregiati da noi, e da tutti i letterati d'Europa ...' (50) should be inferior or defective. But perhaps Huygens has managed to grind his lenses a different shape, or perhaps his double eye piece (two contiguous plano-convex lenses) gives a better effect. If experience (esperienza) and comparison (paragone) do not show whose telescopes are better, then only after eight or nine years, when the anses are again near to disappearing, can it be decided whether at that stage they are round and detached or straight and extended. (51)

Yet, there is a way by which we can secure ourselves of the truth sooner, by means of ordinary telescopes and simple vision. A model of Saturn, surrounded by a ring, in the proportions indicated by Huygens, was constructed and placed at the end of a gallery and illuminated by four torches placed in such a way that they could not be seen by the observer. The model was observed from a distance of 128 braccia (235 feet) through two perspectives, an excellent one of 1½ braccia (33 inches) and a shorter and inferior one. When the ring was positioned so that it was almost in the same plane as the eye of the observer, it was found that through the better prespective it appeared as a continuous streak of light on either side of the globe, but through the inferior perspective, it appeared as two separate little globes flanking the central globe. When the experiment was repeated in daylight, it was found that at a distance of 37 braccia (68 feet) the anses were seen, with the naked eye, as separated globes, but when viewed through a very small perspective, they appeared again as continuous projections from the central globe. It was also found that the model could admirably represent all the other appearances that had been observed, with one exception: the solitary appearance. Borelli thinks that an extremely thin ring is a better solution to the problem of periodic invisibility. However, he points out that Huygens can defend his supposition on Fabri's authority, since Fabri's two light-absorbing satellites are a device exactly analogous to Huygens' ring whose outside edge does not reflect light. (52) As to Fabri's system:

> About the other part of the system of four little planets which form the appearance of the ears of Saturn, not few difficulties are encountered, because the hypothesis, in itself difficult, does not appear perhaps sufficient to satisfy the appearances. (53)

Borelli discusses the discrepancies between certain observed figures of Saturn and the geometry of Fabri's system, showing quite easily that the combination of the two light and two dark globes can only account for the solitary and tri-spherical appearances and is quite inadequate for explaining the other observed appearances. But not only was this demonstrated by geometry, the members of the Accademia had also built a model of Fabri's theory:

> A device was built which represented the system of Saturn according to the position of Father Fabri and put at convenient distances, receiving its light from four torches. With telescopes of various sizes and perfection, it was only possible to represent realistically [al vivo] with this device the first and second figures of the table of Eustachio [i.e. the tri-spherical appearance] and also the solitary appearance of Saturn.... but that continuation of the bright handles which embrace Saturn, as in the tenth figure of Eustachio [i.e. the handled appearance] was never reproduced. though the combinations of the little planets were varied much. (54)

Borelli's discourse ends with a refutation of Fabri's claim that the satellites of Jupiter and Saturn revolve about points behind these planets. With the planet at quadrature, if the satellite revolves about the planet, as in fig. A, it will become invisible, when passing in front of the planet, when it reaches point X on the line from the Earth tangent to the planet and will become visible again at U. When it passes behind the planet, it will disappear at V, and remain invisible until it reaches Z, where it emerges from the shadow cone. If however the satellite revolves about a point behind the planet,

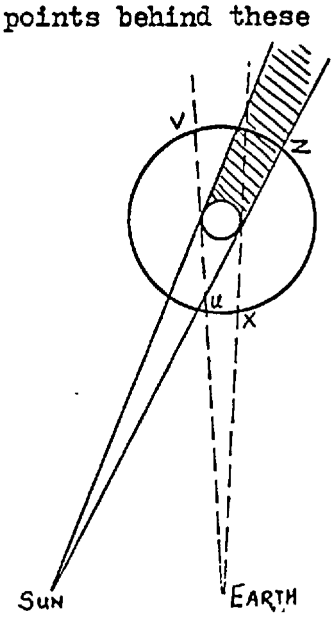


FIG. A

as in fig. B, it will be invisible during quite different intervals, i.e. between X and Y and again between S and Z. This is not borne out by observations, which clearly show the satellite visible and invisible at the intervals predicted by the configuration in fig. A. (55) This issue was of course settled beyond any doubt whatsoever a few years later by Cassini, who observed the shadows of Jupiter's satellites passing across the disc of Jupiter. (56)

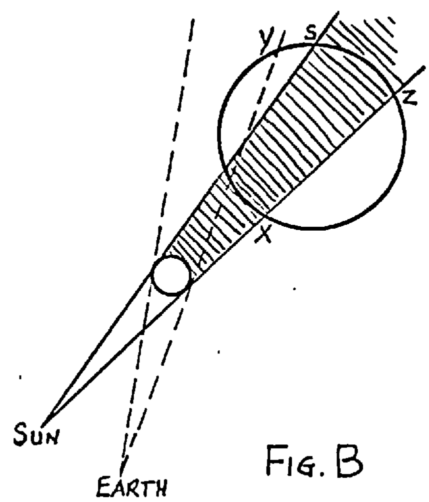


FIG. B

Dati's comments are somewhat more gentle. He finds it difficult to decide between the two theories, but suggests that the best way to ascertain which one is false (and one of them must be false, since they are so opposed) is to measure the length and width of the anses regularly. If the length remains constant and the width varies, Fabri's hypothesis cannot be true, but if the width remains constant while the length varies, then Huygens' theory has to be false. If both length and width vary, both hypotheses must be doubtful. (57) Both hypotheses appear equally strange to him, but then again the appearance of Saturn is very strange itself. He does add however that Fabri's predictions do not agree with the printed observations as far as the diminution of the length of the anses is concerned. The length of the anses in the printed observations does not decrease gradually from a maximum to nothing. Furthermore, the bounding edges of the anses should always be circular according to Fabri's theory, but in reality they are more acute. In fact, both the inside and the outside edges of the anses appeared elliptical to him. (58)

From Borelli's and Dati's comments it must have been abundantly clear to Fabri that the members of the Accademia del Cimento had decided that Huygens' hypothesis was preferable.

Six weeks later, a similar package was sent to Huygens. It was sent by Dati to Heinsius and contained besides Dati's letter to Heinsius a letter from Prince Leopold to Huygens, two reports by Borelli, and one report by Magalotti. In his letter to Heinsius, Dati wrote that although these writings could be shown to whomever it pleased Huygens,

> ... for the moment it is desired that no public mention is made of it. For one thing this is because these men [i.e. the members of the Accademia] are very cautious in affirming anything, not wishing to commit themselves without much consideration and repeated trials ... and for another thing because, having written some rather severe censures against Father Fabri, they would not wish to commit them-

> selves and to be held by the world to be impassioned and partial, which in fact they are not, except for being partial to the truth. In this I commit myself to your prudence. (59)

Obviously, the Accademia did not wish their arguments to be used in a tract that Huygens might write in response to Divini and Fabri. But there was no danger of this as a copy of Huygens' tract in response to Brevis Annotatio was already on its way to Florence when Dati's letter was written.

Prince Leopold's letter to Huygens mentions an observation made on 20 August 1660, which showed a shadow of the body on the ring. Although the observers had not been able to verify this phenomenon on subsequent nights because of poor conditions for observing, Prince Leopold was telling Huygens so that he could try to observe this phenomenon for himself. But the Prince did not tell Huygens that this observation had been made with a Divini telescope! (60)

Prince Leopold makes one further point in this letter. It would be nice if one of the many stars of the Milky Way could be observed through the interstices of the handles. (61) He did not have to say that this would obviously make Fabri's theory untenable.

Borelli's first report begins by showing his esteem for Huygens:

> After the book of the new system about the globe of Saturn, lately published by Christiaan Huygens has been read and seriously considered by the Accademicians of Your Highness, [the report is addressed to Prince Leopold] this great astronomer is accordingly judged to merit his fortune of being the second of those who in the continuous space of so many centuries have brought down new planets from the sky to the view of man, which is the star newly observed by him about the planet Saturn before any other man, with the determination of its period of revolution.
>
> Added to which he has assigned a real stability of shape and aspect to the figure of Saturn which has tortured the minds of the most renowned astronomers of Europe up till now by the variety of his miraculous appearances, crowning him with a band, to which he has added the turning of the planet on its own axis, which is propagated even to the speed of its moon. And he has constructed by his intellect an idea by which this planet must be counted among the most marvellous machines in the Universe.
>
> Nevertheless, this most noble conception of Mr Huygens is also subject to the fortune of things that are at the same time great and new, there being at present no lack of men who have the pretension to oppose themselves to his new and most ingenious hypothesis.
>
> We however have also in this matter inviolably observed the custom of the Academy of Your Highness which is to search out the truth through many experimental proofs, to a degree, however, in which it can be adapted to things so far removed from our senses, and we have fully and dispassionately examined the concept of Mr Huygens and those of the adversaries who oppose him, in the meeting before Your Serene Highness. (62)

Borelli then discusses four 'reflections' which were put forward at this meeting.

The first reflection deals with the problem of explaining the tri-spherical appearance of Saturn by means of the ring-hypothesis. It was suggested that the 'luminous continuation of light' seen by Huygens, instead of two separate globes, was due to inferior telescopes, just as 'in the Milky Way, the whitish light divides itself into an infinity of stars scattered in the darkness of the surrounding aether'. (63) This argument is first countered by a straightforward optical explanation, similar to the explanation put forward by Wren and Huygens: when the ring is almost closed, the connecting arms become very thin and therefore they cannot be discerned with mediocre perspectives. Thus, Saturn appears flanked by two disconnected bodies. These bodies, furthermore, appear round because all objects seen from afar appear rounded off, as, for instance, the luminous horns of Venus. (64) This was then verified by means of a model. Borelli sets out the experiment described above (see pp. 157-158), but he adds several interesting facts. The men who looked at this model knew its real shape and therefore they could trace the fine connections between the apparent lateral globes and the central body. For this reason, a number of people who had no idea as to the model's real shape were called upon to help in the investigation:

> Therefore ... many people, among whom also some very ignorant ones, who had not seen the contrivance of this device from close by, were called upon to observe it. And they were given to see it from a distance of 37 braccia, and made to draw, each by himself, what they appeared to see. And in this way it was obvious that the appearance which they almost all drew was the disc of Saturn in the middle of two little round balls and separated from it by a sensible distance. (65)

Although the experiment proved the point satisfactorily, it was, of course, not free from some human complications:

> Of course, I did not say almost all just because there was one man with such perfect vision, for whom that distance was too short to hide the very thin production of light ... and who drew Saturn surrounded by a band, but because there were some who either because the distance was too great with respect to their vision, or because of the strange conception caused by ignorance and by the novelty of what was put before them, fancied to have to signify it by some strange design such as they made. In not one of those was it ever possible to recognise any resemblance with the object seen. (66)

Obviously the members of the Accademia were not at all unaware of the difficulty of being unbiased in the process of 'seeing'. They recognised that since they knew the real shape of the model, they knew what they were supposed to see, and therefore that their vision was not impartial.

It is difficult to find fault with their method of obtaining unprejudiced information and this experiment amply illustrates the mastery of the experimental method attained by the Accademia del Cimento.

In the second reflection Borelli deals with the 'fit' between the ring-hypothesis and the available information. He points out that critics of the hypothesis maintained that not all the varied observations reported in the table in Systema Saturnium (see p. 129, fig. 1) were accounted for by the ring-hypothesis, and that Huygens was perhaps not very reasonable to reject all those appearances which did not corrrespond to his theory, especially the appearances observed by Hevelius. A rigorous check on the tri-spherical appearance would only be possible some nine or ten years hence, but the test that could decide between Huygens' hypothesis and all the others was that of the length of the anses: did it vary or did it not? Only observers with very good telescopes, such as the Grand Duke himself, Hevelius, Huygens, Divini, and Riccioli could check this. But here Borelli adds a piece of information overlooked by Huygens:

> And he who makes some reflections on the many strange observations of Gassendi [in his Opera Omnia, published in 1658, see pp. 65-67, figs. 20-40], will find that the line which joins the centres of the companions of Saturn proves to be, for most of them, 9 parts, of which parts the diameter of Saturn itself is but 4 parts - an infallible argument for the constancy of this determined length and for the uncertainty of its shortening. (67)

Thus, the proof of the constancy of the length of the anses could even be found in the observations of a man who had never formulated a theory on Saturn. Gassendi's observations served a very useful purpose!

In the third reflection Borelli turns his attention to the ring itself and the shadow effects. First he states his reason for using a very thin ring in the model:

> We have not succeeded in observing with our perspectives any shadowy stripe across the globe of Saturn. On the contrary, we have met with insuperable difficulties in acknowledging a noticeable size of this band. It happens that, having no example in Nature of a material so unapt for the reflexion of light, we cannot imagine that the material, whatever it is, of which a ring of this size is made up, should not be detected, not even by as much as a glimmer or glitter of reflexion, throwing out its lateral emanation into the region of the dark aether. Not only does this difficulty exist with the convex cylindrical surface, but also with the concave surface exposed to our view [i.e. the inside edge of the ring]... Therefore we were reduced to making the ring of considerable thinness, as it seems to us that this subtracts from the difficulty which otherwise was experienced in constructing it. (68)

Moreover, in order to achieve the solitary appearance with the model, all roughnesses had to be removed from the thin ring, for although none of these roughnesses were, in proportion to the model's size, any higher than the highest mountain on Earth, they nevertheless showed up in the form of an extremely thin brilliant line. (69)

But even if the ring is very thin, its width should still produce a noticeable shadow on the disc of the planet (except of course when the ring-plane passes through the Sun). Yet, the academicians had not been able to detect such a shadow in their observations of the planet. Borelli gives an explanation for this. When the observations were made, in the summer of 1660, the planet was near conjunction, as in fig. A. In

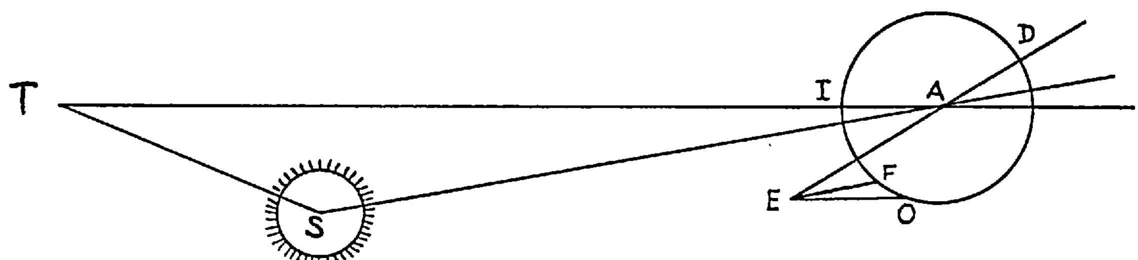


Fig. A

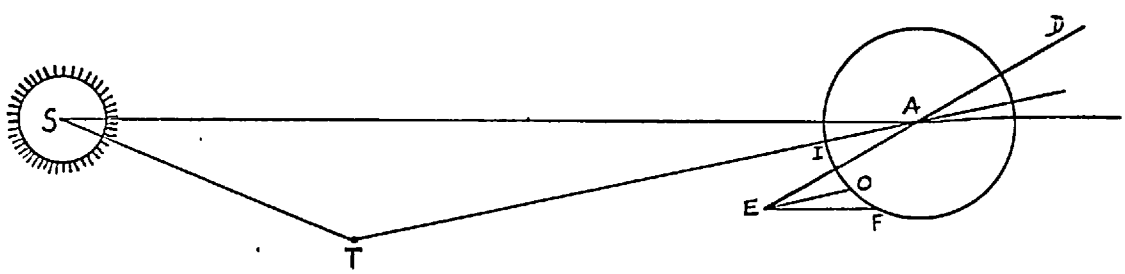


Fig. B

this figure, the shadow cast by the ring DE will be terminated at F (EF // SA), whereas the line of vision from the Earth, EO (EO // TA), will fall below F. Therefore, when Saturn is near conjunction, and the Sun and Earth are on the same side of the ring-plane (as they were in 1660), no shadow can be seen adjacent to the exterior edge of the ring. When, however, Saturn is at or near opposition, as in Fig. B, the visual ray EO (EO // TI) will fall within the shadow zone which is terminated at F (EF // SA) and the shadow will be seen. Such was the case, according to Borelli, in April 1660, and would again be the case in May 1661. (70)

But Borelli's explanation is only partially correct. In the figures shown here, if there is no shadow visible adjacent to the outside edge of the ring, then there must be a shadow visible adjacent to the inside edge of the ring. Yet, the members of the Accademia had not seen such a shadow. This is probably due to the brightness of the ring, which was in 1660 already inclined considerably with respect to the Earth. But the academicians had noticed something else: the shadow of the body on the ring. Borelli goes on to discuss this phenomenon in the fourth reflection:

> The majority of our academicians concurred in having observed during these last two months of July and August a brief streak of shadow on the superior Eastern part of the band of Saturn, by which the apparent attachment of this band to the disc of the planet was lost. It appears that from this a very efficacious argument can be deduced in favour of Mr Huygens ... (71)

This shadow is explained geometrically as being predicted by the ring-hypothesis and Borelli demonstrates that it will shrink until it disappears altogether just before the heliacal setting of Saturn. After Saturn comes out of the Sun it will however show itself again and increase to a maximum by the month of February 1661. (72)

In Borelli's comments, several important points are made. The most important of these is the experimental demonstration that the ring can indeed under certain circumstances give rise to the tri-spherical appearance. Here, finally, was the experimental proof that resolved the long-standing dichotomy between the tri-spherical and the handled appearances. The demonstration, from a source that could hardly be said to be biased, that the length of the anses in the tri-spherical appearance is the same as it is in the handled appearance (i.e. about 2¼ times the diameter of the central disc) was an important contribution, as it provided strong evidence that the theories of Hevelius, Hodierna, and, of course, Fabri were wrong. Finally, although Borelli's treatment of the shadow of the ring on the body leaves something to be desired, the observation of the shadow of the body on the ring could only be accomodated by Huygens' hypothesis.

But Borelli did not stop here. In a separate report to Prince Leopold he goes on to consider the central problem of the ring-hypothesis: the question of the origin and stability of the ring. As shown above, he had been led by the experiments with the model to prefer the more elegant solution for the ring's periodic invisibility, a very thin ring. But this brought up the problem of the constitution of such a ring:

> ... it remains to examine the physical possibility of such a situation. That is, whether the existence or generation of the said ring is

> possible or not; secondly, whether it can endure and maintain itself perpetually; thirdly, whether it can obey and follow the motion of Saturn's turning through the fluid of the aether. (73)

If the ring is solid, there is no difficulty. Just because such a structure is unprecedented, it does not follow that therefore it is impossible,

> ... since the major part of the infinite and inexhaustible treasure of Nature remains unknown to us, and therefore, from time to time, there are discovered some which are without precedent, not known, and not among the usual ones of which Nature makes use. (74)

But Borelli does not believe that the ring is a solid structure, because he discusses this possibility no further. He goes on to discuss in detail the possibility of such a ring being made up of fluid. Such a ring could be generated from vapours emanating from the planet, like the vapours thrown up by our 'volcanoes and Etna's'. (75) These vapours could reach equilibrium at some distance from the planet and remain there if there is no wind on Saturn. The ring might also be generated from a mixture of liquids which becomes opaque when mixed. Aqua fortis 'replete with metals' is opaque, but is rendered transparent by a small addition of oil of tartar. Urine becomes cloudy when it is simply cooled. Suppose that the ambient region of Saturn contains a concentration of some fluid which is rendered opaque only in the equatorial regions by a concentration of some other agent? This could cause a ring such as the one we see. (76).

If the ring is solid and hard, it can endure easily. But if it is not hard it can endure anyway, providing that it is continuously replenished by additional emanations from the planet. This is analogous with the upper regions of the atmosphere of the Earth near the Poles, which are filled with snow which is continuously replenished by the generation of vapours. This region must appear from space as a white opaque ring around the Earth. Furthermore, the shape of the ring will remain constant because

> ... if it is supposed that the whole fluid region about Saturn ... has a natural inclination to approach, gravitate towards, and remain adhering to Saturn, and if it also supposed that in such a region there are no winds but rather that it is completely tranquil, then the reasons for turbulence and variation of figure cease and gravity naturally perseveres to maintain the said region united and adhering to Saturn. The figure of the said ring cannot then in any way alter and change position. A like effect is observed in a glass bottle in which water, wine, and other liquours remain separated like stripes of various colours, the water remaining in the same place, position, and figure all the time, as long as it remains tranquil and is not at all agitated by waves or other internal motion. (77)

The last point considered by Borelli is why the ring remains with Saturn as the planet travels through the aether. To deal with the problem of 'aether drag' Borelli uses the analogy of a torch: if a torch is moved rapidly through the air, a trail of fire will follow behind it, like the tail of a comet. But if the flame is shut in by a lantern, it is neither 'bent' nor extinguished by rapid motion. Thus, if some region of the aether around Saturn, extending beyond the ring, is bound to Saturn

> ... by virtue of its gravity, or magnetic force, or by another, similar cause which keeps it tenaciously bound to Saturn, so that all together they form a system, the said ring of Saturn will be covered and protected from the disturbing influences of the immobile aether ... (78)

Borelli goes on to generalise these remarks:

> But why is it necessary to enquire about other likely causes? Is it not enough to see them judiciously as Nature's work in the sky - effects very much alike, nay rather precisely the same? Jupiter also turns in the aetherial fluid, yet his four Medician planets which surround him have no difficulty in following his motion and it never occurs that they remain behind because of interference from the immobile aether. Venus and Mercury, it is also true, have never abandoned the Sun, nor has the star lately discovered about Saturn remained behind. Therefore, if we are granted a like virtue, Saturn's ring can turn constantly with Saturn with the same ease and therefore, if the virtue which carries the Medician stars resides in Jupiter, we say equally that the force which transports the ring of Saturn resides in that same planet and he who considers that there is, proper to the Medician stars, something either analogous to weight or magnetic virtue, the same can be said of the ring of Saturn. Yes, it makes it permissible for the ring not less than for the Medician planets to be transported together with Saturn. (79)

Clearly, Borelli was at this time already well under way towards generalising the concept of gravity.

Magalotti's remarks deal likewise with the physical constitution of the ring. He begins by expressing sympathy with the ideas of Roberval concerning the constitution of the anses, because these ideas were beautifully simple. Since evaporation and condensation occur on Earth, why should they not also occur on Saturn? He postulates that vapours are continuously generated all over Saturn, but that they congeal into large particles which fall back everywhere except in the region above Saturn's equatorial zone. In this zone the vapours are much more tenuous, as it is a torrid zone. Therefore they ascend to, and come to equilibrium at a greater altitude. Thus, they form a band about the equatorial zone. This band is made up of very small stars of ice. (80)

But why isn't a similar band of frozen vapour observed about the torrid zone of every planet? This is simply because such bands are not necessarily visible and also because they are more likely to form about the planets furthest from the Sun. Jupiter shows bands which are more lucid than the rest of the planet, as Huygens had shown; Mars exhibits a dark band - dark perhaps because the vapours are more tenuous, as this planet is closer to the Sun. Saturn's band is much more easily visible because its vapours are so dense due to the greater distance from the Sun. The Earth has some vapours too, especially around its cooler parts. Venus and Mercury have none because they are too hot. (81)

The light that reaches the Earth from Saturn's ring is not light that is reflected from Saturn and then refracted by the ice cyrystals, but rather sunlight that is directly reflected by the ring. Unlike Borelli, Magalotti thinks that the ice crystals and the ambient air are subject to motion relative to Saturn, like the ebb and flood of the seas on Earth, particularly around Saturn's equator, where the planet's motion is most rapid. This will be especially true if there are no high mountains and no winds in that region. As these little ice crystals are thus repelled by Saturn's motion, they form a stable band around the equator. (82)

Magalotti's speculation is not very subtle and it could have done little to settle the doubts that some men had regarding the physical structure of Saturn's ring. But the total effort of the Accademia del Cimento in the controversy between Huygens and Fabri clearly shows that this was an efficiently operating group of great intelligence. Borelli's astute writings deal with the basic issues of both theories and after an unbiased investigation, the conclusion which is obvious from the reports, although it is nowhere explicitly stated, was that the Accademia del Cimento favoured Huygens' ring-hypothesis. But although the Accademia had discharged its obligation of acting as judge in this dispute and rendered a verdict, the controversy between Huygens and the team of Fabri and Divini was by no means over, as will be shown in the next chapter.

CHAPTER 10

The Controversy Continues

Reports of the publication of a tract against his Systema Saturnium had reached Huygens from a number of sources long before he received a copy of Brevis Annotatio, and he had surmised that it would not be unduly difficult to answer the objections to his theory put forward by Divini and Fabri. Brevis Annotatio was not at all well received, as evidenced by Chapelain's statement:

> il n'y a pas moyen de s'empescher de vous escrire apres avoir lieu le livret d'Eustachio de Divinis sur vostre Systeme de Saturne. Je me doutois tousjours bien que cette Montagne enfanteroit une souris, et en vous attaquant il ma moins surpris quil ne ma fait rire. Mais le principal Assaillant n'est pas celuy qui est entré sur les rangs. Il n'a servi que de couverture au Docteur qui a fort pauvrement imaginé qu'il establiroit sa reputation sur la ruïne de la vostre. Cette presomption n'est digne que de la propre ferule dont il chastie les incongruités de ses grimaux. L'interest qu'Eustachio a dans ce proces n'est autre que d'empescher que vos descouvertes ne deschalande sa boutique et ne descrie les lunettes qu'il a fait, lesquelles il maintient meilleures que les vostres, d'ou il tire un consequence que puisque les siennes ne font point voir dans le Ciel ces phases qui vous y apparoissent par les vostres il faut de necessité que les vostres soient fausses, et que ce que vous dites sur leur foy ne soit pas vray; Sophisme dont il ne faut pas grande subtilité d'esprit pour en desmesler la fallace. (1)

Immediately upon receiving Brevis Annotatio, Huygens set to work on his response, believing that

> ... apres qu'elle sera publiee, l'on ne doutera point ny de l'impudence du Lunettier ny de l'ignorance du bon Pere Fabry. (2)

Due to the fact that Adriaan Vlacq, the printer who usually printed Huygens' works, was busy on several other publications (3), Huygens' response, Brevis Assertio Systematis Saturnii, together with a new edition of Brevis Annotatio, was not finished until the end of September 1660, still only a month and a half after Huygens had received Brevis Annotatio, and copies were sent out as of 30 September. (4) This tract is composed in the form of a letter to Prince Leopold, just as Brevis Annotatio was. In the first paragraph Huygens gives his opinion of that tract:

J'avais donc cru qu'on me ferait quelques objections subtiles que je n'aurais pas prévues, tirées des profondeurs de la science astronomique, et cela avec cette politesse et modestie, qui siérait à un homme qui s'applique aux études liberales. Mais j'ai été absolument deçu dans mon attente, car je vois seulement qu'ils combattent mes observations sans arguments solides, révoquant en doute la plupart d'entre elles et m'accusant assez ouvertement de les avoir inventées contrairement à la vérité. J'écarterai aisément, je l'espère, un soupçon si indigne, et je pense qu'il ne me faudra pas beaucoup de paroles à cet effet, vu que je plaide ma cause devant Votre Altesse dont l'équité souveraine est unie à une égale perspicacité du jugement. (5)

He then turns his attention to Divini's charges, pointing out that longer telescopes are not necessarily better: the quality of the lenses determines how good a telescope is. (6) Divini's observations of the satellite of Saturn are examined and it is found that the positions given by Divini are impossible according to the elements of the orbit of the satellite given in Systema Saturnium. (7) After discussing the inability of Divini's telescopes to make out the dark band across the face of Saturn and mentioning a comparison made in Rome between an English telescope and a Divini telescope, Huygens states:

D'une part celuy [Guisony] qui a fait sur place la comparaison des différentes télescopes juge donc ceux qui provenaient d'Angleterre supérieurs à ceux de Rome, mais de l'autre Eustachio s'obstine pourtant à nier énergiquement cette superiorité, de sorte que même si j'envoyais les miens à Rome cela ne m'avancerait en rien auprès de lui. Que faire de cet homme? Qui, en considérant ceci, ne se croira pas en droit de penser qu'il est si préoccupé de son avantage personnel qu'il ne peut ou ne veut pas discerner ce qui est vrai? (8)

In his discussion of his observations as compared to those of Divini, Huygens repeatedly mentions that his observations have been corroborated by some English observers, especially William Balle. Having again established the superiority of his telescopes, Huygens can afford to be charitable:

Mais j'estime avoir maintenant suffisament (et plus que suffisament) fait comprendre de qu'il faut penser de mes lunettes et de mes observations, et aussi de celles d'Eustachio. Je ne voudrais pas cependant que mes discours lui fassent du tort; mais j'espère plutôt qu'ils le stimuleront à s'appliquer toujours davantage jusqu'à ce qu'il parvienne à surpasser d'abord ses propres télescopes et ensuite les nôtres aussi. En effet, je suis si éloigné d'être envieux des efforts de ceux qui tâchent de perfectionner un art si excellent que j'ai même résolu de publier dans peu de temps tout ce que j'ai trouvé à ce sujet et surtout ce qui se rapporte à la théorie de la Dioptrique; ce que je comprends devoir faire aussi afin que l'on puisse examiner la vérité de mes observations sur le système de Saturne, l'art de fabriquer des lunettes équivalentes aux nôtres ayant été de sorte rendu public. (9)

This was of course easy to say from his supposedly exalted position. But in fact Huygens never published the tracts he mentions here. Both his 'Tractatus de Refractione et Telescopijs', finished in 1653 (!) and his 'Commentarij de Formandis Poliendisque Vitris ad Telescopia' were published for the first time in his Opuscula Posthuma of 1703. At this point Huygens had little idea that within five years his eminent position as a telescope maker would be entirely eclipsed by Giuseppe Campani, and in view of the patronising tone of the above passage, there is perhaps a large measure of justice in this.

Of course Huygens also directs his pen at Fabri and his theory. In shifting his attention from Divini to Fabri, he discusses the darkness of the interstices between the anses and the central body of Saturn. Divini had maintained that these interstices are darker than the surrounding sky, but Huygens counters this with a simple and elegant argument: the only reason why the sky does not appear totally black to us is that the Earth's atmosphere is lit up by the Sun during the day and by the Moon and stars at night. Thus, since the atmosphere is the cause of this, the dark interstices cannot possibly be any darker than the rest of the sky. In fact, he himself had always seen them somewhat lighter than the rest of the sky on account of the light from the neighbouring anses and the central body. (10)

Divini and Fabri had also argued that according to Huygens' theory Saturn could never be seen entirely without anses, because some light would always be reflected from the outside outside edge of the ring, no matter how dark this edge might be. But Huygens points out, as Borelli had done, that if Fabri can postulate two satellites which do not reflect light, then he can postulate such a material for the edge of the ring. (11) Moreover, the contention by Fabri and Divini that it is impossible for the side of the ring illuminated by the Sun to be turned away from the Earth because of the large size of the Sun, is ridiculous. That would only be true if the Sun's diameter was equal to (or greater than) two-fifths of the diameter of the Earth's orbit! (12)

Since Fabri's basic motivation for entering a public controversy with Huygens was the defence of the Earth-centered system of astronomy, it is of interest to quote here Huygens' answer to the charge of being an Aristarchus Heterodoxus:

> Personne à mon avis ne pourrait raisonablement me reprocher d'avoir adapté mon Systeme de Saturne au Systeme de Copernic. Comme cependant Fabri defend à tous les Catholiques de se servir de ce dernier, je m'étonne de ce qu'il ne déclare pas que déjà pour cette seule raison toutes mes fictions doivent être rejetées. Mais il voyait, je

> pense, que je pourrais facilement substituer au Système de Copernic celui de Tycho. En effet, pour les phénomènes en question il importe peu lequel des deux j'emploie. Toutefois la vérité de la chose ne peut être expliquée autrement qu'en suivant Copernic; et de plus notre Système de Saturne corrobore fortement le sien.
>
> Mais je ne comprends pas comment Fabri assure si confidemment que cette théorie de la Terre mobile n'est admise que par les Aristarques hétérodoxes. En effet toutes les fois que j'en parle avec des Catholiques (c'est-à-dire de Catholiques Romains), ceux-ci affirment qu'ils ne sont nullement tenus de se conformer aux décrets qui s'opposent à cette théorie, soit qu'ils émanent de Cardinaux ou qu'ils proviennent du souverain Pontife lui-même. Il est clair qu'ils n'attribuent pas à ces décrets une si grande autorité dans l'explication de l'écriture sainte qu'il faille nécessairement s'y tenir même dans ce qu'ils appelent des controverses de fait; ils sont convaincus que le repos de la Terre doit plutôt être défendu par des raisons que consacré par des documents officiels. Même il est certain qu'en France le Système de Copernic est défendu parfois non pas comme une hypothèse mais comme une vérité acquise, et cela même par des ecclésiastiques et des prêtres qui enseignent ouvertement cette doctrine dans des volumes entiers, sans aucune contradiction que je sache de la part de Rome. Songeant à tout cela, je suis convaincu depuis longtemps qu'outre ceux qui ne connaissent pas l'Astronomie et le public ignorant, quelques Cléanthes seuls (parmi lesquels Fabri) s'attachent encore à l'erreur antique et s'opposent avec un vain effort au mouvement de la Terre. (13)

This is a very frank statement of the situation outside Italy and Spain regarding the Copernican hypothesis. When Brevis Assertio was reprinted in Florence, these two paragraphs were of course deleted. Huygens could indeed have explained the ring-theory in terms of the Tychonic system of astronomy, but it remains to be seen whether he could have conceived the theory if he had not been a Copernican. As long as the Earth occupied a privileged position in one's conception of the Universe, the analogous reasoning that was such a major force in physical astronomy in the second half of the seventeenth century was not possible. Not only were many telescopic discoveries, such as the rotation of Mars and Jupiter based on this (indeed, they were mere verifications of previously postulated analogous behaviour), but the gradual growth of the concept of universal gravitation, a purely theoretical development, would also have been impossible without such reasoning by analogy. For Huygens the Tychonic system was merely a fictional system which saved the phenomena in the manner approved by the conservative powers in Rome. The truth of the matter, however, could only be explained in the Copernican system (Sed rei veritas haud aliter quam Copernicum sequendo explicatur). His whole conception of the Universe was in terms of Copernican circles and Cartesian vortices, and it was this conception, along with the analogy between the Earth and the Moon on one hand and Saturn and its moon on the other, that led to the formulation of the ring-hypothesis. He was of course in good company in his Copernican conviction. The statement about the French Catholics (e.g. Bouillau) who, as members of the clergy, openly defended the Copernican system in speeches, letters, and books was indeed a very pointed reminder of what seemed to Huygens a ridiculous situation.

Even Fabri himself had, in effect, proposed a new system of the World in his discussion of Saturn's motion around the ecliptic. But Huygens does not want to discuss this ridiculous idea. He concentrates instead on Fabri's four moons. This pretty device (commentum bellum) of the two light and two dark satellites reminds Huygens of ' ... quelque truc de prestidigitateur avec des billes blanches et noires, dont tour à tour les unes et les autres sont montrées ou cachées ...' (14) Why hasn't Fabri at least traced the orbits through which these satellites run, instead of merely representing their positions by means of their diameters (see p. 154)? But at any rate, these satellites cannot explain all the phenomena. Even if the tri-spherical appearance, seen by so many observers, is accepted, how does Fabri propose to account for the changes in size of the lateral globes, and how does he account for the handled appearance, in which the dark satellites have to be smaller than the light ones, if all four satellites are always the same size? Furthermore, how can round bodies show an elliptical outline? An even greater problem is presented by the periods of Fabri's satellites. The handled appearance is seen for a number of years in succession, and so is the tri-spherical appearance. What periods of the satellites could possibly account for this? 'Puisse la punition de l'inventeur de ce système ridicule consister dans l'obligation de chercher à scruter les anomalies de ces mouvements.' (15)

Brevis Assertio is clearly a polemical tract rather than a strictly scientific one. Huygens concentrates on the charges levelled against him by Divini and on the theory of Fabri, without taking this opportunity to deal with the objections made by others, such as the invisibility of the edge of the thick ring, and the physical problem of the ring about which, it seems, every one was somewhat uneasy. Even before the publication of Brevis Assertio he had written to Chapelain regarding Brevis Annotatio:

> Mesme vous en avez ecrit une parfaite refutation, qui me fait presque regretter la peine que j'ay prise d'en faire une plus longue. Car certainement Monsieur ces adversaires la ne meritent pas tant. Vous avez tres bien remarquè out l'estat de la controverse, comme aussi les causes qui les ont meu à s'opposer a mes phaenomenes. Sur tout cela j'ay fait dans ma response les mesmes reflexions que vous, et je croy qu'apres qu'elle sera publiée, l'on ne doutera point ny de l'impudence du Lunettier ny de l'ignorance du bon Pere Fabry. Je

> scay desia qu'a Florence l'on est scandalizè de leur procedè, et quant a la veritè de mon Hypothese, ils en demeurent plus persuadez que jamais, voyant que l'on n'y trouve a redire que des choses si frivoles. Tellement que le Sieur Carlo Dati escrivit a Monsieur Heinsius, che tutti erano Hugeniani. (16)

Thus, Huygens knew that the scientists in Florence also found Fabri's theory ridiculous. His statement against Catholic orthodoxy in astronomy shows that he was not trying to convince any one in Rome, and if Chapelain, a poet, could see how ill conceived Fabri's theory was, why then was it necessary at all to write a tract against Fabri? Apparently Huygens did it to further his own reputation.

Brevis Assertio was reprinted in Florence by order of Prince Leopold, who was very impressed by it. But although he was apparently convinced of the superiority of Huygens' theory, the Prince was apparently not convinced that it was true. Moreover, he remained painstakingly fair to Fabri, even in his personal letters to Huygens:

> I have read it [Brevis Assertio] eagerly and attentively and I have shown it to many knowledgeable persons and all are unanimously satisfied, recognising in the same the solid traits of the learning and the wisdom of Your Excellency. In it, I can give no other judgment of your opinion on Saturn except that some things as yet present themselves to me as disagreeing with the observations which I have been able to make, while also, on the other hand, I cannot give perfect justice to the opinions of Father Fabry, if at any rate I were capable of giving it (which I am not), while he has still not declared the periods of the motion of the little planets which he postulates ... (17)

In the meantime, Huygens had received the reports from the Accademia del Cimento, and his remarks on these are of a quite different nature from the content of Brevis Assertio. He wrote to Carlo Dati:

> ... it appears that they [Magalotti and Borelli] are now so persuaded of the truth of my hypothesis that they consider it worth while to inquire diligently into the generation and material of the ring, which I certainly did not dare to do in my tract, not knowing yet the opinion of the learned men on the probability of my system. For if I were now likewise to take up that examination of the ring of Saturn, I do not think I could invent anything better than what the eminent men have brought forward. But there are perhaps some considerations which throw doubt on their opinions. And among these especially it seems to me that whether the ring is said to consist of non-concrete vapours as Borelli maintains, or whether of hard frost as Magalotti maintains, it can hardly be understood why Saturn exhales so much vapour from the circle of smallest latitude, and why, when they have ascended into the heavens, they do not spread out somewhat, but all continue to be contained in a very thin ring. (18)

After pointing out that the opinions of both Borelli and Magalotti as to the genesis of the ring were rash conjectures, Huygens continues:

> Therefore nothing still seems more probable to me than that the ring of Saturn is solid and opaque. Of what material it consists and when and how that form which it has came to be, of this nothing that is worth while to set forth has yet come to my mind. (19)

In a letter to Prince Leopold sent in the same envelope as the letter to Dati, Huygens expresses his great admiration for the experiments and observations of the Accademia, admitting that he has never seen the shadow of the body on the ring because he did not realise that it follows from his hypothesis. But he expresses his intention to look for it and also for the possibility of seeing a little star of the Milky Way through the interstices of the anses. But on the subject of the shadow of the ring on the body he writes:

> On the subject of the obscure line on Saturn's disc, I wish it were true that the most learned men think it not to be observed on the extreme edge of the ring, but judge it rather to be the shadow cast by the ring on the globe of Saturn, since I would see my hypothesis liberated from a conspicuous difficulty, because of which difficulty it was necessary to postulate that material surrounding the outermost circumference of the ring, unapt for the reflection of light. This, it is true, is somewhat crude, but I nevertheless consider it not impossible, since it can be imagined as similar to the surface of still water, which, it is certain, will be quite invisible. And in fact, does it not appear that that great thinness of the ring is hardly reasonable with regard to such width and such mass? And although surely that admitted round phase follows [from the thin ring], yet I did not believe that the obscure line visible to us can arise from such a narrow width of the ring. It is in fact altogether slender because Saturn's latutude is minimum, which the Illustrious Academicians also acknowledge. But I have shown, in refutation of Divini, by the observations of the English as well as by my own, that the dark trace was really observed. And I stated moreover that it appeared to me, for instance, on 26 November 1656, at which time it is to be noted that our eye was more highly elevated above the plane of the ring than was the Sun, and that therefore the shadow preceding the ring [i.e. adjacent to the exterior edge] could by no means be seen on Saturn's globe at that time. Whence, if my observation is true, the darkness consists of that line on the circumference of the ring itself. But inquiries about this can be made again and more surely after eight or nine years, if life is long enough. (20)

Thus, more than a year after the publication of Systema Saturnium, and after having received the comments of all important scientists and of a host of lesser scientists and amateurs, often commenting on the awkwardness of the thick ring, Huygens still steadfastly held the opinion that the ring was thick and solid, with an invisible outside edge.

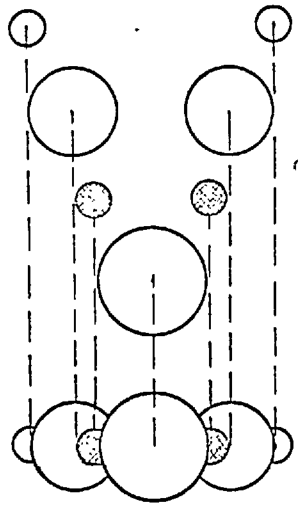
The publication of Brevis Assertio, together with a republication of Brevis Annotatio, had the desired effect. Although Hevelius had great reservations about the ring-hypothesis, he wrote after having read these two tracts:

> I have read through both eagerly. In the first [Annotatio] I find mostly quibbling and I find it very much unworthy of a learned man, if I may speak frankly. In the latter [Assertio], however, I see that everything is treated properly and as it should be. I doubt that he [Fabri] will offer any further reply; it will be better for him to be silent, especially if he wants to preserve his reputation. (21)

It seems thus that Huygens' controversy with Divini and Fabri allowed men such as Hevelius to take a somewhat more detached view on the subject, which had been impossible in the preceding direct confrontation between his own theory and the ring-theory. Thus, whereas the publication of Systema Saturnium had driven Huygens and Hevelius apart, the subsequent controversy with Fabri brought them onto a more friendly footing again.

However, Hevelius' belief that Fabri or Divini would not answer Brevis Assertio was ill founded. On 1 June 1661, Prince Leopold sent Huygens a new tract written by Divini and Fabri. It was entitled Pro Sua Annotatione in Systema Saturnium Christiani Hugenii adversus ejusdem Assertionem (Rome 1661), and it was written in the form of a letter to Huygens. Prince Leopold pointed out that he had absolutely nothing to do with the contents of this tract or with its printing. (22)

Divini takes up the argument about the quality of his telescopes again. Against Huygens' statement that in the final analysis the quality of the telescope depends on how well the lenses are figured and polished, he maintains, quite predictably, that, besides having larger apertures and being longer, his telescopes also have excellently formed lenses, and therefore they must be better than those of Huygens. (23) Then follows a long succession of petty points, such as maintaining that Huygens had not discovered the satellite of Saturn, but that it had been discovered by Fontana (24), and quibbling about the positions of Saturn's satellite when it was observed in Rome in 1657. (25) Fabri's theory is modified to include two more light-reflecting satellites, bringing the total number of satellites around Saturn to seven, five of which are light-reflecting, while the other two are light-absorbing. By means of his six satellites (Huygens' satellite of course plays no part in Fabri's theory) Fabri then explains all the phenomena of Saturn. The handled appearance is approximated as follows: (26)



Obviously things were getting a bit silly at this point, and one begins to wonder if Fabri was serious!

But Fabri appears to have realised his position to some extent, for Pro Sua Annotatione is conciliatory in places and at times it seems as though Fabri is laying the foundation for a graceful retreat. Thus, he states under the guise of Divini:

> ... believe me, Christiaan Huygens, Fabri does not oppose your annular hypothesis; nay rather he affirms most solemnly that he shares your desire that this ring harmonises with the truth. For thence, he is sure, much light will be shed for the settlement of controversies of greater moment. And lest you think that this is imagined by me, since you already doubt my word in other respects, although unjustly, he has demonstrated in fact many properties of the same hypothesis, which I add on to this dissertation. (27)

Indeed, Fabri draws no less than 59 necessary conclusions from Huygens' hypothesis, in which he tries to deal with the ring-hypothesis and some of its difficulties in an honest fashion. For instance, if the outside cylindrical surface of the ring does not reflect light, and the inside cylindrical surface does, and if furthermore the ring is of sensible thickness as Huygens maintains, one side of the anses (i.e. top or bottom) should appear wider than the other. Since this is not the case, the inside edge must also be of a non-reflecting material. (28) Fabri also knows of the observation of the shadow of the body on the ring made by the members of the Florentine academy, and he also knows that this observation was made with a Divini telescope, although he does not mention this. Rather than denying the shadow, he is cautious about it:

> ... some [observers] have said that they have observed such shadows as I had at first opposed to your system. I have never, up to now, been able to observe these, although I made use of a sufficiently long telescope. Nor have you until now observed them, Eminent Sir,

> If indeed you had observed them, you would, I think, have made some mention of them. (29)

Fabri's dilemma is obvious. Knowing that this shadow had been observed with a Divini telescope (although Huygens did not know this) he could not very well deny its existence, as this would be an insult to Prince Leopold and his academy. But if he admitted its existence, he would also have to admit that his theory was wrong, and he was not yet ready to do so.

Fabri had made models of both theories and he frankly admitted that both models could account for the appearances of Saturn. (30) But perhaps the most significant indication of Fabri's struggle with himself is the following passage:

> Moreover, I know that it never came into our minds that the globe of Saturn is surrounded by a ring, before you published your tract [i.e. Systema Saturnium], although we observed him with the same tubes with which we still observe him. Now, however, it seems to us that we perceived that ring, or that horizon as I usually call it, no doubt on account of a preconceived mental pictue [propter praeconceptam imaginationem]. (31)

Clearly, Fabri could not help seeing a ring, once the ring-theory had been proposed, although rationally he was very much opposed to it.

Pro Sua Annotatione ends with a friendly gesture:

> For the rest, I wish you to be persuaded that Fabri has a high opinion of you, as has often been confirmed to me. He asks me again and again to indicate this to you and to give you many greetings in his name. (32)

In view of the tone of the latter part of Pro Sua Annotatione, it is fair to say that Fabri was at this time already half convinced of the correctness of the ring-theory and that he was in fact already establishing a graceful route for his retreat, which, to his credit, he executed publicly a few years later.

Surprisingly, Huygens' opinion of the tract was not very high, and the gestures of reconciliation were also not appreciated by him. He may have toyed briefly with the idea of answering the tract, but he thought better of it:

> Je ne voy pas a quoy serviroit de faire imprimer en ce païs [Holland] ma response a la derniere lettre d'Eustachio, puis qu'il n'y a personne que moy qui l'ait vüe, et d'ailleurs il me semble pas qu'il me soit fort glorieux d'avoir a faire a un homme de sa sorte, car encore que se soit le Pere Fabri qui escrive contre moy, tout se publie pourtant sous le nom de l'autre, qui est une vraye invention de Jesuite. (33)

Nor was Huygens at all convinced that Divini's telescopes were as good as his own: 'I am forced to believe that ... Eustachio's tubes do not surpass my shorter ones in quality by as much as they are excelled by my longer ones. ' (34)

Indeed, Huygens had come out of the controversy as the undisputed winner in the eyes of the world and he had even made a start at convincing his actual opponent. But it must not be thought that the ring-hypothesis was by 1661 universally accepted. Before that could come to pass, certain problems connected with it still had to be eliminated.

CHAPTER 11

The Acceptance of the Ring-Theory: 1660-1675

Although it is true that the ring-theory was not fully accepted until the late 1660's, it became the most popular theory about the appearances of Saturn from the day of its publication. Whereas a number of theories had been developed quite independently before 1659, after the publication of Systema Saturnium, all further ideas and theories were advanced as commentaries on the ring-theory. One reason for this was Huygens' reputation as a brilliant scientist in a number of fields as well as his reputation as a telescope maker. But by far the most important reason was the comprehensive nature of Systema Saturnium. The theories of Hodierna and Roberval were only known through Huygens' commentary on them in Systema Saturnium. Furthermore, besides the description of the ring-theory, the book also contained important information about Huygens' telescopes as well as a full description of the observations made by Huygens of the new satellite and some of the other planets. Systema Saturnium was therefore an important astronomical tract.

Though the ring-theory had some very powerful opponents, such as Hevelius, Bouillau, and Riccioli, it was rapidly accepted by the vast majority of scientists and especially by the younger ones. Thus, in England there was no opposition to the theory in the group of scientists who formed the Royal Society. Wren, Wallis, and Hooke all accepted it without question, although perhaps they did not agree with every detail of it. In France there is no evidence that any of the younger astronomers, Picard, Auzout, and Richer, ever doubted the validity of the theory and even in Italy the opposition was on the part of the more orthodox and older men, Riccioli, Fabri, and Divini. Cassini, who at this time held the chair of astronomy at the University of Bologna, apparently accepted the ring-theory without argument, and tried to deal with the dynamical problem posed by such a ring. In 1705 he wrote:

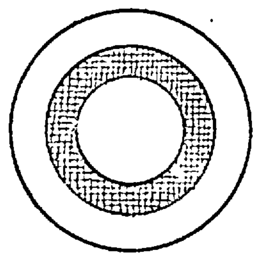
> Cette hypothese fur trouvée admirable, & tèspropre pour expliquer les differentes phases de Saturne, quoiqu'elle ne fut pas reçûë de tous ceux qui étoient prévenus par d'autres hypotheses. Nous n'osâmes pas y comparer une pensée qui nous étoit venuë, que cet anneau pourroit être formé comme d'un essain de petits Satellites qui pourroient

> fair à Saturne un apparence analogue à celle que la voie de lait fait à la terre par une infinité des petites étoiles dont elle est formée; mais avec cette difference qu'elle ne fait point de parallaxe à la terre, au lieu que cette trace en fait une tres-grande a Saturne. (1)

Only with time and with new observations could men like Hevelius, Bouillau, and Riccioli be convinced of the correctness of the ring-theory. But in the meantime, there were some problems with the theory that still had to be solved before the scientists who accepted the theory in principle could be entirely satisfied. In September 1661 Huygens received a letter from Sir Robert Moray in which he was informed that Bernard Frenicle de Bessy had communicated a theory concerning Saturn to the Royal Society by means of Sir Kenelm Digby. (2) To some extent Frenicle's theory has been discussed in chapter 6, in connection with Wren's theory (see pp. 107-108). The theory was transmitted to Huygens from several sides. From Moray he received a copy of Frenicle's letter to Digby (3) and from France he received a long letter from Frenicle himself through the offices of Thevenot. (4) Frenicle had noticed that the observations made in 1660 showed Saturn to be almost totally surrounded by the apparent ellipse of the ring, that is ' ... le bord exterieur de l'anneau passoit a l'extremité de ♄, & rasoit son disque ...' (5) He pointed out that this was hardly possible according to the values for the inclination of the ring to the plane of the ecliptic and the ratio of the diameters of the ring and the body. Huygens had of course fixed the inclination at $23\frac{1}{2}^{\circ}$ and the ratio of diameters at 9:4 (or 2.25:1) and from this it followed that when the ring is at its greatest inclination with respect to the Earth (i.e. $23\frac{1}{2}^{\circ}$) the apparent disc of the central body should protrude on both sides from the apparent ellipse of the ring ($\sin 23\frac{1}{2}^{\circ} < \frac{4}{9}$). In 1661 the ring had not yet reached its maximum inclination with respect to the Earth, and already, according to Huygens' own observations, the minor axis of the apparent ellipse was greater than the diameter of the globe. In order to overcome this problem Huygens had adjusted the ratio of diameters from 9:4 to 17:6. (6) But Frenicle claimed that even this adjustment was not enough to account for the phenomena. Instead of making adjustments to the ratio of diameters or the inclination of the ring to the ecliptic, Frenicle proposed something different:

> J'estime donc qu'il est plus a propos de laisser les proportions qui ont été trouvées cy devant, & d'attribuer la cause de ce débordement de l'anneau, auquel je m'attendois bien, a un mouvement qu'a l'anneau du Nort au Sud, & possible que tout le cors de ♄ participe a ce mouvement, afin que ses poles puissent aussy jouir quelques fois des douces influences du ☉, etans en cela plus privilegies que ceux de notre terre qui ne le voyent que fort obliquement: (7)

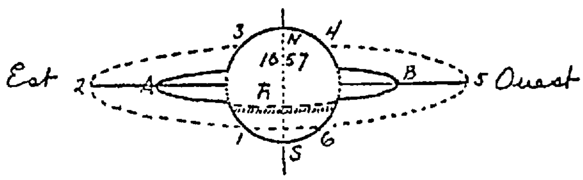
What Frenicle proposed in effect was to give the ring a revolving or reciprocating motion about an East-West axis. It would be easy to decide whether the ring makes a complete revolution or reciprocates through 180°, by means of the shadow of the ring on the body. If, after the ring has reached an inclination of 90° to the plane of the equator, the shadow retraces its steps, the ring reciprocates. If however the shadow reappears on the opposite side of the globe, (i.e. disappears at the North Pole and reappears at the South Pole) the ring revolves. Moreover, if the ring revolves, the motion of the moon of Saturn will be reversed, since it always remains in the plane of the ring. (8) But surely, the figure predicted by Frenicle's theory for the year 1664, shown here, had never been observed! Frenicle was well aware of this and tried to explain the appearances seen in the late 1640's to accomodate his theory. But, at any rate,



> Or, si l'ombre se voit ainsi [i.e. indicates that the ring makes a complete revolution] & que neantmoins ♄ ne paroisse pas comme je l'ay descript il faut que son anneau ne soit pas un cercle mais une Ellipse ... (9)

This ellipse will of course touch the globe. (10) Obviously, this is where Frenicle's theory and Wren's theory become almost identical.

But if the ring, the satellite, and the globe all spin about Saturn's polar axis, and the polar axis itself revolves about an East-West axis, then the spinning motion of the ring and the satellite will become retrograde with respect to the fixed stars. For this reason Frenicle states:

> Il faut donc donner deux mouvements a ♄ & a son anneau. l'un sur les poles 2. 5. qui sont les deux points de l'intersection de l'Ecliptique et de l'Equateur decrit sur ♄ par lequel la partie superieure de
>
> 
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>
> l'anneau qui sera en N. passant entre nous & ♄. baisse vers S. & la partie inferieure S. passant par derriere ♄ monte vers N. L'autre mouvement se fait sur l'axe NS, qui passant par le centre de ♄, aussy bien que le precedent, est perpendiculaire au plan de l'anneau: & ce mouvement se fait pendant 15. ans selon l'ordre des signes; l'an 1664. l'anneau prendra un mouvement contraire savoir d'Orient en Occident contre l'ordre des signes & continuera 15. autres années. Or on ne donne a ♄ ce 2.ond mouvement, qu'a cause de sa ☾.(11)

Apparently Frenicle thought that some time in 1664 the rotation of Saturn, the ring, and the satellite about Saturn's polar axis would stop and reverse itself.

Unfortunately, Huygens' reply to Frenicle has been lost, but his opinion of Frenicle's and Wren's theories (the latter of which he learned in full only in 1661) are evident from his references to them in his correspondence of 1661 and 1662. Frenicle had also predicted that by 1662 the ring would no longer touch the body, and Huygens confidently maintained that the events would prove Frenicle wrong.

In the letter from Wren to Neile accompanying the only original copy of *De Corpore Saturni*, Wren made the following statement in explaining his reluctance to let the tract be circulated:

> Neither had I now been persuaded to it [i.e. surrendering his only copy of the tract], but that I could not endure a Regresse in Reall Learning, having alwaies had a Zeale for the Progresse of it; & to see ingenious men, neglecting what was well determined before, to doe worse on the same subject because they would doe otherwise, was alwaies wont to make me passionate; & therefore I could not with Charity suffer a persone (whose great Wit unusefully applied would be a losse to the world) to trouble himselfe with this lesse considerable Hypothesis, wch if he had known not to be new, he had possibly dispised; & yet it is very well advised of him, that wee should not so build upon Hugenius' Hypothesis, as to neglect the observations, about the Full phasis, wch till they are obtained little more can be determined in this thing then what Hugenius hath don. And therefore though I might have taken occasion together with this old paper to have lent some new thoughts, & to have suggested some new Hypotheses, yet considering they would as yet be but meer conjectures, I have let alone those thoughts. (12)

This remark was obviously directed at Frenicle, and, as happened so often, the letter was copied and sent to Frenicle (as well as to Huygens). Frenicle of course took offence at it and complained to Digby that he had been unfairly treated by the English and that it had never been his intention to substitute another theory for the ring-theory:

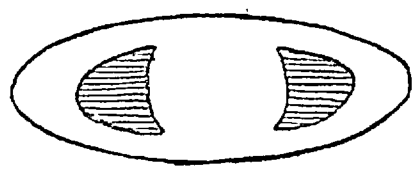
> ... mon principal dessein n'a esté que de mouvoir Messieurs de vostre Nation, & Monsieur Huguenes aussy ... d'observer ♄, pour cognoistre si ma pensée etoit vraye ou non; & j'avois eu bien plus de satisfaction de recevoir quelque chose de leurs observations de cette année, plutost que des censures, inutiles en choses qui ne regarde point le sujet dont il s'agit ... (13)

But Frenicle also sent a detailed critique of Wren's theory to England, and this could have developed into another unnecessary scientific controversy, had it not been for Wren, who would not reply:

> ... il [Wren] n'en a point d'envie; disant que tout ce qu'il a a dire, est que Monsieur Frenicle s'est imagine, qu'il a des opinions qu'il n'a point. (14)

A controversy therefore never developed and by the summer of 1662 observations of Saturn had more or less settled the issue between Frenicle and Huygens:

> ... le debat entre ces deux scavantes personnes ne se pouvoit eschauffer qu'a mon avantage, car en effect il y en a d'estre cité par eux, et de tous les deux partis avec eloge. Je devrois les prier et particulierement Monsieur Frenicle d'en estre moins liberal; mais je prevoy que le combat se terminera bien tost apres qu'ils auront connu la veritable phase de ♄ qui est a present, ou par leur propres observations, ou en adjoutant foy aux mienes, car elle ne favorise non plus l'une que l'autre de leur hypotheses, mais confirme entierement la miene estant telle que voicy ...



> Les anses comme vous voiez sont bien larges aux endroits ou elles sont attachées au globe, contre ce qui devroit estre selon Monsieur Wren, qui les supposoit là fort estroites. Vous voyez aussi qu'il s'en faut encore beaucoup que l'interieure ellipse des anses ne passe par dessus le globe et par dessous sans le toucher, comme Monsieur Frenicle l'avoit attendu, ou du moins qu'il s'en faudroit tres peu. (15)

But Huygens ignored here the problem which occasioned this whole exchange between Frenicle, the English, and himself. For in fact, the observation shown above does not at all confirm his hypothesis, at least not the quantitative prediction as to the appearance of the handled phase, because the globe in this figure does not protrude from the apparent ellipse of the ring. Huygens thought himself secure when he had adjusted the ratio of diameters to 17:6, but he had not done this on the basis of direct measurement:

> J'ay observé tous ces jours passéz Saturne avec les mienes, et je voy distinctement qu'il ny a pas la moindre partie du globe de Saturne qui avance hors de l'ovale de l'anneau par dessus ny par dessous, ce qui ne devroit pas estre ainsi, selon la proportion des diametres de l'anneau et dudit globe que j'avois mise de 9 ad 4. J'ay donc connu qu'il faut poser l'anneau plus grand a proportion, et que son diametre a celuy du globe doit estre pour le moins comme 17 à 6. (16)

In other words, he had made the adjustment based on a calculation of what the ratio of diameters should be in order to accomodate the disc within the apparent ellipse of the ring. Quite rightly, Frenicle had rejected this adjustment:

> vous dites que vous croyez que l'anneau doit etre plus grand que vous ne l'aviez posé, a cause que vous avez observé qu'il excede le globe interieur de ♄: de maniere que ce qui vous fait augmenter son diametre, n'est pas que vous l'ayez mesuré mais seulement a cause, que si on le laissoit comme on l'avoit trouvé, l'observation ne repondroit pas a votre hypothese; mais prenez garde que les observations precedentes que vous avez faites, & celles qu'on a fait aussy tres exactement a florence contrairent a votre pensée; car les differentes situations de l'anneau ne peuvent pas changer la longeur apparente de son diametre ... (17)

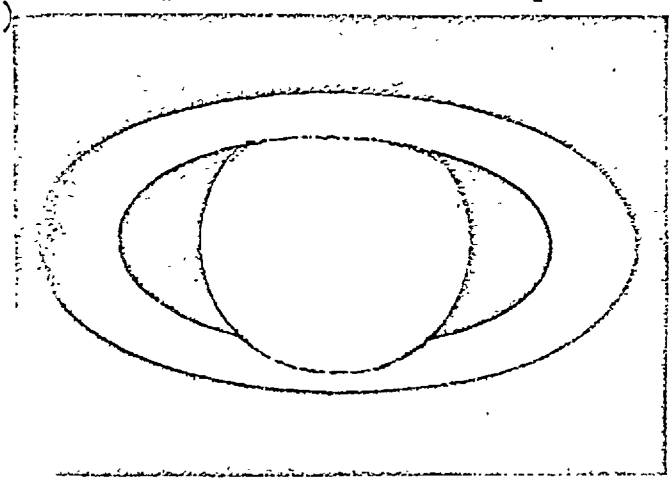
But Huygens never appears to have measured the ratio of diameters during this period, and furthermore, he apparently ignored all previous observations, including his own, which clearly showed the ratio of diameters to be 9:4, or close to it. The ratio in the observation shown on the previous page is just about 17:6, indicating that Huygens took this ratio whenever he depicted an observation at this time. And lest it be thought that this was perhaps not by design, Huygens says about the figure: 'et je la peindra un peu mieux que de coustume, a fin qu'elle ne cause pas des abus comme celle de l'an passè.' (18) Thus, by 1662 Huygens had still not solved the problem first pointed out to him by Bouillau in 1659 (see pp. 138-139).

But this problem can be solved in a much better way. If the 9:4 ratio of diameters was in fact correct, then the only other solution was an adjustment in the inclination of the ring to the ecliptic. In effect, Frenicle had moved in this direction, but much too far. It is indeed curious that after Huygens had initially forwarded such an adjustment as the answer to Bouillau's objection (see p. 140), neither he, nor any one else spoke of such an adjustment until after 1665. One possible explanation for this is that the analogy of the ring's inclination with the Earth's equator's inclination (to the ecliptic) was very powerful. Therefore it did perhaps not occur to astronomers for quite some time that this inclination could be greater (or less) than $23\frac{1}{2}^{\circ}$ without upsetting the entire hypothesis.

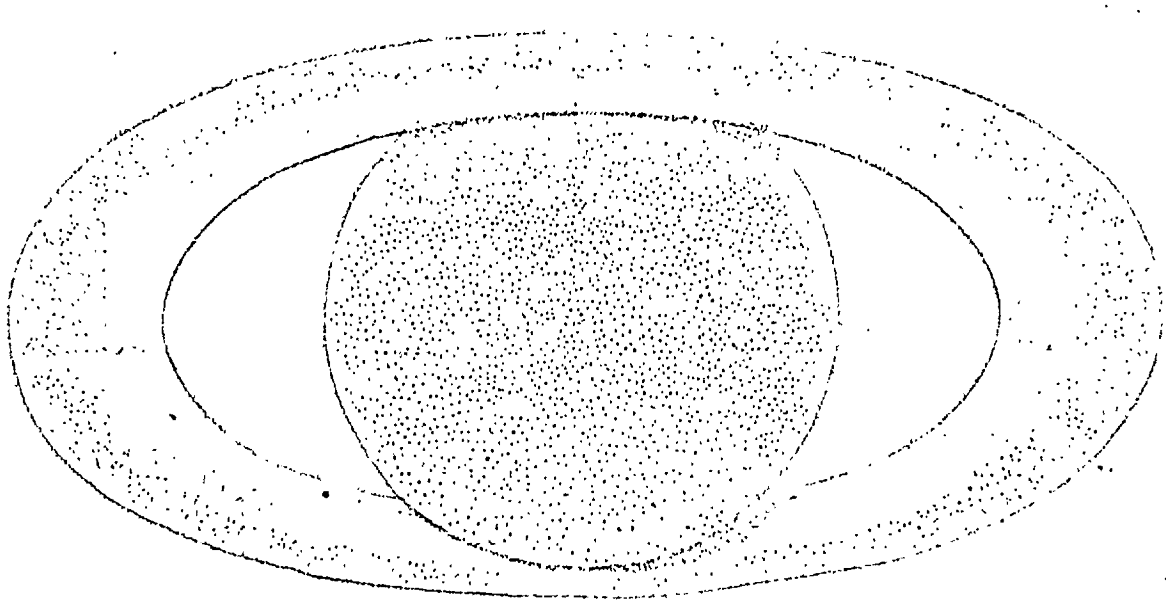
Although Huygens of course made fairly regular observations of Saturn during the 1660's, and indeed during the rest of his life, he never again made any new discoveries concerning this planet or any other. As has been shown in chapter 9, the first observation of the shadow of the body on the ring was made in Florence in 1660, with a Divini telescope, while Huygens did not manage to observe this shadow (knowing its existence) until 1664. During the 1660's a new crop of observational astronomers came to the foreground, and these astronomers made observations which precluded any further denial of the ring-theory: in other words, they proved what hitherto had been a theory to be a fact.

In 1663, Giuseppe Campani (c. 1620-1695) the Roman telescope maker who shortly afterwards became regarded as by far the best telescope maker in Europe, observed Saturn with telescopes of 13 and 18 feet, and noticed:

> ... that Saturn is surrounded by a circle, in appearance in the shape of an ellipse, situated in such a manner about the globe that the superior part, towards the north-pole, hides a portion of the said globe while, on the contrary, the inferior portion of the circle, that is that portion that is towards the South, is hidden and partly covered by the same globe, so that the inferior part remains behind, and the superior part in front of the star, as is sensibly understood from the apparent place and position of the circle and from the same lightly shadowed outlines of the same circle, as well as of the globe or disc of Saturn, conforming to the figure adjoined here, which I have recently delineated with my own hand, as best I could, but reported and described in the reversed position as the telescope with two convex lenses shows it. (19)



In the same year as he published the above figure in his Ragguaglio di due nuovo Osservazioni (Rome, 1664), Campani also put out a single page with a figure of Saturn (as well as a figure of Jupiter): (20)



In a letter to the Abbé Charles, Campani explained that he had noticed several peculiarities in his observations of Saturn and that therefore he had this single page engraved. These peculiarities were:

> 1) The circle, in the outer part, that is towards the exterior circumference, is less lucid and clear, up to the middle of its plane, and from the middle towards the disc of Saturn it is more lucid and clear than the same disc.
>
> 2) Here and there, towards the superior part, the extremity of the disc appears a bit darkened, that is, less clear than the remainder of the disc ... which I have not said nor ever believed to be caused by the shadow of the circle, leaving the judgment of this to Astronomical Men, while it is solely my duty to note exactly the appearance in the same way that I see it, without involving myself in other things.
>
> 3) The circle is a bit shadowed by a band neighbouring the apparent inferior part of the Globe. (21)

It is clear that Campani had noticed the differences in brightness between the outer ring and the second ring, although he had not seen the division between these rings. It also appears that he had seen the so-called crepe ring, not 'discovered' until the nineteenth century. But most importantly, Campani's observations clearly showed that the ring passes in front of and behind the globe of Saturn. Therefore, any one who accepted Campani's observations had to reject the theories of Fabri, Hevelius, Hodierna, and even Wren. It is interesting to note here that in the same letter in which Huygens comments on these new observations by Campani (he calls him Montani in this letter) he also mentions having (finally) observed the shadow of the body on the ring. (22)

The Ragguaglio, which launched Campani into international fame, was answered in the following year by Adrien Auzout (1622-1691), in a tract entitled Lettre a Monsieur L'Abbé Charles, sur le Ragguaglio ... da Giuseppe Campani (Paris, 1665). In this letter, Auzout discussed Campani's telescopes (made by turning lenses on a lathe, without a mold) and his observations, as well as Hooke's comments on telescopes in Micrographia. Auzout quibbled with Campani about where the shadow of the ring on the body should appear at certain times and positions of Saturn, but he also discussed the other dark band seen by Campani:

> Pour l'ombre d'en haut, qu'il dit que l'Anneau fait sur le corps de Saturne, il ne peut pas l'avoir veuë, puisqu'il n'y en doit point paroitre à cause de sa Latitude Septentrionale, comme il est aisé de le juger; si ce n'etoit dans le mois d'Octobre, au cas qu'elle soit assez forte pour estre visible. Il faut donc qu'il ait un peu de prejugé en ce recontre. Mais il n'est rien de si naturel; car quand on a ouy dire que ce que l'on voit autour de Saturne est un Anneau

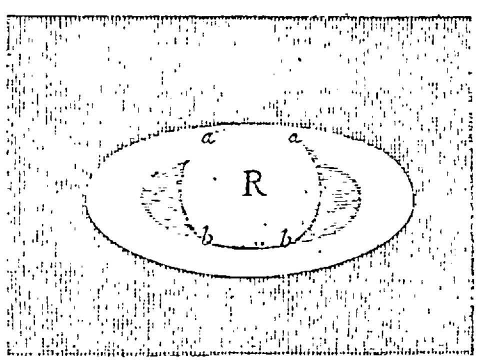
> qui l'environne, on ne peut presques s'empecher en voyant deux pointes obscures, de se les representer continuées de l'une à l'autre, particulierement quand l'air ou la Lunette tremble: & j'avouë que depuis que j'ay veu sa figure, il m'a semblé quelquefois que je voyois cette continuation, sur tout, comme j'ay dit, quand l'air trembloit, quoyque regardant sans songer à cette figure ny à aucune autre, comme je tache toûjours de faire, cela me paroisse, comme si ce deux Corps n'en estoient en cet endroit qu'un continue. (23)

Obviously Auzout too was aware of the tricks our vision can play on us, although in this case he was using that knowledge to attempt to explain away an observation which much later was proven to be correct.

After stating that he himself had been one of the first to observe the shadow of the body on the ring, in 1662, Auzout states:

> Il est vray que je n'ay pas esté en état d'observer Saturne dans son Quadrat Oriental; mais je ne doute point que l'ombre ne paroisse du costé gauche, puisqu'il me semble qu'on [ne] peut plus douter de l'existence de l'Anneau apres tant d'observations de l'ombre que le corps de Saturne jette dessus, conformement à ce qui en doit arriver suivant cette hypothese; n'y ayant pas de raison pourquoy il en jetteroit d'un côté & non pas de l'autre. (24) [ne?]

Auzout was quite right in this. Observations of these shadow effects had been made in Italy, France, England, and Holland. In the following year, an observation by Robert Hooke, published in the Philosophical Transactions, showed almost exactly the same as what Campani's observation of 1664 had shown: (25)



This observation too has been interpreted as including an early sighting of the crepe ring.

It was precisely such observations that conquered the last resistance to the ring-theory. In early 1665 Huygens was informed by Matteo Campani, Giuseppe's brother, that Fabri had abandoned his own theory and had embraced the ring-theory. (26) In the same year Fabri published his Dialogi Physici, in which he stated:

> You [this part of the book is an open letter to Claude Basset] say, it is good and I congratulate you on changing your mind. For you denied that the others go below Jupiter [i.e. the satellites of Jupiter] and you also opposed the Saturnian ring. Now you affirm both. But I have not changed my mind ... I do not remember that I in fact entirely denied the Saturnian ring. In fact, it has merely been asserted by me that all the figures of the Saturnian system, until then delineated by various authors, can be explained by means of the little globes. But this does not prevent me from thinking differently when new observations are brought to my attention, such as they exhibited themselves to me these last few months. Although it is probable to me that that ring goes around Saturn, and that the present phenomenon can hardly be explained otherwise, I nevertheless wait until Saturn is positioned in the Milky Way, which in fact will be the case shortly, and until he returns to the Equator, where he will not be until seven or eight years from now. (27)

In the text of Dialogi Physici Fabri points out that the parallelism between the ring-plane and the Earth's equatorial plane agrees wonderfully with the Tychonian system of the World. (28) Two years later, in his Synopsis Optica (Lyons, 1667), Fabri gave a full exposition of the ring-theory and expressed his total agreement with it. (29)

Although Fabri's conversion certainly was the watershed, it took some time for the last resistance to disappear. In 1665 Riccioli published his Astronomiae Reformatae, in which he raised some questions about the ring-theory. Besides pointing out that according to Huygens the central disc should always protrude from the ring, Riccioli also felt that Huygens had not sufficiently explained how the various spurious appearances were caused by imperfect telescopes. But most importantly, he did not think it necessary to assume a ring which is parallel to the equator. In fact, he preferred an elliptical ring, touching the central body at two points and either revolving or librating about the major axis of the ellipse: 'armilla cingitur, plana, Elliptica, duobus locis cohaerente; sive parallela Aequatori; sive in se circumvolbili, aut libratili versus Mundi Polos'.(30) Moreover, Riccioli thought that it was probable that the elliptical ring was wider at the extremities and narrower where it touches Saturn. This would explain why at some times the connections between the extremities and the globe are invisible. This shows that the theory first formulated by Wren, almost ten years before, and since that time independently arrived at by Bouillau and Frenicle, was still a very attractive one. It should be pointed out however that while Riccioli disagrees with Huygens, the whole section on Saturn in Astronomiae Reformatae is a commentary on Systema Saturnium. Riccioli was probably somewhat out of touch with the latest developments on this subject, because the last observation mentioned is dated 13 May 1661. (31)

Clearly then, by 1665 men like Riccioli were in the minority. It is unimportant to trace the exact dates of the last conversions to the ring-theory (if indeed that were possible). Suffice it to say that by 1670 Huygens' theory was universally accepted; even Hevelius had been won over. (32) But by 1670 the ring-theory had been changed somewhat. It had been apparent since the early 1660's that it did not accurately predict the appearance of the most open phase, and we have dealt with some of the problems caused by this deficiency earlier in this chapter. The obvious solution, adjusting the inclination of the ring to the ecliptic, took some time to be implemented. Indeed, for the time being, astronomers preferred to find fault with the ratio of diameters shown in contemporary observations. Thus, Auzout stated on the subject of Campani's observations presented in the Ragguaglio and in the single sheet of observations of the same year:

> Pour la figure de Saturne, je n'ay rien à ajoûter a ce que je vous ay écrit; car celle de son imprimé; si ce n'est qu'il donne à sa largeur encore un peu plus que la moitié de sa longueur, & ainsi il faudroit que l'Anneau eust plus de 30. degrez d'Inclination. Vous sçavez combien mes mesures sont esloignees de cela ... (33)

But Auzout was not sure what the inclination was and on 6 March 1665 he wrote to Huygens:

> Je seray bien aise aussi que vous me mandies ce que vous aves observé touchant la longueur et la largeur de l'anneau de Saturne et si vous aves trouvé que l'anneau debordoit par dela le corps de Saturne et de combien et quel angle de declinaison lanneau faisoit avec lEcliptic. (34)

Later that year he wrote:

> prenons sil vous plaist bien garde cette année a la proportion de lanneau car je ne la trouve pas si approchante de la triple [i.e. 17:6] que vous et si Saturne en ce temps la ne debordoit point je craindrois que la declinaison ne fust plus grande que 23.30. (35)

In 1667 Huygens (now settled in Paris) made observations to determine anew the inclination of the ring-plane to the ecliptic and to the equatorial plane. On the 16th of July he observed Saturn with Buot, on the 15th of August with Buot, Picard, and Richer, and on the 17th of August of the next year with Picard. This latter observation was published in the Journal des Scavans of 11 February 1669 in an article entitled 'Observation de Saturne faite a la Bibliotheque du Roy'. In the article Huygens reported the new value for the inclination of the ring-plane to the ecliptic as '31 degrez environ'. (36) This value was closer to the actual inclination (28° 12'⅔ at that time) and now the error was on the larger side so that there was no longer a problem of accomodating the disc of the planet within the apparent ellipse of the ring. With this problem solved, Huygens reverted to a value of 9:4 for the ratio of diameters of ring and body, which is very close to the modern value.

With the inclination thus adjusted, the next major test for Huygens' theory was whether or not his predictions concerning the round phase of 1671-1672 would be borne out. In Systema Saturnium Huygens had predicted that the invisibility of the anses would begin in July or August 1671 and last until July or August 1672. (37) In accordance with the predictions (or pretty nearly), Saturn lost its anses towards the end of May 1671, but on 14 August of the same year they became visible again! As soon as this happened, Huygens predicted that the anses would disappear again towards the end of the year. (38) What had happened was that when the ring-plane entered the Earth's orbit, the Earth was barely ahead of it, so that the ring was nearly edge on, and therefore invisible with seventeenth century telescopes. But as the velocity of the Earth is greater than the velocity of the ring-plane through the Earth's orbit, the Earth pulled away from the ring-plane sufficiently to make the ring visible again. In other words, when the ring-plane entered the Earth's orbit, Saturn was near its station and when it became retrograde the ring became visible again. Huygens realised that as soon as Saturn should start moving in consequence again, the ring-plane would 'catch up' with the Earth and therefore he predicted that the anses would disappear again towards the end of the year. He was of course very anxious to explain to the scientific community that this reappearance of the anses was only temporary and that it agreed entirely with the ring-theory, although it had not been predicted in Systema Saturnium. To this end he wrote a note about it to Cassini, and asked him to see to it that it was published, as he himself was about to spend some time in the country. Cassini included Huygens' comments in Suite des Observations des Taches du Soleil faites a l'Academie Royale. Avec quelques autres Observations concernant Saturne, published early in November 1671, which was reproduced in the Philosophical Transactions of 18 December (o.s.) 1671. In his letter to Oldenburg accompanying a copy of the tract Huygens wrote:

> Il y a à la verité quelque chose qui m'a fait retarder, d'une semaine à l'autre, de vous faire celle-ci, qui est l'imprimè dont vous la voyez accompagnee. Car ce qu'il y a la-dedans des observations de Saturne, je l'avois donnè il y a deux mois devant que m'en aller a la campagne mais M. Cassini s'estant proposè de publier en mesme

> tems la suite de ses observations des taches du soleil, la gravure des figures et autres circonstances y ont apportè cette longueur, que tout cela ne paroit que maintenant et a mon grand regret, parce qu'ayant predit le retour de la forme ronde de Saturne vers la fin de l'année, peu s'en faut que la prediction ne soit accomplie devant qu'on en ait etè averti, - je dis pour les pays estrangers, car nos Messieurs savent bien, qu'aussi tost que M. Cassini m'eut appris que les bras de Saturne estoient revenues, je dis qu'assurement ils disparoitroient devant la fin de l'année. Je les observay encore hier au soir, mais si foibles et obscurs qu'on avoit de la peine a les discerner; de sorte que dans peu de jours ils ne paroitront plus de tout. Ceci confirme tout à fait mon hypothese de l'anneau, qui presentement disparoit a nos yeux, a mesure que les rayons du soleil en eclaircit obliquement la surface plate tournee vers nostre vue. Et les apparences de cette année donneront moyen de predire le retour de la figure ronde avec bien plus de justesse qu'auparavant. (39)

As indicated here by Huygens, the disappearance of the anses, with Saturn at station, was due to the very oblique illumination of the ring by the Sun. Thus, although the anses retained their width, they became progressively dimmer:

> ... on leur voyoit perdre peu à peu leur clarté, quoy-qu'ils demeurassent toûjours assez larges pour estre vûs; ce qui estoit une marque certaine que les rayons du Soleil éclairoient fort obliquement la surface de l'anneau de Saturne qui estoit tournée vers nous, & qu'à la fin ils ne l'éclairoient plus du tout, mais bien l'autre surface opposée. Dans l'apparition precedente de la figure ronde, depuis la fin de May jusqu'au 14 d'Aoust les bras n'estoient devenus invisibles faute d'estre éclairez, mais à cause que nostre vûë étoit tres-peu ou point du tout élevée sur la surface de l'anneau que le Soleil regardoit. (40)

According to Huygens' prediction, Saturn lost its anses again in December 1671. The ring-plane passed through the Sun and through the Earth after Saturn's heliacal setting. With modern telescopes the ring would have remained visible through the entire period of Saturn's visibility in 1671, since with these telescopes the ring is only invisible when the ring-plane passes through the Earth. But with the telescopes used by Huygens and Cassini, the ring was invisible when Saturn was within a few degrees of its equinoxes, although Huygens had adjusted this value from 6°, given in Systema Saturnium, to 2°, an improvement which he quite rightly ascribed to the improvement of telescopes. (41)

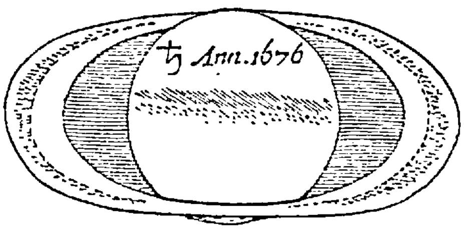
After its heliacal rising in June 1672, Saturn showed its anses clearly. (42) Thus, neglecting the interruption in the period of invisibility of the anses, Huygens' predictions were only in error by a few months, whereas Hevelius' predictions in De Nativa Saturni Facie (see p. 76) were found to be a whole year in error. On the basis of the observations of 1671 and 1672, the predicted dates for the solitary appearances of 1685 and 1701 were adjusted by Huygens to dates very close to the actual occurrences. (43)

Thus, by 1672 the 'system of Saturn' had been sufficiently adjusted to be an entirely satisfactory and elegant astronomical explanation of the appearances of Saturn, and it was indeed never seriously challenged again. But the physical question about the stability of the ring had by no means been answered. Was it a thick solid ring as Huygens claimed, or was it a very thin structure made up of liquids, vapours, or a very large number of very small satellites? Huygens held fast to the idea of a thick ring. On 18 July 1672 he saw a dark trace across Saturn's disc which he judged to be the dark exterior edge of the ring (44), and on a similar observation made on 1 April 1685 he remarked: 'A rather dark line clearly appeared across the middle disc, continuous with the lowest trace of the arms. This had to be the exterior superfice of the ring, as I have concluded from the position of ♄.' (45) In Cosmotheoros, published posthumously in 1698, Huygens wrote: 'Quant à son épaisseur, les observations font voir qu'elle est petite; cependant, par rapport au diametre, cette exiguité ne sera pas excessive: l'épaisseur peut même être estimée de plus de six cents milles Germaniques [about 2400 miles].' (46) Thus, until the end of his life Huygens believed the ring to be thick and, in view of this last statement, also solid.

But the question of the composition of the ring was curiously neglected in the seventeenth century. Newton, for instance, does not refer to it at all in his Principia. It remained a matter of speculation until Maxwell's theoretical paper of 1857, while the experimental proof of the satellite nature of the ring was not supplied until 1895, by Keeler (see Appendix A). Cassini, whose opinions were greatly respected, thought until the end of his life that the ring was made up of a large number of satellites. This opinion was shared by his son, Jacques Cassini (1677-1756), who wrote:

> On peut donc supposer avec beaucoup de vrai-semblance que l'anneau de Saturne est formé d'une infinité des petites Planetes fort près l'une de l'autre, qui étant comprises dans son Atmosphere, sont entraînées par le mouvement qui fait tourner Saturne autour de son centre, & que dans cette Atmosphere il y a de grands nuages paralleles au plan de l'anneau, qui interceptant une partie des rayons du Soleil, paroissent sur Saturne en forme de bandes paralleles à cet anneau. (47)

To Jean Dominique Cassini belongs the honour of having added more important observations to the ones already made of Saturn. In 1671, 1673, and 1684 he discovered four new satellites, all, of course, with Campani telescopes (48), and in 1675 he discovered the division between the dimmer outside part of the ring and the brighter inside part. (49) Although it has been advanced that this division had already been discovered by William Balle in 1665 (50), that division quite deservedly bears Cassini's name.



From Phil. Trans., XI (1676), No. 128, opposite p. 710

CONCLUSION

It is apparent from the preceding discussion that the problem of Saturn involved a great deal more than the improvement of the telescope. Indeed, all the interesting questions connected with this problem had little to do with the improvements made in this instrument.

Clearly, Galileo was not a methodical observer as was Tycho Brahe or Johannes Hevelius, and his interest in Saturn is limited to certain specific periods, 1610, 1612-1613, and 1616. In the first year he first observed Saturn, in 1612 the planet lost its 'companions' which it regained the next year, and in 1616 Galileo first observed the appearance that came to be known as the 'handled appearance'. His references to Saturn after this time are all in response to queries and observations made by his correspondents. There is no evidence to suggest that after some initial attempts Galileo ever seriously worked towards a solution of the problem. Perhaps his admonition to Benedetto Castelli in 1640 (see p. 21) is indicative of the change in role of the telescope from a toy to an astronomical tool.

Serious work towards the solution of the problem started in 1642, when the solitary appearance renewed interest in Saturn. As shown in chapter 3, Hevelius, Bouillau, and Riccioli all began their observations of Saturn around this time, while Gassendi, who had made a few observation of the planet before that time, made numerous observations starting in 1642. And this is not true for Saturn alone. The serious contemplation of the planets <u>for its own sake</u> goes back to the early 1640's and therefore telescopic observational astronomy can be said to have started at this time. Gassendi was the outstanding figure in this new field until about 1650.

It is indeed intriguing that none of these men formulated a theory about Saturn's appearances until 1656. Gassendi knew that Galileo had seen the planet solitary in 1612, and he himself had seen it fully handled in 1633 and 1634, solitary in 1642, and since then he had observed a continuous series of appearances gradually leading to the fully handled appearance of the late 1640's. He might thus have realised that he had witnessed a complete cycle of Saturn's phases between 1633 and 1648, but he observed Saturn for another seven years without ever formulating a theory. The realisation, which first came to Wren and Huygens in the middle 1650's that not all appearances depicted were equally trustworthy, appears never to have occurred to Gassendi, or, for that matter, to Hevelius, Bouillau, or Riccioli, before it was brought to their attention by others. Thus, taking all his own observations, as well as those of e.g. Fontana and Hevelius, at face value, Gassendi was faced by a formidable task. It is very difficult indeed to imagine any theory that could account for all those appearances. But if Gassendi had rejected some observations, it is equally likely that he would have rejected the handled appearance as the tri-spherical appearance, and perhaps more likely. The tri-spherical appearance was the primary appearance, corresponding with the vague model in which Saturn's appearances were somehow explained by the motion of satellites around the central globe. Thus, Gassendi, in the absence of the critical tools with which to evaluate observations, was left with a mass of conflicting information. But although Gassendi never arrived at a theory, it must not be thought that his observations were all in vain. The fact that they could supply very useful data is amply illustrated by Borelli's use of them to show the constancy of the 9:4 ratio of the diameters of the ring and the body.

As has been pointed out in chapter 4, since Hevelius did not question the validity of his own observations sufficiently, he never solved the dichotomy between the handled and the tri-spherical appearances. But he did firmly establish the handled appearance as the primary one. Actually, Hevelius' theory is only an adjunct to the more important point of _De Nativa Saturni Facie_: the period and progression of the phases.

Although perhaps more evidence is needed to prove the point, it is questionable whether Huygens' 12 foot telescope of 1655, with which he discovered Titan and which he used at the time when he formulated the ring-hypothesis, was in fact better than the best telescopes of Divini, Hevelius, Neile, or even Wiesel. The important observations made by Huygens and Wren between 1655 and 1657 were probably largely due to their mental attitude. Hevelius already knew, based on his experience of 1642 and 1643, that just before and after the solitary appearance the anses would appear as detached globes, and he verified this in 1654 and 1655. On the other hand, Wren had already tentatively formulated his hypothesis when he made the crucial observation which showed that the 'anses kept their length'. Both Wren and Huygens were less prejudiced in their observations than were the older observers. In fact, the important observation made by Huygens in 1655, which showed the anses fully extended, thin, and attached to the central body, was his very first recorded observation of Saturn, and on the same night he also made his first recorded observation of Jupiter! Furthermore, the shadow of the body on the ring was discovered in Florence in 1660, with a Divini telescope.

Huygens did not see this shadow (although he knew of its existence) until 1664, but the Divini telescope with which it was first seen had been given to Grand Duke Ferdinand II by Sir Kenelm Digby, and this means that that telescope was made at the latest in the early 1650's. Divini himself insisted that he had never seen any shadows and that he had seen the anses as separated globes before and after the solitary appearance of 1656. How can such facts possibly be explained by ascribing the solution of the problem of Saturn to Huygens' superior telescopes?

Clearly, the theories of Wren and Huygens represent, as it were, a complete break with the past. Both men were young - in their middle twenties - and not subject to the same prejudices as the older observers. In Huygens' case, his conception of the ring-hypothesis is inextricably connected with his ideas about the Universe in general and about our solar system in particular. Although this is conjecture (since Huygens does not state it specifically) it is difficult to reject the obvious connection between Cartesian vortices and the mental processes described by Huygens that led to the realisation that a ring causes all the appearances. Would Hevelius or Riccioli have thought in terms of a rapid rotation of Saturn on its axis, which rotation was somehow connected with the circular motion of Titan and all the matter between Saturn and Titan? This is hardly likely! But if that is so, then cosmological ideas had a great deal to do with discoveries in the heavens.

The great inconsistency in Huygens' ideas about Saturn is his tenacious belief in a thick solid ring, right up to the end of his life, although he was attacked on this subject numerous times. Apparently his belief was based on several observations of a black band adjacent to the outside edge of the ring. These apparently erroneous observations were not limited to the period 1656-1659, but were repeated on several occasions until late in Huygens' life. However they were never corroborated by any one else. This seems to indicate that the idea of a thick solid ring, once formulated in Huygens' mind, gave rise to erroneous observations. However, was this idea itself based on an accidental error in observing or depicting an observation? This is possible, but I believe that it is more likely an error of interpretation which caused the idea of a thick solid ring to be formulated. It seems most reasonable to suppose that right from the beginning Huygens interpreted the dark band across Saturn's disc as the edge of the ring. In this context it should be noted that not once, in Systema Saturnium, does Huygens raise the possibility that the dark band, so often seen across Saturn's disc, could be the shadow of the ring on the body.

This of course brings up the question as to how good an observer Huygens really was. There is little doubt that he was a better observer than Divini and even Hevelius, but it is clear that he was not as good an observer as one might think. After the shadow of the body on the ring had been observed in Florence in 1660, it was seen by Auzout in 1662 and by Campani well before 1664. Yet Huygens did not perceive it until 1664, four years after it had first been brought to his attention. But perhaps the best indication is the fact that in 1675 both Huygens and Cassini made observations of Saturn with the splendid 34 foot Campani telescope installed in the observatory in Paris. Huygens saw the difference in brightness between the outside half and the inside half of the ring (first noticed by Campani in 1664), but Cassini discovered the separation between these two parts. Now it may well be that when Cassini made this discovery the visual conditions were better than at the times when Huygens observed the planet, but from the wording of Cassini's announcement in Journal des Sçavans (1 March 1677, V:32-33) it appears as though the division was observed on numerous occasions after its initial discovery. Yet, as far as can be determined, Huygens never observed Cassini's division and does not even mention it in his Cosmotheoros

In summing up, I believe that Huygens was a maker of excellent telescopes. His early telescopes (made between 1655 and 1660) were indeed among the best in Europe at that time. But they were almost certainly not superior to those of Divini and probably equalled by those of Neile, Hevelius, and Wiesel. He was probably surpassed in this art by Divini in the early 1660's, but both Huygens and Divini were eclipsed by Campani. As an observer Huygens must be rated highly, but he was certainly not as good an observer as Cassini, and probably not as good as Flamsteed. The only necessity for this evaluation is the traditional belief that Huygens' telescopes were vastly superior to those of his contemporaries (with the exception of those of Campani). Surely, Huygens' achievements do not have to be exaggerated. He towered above all the men we have dealt with in this thesis; indeed, he was certainly the greatest scientist in Europe between 1655 and 1685, and no keener example of his brilliantly incisive mind exists than his solution of the problem of Saturn.

Besides the expected objections to the ring-theory, the unprecedented new shape in the heavens, the awkwardness of the thick ring, etc., there were some objections which strike one as being not strictly scientific. Hevelius in all his fury retained his integrity as a scientist: his objection were in the final analysis justified by weaknesses in the ring-theory or by his own observations. But the same is not true for Bouillau, Riccioli and Frenicle. The criticisms of all three were based justly on the predicted shape of Saturn when the ring would be most open. Huygens did in fact point out to Bouillau in 1659 that this objection could be overcome by adjusting the obliquity of the ring. But why did not Bouillau, Riccioli, and Frenicle realise this in the first place? There is in all three cases a large gap in logic. Surely, logic demands that a theory should not be rejected until it had been shown that it cannot account for the phenomena qualitatively. If a minor adjustment in some quantitative aspect can bring the theory in harmony with the phenomena, then that adjustment should be preferred. But perhaps these men considered the $23\frac{1}{2}^{\circ}$ inclination of the ring-plane to the ecliptic a qualitative aspect without which the whole theory would cease to be a coherent theory. If that were true (and the $23\frac{1}{2}^{\circ}$ inclination was a powerful analogy indeed), we might expect these adversaries to have stated this. Yet not one of them did so. Therefore, their failure to realise that a relatively simple adjustment would eliminate their objections without materially altering the ring-theory seems to indicate a lapse of logic in all three.

But what occasioned such a lapse in logic? Bouillau was a man of great intelligence and a mathematical astronomer; Riccioli was nothing if not fair (indeed, he devoted his scientific life to convincing scientists of the incorrectness of the Sun-centered hypothesis by scientific arguments, not by dogma); Frenicle was a highly skilled mathematician. The thing that all three have in common in this matter is that they all had a theory (very similar theories, as it turned out). Is it perhaps the case that one's scientific judgment is likely to suffer when one is committed to a theory of one's own?

A number of tests were proposed to check the validity of the ring-theory. By far the most ingenious and conclusive of these was the test made by the Accademia del Cimento. The dichotomy between the handled and the tri-spherical appearances had been explained away by Huygens (as indeed by Wren), but mere explanation was not conclusive. The model of the ring-hypothesis built in Florence proved in no uncertain way that the ring could indeed give rise to a tri-spherical appearance. One may assume that Wren had already satisfied himself of the same in 1658 by means of his model, but he did not discuss this fully. Although Huygens never revealed the experiments and comments of the Accademia (in accordance with Dati's request), the learned world was informed of such experiments by Fabri in Pro Sua Annotatione, and by Campani in his Ragguaglio, and one may assume that by about 1665 all astronomers had been convinced that a ring can give rise to the tri-spherical appearance, although initially it had been thought that only in 1670 or 1671 could this issue be settled. Apparently Hevelius persisted in this belief, because evidence of his acceptance of the ring-theory does not appear until 1670.

It is interesting to note here that had the events of 1671-1672 really been a test for the ring-hypothesis, Huygens would have had a difficult task in explaining that his theory could in fact account for the phenomena observed in those years. Dati had written in 1660 that if the anses did not decrease in length but became narrower, Huygens had to be right, but if they became shorter without a decrease in width, this would be evidence for Fabri's theory. If they became both narrower and shorter, both theories would be in doubt. As it happened, in the autumn of 1671 the anses retained their length and width but became progressively dimmer until they finally disappeared altogether. Huygens' hypothesis could of course account for this phenomenon, but he had not predicted it in his Systema Saturnium, and if the ring-theory had still been in doubt his explanation might have sounded rather ad hoc. But in fact, by 1671 Huygens no longer had to convince any one of the correctness of the ring-theory, and thus he was not addressing himself to a skeptical audience, as had been the case in 1659.

One reason why Huygens had not foreseen the events of 1671 was that he had postulated in 1659 that the anses would be invisible within a space of 6^{o} on either side of Saturn's true equinoxes, regardless of the position of the Earth. This dictated that the anses would indeed be invisible for a whole year in 1671-1672. But by 1671 telescopes had been sufficiently improved - at any rate those made by Campani - to force Huygens to narrow these limits to 2^{o} on either side of the true equinoxes. Based on the experiences of 1671-1672 Huygens adjusted his predictions concerning the period of invisibility of the ring in 1685. In 1672 he predicted that the ring would be invisible from July to November 1685. In fact, the ring was invisible in that year from mid-July until October. Thus it appears that the improvements made in the telescope were much greater between 1656 and 1671 than between 1671 and 1685.

Although speculation as to the ring's constitution may have been useless in the seventeenth century because it was impossible to decide for certain whether the ring was solid or a swarm of very small satellites, it is nevertheless surprising that after the initial discussion sparked off by the publication of Systema Saturnium so little speculation as to the ring's constitution is to be found. As mentioned in chapter 11, Newton never mentions the ring's constitution in his Principia, and astronomy books ignored the question also. Opinion on this subject appears to have been divided in the eighteenth century. (see also Appendix A)

In studying the problem of Saturn, one is struck time and again by the large role played in astronomical speculation by analogies. When, of course, one speculated about the planets, one had only the example of the Earth - and to a minor extent that of the Moon - as a basis for detailed knowledge. Quite naturally therefore, in the type of popular cosmological speculation exemplified by Fontenelle's Entretiens sur la Pluralité des Mondes and Huygens' Cosmotheoros the examples of living beings and all their concomitants are extrapolated from the Earth to other planets. The foundation of such speculation goes back at least to Giordano Bruno. But in professional astronomy such analogies were powerful tools too. Huygens' use of the analogy between the Earth-Moon system and the system of Saturn and its newly discovered satellite in his train of thought that led to the ring-theory is perhaps the best example of how fruitful such analogies could be. But the discovery of the rotation of Mars and Jupiter, a phenomenon accepted on faith before the actual discovery, is also a good example of such an analogy. Borelli's and Magalotti's conjectures on the formation and stability of the ring are replete with analogies between Earthly phenomena and possible Saturnian counterparts. All this shows a fundamental scientific faith in the constancy of Nature, or the universality of Nature's laws, which pervaded the air in the second half of the seventeenth century. This faith was particularly important in astronomy. The older generation of astronomers, e.g. Gassendi and Riccioli, never thought in those terms. It appears to represent an almost complete change in conception of the Universe, and most likely Descartes did more than any one to bring about this change. It is almost as though only after Descartes had scientists freed themselves from ancient prejudices. This is not to say that before Descartes they did not think about the similarity between the Earth and other planets, but it does mean that they did not perceive or think of the Universe subconsciously as being uniform and subject everywhere to the same laws. Certainly, the most important result of this new way of considering the Universe was the identification of the force that keeps Saturn's ring tied to Saturn and that ties the satellites of Saturn and Jupiter to their parent bodies with the force that keeps the planets tied to the Sun and the force that attracts heavy bodies to the centre of the Earth. This concept, impossible to arrive at without a conviction of the fundamental unity of Nature and the universality of Nature's laws, was a direct result of analogous thinking and it was 'in the air' long before

Newton mathematised it.

Observational astronomy fed on the principle of the uniformity of Nature and in turn reinforced it. In doing so it made important contributions to cosmology. The new accuracy gained by joining telescopes to measuring instruments allowed astronomers to fix the distances within our solar system to within 10% of their modern values, and by thus fixing the base-line, allowed a rough determination of at least the order of magnitude of the distance to the nearest fixed stars. Careful measurements also supplied some of the important numerical data used by Newton to prove his new laws and some of the old ones, e.g. the tables of the satellites of Jupiter and Saturn, used to illustrate Kepler's third law. It supplied the data which allowed men like Halley and Newton to solve the problem of comets. In fact, the only notable failure in this field was the case of the theory of the Moon, and this failure was as much Newton's fault as it was Flamsteed's. It is therefore fair to say that observational astronomy played an absolutely essential role in the development of the Newtonian conception of the Universe.

NOTES

Abbreviations

G.G.	Le Opere di Galileo Galilei (Edizione Nazionale), Florence, 1929-1939 (reprint of the 1890-1909 edition.
H.O.	The Correspondence of Henry Oldenburg, London, 1964 -
Hist. & Mem.	Histoire de l'Académie des Sciences avec les Mémoires de Mathématique et de Physique tirés des Registres de cette Académie, Paris, 1699-1790.
Mem.	Mémoires de l'Académie des Sciences, contenant ~~les~~ les Ouvrages adoptés par cette Académie avant son Renouvellement en 1699, Paris, 1729-1733, vols III-XI. The first two volumes of this series have the title Histoire de l'Académie des Sciences, depuis son Établissement en 1666 jusqu'a son Renouvellement en 1699, Paris, 1733.
Notes and Records	Notes and Records of the Royal Society of London.
O.C.	Oeuvres Complètes de Christiaan Huygens, The Hague, 1888-1950.
Phil. Trans.	Philosophical Transactions of the Royal Society.

Notes to pp. 11-17

CHAPTER 1

[1]Galileo, The Starry Messenger, in Discoveries and Opinions of Galileo, ed. S. Drake, (New York, 1957), p. 58.

[2]G.G., X: pp. 409-410. Unless otherwise indicated, the translations are mine.

[3]E. S. Carlos, tr., A Part of the Preface to Kepler's Dioptrics, forming a Continuation of Galileo's Sidereal Messenger, (London, 1880), p. 88.

[4]Ibid.

[5]'T. Harriot Mathematical Papers', British Museum MSS., Add. 6788, ff. 250v-252v.

[6]G.G., X: p. 474.

[7]Ibid.

[8]Carlos, op. cit., pp. 91-92. Geryon: a mythical king in Spain having three bodies.

[9]G.G., X: pp. 484-485.

[10]G.G., XI: pp. 92-93.

[11]G.G., XI: pp. 77-78.

[12]J. Christmann, Nodus Gordius ex Doctrinae sinuum explicatus; accedit appendix observationum, quae per radium artificiosum habitae sunt circa Saturnum, Jovem et lucidiores stellas fixas, (Heidelberg, 1612), p. 41. Christmann has a good claim to being the first astronomer to apply the telescope to a measuring instrument - in this case the sextant. See H. Ludendorff, 'Über die erste Verbindung des Fernrohres mit astronomischen Messinstrumenten', Astronomische Nachrichten, Band 213 (1921), No. 5112, pp. 385-390.

[13]G.G., V: p. 31.

[14]G.G., V: pp. 110-111

[15]G.G., V: pp. 237-238.

[16]Ibid.

[17]G.G., XI: p. 532

[18]A. Koyré, 'A documentary History of the Problem of Fall from Kepler to Newton', Transactions of the American Philosophical Society, new series, XLV (1955), pp. 330-331.

Notes to pp. 17-23.

19 G. Locher, Disquisitiones Mathematicae, (Ingolstadt, 1614), p. 88.

20 Ibid.

21 G.G., V: p. 238.

22 Galileo, Dialogues concerning the Two Chief Systems of the World, tr. S. Drake, (Berkeley, 1953), p. 263.

23 E. Zinner, Entstehung und Ausbreitung der Copernicanischen Lehre, (Erlangen, 1943), p. 493.

24 C. Scheiner, 'Tractatus de Tubo Optico', Bayrische Staatsbibliothek (Munich) MSS. Clm. 12425, pp. 64-65.

25 The entries on the page surrounding the sketch are all dated between June and October 1616, G.G., XII: p. 276, n. 1.

26 G.G., XII: p. 276. See also, A. Favaro, 'Intorno alla Apparenza di Saturno osservata da Galileo Galilei nell'Agosto dell'Anno 1616', Atti del Reale Istituto Veneto di Scienze, Lettere ed Arti, LX (1900-1901), parte seconda, pp. 415-432.

27 G.G., XIII: p. 13.

28 G.G., VI: p. 361.

29 G.G., XIII: pp. 286-287.

30 Galileo, Dialogues concerning the Two Chief Systems of the World, op. cit., p. 263.

31 G.G., XVIII: pp. 238-239.

32 J.B. Cysat, Mathematica Astronomica, (Ingolstadt, 1619), p. 36.

33 G.A. Baranzani, Uranoscopia, (Geneva, 1617), p. 124.

34 G. Biancani, Sphaera Mundi, (Modena, 1620; second edition Modena, 1635), p. 155.

35 C. Borri, Collecta Astronomica, (Lisbon, 1631), p. 158.

36 J. Kepler, Epitomes Astronomiae, (Linz, 1618-1621), Book IV, p. 450.

37 J. Wilkins, A Discourse concerning a New World and another Planet, (London, 1640), p. 49.

Notes to pp. 24-30.

CHAPTER 2

[1]C. de Waard, De Uitvinding der Verrekijkers, (The Hague, 1906); E. Rosen, The Naming of the Telescope, (New York, 1947); V. Ronchi, Galileo e il Cannocchiale, (Udine, 1942).

[2]Peiresc to Galileo, 17 April 1635, G.G., XVI: pp. 259-262.

[3]P. Gassendi, Opera Omnia,(Lyons, 1658), IV, passim.

[4]A. Favaro, 'Intorno ai Cannocchiali costruiti ed usati da Galileo Galilei', Atti del Reale Istituto Veneto di Scienze, Lettere ed Arti, LX (1900-1901), parte seconda, pp. 317-342.

[5]G. Abetti, 'I Cannocchiali di Galileo e dei suoi Discepoli', l'Universo, Sept. 1923, pp. 685-692; V. Ronchi, 'Sopra i Cannocchiali di Galileo', ibid., Oct. 1923, pp. 791-804.

[6]Castelli to Galileo, 18 July 1637, G.G., XVII: p. 139.

[7]G. Locher, Disquisitiones Mathematicae, (Ingolstadt, 1614), p. 58.

[8]M. Niesten, 'Sur la Carte de la Lune par van Langren', Ciel et Terre, IV (1884), p. 313; for Gassendi's moonmap see A. Tiberghien, 'Cartes Lunaires peu connues: II: C. Mellan 1634-5', Ciel et Terre, XLVII (1932),

[9]Johann Wiesel's pricelist of 1647, reproduced in T. H. Court and M. von Rohr, 'New Knowledge of Old Telescopes', Transactions of the Optical Society, XXXII (1930-1931), pp. 118-119.

[10]C. Scheiner, Rosa Ursina,(Rome, 1630), p. 130.

[11]F. Fontana, Novae Coelestium Terrestriumque Rerum Observationes, (Naples, 1646), p. 21.

[12]M. Hirzgarter, Detectio Dioptrica, (Frankfurt, 1643), pp. 12-13. This book showed some of the observations of Fontana.

[13]A. M. S. de Rheita, Oculus Enoch et Eliae, (Antwerp, 1645), p. 339.

[14]Ibid., p. 350.

[15]Ibid., p. 356.

[16]Sir Charles Cavendish to J. Pell, 20 October 1644, 16 November 1644, 20 December 1644 (o.s.), J. O. Halliwell, A Collection of Letters illustrative of the Progress of Science in England, (London, 1841), pp. 85-88.

[17]B. de Monconys, Journal des Voyages de Monsieur Monconys, (3 vols.), (Lyons, 1664-1666), I: p. 117.

[18]Wiesel, loc. cit.

[19]I quote here from Court and Rohr, op. cit., p. 120.

Notes to pp. 31-35.

[20]J. Hevelius, Selenographia, (Danzig, 1647), p. 12.

[21]Ibid., p. 14.

[22]Ibid., plate opposite p. 40.

[23]G. B. Riccioli, Almagestum Novum, (Bologna, 1651), p. 204.

[24]C. Huygens, De Saturni Luna Observatio Nova, (The Hague, 1656), O.C., XV: pp. 173-177.

[25]H. J. Klein, 'Eine Prüfung alter Fernrohrobjektive von Huygens und Campani', Sirius, XXXII (1899), pp. 277-280.

[26]Ibid., pp. 277-278.

[27]Ibid., p. 279.

[28]H. C. King, The History of the Telescope , (London, 1955), pp. 48 ff.

[29]Ibid., p. 51.

[30]Galileo, Il Saggiatore, in The Controversy on the Comets of 1618, tr. S. Drake and C. D. O'Malley, (Philadelphia, 1960), pp. 212-213.

[31]Rheita, op. cit., p. 353.

[32]Although the 18 ft. objective ($10\frac{1}{4}$ braccia) is still preserved in Florence, Ronchi and Abetti did not perform any tests on it. See also Antinori's introduction to the 1841 edition of the Saggi (p. 24), in which he also ascribes a telescope of 18 braccia to Torricelli. But this is without basis. As shown in Appendix C, this telescope was made by Divini.

[33]Hevelius to Huygens, 7 September 1656, O.C., I: p. 487.

[34]Huygens, Systema Saturnium, (The Hague, 1659), O.C., XV: pp. 228-230.

[35]Wallis to Huygens, 22 March 1656, O.C., I: pp. 396-397.

[36]Wren to Neile, 1 October 1661 (o.s.), in C. A. Ronan and Sir Harold Hartley, 'Sir Paul Neile F.R.S. (1613-1686)', Notes and Records, XV (1960), p. 163.

[37]Borelli to Viviani, 17 August 1658, Biblioteca Nazionale (Florence) MSS. Gal. 254, ff. 110-111.

[38]E. Divini, Brevis Annotatio in Systema Saturnium, (Rome, 1660), O.C., XV: p. 409.

[39]Chapelain to Huygens, 25 February 1669, O.C., VI: p. 19; Huygens to Oldenburg, 10 August 1669, O.C., VI: p. 480. In this last letter Huygens writes: 'Les miens que j'ay fait jusqu'icy ne sont que de 45 pieds et j'ay estè tellement traverse par la deffaut de la matiere qu'a peine en ay je encore un qui soit bon.'

[40]Auzout to Huygens, 6 November 1665, *O.C.*, V: pp. 526-528.

[41]Christiaan Huygens to Constantijn Huygens (brother), 30 November 1668, *O.C.*, VI: p. 300.

[42]Hevelius to Oldenburg, 3 June 1668 (o.s.), *H.O.*, V: p. 447.

[43]Oldenburg to Hevelius, 2 August 1669 (o.s.), *H.O.*, VI: pp. 165-171.

[44]*H.O.*, VI: p. xxiii.

[45]A. Danjon and A Couder, *Lunettes et Télescopes*, (Paris, 1935), pp. 645-647.

[46]C. Wolf, *Histoire de l'Observatoire*, (Paris, 1902), pp. 168-172.

[47]A. Armitage, *Edmond Halley*, (London, 1966), p. 41.

[48]Wolf, *op. cit.*, p. 164.

[49]Christiaan Huygens to Constantijn Huygens (brother), 1 September 1693, *O.C.*, X: p. 488. Note that after the discovery of the last two satellites, Cassini could see all five satellites with his 34 foot Campani telescope.

[50]G. Tabarroni, 'La Lente spezzata del Campani conservata nell'Istituto di Fisica dell'Universita di Bologna', *Strenna Storica Bolognese*, XVII (1967), pp. 433-441.

CHAPTER 3

[1]G. Biancani, *Sphaera Mundi*, (Modena, 1635), p. 155.

[2]M. Hortensius, *De Mercurio sub Sole viso et Venere invisa*, Leyden, 1633, p. 58.

[3]P. Gassendi, *Opera Omnia*, ed. H. L. Habert de Montmor,(Lyons, 1658), IV: p. 142 (entry 19 June 1633).

[4]*Ibid.*, IV: p. 183 (entry 13 April 1634).

[5]*Ibid.*, IV: p. 441.

[6]'Collection Boulliau' XL, Bibliothèque Nationale MSS. FF 13058, f. 13058 *et seq.*

[7]*Ibid.*, f. 63.

[8]J. Hevelius, *Selenographia*, (Danzig, 1647), p. 42. *Idem*, *Dissertatio de Nativa Saturni Facie*, (Danzig, 1656), p. 7.

[9]G. B. Riccioli, *Almagestum Novum*, (Bologna, 1651), p. 488.

[10]Gassendi, *op. cit.*, IV: p. 441.

Notes to pp. 46-49.

[11]Peiresc and Gassendi to Galileo, 24 February 1637, G.G., XVII: p. 34.

[12]At the 'Journées Gassendistes' held at the Centre International de Synthèse in 1953, Alexandre Koyré gave an address on Gassendi as a 'savant', in which he stated: 'Gassendi, en effet, n'est pas un grand savant, et dans l'histoire de la science, au sens strict du terme, la place qui lui revient n'est pas très important. Il est clair que l'on ne peut pas le comparer aux grands génies qui illustrèrent son époque - à un Descartes, un Fermat, un Pascal; ni même à un Roberval ou à un Mersenne. Il n'a rien inventé, rien découvert et ... il n'existe pas la loi de Gassendi. Même de loi fausse.' (p. 60)
But, to be fair to Koyre, he does make the remark: 'Mais ne soyons pas trop sévères, et tâchons d'éviter l'anachronisme. Car si, pour nous, Gassendi n'est pas un grand savant, pour ses contemporains c'en était un, et même un tres grand, l'égal et le rival de Descartes.' (p. 61)
Koyré also describes Gassendi's astronomy. (pp. 62-63)
See: Centre International de Synthèse (Rochot, Koyré, et. al.), Pierre Gassendi sa Vie et son Oeuvre 1592-1655, (Paris, 1955).

[13]Rheita, Novem Stellae circa Jovem circa Saturnum sex, circa Martem non-nullae, (Louvain, 1643), pp. 12-59.

[14]Ibid., p. 127.

[15]M. Hirzgarter, Detectio Dioptrica, (Frankfurt, 1643), p. 12.

[16]Ibid., p. 21.

[17]Rheita, Oculus Enoch et Eliae, (Antwerp, 1645), pp. 277-280.

[18]F. Fontana, Novae Coelestium Terrestriumque Rerum Observationes, (Naples, 1646), pp. 128-140.

[19]A. Kircher, De Ars Magna Lucis et Umbrae, (Rome, 1646), p. 17.

[20]Hevelius, Selenographia, pp. 42-44.

[21]reproduced in Bullettino di Bibliografia et di Storia delle Scienze matematiche e fisiche pubblicato da B. Boncompagni, XX (1887), facing p. 614.

[22]P. Gassendi, Epicuri Philosophia, (Lyons, 1649), pp. 904 ff.

[23]Riccioli, op. cit., pp. 487-488.

[24]Rheita, Novem Stellae, pp. 12-59.

[25]Hirzgarter, op. cit., p. 22.

[26]Rheita, Oculus Enoch et Eliae, p. 277.

[27]Fontana, op. cit., pp. 128-140.

[28]Ibid., pp. 136, 138, 140.

[29]Ibid., p. 127.

[30]Hevelius, Selenographia, p. 42.

Notes to pp. 49-71.

[31]*Ibid.*, p. 44.

[32]Gassendi, *Epicuri Philosophia*, p. 904.

[33]Riccioli, *op. cit.*, p. 723.

[34]J. Phocylides Holwarda, *Philosophia Naturalis seu Physica Vetus-Nova*, (Franeker, 1651), pp. 259-260.

[35]Hevelius, *Selenographia*, p. 44.

[36]Gassendi, *Epicuri Philosophia*, pp. 904-905.

[37]Hevelius to Bouillau, 18 February 1650, 'Collection Boulliau', *op. cit.*, XXV (FF 13043). f. 20v.

[38]*Astronomische Nachrichten*, 1833, no. 252, p. 199.

[39]A. Favaro, 'Intorno alla Apparenza di Saturno osservata da Galileo Galilei nell'Agosto dell'Anno 1616', *Atti del Reale Istituto Veneto di Scienze, Lettere ed Arti*, LX (1900-1901), parte seconda, p. 428. The Italian text is as follows: 'Questa figura ... e per se stessa e per la dichiarazione che l'accompagna, porge una apparenza di Saturno così vicina a quella che quarant'anni più tardi fu rivelata dall'Huygens che non siano quelle vedute e descritte dallo Scheiner, dal Riccioli, dall'Evelio, dal Gassendi, dal Divini, dall'Odierna, dal Bouillaud, dal Fontana e dal Biancano, poiche in nessuna di queste l'anello al quale l'Huygens lego meritamente il suo nome, è così fedelmente rappresentato come lo è nella figura che Galileo ne traccio nell'agosto dell'anno 1616.'

[40]N. R. Hanson, *Patterns of Discovery*, (Cambridge, 1961), p. 11.

CHAPTER 4

[1]The history of science in the twentieth century has not done justice to Hevelius. Except for brief comments in a number of standard sources and some discussions of specific aspects of his work, no up to date biography exists. Therefore, the best and only complete biography of Hevelius is still L.C. Beziat's 'La Vie et les Travaux de Jean Hevelius', *Bullettino di Bibliographia e di Storia delle Scienze matematiche e fisiche*, VIII (1875), pp. 497-558, 589-669. See also E.P. Blech, *Rede bey der Gedächtenissfeyer Hevelii den 28. January 1787 gehalten*, (Danzig, 1787); F.A. Brandstäter, *Johannes Hevelius der berühmte Danziger Astronom. Sein Leben und seine Bedeutsamkeit*, (Danzig, 1861). For a complete description and analysis of Hevelius' work see M. Delambre, *Histoire de l'Astronomie Moderne*, (Paris, 1821), II: pp. 433-495.

[2]On his passage by ship to Holland, in 1630, he observed an occultation of Saturn by the Moon; the observation was made near Hveen, the location of Tycho's famous observatory. On 10 November 1630 he observed an eclipse of the Moon at Leyden.

Notes to pp. 71-75.

[3]It is said that Hevelius never missed a clear night, and this seems to be borne out by the prodigious number of observations made by him. But more importantly, his observations were published with great regularity, which was very important then, as it is now.

[4]See E.F. MacPike, Hevelius, Flamsteed and Halley, (London, 1937); this also contains a brief biography of Hevelius (pp. 1-16); A. Armitage, Edmond Halley, (London, 1966), pp. 37-43. Delambre (op. cit., p. 476) compares the accuracies of the angluar separations of eight stars on the ecliptic, whose total should add up to 360° longitude, of Tycho, Hevelius, and Flamsteed, using Piazzi's values as standards:

	Hevelius			Tycho			Flamsteed			Piazzi			ΔH		ΔT		ΔF	
	°	'	"	°	'	"	°	'	"	°	'	"	'	"	'	"	'	"
α♈ – α♉	35	32	15	35	32	10	35	32	5	35	32	2		13		8		3
α♉ – β♊	45	3	40	45	5	5	45	3	35	45	2	48		52	2	17		47
β♊ – α♌	37	0	0	36	59	30	37	0	55	37	1	34	1	34	2	4		39
α♌ – α♍	54	1	55	54	2	0	54	2	10	54	3	59	1	4		59		49
α♍ – δ Oph.	42	33	0	42	33	20	42	32	50	42	32	58		2		22		8
δ Oph. – α Aquil.	55	19	0	55	17	20	55	19	0	55	20	43	1	43	3	23	1	43
α Aquil. – α Peg.	47	48	10	47	49	40	47	48	0	47	47	10	1	0	2	30		50
α Peg. – α♈	43	37	25	43	37	15	43	37	30	43	37	40		15		25		10
Total	360	55	25	360	56	25	360	56	6	360	57	54	2	29	1	29	1	48

[5]R. Grant, History of Physical Astronomy, (London, 1852; reprint London, 1966), p. 57.

[6]Beziat, op. cit., pp. 539-541.

[7]Philips Huygens to Christiaan Huygens, 6 May 1656, O.C., I: pp. 411-412.

[8]The separate note containing this anagram was by mistake entered as an appendix to Hevelius' letter to Huygens of 22 June 1656, accompanying a copy of De Nativa Saturni Facie (O.C., I: pp. 434-435), but this mistake was later corrected by the editors (O.C., XV: p. 181 n. 3).

[9]Hevelius to Huygens, 22 June 1656, loc. cit. On the same date Hevelius sent copies of the tract, accompanied by letters, the Bouillau, Nucerio, and Goldmann (and probably to many others). Observatoire de Paris MSS. 'Epistolae Hevelium', IV: nos. 458, 467, 478, resp.

[10]Hevelius to Bouillau, 22 June 1656, loc. cit.: 'Ab aliquot iam annis, foetum istum de motu phasium Saturni in mente fovi; sed nolui, antequam Saturnum denuo Monosphericum conspexissem, ea quae conceperam animo typis de [?] vulgare.'

[11]This 'data sheet' would not be out of place in a modern scientific publication. Hevelius had a great penchant for ordering and classifying. At times this could lead to some very cumbersome results, as e.g. his complicated nomenclature for all the phases of the Moon, in his Selenographia.

Notes to pp. 76-81.

[12]N. Copernicus, De Revolutionibus Orbium Caelestium Libri Sex, Book V, Ch. VI.

[13]Vatier to Hevelius, 3 April 1657, British Museum MSS. Sloane 652, pp. 189-190. 'Mihi tamen unus aut alter haesit scrupulus, quem si Tua permittet humanitas evertendum, proponam. Et ille quidem gravior mihi videtur, quod non satis mihi appareat quomodo Saturni ansae, quae totam ejus disci latitudinem seu diametrum, adaequant apertione sua, possint aut contrahi in globulos duos, qui Saturniae diametri, non nisi tertiam obtineant partem, quales nunc videntur, aut omnino secundum extremas partes evanescere. Nam Tubus, qui ansarum seu quasi cornuum acumina spectanda dedit in Apogeo seu Aphelio, idem ea deberet exhibere in media distantia.

Alterum mihi dubium est, quomodo cavitas ansarum verti possit in curvitatem Circuli, seu, quomodo videamus convexum, id quod concavum est. Interior enim ansarum pars cava est: at globulorum facies vicinae, convexae sunt, ut quidem meis Tubis non optimis, & Tubo D. Gassendi exhibentur. In tuis eos figuris video pingi non circulares: sed contra oppono sensum plurimorum, qui tunc trisphaericam, & cum globulis Saturnum apparere contendunt, quare circulares illos globulos esse supponunt.'

Vatier also refutes the application of the Keplerian hypothesis: 'Mitto ego quod de Fibrarum diversione Keppleriana sumpsit, ad explicandum istam hypothesin. Puto enim ei qui Magneticorum diversiones probe examinavit, & cum experientia contulerit, non posse illam Keppleri doctrinam probari, cum evidentissimum sit, quod si Sol uno sui termino, unum Planetae terminum trahit & amat: ipse Sol altero sui termino illum eundem planetae terminum oderit & fugiet. Quare Sol intra dies 28. aut 30. conversus, ut vult Kepplerus, non poterit Planetam ullum trahere per menses duos continuos, sed post 14. dies repellet, quem illis 15. diebus traxerat. Evidens autem est, quod Corpus magneticum neque trahit neque pellit, ac proinde neque convertit trahendo aut pellendo Corpora alia, nisi conversos habeat ad ipsa suos seu sui axis Magnetici terminos. Sed ista non sunt ad hypothesis Tua veritatem necessaria: quare ipsa nihilo facio.'

[14]Huygens to Hevelius, 25 July 1656, O.C., I: p. 463.

[15]C. Wren, De Corpore Saturni (1658), see ch. 6, below.

[16]O.C., I: p. 488.

[17]Ibid., pp. 488-489.

[18]Ibid., p. 489.

[19]Hevelius to Huygens, 22 June 1656, loc. cit.

[20]Hevelius to Bouillau, 9 December 1659, see Appendix B, p. 252.

[21]Bouillau to Hevelius, 4 May 1657, Bibliothèque Nationale MSS. 'Collection Boulliau' XXV (FF 13043), f. 80r.: 'Totum librum tuum legi, & inter legendum mentem saepissime dubiit ingenii tui praestantia, indefessamque diligentiam tuam miratus sum. Summa cum animi voluptate Saturni varias phases verissime, utque mihi & alias semper exhibit[as] sunt, descriptas, periodumque talium phaenomenon detectam vidi.'

Notes to pp. 87-91.

CHAPTER 5

[1] L. Auger, Un Savant Méconnu: Gilles Personne de Roberval, (Paris, 1962), p. 10. See also E. Walker, A Study of the Traité de Indivisibles of Gilles Personne de Roberval, (New York, 1932).

[2] Huygens to Roberval, March 1656, O.C., I: p. 396.

[3] O.C., XV: pp. 174-176.

[4] Roberval to Huygens, 4 July 1656, O.C., I: pp. 451-452.

[5] Huygens to Roberval, 20 July 1656, O.C., I: p. 457.

[6] Huygens to Roberval, 27 July 1656, O.C., I: p. 466.

[7] Roberval to Huygens, 4 August 1656, O.C., I: pp. 474-476.

[8] Chapelain to Huygens, 12 April 1658, O.C., II: pp. 165-166.

[9] O.C., XV: pp. 288-290.

[10] Roberval to Huygens, 4 August 1656, loc. cit.

[11] Ibid.

[12] Huygens to Roberval, August 1656, O.C., I: p. 486.

[13] Chapelain to Huygens, 12 April 1658, O.C., II: pp. 165-166.

[14] Chapelain to Huygens, 10 May 1658, O.C., II: p. 175.

[15] Huygens to Chapelain, 6 June 1658, O.C., II: pp. 180-181.

[16] O.C., XV: pp 288-290.

[17] Bouillau to Huygens, 10 October 1659, O.C., II: p. 492.

[18] For biographical information on Hodierna see: A. Mongitore, Biblioteca Sicula sive de Scriptoribus Siculis, (2. vols.), (Palermo, 1708-1714), II: pp. 330, 42A; G.E. Ortolani, Biographia degli Uomini Illustri della Sicilia, (4 vols.), (Naples, 1817-1821), I; P. A. Saccardo, 'La Botanica in Italia', Memorie del R. Istituto Veneto, XXV (1895), no. 4, XXVI (1901), no. 6; A. Licitra, Studio su la Vita e su le Opere di Giovanni Battista Odierna, (Ragusa, 1899).

[19] Hodierna, L'Occhio della Mosca, Discorso Philosofico, (Palermo, 1629).

[20] Idem, Thaumantiae Miraculum seu de Causis quibus Objecta singula per Trigoni Vetrei transpicuam substantiam visa, legantissima Colorum Varietate Ornata cernuntur, (Palermo, 1652).

[21] Idem, De Systemate Orbis Cometici, deque admirandis Coeli Characteribus, (Palermo, 1654).

Notes to pp. 91-98.

[22]Idem, Menelogiae Jovis Compendium seu Ephemerides Medicaeorum nunquam hactenus apud Mortales Editae ..., (Palermo, 1656).

[23]Idem, Protei Caelestis Vertigines seu Saturni Systema, (Palermo, 1657), p. 22.

[24]Bartholin to Viviani, 26 July 1656, Biblioteca Nazionale (Florence), MSS. Gal. 254, f. 45.

[25]pp. 3-4.

[26]Hodierna refers here to Fontana's Novae Coelestium Terrestriumque Rerum Observationes, (Naples, 1646), p. 126.

[27]Huygens to Hodierna, 14 September 1658, O.C., II: pp. 223-224.

[28]O.C., XV: pp. 290-293.

[29]Guisony to Huygens, 20 October 1660, O.C., III: p. 140.

CHAPTER 6

[1]J. Wallis, A Defence of the Royal Society of London and the Philosophical Transactions, (London, 1678), see D. McKie, 'The Origins and Foundations of the Royal Society of London', Notes and Records, XV (1960), p. 12.

[2]His publications on this subject were: Discovery of a New World, or a Discourse tending to prove that it is probable that there may be annother habitable World in the Moon, (London, 1638), and Discourse of a New Planet, tending to prove that it is probable our Earth is one of the Planets, (London, 1640).

[3]Ward, In Ismaelis Bullialdi Astronomiae Philolaicae Inquisitio Brevis, (Oxford, 1653).

[4]In 1654, Wallis observed a solar eclipse with the help of Wren and a certain Richard Rawlinson. He published this observation the following year: Eclipsis Solaris Oxonii anno 1654 visae Observatio, (Oxford, 1655).

[5]C.A. Ronan, 'Laurence Rooke (1622-1662)', Notes and Records , XV (1960), pp. 113-118.

[6]W.S.C. Copeman, 'Dr Jonathan Goddard, F.R.S. (1617-1675)', Ibid., pp. 69-77; C.A. Ronan and Sir Harold Hartley, 'Sir Paule Neile, F.R.S. (1613-1686)', Ibid., pp. 159-165.

[7]The best biography of Wren is J. Summerson, Christopher Wren, (London, 1953). But even this book does not treat Wren's scientific career adequately. See also J. Summerson, 'Sir Christopher Wren, P.R.S. (1632-1723)', Notes and Records, XV (1960), pp. 99-105; D.T. Whiteside, 'Wren the Mathematician', Ibid., XV (1960), pp. [illegible]; 107-111

Notes to pp. 98-99.

A.R. Hall, 'Wren's Problem', Ibid., XX (1965), pp. 140-144; A.K. Biswas, 'The Automatic Rain-Gauge of Sir Christopher Wren, F.R.S.', Ibid, XXII (1967), pp. 94-104; A. Van Helden, 'Christopher Wren's De Corpore Saturni', Ibid., XXIII (1968), pp. 213-229 (see appendix D); Parentalia, (London, 1750); J. Ward, The Lives of the Professors of Gresham College, (London, 1740). For a complete list of Wren's scientific achievements see: H.W. Jones, 'Sir Christopher Wren and Natural Philosophy: with a checklist of his scientific Activities', Notes and Records, XIII (1958), pp. 19-37.

[8]His method for observing solar eclipses was published in 1680 by Flamsteed in his Doctrine of the Sphere grounded on the Motion of the Earth.

[9]'Lex Naturae de Collisione Corporum', Phil. Trans., no. 43 (11 January 1668/9), pp. 867-868, translated in H.O. V: pp. 320-322.

[10]See e.g. F. Cajori, ed., Sir Isaac Newton's Mathematical Principles of Natural Philosophy, (Berkeley, 1960), pp. 46, 106, 158.

[11]Wren, De Corpore Saturni, see Appendix D, p.283; Wallis to Huygens, 1 April 1656, O.C., I: p. 396.

[12]Ward to Isham, 27 February 1652, Notes and Records, VII (1950), p. 70.

[13]Wren, op. cit., p. 283.

[14]Wallis to Huygens, 17 April 1656, O.C., I: p. 401; 1 January 1659, O.C., II: p. 306.

[15]For a discussion of Balle's astronomical activities see: A. Armitage, 'William Ball, F.R.S. (1627-1690)', Notes and Records, XV (1960), pp. 167-172.

[16]Wren, op. cit., pp.285 -286; Wallis to Huygens, 22 August 1656, O.C., I: p. 481.

[17]Huygens to Wallis, 13 June 1655, O.C., I: pp. 331-332.

[18]Wallis to Huygens, 1 July 1655, O.C., I: pp. 335-338.

[19]Wallis to Huygens, 1 January 1659, O.C., II: pp. 305-306.

[20]Ibid.

[21]Huygens' anagram containing the discovery of the satellite was not merely a random collection of letter, but was partially in the form of a line of poetry from Ovid: Admovere oculis distantia sidera nostris, VVVVVVVCCCRRHNBQX, e.g. O.C., I: p. 332.

[22]Wallis to Huygens, 1 January 1659, O.C., II: p. 306.

[23]Wren to Neile, 1 October 1661 (o.s.), Ronan and Hartley, op. cit., p. 163.

[24]Wallis to Huygens, 17 April 1656, O.C., I: p. 403

[25]Wallis to Huygens, 22 August 1656, O.C., I: p. 480.

[26]Ibid., pp. 481-482.

[27]Wren to Neile, loc. cit.

[28]Ibid.

[29]Ibid.

[30]Ibid.

[31]Ibid.

[32]O.C., III: pp. 419-424.

[33]Wren., op. cit., p. 282.

[34]Ibid., pp. 282-283.

[35]Ibid., p. 283.

[36]The oblateness of Saturn was not discovered until 1782, by William Herschel. See R. Grant, History of Physical Astronomy, (London, 1852, reprinted London, 1966), p. 252.

[37]Wren, op. cit., p. 283.

[38]Ibid., p. 285.

[39]Ibid., p. 287.

[40]Wren to Neile, loc. cit.

[41]Oldenburg to Huygens, 17 September 1661, O.C., III: p. 332; Moray to Huygens, 19 October 1661, O.C., III: p. 368; T. Birch, History of the Royal Society, (London, 1756), I: p. 43.

[42]Wren to Neile, loc. cit.

[43]Ibid., p. 164.

[44]Besides the copy published in O.C., I have found the following MS. copies: Royal Society MSS. 'Boyle Papers', XX: ff. 19-31; 'A Hill Philosophical Papers', British Museum MSS. Sloane 2903, ff. 102-103 (incomplete); 'Miscellaneous Collections relating to Gresham College, Volume the first by I. Ward', British Museum MSS. Add. 6193, ff. 83-95; British Museum MSS. Sloane 243, ff. 144-150.

[45]Huygens to Moray, 24 June 1661, O.C., III: p. 283.

[46]Huygens to Moray, 9 June 1662, O.C., IV: p. 151.

[47]Frenicle to Wren, December 1661, O.C., IV: pp. 40-44.

Notes to pp. 110-113.

CHAPTER 7

[1]A. E. Bell, Christian Huygens and the Development of Science in the Seventeenth Century, (London, 1947).

[2]U.I. Frankfourt and A.M. Fraink, Christian Huygens, (Moscow, 1962).

[3]He made some contributions to van Schooten's 1649 edition of Descartes' Geometrie, O.C., XIV: pp. 409-417. See also: Theoremata de Quadratura Hyperboles, Ellipsis et Circuli, ex dato Portionum Gravitatis Centro, (Leyden, 1651), O.C., XI: pp. 271-337; De Circuli Magnitudine Inventa, (Leyden, 1654), O.C., XII: pp. 91-215.

[4]F. Fontana, Novae Coelestium Terrestriumque Rerum Observationes, (Naples, 1646), p. 126; Rheita, Novem Stellae circa Jovem circa Saturnum sex, circa Martem non-nullae, (Louvain, 1643).

[5]O.C., I: pp. 332, 335. This anagram was also inscribed on the objective lens of Huygens' 12 foot telescope, with which he discovered the satellite, see O.C., XV: p. 11.

[6]Ovid, Fasti, v. 297 ff.

[7]W.H. Lewis, The Splendid Century, (New York, 1957), p. 24.

[8]Systema Saturnium, O.C., XV: p. 228

[9]Colvius to Huygens, 20 January 1656, O.C., I: p. 375.

[10]Huygens to Colvius, 8 February 1656, O.C., I: p. 380.

[11]Ibid.

[12]van Schooten to Huygens, 11 February 1656, O.C., I: p. 381; Huygens to Hevelius, 8 March 1656, O.C., I: p. 388.

[13]Huygens to van Schooten, 15 February 1656, O.C., I: p. 383: 'Effecimus 24 pedum longitudine praestantissimum, sed adhuc loci commoditas deest ubi observationes instituere possimus.' See also Huygens' reference to this in Systema Saturnium, O.C., XV: pp. 246-247.

[14]Bell, op. cit., p. 31: 'A new twenty-three foot telescope with the best lenses he could make was assembled with all speed and this was in use after February 19th, 1656. With this instrument the planet appeared much more distinctly and he at last made a drawing of it showing it surrounded by a ring.' This error, although not earth-shaking, is indicative of the level of scholarship of Bell's book.

[15]Systema Saturnium, O.C., XV: pp. 238-239. See also O.C., I: p. 322.

[16]There has been some confusion on this subject in the standard sources; e.g. H.C. King writes in his History of the Telescope, (London, 1955): 'Towards the end of 1655, the rings were still almost edge-on, but a new 23-foot telescope made at this time revealed the tapering of the ansae at their extremities. Another feature, seen

with both instruments, was a darkish line passing over the disk - the shadow of the rings upon the body of the planet. By January, 1656, Saturn appeared quite round and without appendages, even when examined with a 123-foot telescope.' (my italics). This '123-foot telescope' found its way into S.A. Bedini's 'The Aerial Telescope', Technology and Culture, VIII (July 1967), no. 3, pp. 398-399. This atricle contains numerous other errors, and is completely unreliable.

[17] Systema Saturnium, O.C., XV: pp. 294-297.

[18] O.C., I: p. 381.

[19] O.C., I: p. 388.

[20] De Saturni Luna Observatio Nova, (The Hague, 1656), O.C., XV: pp. 176-177.

[21] Ibid., pp. 174-175.

[22] O.C., I: p. 525

[23] Huygens to Bouillau, 19 September 1658, O.C., II: pp. 220-221.

[24] Huygens to Bouillau, 7 August 1659, O.C., II: p. 453.

[25] Huygens to Bouillau, 26 December 1657, O.C., II: pp. 109-110.

[26] Huygens to Chaplelain, 28 March 1658, O.C., II: pp. 157-161.

[27] Chapelain to Huygens, 12 April 1658, O.C., II: p. 166; Huygens to Chapelain, 18 April 1658, O.C., II: p. 169; Chapelain to Huygens, 10 May 1658, O.C., II: pp. 173-176. See also H. Brown, Scientific Organizations in Seventeenth Century France, (Baltimore, 1934), p. 84.

[28] Huygens to Chapelain, 18 April 1658, O.C., II: p. 169.

[29] The meeting at which Chapelain revealed the ring hypothesis was attended by more than forty people: Chapelain to Huygens, 10 May 1658, O.C., II: p. 174.

[30] Wallis to Huygens, 1 January 1659, O.C., II: p. 306; Huygens to Wallis, 31 January 1659, O.C., II: pp. 330-331.

[31] Bouillau to Huygens, 27 September 1658, O.C., II: p. 226; Chapelain to Huygens, 30 October 1658, O.C., II: p. 268; Bouillau to Huygens, 21 March 1659, O.C., II: p. 379.

[32] O.C., II: p. 430.

[33] O.C., XV: pp. 214-215.

[34] Borelli had to disguise his thoughts about the solar system as being about the Medician planets.

Notes to pp. 117-128.

[35] Condorcet wrote in his 'Eloge de Huygens' : L'honneur de decouvrir les autres satellites de Saturne etait reserve a l'illustre Cassini: Huyghens ne chercha pas meme a les voir; on aura peine a croire par quelle raison. Cet homme celebre tenait a des prejuges antiques, que Descartes n'avait pu deraciner absolument. Il croyait que le nombre des satellites ne devait pas surpasser celui des planetes principales. On ne sait pas combien des idees superstitieuses de toute espece ont fait manquer de decouvertes: le genie peut bien se trainer malgre ses fers; mais il vole quand il a su les briser.' Condorcet, Eloges des Academiciens de l'Academie Royale des Sciences ... , (Paris, 1773), pp. 117-118.

[36] Perhaps this discussion is in answer to a query by Petit about Huygens' method for determining the magnification of his telescopes. (29 November 1658, O.C., II: p. 279.)

[37] Hevelius, Selenographia, p. 37.

[38] Unknown to Huygens, this nebula had already been observed by Cysat in 1619.

[39] E. Halley, 'A Correction of the Theory of the Motion of the Satellite of Saturn', Phil. Trans., XIII (10 March 1683), pp. 82-88.

[40] The name Titan was given to this satellite in the nineteenth century.

[41] O.C., XV: p. 45.

[42] O.C., XV: p. 270. I quote here from the French translation made by the editors of O.C. In each case the Latin text is found on the adjoining page.

[43] Ibid.

[44] Hevelius' reaction to Systema Saturnium will be discussed in Chapter 8.

[45] He probably learned of Wren's theory during his first visit to England in 1660-1661.

[46] O.C., XV: p. 294.

[47] O.C., XV: p. 299.

[48] O.C., XVI: p. 254 n. 1.

[49] Hevelius to Bouillau, 9 December 1659, Appendix B, p.252.

[50] E.J. Dijksterhuis, 'Christiaan Huygens', Centaurus, II (1953), p. 271.

[51] O.C., XV: p. 153.

CHAPTER 8

[1]Bouillau to Huygens, 4 January 1658, O.C., II: p. 117.

[2]Bouillau to Huygens, 1 August 1659, O.C., II: p. 448.

[3]Chapelain to Huygens, 10 May 1658, O.C., II: p. 174.

[4]Ibid., p. 175.

[5]Wallis to Huygens, 28 February 1659, O.C., II: p. 358.

[6]Ibid.

[7]Sluse to Huygens, 22 August 1659, O.C., II: p. 471.

[8]St Vincent to Huygens, 5 November 1659, O.C., II: p. 505.

[9]Huygens to St Vincent, December 1659, O.C., II: p. 542.

[10]Kinner von Löwenthurn to Huygens, 1 January 1660, O.C., III: p. 5.

[11]Petit to Huygens, 30 January 1660, O.C., III: p. 18.

[12]Astronomia Physica, (Paris, 1660), p. 151.

[13]Ibid., p. 5A (Letter to Petit).

[14]Ibid., pp. 5-6A.

[15]Ibid., p. 11A.

[16]Wallis to Huygens, 4 December 1659, O.C., II: p. 520.

[17]Wren to Neile, 1 October 1661 (o.s.), in C.A. Ronan and Sir Harold Hartley, 'Sir Paul Neile, F.R.S. (1613-1686)', Notes and Records, XV (1960), p. 163.

[18]Ibid.

[19]Moray to Huygens, 23 December 1661, O.C., III: p. 415.

[20]Guisony to Huygens, 25 March 1660, O.C., III: p. 45.

[21]St Vincent to Huygens, 26 April 1660, O.C., III: p. 72; Gottigniez to St Vincent, April 1660, O.C., III: p. 73.

[22]Bouillau to Huygens, 10 October 1659, O.C., II: p. 492.

[23]Bouillau to Huygens, 21 November 1659, O.C., II: pp. 510-511.

[24]Ibid., p. 511.

[25]From Systema Saturnium, O.C., XV: p. 311.

Notes to pp. 139-147.

[26]Bouillau to Prince Leopold, 21 May 1649, Biblioteca Nazionale (Florence) MSS. Gal. 275, f. 21r.

[27]Bouillau to Des Noyers, 9 January 1660, Observatoire de Paris MSS. 'Epistolae Hevelium', IV, no. 589. Although this letter is not addressed, it is clear from a letter written by Bouillau to Huygens on the same day (O.C., III: p. 8) that this letter was addressed to Des Noyers.

[28]Prince Leopold to Bouillau, 9 October 1659, O.C., III: pp. 469-470; Bouillau to Prince Leopold, 19 December 1659, O.C., II: p. 533.

[29]Ibid.

[30]Huygens to Bouillau, 11 December 1659, O.C., II: p. 524.

[31]Chapelain to Huygens, 4 March 1660, O.C., III: p. 35.

[32]Ibid.

[33]Huygens to Chapelain, 28 April 1660, O.C., III: p. 76.

[34]Huygens to Boddens, 29 January 1660, O.C., III: p. 16.

[35]Hevelius to Bouillau, 9 December 1659, Appendix B, pp. 244-256.

[36]Bouillau to Huygens, 16 January 1660, O.C., III: p. 9.

[37]Hevelius to Bouillau, 9 December 1660, Appendix B, pp. 244-256.

[38]Ibid., p. 250.

[39]Philips Huygens to Christiaan Huygens, 30 May 1656, O.C., I: p. 420.

[40]Hevelius to Bouillau, 9 December 1659, Appendix B, pp. 250-251.

[41]Ibid., pp. 251-252.

[42]Ibid., p. 252.

[43]Ibid., pp. 252-253.

[44]O.C., XV: p. 175.

[45]Hevelius to Bouillau, 9 December 1659, Appendix B, p. 254.

[46]Ibid., p. 255.

[47]Hevelius to Huygens, 13 July 1660, O.C., III: p. 91.

[48]Ibid., p. 96.

[49]Huygens to Hevelius, 4 October 1660, O.C., III: p. 134.

CHAPTER 9

[1]Heinsius to Dati, 14 August 1659, O.C., II: p. 462

[2]Prince Leopold to Bouillau, 9 October 1659, O.C., III: p. 469.

[3]Huygens to Kinner von Löwenthurn, 28 November 1659, O.C., II: p. 514.

[4]Guisony to Huygens, 25 March 1660, O.C., III: p. 45; 20 October 1660, O.C., III: p. 144.

[5]Guisony to Huygens, 25 March 1660, loc. cit.

[6]Guisony to Huygens, 20 October 1660, loc. cit.

[7]Guisony to Huygens, 27 August 1660, O.C., III: p. 116.

[8]Huygens to Bouillau, 20 November 1659, O.C., II: p. 510.

[9]Bouillau to Prince Leopold, 19 December 1659, O.C., II: pp. 532-533.

[10]Huygens to Bouillau, 20 November 1659, loc. cit.

[11]Bouillau to Huygens, 4 July 1659, O.C., II: p. 430.

[12]Huygens to Bouillau, 1 January 1660, O.C., III: p. 5.

[13]Huygens to Bouillau, 22 January 1660, O.C., III: p. 13.

[14]Dati to Heinsius, 16 March 1660, O.C., III: pp. 42-43

[15]Huygens to Héinsius, 7 April 1660, O.C., III: p. 60.

[16]Dati to Heinsius, 13 July 1660, O.C., III: pp. 502-503.

[17]O.C., III: p. 31 n. a.

[18]Huygens to Prince Leopold, 13 August 1660, O.C., III: pp. 109-110; Prince Leopold to Huygens, 1673, O.C., VII: pp. 281-286.

[19]Systema Saturnium, (The Hague, 1659), p. 37, O.C., XV: p. 279.

[20]Gottigniez to St Vincent, March 1660, O.C., III: pp. 59-60.

[21]Guisony to Huygens, 25 March 1660, O.C., III: pp. 45-46.

[22]Dati to Heinsius, 25 May 1660, O.C., III: p. 83.

[23]Divini to Prince Leopold, 10 July 1660, Biblioteca Nazionale (Florence) MSS. Gal. 276: f. 33r.

Notes to pp. 152-157.

[24]Brevis Annotatio in Systema Saturnium Christiani Hugenii, (Rome, 1660), reprinted in the same year with Huygens' response: Brevis Annotatio in Systema Saturnium ... una cum Christiani Hugenii Responso, (The Hague, 1660). The text of this latter edition is identical with that of the Rome edition, and is reproduced in O.C., XV. I quote here from the French translation by the editors of O.C., pp. 404-406.

[28]Ibid., p. 406

[29]Bullettino di Bibliografia etdi Storia delle Scienze matematiche e fisiche, XX (1887), opposite p. 614.

[30]Divini, Brevis Annotatio, O.C., XV: p. 406.

[31]Ibid., pp. 406-408.

[32]O.C., XV: p. 279.

[33]O.C., XV: pp. 408-410.

[34]Ibid., p. 410.

[35]Ibid., pp. 412-414.

[36]In his first letter on sunspots, G.G., V: pp. 110-111. See Chapter 1, p. 14.

[37]O.C., XV: p. 414.

[38]Ibid., p. 416.

[39]Ibid.

[40]Ibid., p. 418.

[41]Ibid., p. 422.

[42]Ibid.

[43]Ibid., pp. 426-430.

[44]Huygens to Prince Leopold, 13 August 1660, O.C., III: p. 110.

[45]Ricci to Prince Leopold, 26 July 1660, Biblioteca Nazionale (Florence) MSS. Gal. 276: ff. 42r-42v.

[46]Dati to Heinsius, 27 July 1660, O.C., III: p. 504.

[47]Prince Leopold to Fabri, 13 August 1660, Biblioteca Nazionale (Florence) MSS. Gal. 289: f. 9r.

[48]Magalotti to Ricci, 17 August 1660, ibid., f. 11r.

[49]Ibid., ff. 14r-14v.

Notes to pp. 157-165.

[50]Borelli to Prince Leopold, August 1660, *ibid.*, f. 15r.

[51]*Ibid.*, f. 15v.

[52]*Ibid.*, f. 17r

[53]*Ibid.*, f. 16r.

[54]*Ibid.*, ff. 18v-19r.

[55]*Ibid.*, ff. 19r-19v.

[56]This observation was first made known through a single printed sheet published by Campani, see O.C., V: opposite p. 118.

[57]Dati to Prince Leopold, August 1660, Biblioteca Nazionale (Florence) MSS. Gal. 289: f. 20v.

[58]*Ibid.*, ff. 20v-21r.

[59]Dati to Heinsius, 5 October 1660, O.C., III: pp. 149-150.

[60]See 'Eustachio Divini versus Christiaan Huygens: a Reappraisal', Appendix C, p. 266.

[61]Prince Leopold to Huygens, 4 October 1660, O.C., III: p. 151.

[62]Borelli to Prince Leopold, O.C., III: p. 152.

[63]*Ibid.*, pp. 152-153.

[64]*Ibid.*, p. 153.

[65]*Ibid.*, p. 154.

[66]*Ibid.*

[67]*Ibid.*, p. 155.

[68]*Ibid.*

[69]*Ibid.*, p. 154.

[70]*Ibid.*, p. 156.

[71]*Ibid.*, p. 157.

[72]*Ibid.*, p. 158.

[73]*Ibid.*, p. 159.

[74]*Ibid.*

[75]*Ibid.*

[76]*Ibid.*, pp. 159-160.

Notes to pp. 165-174.

[77]*Ibid.*, p. 161.

[78]*Ibid.*

[79]*Ibid.*, pp. 161-162.

[80]Magalotti to Prince Leopold, *O.C.*, III: pp. 163-164.

[81]*Ibid.*, p. 165.

[82]*Ibid.*, pp. 165-166.

CHAPTER 10

[1]Chapelain to Huygens, 26 August 1660, *O.C.*, III: p. 114.

[2]Huygens to Chapelain, 2 September 1660, *O.C.*, III: p. 119.

[3]Heinsius to Dati, 3 September 1660, *O.C.*, III: p. 121.

[4]Huygens to Prince Leopold, 30 September 1660, *O.C.*, III: pp. 132-133.

[5]*Brevis Assertio Systematis Saturnii*, (The Hague, 1660). I quote here from the French translation, *O.C.*, XV: p. 440.

[6]*Ibid.*, p. 442.

[7]*Ibid.*, pp. 442-446.

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

[9]*Ibid.*, p. 452.

[10]*Ibid.*

[11]*Ibid.*, p. 454.

[12]*Ibid.*, pp. 454-456.

[13]*Ibid.*, pp. 458-460.

[14]*Ibid.*, p. 462.

[15]*Ibid.*, pp. 462-464.

[16]Huygens to Chapelain, 2 September 1660, *O.C.*, III: p. 119.

[17]Prince Leopold to Huygens, 5 November 1660, *O.C.*, III: p. 171.

[18]Huygens to Dati, 28 November 1660, *O.C.*, III: p. 194.

[19]*Ibid.*

Notes to pp. 174-180.

[20]Huygens to Prince Leopold, 28 November 1660, O.C., III: pp. 195-196.

[21]Hevelius to Huygens, 1 August 1661, O.C., III: pp. 308-309.

[22]Prince Leopold to Huygens, 1 June 1661, O.C., III: pp. 274-275.

[23]*Eustachius de Divinis Septempedanus Pro Sua Annotatione in Systema Saturnium Christiani Hugenii adversus ejusdem Assertionem*, (Rome, 1661), pp. 5-8.

[24]*Ibid.*, p. 10. Divini refers here to Fontana's statement in *Novae Coelestium Terrestriumque Rerum Observationes*, (Naples, 1646), p. 126: 'Insuper praeter ordinarios Saturni spectatores aliquando duas stellas, quandoque tres inspexi, quas judico non esse firmamenti sydera ...'

[25]Divini, *Pro Sua Annotatione*, pp. 11-15.

[26]*Ibid.*, pp. 56-58.

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

[28]*Ibid.*, p. 72.

[29]*Ibid.*, p. 78.

[30]*Ibid.*, p. 101.

[31]*Ibid.*, p. 106

[32]*Ibid.*, p. 115.

[33]Huygens to Lodewijk Huygens, 3 May 1662, O.C., IV: pp. 125-126.

[34]Huygens to Prince Leopold, 1662, O.C., IV: p. 286.

CHAPTER 11

[1]J. D. Cassini, 'Reflexions sur les Observations de Satellites de Saturne & de son Anneau', *Hist.* & *Mem.*, 1705, (Paris, 1730), Memoires, pp. 17-18.

[2]Moray to Huygens, 16 September 1661, O.C., III: p. 322; Frenicle to Digby, 31 August 1661, O.C., III: pp. 337-339.

[3]*Ibid.*

[4]Thevenot to Huygens, 25 September 1661, O.C., III: p. 346; Frenicle to Huygens, 26 August 1661, O.C., III: pp. 349-354.

[5]Frenicle to Huygens, 26 August 1661, O.C., III: p. 351.

Notes to pp. 180-188.

[6]Huygens to Moray, 24 June 1661, O.C., III: p. 283.

[7]Frenicle to Huygens, 26 August 1661, O.C., III: pp. 352-353.

[8]Ibid., pp. 350-351.

[9]Frenicle to Digby, 31 August 1661, O.C., III: p. 338.

[10]Frenicle to Huygens, 26 August 1661, O.C., III: p. 351.

[11]Ibid.

[12]Wren to Neile, 1 October 1661 (o.s.), C.A. Ronan and Sir Harold Hartley, 'Sir Paul Neile, F.R.S. (1613-1686)', Notes and Records, XV (1960), pp. 163-164.

[13]Frenicle to Digby, 20 December 1661, O.C., IV: p. 38.

[14]Moray to Huygens, 9 February 1662, O.C., IV: p. 34.

[15]Huygens to Chapelain, June 1662, O.C., IV: pp. 145-146.

[16]Huygens to Moray, 24 June 1661, O.C., III: p. 283.

[17]Frenicle to Huygens, 26 August 1661, O.C., III: p. 352.

[18]Huygens to Chapelain, June 1662, O.C., IV: pp. 145-146.

[19]G. Campani, Ragguaglio di due Nuove Osservazioni, (Rome, 1664), p. 18.

[20]This single page is extremely rare. A reproduction can be found in O.C., V: opposite p. 118.

[21]Campani to Charles, quoted in O.C., V: pp. 117-118 n. 1.

[22]Huygens to Moray, 29 August 1664, O.C., V: pp. 107-108.

[23]A. Auzout, Lettre a Monsieur L'Abbé Charles, sur le Ragguaglio ... da Giuseppe Campani, (Paris, 1665), p. 7.

[24]Ibid., p. 9.

[25]Phil. Trans., I: pp. 108-109, plate.

[26]M. Campani to Huygens, 2 December 1664, O.C., V: p. 193.

[27]H. Fabri, Dialogi Physici, (Lyons, 1665), p. 212.

[28]Ibid., p. 91.

[29]H. Fabri, Synopsis Optica, (Lyons, 1667), pp. 49-59.

[30]G.B. Riccioli, Astronomiae Reformatae, (Bologna, 1665), pp. 367-368.

[31]Ibid., p. 365.

Notes to pp. 189-193

[32]J. Hevelius, Machina Coelestis II, (Danzig, 1679), p. 76: Observation of 19 November 1670: 'Saturnus hac die fere adhuc eadem facie apparebat, qua Mense August. & Sept. videbatur, nisi quod annulus ex parte compressior videretur.' (my italics).

[33]Auzout, op. cit., p. 19.

[34]Auzout to Huygens, 6 March 1665, O.C., V: p. 257.

[35]Auzout to Huygensm 5 June 1665, O.C., V: p. 368.

[36]For the inclination of the ring to the ecliptic Huygens found the following values in 1667 and 1668: 31° 22', 32° 0', 31° 38', 30° 42'. For the inclination of the ring to the plane of the equator, which in 1659 he had calculated at 3° 48', he found: 8° 58', 9° 32' 50", 9° 13', and 9° 20'. (See O.C., XV: p. 478).

[37]Systema Saturnium, O.C., XV: p. 338.

[38]Huygens to Lodewijk Huygens, 21 August 1671, O.C., VII: p. 101.

[39]Huygens to Oldenburg, 7 November 1671, O.C., VII: pp. 115-116.

[40]'Extraict d'une Lettre de M. HUGENS de l'Academie Royale des Sciences a l'Auteur du Journal des Sçavans, touchant la figure de la Planete de Saturne', Journal des Sçavans, VIII, 12 December 1672, O.C., VII: pp. 236-237.

[41]Ibid., p. 237.

[42]Ibid., p. 236.

[43]Ibid., p. 237.

[44]O.C., XV: pp. 109-110.

[45]O.C., XV: p. 153.

[46]Christiani Hugenii Cosmotheoros. sive de Terris Coelestibus, Earumque Ornatu, Conjecturae, (The Hague, 1698). I quote here from the French translation in O.C., XXI: p. 786.

[47]Hist. & Mem., 1715, (Paris, 1718), Memoires, p. 48.

[48]Memoires, X (Paris, 1730), pp. 584-586, 694-702.

[49]'Observations Nouvelles de M. Cassini, touchant le Globe & l'Anneau de Saturne', Journal des Sçavans, 1 March 1677 (XII), pp. 32-33. See also Phil. Trans., XI (1676), pp. 689-690 and fig. opposite p. 710.

[50]This claim is founded on an article in Phil. Trans., I (1665/6), No. 9, p. 153. Balle's figure was omitted in some copies of this issue. See A. Armitage, 'William Ball, F.R.S.(1627-1690)', Notes and Records, XV (1960), pp. 169-170. For an example of the claim for Balle's discovery of the division in the ring see R. Grant, History of Physical Astronomy, (London, 1852, reprinted London, 1966), p. 526.

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APPENDIX A

Major Developments in the Knowledge about Saturn's Rings, 1675-1895.

Although it is clearly beyond the scope of this thesis to deal with all subsequent developments - in theory as well as in observation - leading to our present knowledge of the rings of Saturn, it is nevertheless essential to treat briefly the major developments.

Speculation as to the constitution of the two rings known by 1675 seems to have been limited during the eighteenth century. Not until its latter part did the planet Saturn again capture the interest of astronomers. This was mainly due to the work of two men, William Hersohel and Pierre Simon de Laplace. Herschel made a great number of observations of Saturn, and this planet, along with its ring and its satellites, was the subject of no fewer than eight of his scientific communications. Besides discovering two new satellites of Saturn, Herschel observed surface features from which he deduced a period of rotation of the planet of 10h. 16min., made observations of the shape of the central globe, and estimated the thickness of the rings.

As to the constitution of the ring, Herschel thought initially that the ring was thick and solid, not even believing that the dark trace discovered by Cassini was a division, but rather that it was '... most probably owing to some permanent construction of the surface of the ring itself ...' (1),

> As to the surmise, which might occur to us, of a division of the ring, or rather of two rings, one about the other, with a distance of open space between them, it does not appear eligible to venture on so artificial a construction, by way of explaining a phaenomenon that does not absolutely demand it. If one ring, of a breadth so considerable as that of Saturn, is justly to be esteemed the most wonderful arch that, by the laws of gravity, can be held together, how improbable must it appear to suppose it subdivided into narrow slips of rings, which by this separation will be deprived of a sufficient depth, and thus lose the only dimension which can keep them from falling upon the planet?
>
> With regard to the nature of the ring, we may certainly affirm, that it is no less solid and substantial than the planet itself. (2)

In the same paper Herschel showed that the thickness of the ring was not greater than one third of a second of arc. (3) But he did not think that the ring disappeared because of its thinness. He believed that the outside edge was rounded or bevelled and also '... that there are other very strong reasons to induce us to think, that the edge of the ring is of such a nature as not to reflect much light.' (4) His subsequent observations of the other side of the ring, showing the same dark band as the first side, led him to admit that this dark band was indeed a division of the ring, but it does not appear that he ever changed his mind as to the solid nature of the ring.

The Marquis Pierre Simon de Laplace worked on the ring of Saturn at the same time, but he approached the problem from a mathematical point of view. Saturn's rings (he believed, on the basis of Short's observations, that there were numerous divisions) are treated in all Laplace's major works, Méoanique Céleste (book 3, ch. 6; book 5, ch. 3; book 14, ch. 3) and Exposition du Système du Monde (book 4, ch. 9), but all Laplace's work on Saturn's rings was done in 1787 and the subsequent discussions of the constitution of the rings in his works are merely paraphrases of the paper 'Memoire sur la Theorie de L'anneau de Saturné', printed in Mémoires de l'Académie Royale des Sciences de Paris of 1789. In this memoir Laplace accepts the supposition that the rings are solid and investigates the conditions under which a series of solid concentric rings can be in stable equilibrium. In order to perform this mathematical investigation he assumed

> ... qu'une couche infiniment mince de fluide, répandue sur la surface de ces anneaux, y serait en equilibre en vertu des forces dont elle serait animée. (5)

Thus, Laplace did not believe that these rings could be held together by their strength (l'adherence de leurs molecules). His results were that the various rings could only endure if they were

> ... des solides irréguliers d'une largeur inégale dans differents points de leurs circonférences, en sorte que leurs centres de gravité ne coïncident point avec leurs centres de figure. Ces centres de gravité peuvent être considerés comme autant de satellites qui se meuvent autour du centre de Saturne, à des distances dépendantes de l'inégalité des parties de chaque anneau, avec des vitesses de rotation égales à celles de leurs anneaux respectifs, (6)

This condition is, of course, over and above the simple condition that the attractive force of the planet just balances the 'centrifugal' tendency.

Laplace's mathematics were highly respected and all further mathematical work on the rings of Saturn had to begin with Laplace. But by the middle of the nineteenth century the solid nature of the rings had by and large been rejected. Thus, Benjamin Peirce of Harvard

College, an excellent mathematician who was thoroughly familiar with Laplace's Mécanique Céleste (which had been translated into English by his friend Nathaniel Bowditch), stated in 1851:

> I maintain, unconditionally, that there is no conceivable form of irregularity and no combination of irregularities, consistent with an actual ring, which would serve to retain it permanently about the primary, if it were solid. (7)

It was at Harvard that the third ring, the so-called 'crepe ring' was 'discovered' by G.P. Bond and C.W. Tuttle, in 1850.(8), and this crepe ring gave further indication that the rings could not be solid, since the planet could be seen through it. It is thus fair to say that by 1855 everyone took the non-solid nature of the rings for granted.

But it was still enough of a problem to merit being put forward as the subject of competition for the first Adams prize to be awarded by Cambridge University. James Clerk Maxwell's solution of this problem showed mathematically that the rings could hardly be solid:

> We found that the stability of the motion of a solid ring depended on so delicate an adjustment, and at the same time so unsymmetrical a distribution of mass, that even if the exact condition were fulfilled, it could scarcely exist long, and if it did, the immense preponderance of one side of the ring would be easily observed, contrary to experience. These considerations, with others derived from the mechanical structure of so vast a body, compell us to abandon any theory of solid rings.
>
> We next examined the motion of a ring of equal satellites, and found that if the mass of the planet is sufficient, any disturbances produced in the arrangement of the ring will be propagated round it in the form of waves, and will not introduce dangerous confusion. If the satellites are unequal, the propagation of the waves will no longer be regular, but disturbances of the ring will in this, as in the former case, produce only waves, and not growing confusion. Supposing the ring to consist, not of a single row of large satellites, but of a cloud of evenly distributed unconnected particles, we found that such a cloud must have a very small density in order to be permanent, and that this is inconsistent with its outer and inner parts moving with the same angular velocity. Supposing the ring to be fluid and continuous, we found that it will necessarily be broken up into small portions.
>
> We conclude therefore that the ring must consist of disconnected particles; these may be either solid or liquid, but they must be independent. The entire system of rings must therefore consist either of a series of many concentric rings, each moving with its own velocity, and having its own systems of waves, or else of a confused multitude of revolving particles, not arranged in rings, and continually coming into collision with each other. (9)

As the former is in the final analysis unstable,

> The final result, therefore, of the mechanical theory is that the only system of rings which can exist is one composed of an indefinite number of unconnected particles, revolving round the planet with different velocities according to their respective distances. (10)

Maxwell's mathematical treatment was very thorough indeed, and as his conclusions coincided with contemporary beliefs as to the composition of Saturn's rings, they were universally accepted. The only thing lacking now was direct experimental proof of the particle constitution of the rings. This proof only became possible when spectroscopic analysis of the light emitted by heavenly bodies became possible.

The first attempt at detecting a Doppler shift in the spectrum of Saturn's rings was made by James A. Keeler, the director of the Allegheny Observatory, in 1893. As his equipment did not lend itself well to such a study, Keeler's first attempt ended in failure. (11) But two years later, after suitable alterations in equipment had been made, he was more successful, and on 9 and 10 April 1895 Keeler obtained two spectra of Saturn and its ring (his spectra did not show the crepe ring and did not register the separation between the two bright rings) which proved conclusively that the outside of the ring moves more slowly than the inside of the ring, and that the relative velocities obey Kepler's third law. (12) His results were corroborated in the same year by William W. Campbell at the Lick Observatory (13) and by H. Deslandres at the Meudon Observatory. (14) All three agreed fairly closely on the velocieties of various parts of the ring system. Thus, only after 1895 can it be said that the problem of the constitution of Saturn's rings had been completely solved.

Notes to Appendix A

[1]W. Herschel, 'Account of the Discovery of the Sixth and Seventh Satellites of the Planet Saturn; with Remarks on the Constitution of its Ring, its Atmosphere, its Rotation on a Axis, and its Spheroidal Figure', The Scientific Papers of Sir William Herschel, I: p. 371.

[2]Ibid., pp. 371-372.

[3]Ibid., p. 373.

[4]Ibid., p. 374.

[5]I quote here from Mécanique Céleste, book 3, ch. 6, Oeuvres Complètes de Laplace, II: p. 166.

[6]Ibid., p. 176.

[7]B. Peirce, The Astronomical Journal, II (1851), No. 3, p. 17.

[8]W.C. Bond, 'On the New Ring of Saturn', The Astronomical Journal, II (1851), No. 1, p. 5. I put the word 'discovered' in inverted comma's because the crepe ring had been sighted unknowingly on numerous occasions before 1850. The real nature of the events of 15 November 1850 are best described in Annals of the Astronomical Observatory of Harvard College, II, part 1, p. 48 n: 'On the evening of the 15th the idea was first suggested by Mr. Tuttle of explaining the penumbral light bordering the interior edge of the bright ring outside of the ball, as well as the dusky line crossing the disc on the side of the ring opposite to that where its shadow was projected on the ball, by referring both phenomena to the existence of an interior dusky ring, now first recognized as forming part of the system of Saturn. This explanation needed only to be proposed to insure its immediate acceptance as the true and only satisfactory solution to the singular appearances which the view of Saturn has presented during the past season, and which we had previously been unable to account for.'

[9]J.C. Maxwell, 'On the Stability of the Motion of Saturn's Rings', The Scientific Papers of James Clerk Maxwell, I: p. 372.

[10]Ibid., p. 373.

[11]J.A. Keeler, 'A Spectroscopic Proof of the Meteoric Constitution of Saturn's Rings', The Astrophysical Journal, I (January-May 1895), p. 417.

[12]Ibid., pp. 416-427.

[13]W.W. Campbell, 'Spectrographic Determination of Velocities in the System of Saturn', Ibid., II (June-December 1895), pp. 127-135.

[14]H. Deslandres, 'Recherches Spectrales sur les Anneaux de Saturne', Comptes Rendus de l'Académie des Sciences, CXX (1895), pp. 1155-1158.

APPENDIX B

Hevelius to Bouillau

9 December 1659

From the original in Bibliotheque Nationale MSS.

'Collection Boulliau' XXV (FF 13042), ff. 87r-92v.

Clarissime, atqûe Eruditissime Vir,

amice plurimum honorande

Quo rariores hactenus Tuae extiterunt literae, eo profecto mihi fuerunt gratiores Tuae responsoriae, quas Lutetiae, die 6 Junij ad me exarasti. Cum me inprimis de valetudine Tuae atque Vestro exoptatissimo pacifico statu certiorem reddant. O utinam et nobis tandem hic in Borussia si liceat esse beatis! Nam Regio haec nostra civitasque, vix diutius tantas belli calamitates sustinere valet. Omnia negotia enim terra marique propemodum possum eunt, sic ut inde nullus non hic apud nos inveniatur, qui non hocce turbulentissimo et calamitoso rerum statu, in suis negotijs atque occupationibus nimium quantum turbetur; ut ipsemet satis superque experior. Quicquid tamen sit, Laus sit Altissimi, quod nihilominus mihi, inter tot tantesque belli strepitus, post curas publicas humeris meis impositas, tantillum otij nonnunquam clementissime concesserit, ut studijs, et speculationibus nostris Uranicis invigilare potuerim; quibus, ut verum fatiar, vix ullo alio tempore diligentius, et cupidiore animo, quam proximis elapsis aliquot annis incubui, et quidem nocturnis, tam stellarum fixarum, quam erraticarum Observationibus. Nam, cum a multis iam annis bene perspexerim, circa stellas fixas corrigendas, etiam ab ipso magno Tychone aliquid posteritati relictum esse, nescio, sane, quo stimulo interno excitatus fuerim ad istud arduum negotium, ut ut laboriosissimum, et plerisque fere fastidiosissimum in me suspiciendum. Certum enim est, ut me tacente optime nosti, nisi fixae rite, ut accuratissime restituantur, frustra, tum circa genuina loca Planetarum, tum Cometarum, nec non eorundem parallaxes laborari. Quamobrem adductus fui, ut ut quamplurima vastissima instrumenta, qualia Tycho habuit, ex ligno scilicet, laminis orichalcicis superinducto olim iam possideam. ut nihilominus adhuc alia longe accuratiora, ex solido metallo, singulari plane, et nova ratione fabricandum curaverim, utpote Quadrantes, Sextantes, Octantes, quoad radium sex, septem, octo et amplius pedum; non solum singula minuta, sed et quina secunda, imo bina accurate commonstrantia; quae in ijs non circiter tantum dijudicare, sive divinare, ut olim obtigit, sed distincte deprehendere et num[e]rare possumus. Adhaec speculam pro hisce organis, plane aliam, longe adhuc commodiorem, quam hactenus possedi. undique scilicet patentem, satisque amplam, tum peculiari modo prorsus mobilem, in culmine aedium mearum exaedificavi. Quibus feliciter absolutis omnibus, negotium istud Uranicum, bene cum DEO, aggressus sum, non quidem solus (scies enim id minime fieri posse, ut distantiae siderum observentur) sed un[o] cum viro quodam juveno dantiscano, rerum Mathematicarum optime perito, ac summe industrio, quem eum in finem ab Academia Rostochiensi, ante triennium circiter evo[ca?]vi, et ab eo tempore in domo mea alui. Quo in labore etiam, adspirante divino Numine, progressus sum, ut iam aliquot millia observationum, distantiarum scilicet fixarum a se invicem, non neglectis reliquorum Planetarum omnium, acquisiverimus, imo spatio unius anni, duo circiter millia: et quo minus hoc elapso anno caelum habuerimus propitium, eo, crede, diligentiores fuimus, sic ut nullam omnino horulam, nedum noctem praeterlabi passi simus, cujus non summa cupiditate habuerimus rationem. Quanti autem laboris constiterit, tot observationes debita diligentia peragere, periti rerum facile judicabunt; quinimo ij qui unicam saltem distantiam omnino correcte dimetiri periculum facient. Siquidem non unica vice ea acquiritur; sed multoties eandem examinando ac repetendo, et quidem diversissimis instrumentis, diversoque tempore. Ricciolus, qui huic negotio, cum P. Francisco Grimaldo (uti legere est in suo Almagesto Lib. VI, Cap. X. pag. 425) licet incredibili tolerantia (ut ipsemet loquitur) per annos 4 operam dedeni, tamen non nisi 150 distantias, si recte numerentur, (nam plurimas bis annotavit) spatio istius quadriennij acquisivit: cum, nos, ut di[x]ximus, (absit gloria) annuente divina gratia, unico anno duo millia acquisiverimus. Quaeras autem quomodo unquam id fieri potueret, cum unicam distantiam non semel aut bis, sed multoties, decies scilicet vicies et amplius, ac diversis plerumque instrumentis dimensus fuerim? Id hac vice brevibus dicere haud valeo, sed rejiciendum id ipsum est in aliam commodiorem occasionem, vel forte in id ipsum tempus, quo nos hic ipse invisas: de quo bono Tuo proposito aurem mihi communis noster amicus nuper vellicavit; quod ut fiat, faxit DEUS O.M.!

Profecto gratissimus exoptatissimusque mihi eris hospes. Interim hoc certo credas velim, me omnino singularem, et multo certiorem, longeque Tychonico faciliorem organa tractando modum, hactenusque plane ignotum habere: imo, ut ut instrumenta mea aenea sint ponderosissima, triplo scilicet quadruplo et amplius ipsis Tychonicis ponderosiora, nihilo tamen minus longe sunt leviuscula in tractando, dirigendo, inclinando, elevando deprimendoque. Ipse namque solus, non unica duntaxat manu, sed digito ad nutum velocissime dirigo, ea nimirum ipsa Organa, quae 300, 400 libras et amplius pendunt: cum tamen Tychonica tantum 80 ponderantia duobus tribusve observationibus satis facesserint negotij, ac vix quidem illis obtemperaverint. Verum de his satis, cum plura hac vice animi moeror, quo nuper sum afflictus, haud permitat. Proh dolor enim! diligentissimus meus socius M. Kretzchmorus, quem unico ob egregias animi dotes animitus amavi, ex improviso acuta fobri die 30 Octob., post finitam statim Eclipsin Lunarem, quam adhuc mecum observat, correptus, die 5 Novemb, mortem cum vita commutavit: cujus praematurum decessum profecto ex toto corde doleo. Sperassem namque hoc autumnali et brumali tempore me strenue porrecturum in nostris susceptis Uranicis laboribus; sed cum DEO aliter visum fuerit, lubens ei totum committo negotium; qui pro sua divina voluntate, si nostris annuet conatibus, alium quempiam virum solertem suscitabit, oujus auxilio observationis nostras, ad nominis sui gloriam, peragere suo tempore non nequeamus. Interea cum automaturgo meo, quem in aedibus meis pro instrumentis fabricandis sustento, homine alias literarum plane rudi, in sua arte tamen bene posito, observationes instituo, et quidem coelesti ope satis feliciter: quae ut porro ex voto succedant, DEUM O.M. supplex veneror. Quod attinet novam istam stellam in pectore Cygni, novam ante pectus nominatam, hanc cum Kepplero plane novam, ac anno 1600 circiter primum ortam existimo, quemadmodum id evidentibus profecto rationibus, in sua narratione Astronomica, super modo dictam stellam institutam, demonstrat; sic ut impossibile prorsus fuisset, Veteros illos diligentissimos rerum coelestium scrutatores, si aliqua ex vetustioribus extitisset, illam penitus non animadvertisse, et quidem stellam, quae adhuc tertio ordini haud male annumerari potest. Minor quidem est illa in pectore Cygni vetustiori, nihilominus tamen optime aequatur Extremae aliae Cygni, nisi quod lumine quodammodo obtusiori, et rubicundiori gaudeat, quali facie istam ab aliquot jam annis semper observavi, nec memini, quoad quantitatem et splendorem hucusque quicquam esse immutatam. In hac mea opinione amplius confirmor, novam penitus esse exortam, ac veteribus plane extitisse invisam, quod diversas Veterum quorundam Observatorum delineationes siderum invenerim, in quibus illam, ut ut minores, utpote illam in medio collo Cygni quartae, et in capite quintae magnitudinis rite depinxerint, hanc tamen novam ante pectus omnino omiserint. Inprimis ex schemate quodam, occasione Cometae anni 1577, a Solertissimo, et Matheseos bene perito Gemma Frisio, accurate delineata, in quodam opusculo de prodigiosa specie naturaque istius Cometae, Antwerpiae anno 1778 [sic] edito. Hic antes, licet pariter longe minores, et minus conspicuas stellulas, hinc inde etiam in ipso Cygno satis diligenter adumbraverit, nihilominus hanc, de qua nobis sermo est, minime prorsus apposuit; quod si adfuisset ibidem in caelo, haud, crede, istam neglexisset. Caeterum gratias habeo debitas, pro communicatis ijs, quae circa Eclipses nuperas a Te alijsque annotata sunt. Libenter vicissim omnia et singula communicarem, qua circa quatuor Eclipses satis benigno caelo observavimus: sed epistola haec nimium excrescent: idcirco rejiciendum ejusque, dum schemata in aes fuerint incisa, ac reliqua typis vulgata. Interim tamen tene praecipua observationum capita. De Eclipsi Lunae anno 1657, die 25 Junij visibili, non nisi, uti ex posteriori mea epistola intellexisti, decrescentes phases circiter quindecim annotavimus, sic ut hora

10	50'	30"	Sex adhuc digiti fuerint obscurati
10	56	30	5
11	2	30	4
11	8	30	3
11	14	30	2
11	20	0	1
11	25	30	Finis
11	37	38	Finis Penumbrae

De Eclipsi vero ☽, Anno 1657. die 20 Decembr. aliquid amplius deprehendimus; ipsum quidem initium, ut et, ob nubes intercurratis, haud conspeximus; attamen ex reliquis accurate admodum delineatis duodecim phasibus, satis praecise, ut opinor, tam initium quam finem assequuti sumus. Ratione autem calculi initium in hac Eclipsi tardius, et finis citius ingruit. Quantitas tantum fuit 3½ digit., cum calculus integro digito majorem esse voluerit. Apparuit itaque initium Hora 7 18' Vesp.

1 dig.	Hor. 7 28	Sectio maximae obscurationis transebat paulo Supra Pl. Alabastrenum, Stringebat Ins. Corsicum a parte inferiore; per ins. Macrum, stringens M. Trapezum, transiensque Montes Amadocos.
2 digit.	Hor. 7 42	
3 dig.	Hor. 7 59	

$3\frac{1}{2}$ dig.	Hor.	8 18
3 digit.	Hor.	8 37
2 digit.	Hor.	8 54
1 Digit.	Hor.	9 8
Finis	Hor.	9 18
Finis penumb.		9 31

Tertiam Eclipsin Lunae hujus anni, die 30 Octobr. ut ut nebulae totum caelum tempore Eclipseos obduxissent, nihilominus tamen, ope nostro praeclarissimo tubo optico 12 pedum longo, pro voto 35 phases, praeter ipsum finem, quem annotare densissimae nubes impediebant, observavimus.
Initium penumbrae accidit Hor. 2 30'.
Ipsum Initium Eclipseos Hor. 3 3

1. digit.	Hor.	3 10
2. dig.	Hor.	3 18
3 dig.	Hor.	3 29
4 dig.	Hor.	3 39
5. digit.	Hor.	3 53
$5\frac{3}{4}$ digit.	Hor.	4 21
5 dig.	Hor.	4 51
4. dig.	Hor.	5 5
3. dig.	Hor.	5 15
2. dig.	Hor.	5 24
1. dig.	Hor.	5 32
Finis	Hor.	5 39
Finis penumb.	Hor.	6 11

Circa hanc Eclipsin inprimis notatu dignum occurrit, quod penumbra ipsam veram umbram 33' praecesserit, et 32' circiter subsequuta; quod in alijs ☽ae defectibus vix me observasse memini. Quantitas hujus Eclipsis haud extitit major $5\frac{3}{4}$ digit. Cum calculus longe eam exhibeat majorem. Sectio maximae obscurationis transivit supra M. Climacem per Ins. Leam, per superiorem partem Pl. Olympi, Pl. Amanum, et supra Ins. Caspiam Majorem.
Eclipsin ☉is quod spectat posteriorem, quae die 14 Novemb. hoc anno circa occasum Solis contigit, ejus quidem initium, ob nubes quidem minime annotare concedebatur, aliquas tamen phases, numero 8, satis bene acquisivi. Inveni itaque hanc Eclipsin nimium quantum a calculo discrepare; siquidem initium extitit 3 18': cum tabula 30' tardius id statuerent. Quantitas in horizonte nostro, si conspicuus Sol ad occasum usque permansisset, me judicio, extitisset $6\frac{1}{2}$ digit. circ. cum

tabula multo minorem referent. Sequentes autem phases accurate sunt delineatae.

Hor.	3	25'	34"	1. dig.
Hor.	3	26	13	$1\frac{1}{4}$. dig.
Hor.	3	27	4	$1\frac{3}{8}$. dig.
Hor.	3	28	12	$1\frac{5}{8}$. dig.
Hor.	3	31	5	2. dig.
Hor.	3	32	44	$2\frac{3}{8}$. dig.
Hor.	3	44	43	$4\frac{1}{8}$. dig.
Hor..	3	45	34	$4\frac{1}{4}$. dig.
Hor.	3	55	0	$5\frac{3}{8}$. dig.

Plura et longe specialiora suo tempore expectabis. Quid Tu vero deprehenderis, pariter meo[um?] communicare haud gravaberis.
Misit mihi nuper, communis noster amicus Dn. Nucerius indicem alicujus magni, ut conjicio, voluminis perlegendum, quem Tu, sine omni dubio, jam etiam accepisti; de quo, ut judicium meum, quod admodum tenue esse duco, aporirem, rogavit. Perlectis autem ac ponderatis omnibus, quod tamen pace Clarissimi istius Viri dictum velem, videtur mihi, multa quidem promittero; num autem, inprimis rationem istam accuratissime in ipsis minutis computandi motus corporum caelestium omnium ad quaecunque secula tam praeterita quam futura praestiterit valde dubito. Profecto, nec Eximius ille Tycho, nec ij, qui rem Astronomicum non sub tuguriolo, aliquo, vel post fornacem, sed sub dio excoluerunt, id fieri posse sibi unquam sunt persuasi: quemadmodum Acutissimus noster Robervallus in suo Aristarcho Samio pag. ultima, de eadem hac materia recte sentit, cui et ego omnino adstipulor: *Ne quis* (inquit) *imposterum se jactet de perpetua quadam Planetarum theoria, seu motuum Astronomicorum perfecta scientia; quae forsan tot tamque frequentibus irregularitatibus obnoxia est, causis adeo in abstruso latentibus, ut ipsas detegere, aut etiam intelligere, captum longe excedat humanum. Adhaec ut ut permulta de suo primo mobili et diurno*, ficto nempe somnio in medium postulerit, tamen hypothesin Copernicaeam, nec mihi, nec forte etiam Tibi, alijsque permultis, qui sano gaudent cerebro, exstirpabit ex animo; multo minus nobis persuadebit, nec Venerem nec Mercurium, et per consequens maculas Solares posse Eclipsare, nec maculare Solem, in aliquali ipsius particula, quicquid etiam (ut ipse loquitur) per Telescopia senserint hac aetate recentiores, ut taceam, quod sint hallucinia, quae fiunt et introducuntur in re literaria per Telescopia, circa apparentias in Sole, Luna et reliquis Planetis. Bone DEUS, quomodo Vir iste se se Orbi literato prostituet, quod etiam ea, quae in sensus ocularesque adeo aperte incurrunt, prorsus negare velit. Caecis haec deliramenta forti persuaderi posse concedo,

qui nihil unquam de veris istis phenomenis vel audiverunt, vel viderunt; sed alijs, sane, nullo modo, qui oculis fruuntur, usum perspicillorum bene intelligunt, ipsimetque phaenomena ista, verum ope sunt contemplati.
Profecto, quisquis etiam ille sit auto, Telescopium, inventum scilicet illud divinum, per quod, mea opinione, Astronomia haud parva acquisivit incrementa, nunquam nobis eripiet; sed operam, DEO favente, potius dabimus sedulam, at adhuc meliora construere possimus, ad corpora coelestia adhuc clarius accuratiusque pervestiganda. Videtur, autorem istum ex innumero verum esse (prout Gassendus scite et recte loquitur in Excercitatione Epist. contra Fludd. pag. 227) qui coelestia loquuntur aut scribunt, et omnes tamen non ex ipso caelo, sed ex proprio cerebro opiniones comminiscuntur. Sine dubio, quantum apparet, egregium Astrologum in isto opere se praestabit autor; num autem Astronomum, tempus docebit. Verum, quid Tu, de his omnibus, statuas, rogo, ne graveris, pariter communicare, facies certe rem nobis pergratam; inprimis si adjicies judicium tuum, de novo isto Systemate Saturnio Hugenij, quod nuper ad me transmittit autor. Meam sententiam cum percipiendi haud minus percupidum Te esse probe sciam, nunc simul libere more meo, aperiam. 1. Totius Hypotheseos Hugenianae fundamentum in eo consistit, quod statuat Saturnum, quoad corpus suum intermedium, plane Sphaericum; nostra vero in eo, quod idem corpus Saturni a me habeatur pro corpore Elliptico. Atque exinde, si haec nostra facies (sicuti probandum erit) genuina ipsius Saturni est, utique tota Hugenij splendida illa annularis machina plane corruit. At, inquies, Hugenius prae alijs omnibus observationibus majorem fidem sibi conciliat, pag. 35, suis praeclarissimis tubis, quibus quoties libet novum Saturnalem clare intuetur, quin alij spectare nequeunt; hinc observationes reliquas suas veriores contendit, quam reliquorum observatorum omnium. Respondeo, me itidem Telescopia, et quidem varij generis non solum ex lentibus duobus, sed etiam ex tribus; quinque et pluribus, diversissima longitudine possidere, quorum lentes, partim mea, partim aliorum industria sunt elaborata, inprimis ab isto Solertissimo Augustuno Optico Wiselis mihi comparavi pro 500 flor. polon. seu gallic., quorum diversorum beneficio aeque accurate Comitem istum Saturni, ab aliquot jam annis, ut observationes meae testantur, me observare scias; imo memini me istum ante decennium et amplius circa Saturnum jam notasse; sed nimium securus tum temporis eum pro fixa habuisse: sic ut in hac parte ipsi minime cedam. Nihil itaque video, cur nostrae observationes non eandem fidem, quam Hugenianae mereantur. Attamen Hugenius acriter insistit, corpus istud intermedium Saturni, suis perspicillis non aliter unquam deprehensum esse, quam

rotundum. Id quod, certe, ultro concedo; sed quaeritur, quo tempore, et quam diu Saturnum observaverit? Profecto, ni fallor non nisi quadriennij spatio, ab anno scilicet 1655 hucusque, observationes instituit, nec vetustiores unquam produxit: quemadmodum id non solum ex ejus opusculo modo edito clare patet, sed etiam ex epistola sua omnium priori ad me data, Anno 1656, die 8 Martij Hagae Comitis: *dubio procul* (enim inquit) *observationes ab anno 1645 usque in praesens tempus continuasti, quae non omnes eadem facie, quae in libris Selenographiae eum Tibi retulere. Quod si ijs varietatibus conspectum cognoscerem, quae ex hypothesi mea consequuntur, plurimum ex convenientia gauderem. Mihi anno demum praecedenti telescopiorum ar~~k~~s innotuit, neque proprias observationes antiquiores habeo.* Ex quibus vides, quod tantummodo ab anno 1655 Saturni faciem conspexerit, at isto tempore, non nisi Saturnum aut omnino sphaericum, aut sphaerico-cuspidatum observare licuit. Nam alia forma, uti ex tabula etiam nostra; tam phasium Saturni speciali, quam Ephemeride nostra pag. 17 dissertat. nostra inserta, clare perspicitur, nunquam tum extitit. Hinc recte concludimus, Hugenium nunquam adhuc Saturnum suismet oculis Elliptico-ansatum animadvertere potuisse. Attamen sic argumentatur, quoniam Saturnum nunquam Elliptico-ansatum vidi, ergo non nisi corpore constat sphaerico; verum qualisnam haec sit consequntia, tu facile intelligis. Expectet, quaeso, annum 1663 et 1664, non dubito, quin Saturnum evidenter forma Elliptica constare deprehendat. Hac siquidem non semel aut bis, sed multoties, nec solus, sed cum praeclarissimis Viris rerum Astronomicarum optime peritis observavi: quippe qui Saturnum non per quatuor tantum, verum per 17 annos, ab anno nimirum 1642 hucusque contemplor, ac per integros tres annos, ab anno scilicet 1647 usque 1650 continue haud alia forma, quam di~~s~~ota Elliptica mihi apparuisse, sicut observationes nostrae testantur. An putet forsitan Hugenius, me cum alijs haud posse discernere, quid sit Ellipticum vel sphaericum, aut id ex cerebro meo finxisse, ut scribit pag. 39, vel potius somniasse? non me hercule. Verum, cum ea figura sphaeroidis oblongae, eo tempore distincte admodum cum alijs Saturnum conspexerim, haud potui aliter, quam firmiter concludere, Saturnum ea forma esse praeditum, etiam suo tempore, in ♊ scilicet et ♐, eadem specie denuo appariturum. Hugenius vero, qui nunquam adhuc integram revolutionem phasium Saturni observavit, quomodo, quaeso, de hoc phaenomeno tam audacter audet statuere certi, nisi potius fingere velit cum semper apparere sphaericum? Tempus. certe, docebit, et ipse suismet Telescopijs, suo tempore, clare deprehendit, Saturnum interdum etiam lucere facie Elliptica, Quae figura ut annulo male admodum includitur, sic etiam phases ♄ omnes, ea ratione haud bene, meo quali quali judicio, demonstrantur. Adhaec, concessa etiam ejus hypothesi annulari, Monosphaericam tamen figuram Saturni evidenter satis demonstrare nequit: multum et pluribus quidem allaborat, praevidens optime rei perplexitatem, ut se istis laqueis tricisque extricare queat, quae affatim pag. 62 et sequentibus pag. 67, 68, 70, usque 73 circa istud phaenomenon ipsi occurrunt: inprimis, cum non negare possit, annulum istum fictum, certa constare crassitie. Siquidem talis ingens machina, oportet ut habeat bene notabilem crassitudinem seu profunditatem, quo in motu isto velocissimo non penitus corruat, vel dissolvatur; quod alias, sane, non sine magno coelestium corporum detrimento et confusione fieret! Posito igitur tali corpore, non capio, quomodo prorsus exutis omnino ab omni parte brachijs, ansis vel lineis rectis apparere possit; cumprimis ei, qui omnium optimo et clarissimo gaudet Telescopio. Persuadere quidem nobis conatur, ab istis partibus lateralibus vel marginalibus annuli, solis lumen vel nihil prorsus, vel leviter admodum posse reflecti, sive materiam istam aquae similem, aut certe laevi et splendida facie esse praeditam, atque extrema annuli praecingere, minime vero asperam montibusque esse obsitam, ut pag. 70 loquitur; sed hae rationes, quam sint frigidae, et quantum contrariae ipsi facessant negotij, quis non perspicit? Profecto, si istius annuli limbus, seu margo, ex tali materia constet, ut vult Hugenius, puto etiam reliquas partes, utpote quae secundum utramque planitiem annuli sunt positae, ex simili materia constare; sed ejusmodi obscuritates et disparentias, in plana superficie, haud ipsemet notavit, quomodo igitur in dictis marginalibus partibus id ei concedamus? Atvero ex mea hypothesi, nullo negotio quilibet apprehendit, quod certo tempore in ♍ et ♓ Saturnus necessario rotundus omnino apparet, sic ut ad talia diverticula confugere minime sit necessum. Quae hac vice sufficiant, de ratione nempe ista demonstrandi phases Saturni: cum non adeo multum intersit, qua hypothesi phaenomenon istud demonstretur, quam quomodo periodus omnium phasium Saturni, atque tempora vicissitudinum recte prius inveniantur ac defineantur. Haec autem periodum ego primus omnium (quemadmodum id nemo inficias ire poterit) et detexi, et publici feci juris: siquidem mea dissertatio de ♄ facie, longe prius erat conscripta, atque edita, quam pagella ejus de Saturni Luna a frateri Hugenij mihi erat oblata. Putabam quidem initio in ipsius grypho lateri ipsius periodum: inquit enim ibidem: Observationes praeterito praesentique anno collectas, quibus periodus ipsius demonstratur, tunc una edituri sumus, cum integrum ♄i Systema perfecerimus. Cujus interea summam sequenti grypho consignare visum est. Verum ex ipsius Systemate Saturni modo edito, atque aenigmate enodato, clari perspicuo anno 1656, adhuc ipsam periodum nescivisse, quandoquidem tantum Saturni formam in eo comprehendit: cum gryphus detectus sic se habeat: annulo cingitur, tenui, plano, nusquam cohaerente, ad Eclipticam inclinato; tum etiam ex ipsa dicta pagella abunde constat, ipsum penitus latuisse phasium periodum, erumpit enim in haec verba: et haerum quidem vicissitudinum tempora in futurum definire non erit difficile, si duorum adhuc mensium observationibus attendere licuerit; quae videndum an hypothesi nostrae consentiant. Quae cum ita revera ut ut sint, ex meo etiam grypho hoc, Hugenio per ejus fratrem transmisso (aaaaa ccc d eeee g h iiiiiiiii ll mm nnnnn ooo p q rrrrrr ttttt uuuuuuu./. Integra phasium Saturni revolutio absolvitur quindecim circiter annis) optime intellexerit me periodum vicissitudinum haud ignorasse, nihilominus tamen nec verbulo in systemate suo tetigit, me nempe primum istius rei esse inventorem; sed potius omnibus viribus conatur, pro phasi monosphaericae, et periodo reliquarum phasium aliquid limatius substituere: num autem id praestiterit, Tu alijque rerum coelestium strenui pervestigatores dijudicabunt. Hugenius in gradu 21 ♍ et ♓ circiter Saturnum spectari semper rotundum asserit; ego vero, non ne pariter in 20 et 21 grad. ♍ et ♓ Saturnum dedi Monosphaericum, si nempe locum eccentricum Planetae praesupponas, prout pag. 16 clare diximus? Profecto, non solum id ex tabula nostra phasium Saturni speciali pag. 15 inserta satis liquet; sed etiam ex ijs pag. 18, ubi de phasium libratione loquimur. Verum quidem est, me tam dictam ♄i phasin, quam reliquas omnes uni certo gradui non alligasse, imo nec voluisse, sed studio (id quod sincere dico) ad unum aut alterum gradum citra ultraque extendisse phasium terminos, quam initio quidem penitus statueram; etiam bene praevideram, aliquanto arctius posse eos coarctari: qua omnes reliqui observatores tales praelongos et perfectos tubos haud possidentis, aeque bene phases eas contemplari possent. Fatetur enim ipsemet Hugenius minoribus tubis, citius tardiusque appareri corpus ♄ rotundum: hinc quoque eorum gratia oportuit utrumque terminum aliquanto producere. Verum quid multis in hocce negotio tanta acribeia haud est opus, suffecit posse praesagire quo anno haec vel illa phasis continget: siquidem et ipse autor, pag. 74, 75 et 76 in dubio quasi relinquit, an phasis, inprimis rotunda omni tempore ita respondere possit: inquit enim: in quorum praedictione si a veritate aut nihil aut pauxillum tantum aberrasse non invenient, tum procul dubio causas quoque horum phaenomenon germanas qualesque reverra sunt sibi explicatas credant. Sin longe hallucinati fuerimus, adeo ut brachijs Planeta cernatur, quo tempore ex sententia nostra vel maxime ijs carere deberet; indicio id est, quaedam circa rotundam phasin accidere nobis nondum perspectum, nec ulli mortalium forsitan pervidenda. Ex quibus, cum perspicuum sit, posse ex sua etiam sententia forsan aliter accideri circa ista phaenomena; quid igitur opus est terminum phaseos Monosphaericae ad semigradum usque alligare? Quod autem sibi persuadeat, si aliquid in hocce negotio adhuc desideretur, nominem unquam ex Posteritate id pervestigaturum; eo in parte, certe, meo quidem judicio, nimium sibi suoque tribuit ingenio: quasi vero et Posteritati non aliquid semper reliquantur? profecto quamplurima: prout Seneca recte loquitur: Multa seculis tunc futuris, cum memoria nostri exeleverit reservantur. Nam quemadmodum, nostra aetate, non pauca nova, et abstrusissima, Veteribus prorsus ignota, sunt detecta, ita et Posteritas, sine omni dubio, et multa alia, quae nobis modo videntur impossibilia in lucem extrahet, praesertim si labor non cessat. Sed pergamus in nostro proposito, inquirentas, an circa reliquas phases ansatas, Hugenius aliquid liter ac nos medium protulerit? Ille anno 1672, 1685 1686, et 1701 rotundum praedixit, ego itidem, uti ex utraque tabella pag. 16 et 17 apposita clare liquet. Ille ait pag. 75, singulis quatuordecim, vel quindecim annis, nimirum bis ad singulas Saturni in sua orbita revolutiones, rotunda forma conspicienda dabitur; neque enim unquam aequinoctij sui locos transire, quin brachia amittat, potest; item pag. 76. phasin ansatarum latissimam medio tempore inter duas rotundas incidere; at ego, pg. 5, dixi, totam phasium Saturni revolutionem absolvi spatio dimidij temporis, quo Saturnus totum percurrit signiferum, hoc est, quindecim circiter annorum spatio: in utraque nempe apside, in ♐ et ♊ Saturnum observari Elliptico ansatum, in utraque vero media distantia in ♍ scilicet et ♓ omni, tempore Monosphaericum. Ex quibus vides Hugenium nihil novi hac in parte, sed illud ipsum protulisse, quod a nobis satis perspicue, quamvis alijs verbis jam dictum est. Porro latissimam phasin ansatarum reversuram dicit pag. 76, circa annum 1633 ey 1664; iterumque anno 1678 et 1679, ac postea anno 1693. Nos plane idem in Ephemeride nostra pag. 17 praediximus. Atque adeo manifestum est, circa periodum phasium Saturni, quod praecipuum est, quod inveniri potuit, nihil istis prorsus novi attulisse, sed meo arasse vitulo. Denique de phasibus istis Saturni, quas in tabella pag. 35 adjunct. delineatas dedit. haec Te scire volo, quod phasin IV. V. VI. et VII, quas pro meis venditas, nullo modo pro meis genuinis, sed pro adulterinis omnino agnoscam. Confer enim quaeso phasin meam I cum sua VII, meam III, cum sua IV, IV cum sua V, itemque V meam cum IV sua, omnesque immoto oculo accurate perlustra, animadvertes, esse prorsus dissimules, tam quoad formam, figurumque quam proportionem, et sectiones corporis intermedij, brachiorumque sive ansularum. Debuisses sane majori diligentia eas delineare, et genuinas plane exhibere, ne autori aliquid alieni affingatur, neque in ejus vergere possit praejudicium. In summa gratulor et Hugenio et mihi, mihi quidem, quod primus fuerim, qui periodum phasium mundo detexerim, atque earum rerum hypothesin non adeo absurdam exhibuerim; Hugenio vero, quod vestigijs meis insistendo novam rationem phases demonstrandi, veritatis gratia excogitaverit. Habebunt igitur literati utram digant, et approbent. Interim tamen minime dubito, possi forte adhuc aliter, imo melius vicissitudines illas suo tempore demonstrari; quod ut fiat in rei Astronomicae commodum animitus aptamus. Vale optime diuque Uraniae nostrae ingens decus. Dabam Gedani Anno 1659, die 9 Decembris.

Tuo honori ornamentisque ex animo
applaudens, applausurusque

Johannes Hevelius
Consul Veteris Civit.
Gedanensis

Ut videas, Clarissime amice, dictum factum esse, en tibi observationes quasdam, quae circa Comitem Saturni meis Telescopijs feliciter habui.

Anno 1657, die 21 Martij, hora 9 vespertina, Comes Saturni visus est ad Orientem in maxima remotione, et cum globulis seu ansulis ♄^{i} prorsus in linea recta. Interstitium inter dictum Satellitem, et Saturnum ipsum vix aequabatur interstitio inter Montem Argentarium, et Lacum nigrum majorem in Luna: atque ita meo judicio, vix tribus minutis a corpore ♄ distitit.

Die 23 Martij, vicinior erat Saturno

Die 24, 25, 26, 27, et 28 vero nusquam plane apparebat

Die vicissim 30 Martij hora 9 vesp. Comes in latere jam occiduo micabat, distans tantum $1\frac{1}{2}$ minut.; Sed vix ac ne vix cernebatur, ob Lunam, ut autumo, eo tempore Saturno admodum vicinam.

Die 7 Aprilis vesp. denuo a latere ♄^{i} ortivo, in maxima circiter elongatione apparuit.

Die 8, propius adstitit Saturno.

Die 10 et 12, omnino disparuerat.

Die 13, rursus a parte occidua, respectu Saturn. deprehendi Comitem.

Die 14, in eadem plaga Comitem adhuc magis a ♄ remotum vidimus.

Die 15, motu retrogrado, propinquior ♄^{o} extitit, sic ut die 16 ex conspectu plane se se subduxerit.

Die 20 Aprilis, Corpori ♄ ab ortu pene adhaerebat; Spectaculum profecto erat jucundissimum.

Die 8 Maij, notabili spatio, pariter ab ortu erat a ♄° elongatus.

At die 16 ejusdem mensis, rursus ad occidentem in maxima circiter digressione deprehensus est.

Hujus generis observationes quamplurimas possideo, suo tempore Orbi literario communicandas. Id quod autem, priusquam labores meos Cometographicos ad finem perduxere, vix fieri poterit: atque tum DEO juvante, cum alijs quamplurimis rerum coelestium observationibus, praesertim Fixarum et Planetarum omnium magno labore acquisitis, in publicum preferam. Vale iterum.

APPENDIX C

Eustachio Divini versus Christiaan Huygens:

a Reappraisal

(Accepted for publication by Physis)

When a simple artisan does battle with an eminent scientist in a public controversy, the results can be disastrous for the artisan. Such was the case of Eustachio Divini who became involved in a controversy with Christiaan Huygens that was not entirely of his own making. The occasion for this controversy was the publication of Huygens' Systema Saturnium in 1659, in which he proposed his famous ring-theory as an explanation of the strange appearances of the planet Saturn. These appearances had confounded astronomers ever since 1610, when Galileo first observed Saturn through a telescope. At the time of the publication of Systema Saturnium Huygens was 30 years old and his reputation had been established already by several mathematical publications, his discovery of a satellite of Saturn, and his successful construction of a pendulum clock, described in his Horologium of 1658. At this time Divini was considered by many the best telescope maker in Europe. But his reputation suffered greatly in the controversy because it became connected with Fabri's theory about Saturn's appearances and this theory was not held in high esteem.

The controversy started ostensibly when Divini took offence at certain remarks made by Huygens concerning the figure of Saturn shown by Divini in his advertising sheet of 1649 (see fig. 1). (1) On the subject of the so-called 'handled' appearance of Saturn Huygens wrote:

> C'est aussi sous cette dernière forme qu'Eustachio Divini les a dessinées en 1646, 1647 et 1648; ... Vu qu'il est considéré comme un très excellent fabricateur de télescopes, il est croyable que c'est lui qui nous a montré la forme de Saturne la plus rigoureusement vraie, à cela près qu'il y a ajouté de son cru, me semble-t-il, les ombres qui apparaissent dans la figure. (2)

But surely this comment was, if anything, flattering and not an accusation of fraud as Divini called it. (3) Furthermore, Divini freely admitted that he had added these shadows. (5) There is only one other reference to Divini in Systema Saturnium in which Huygens voices no opinion on his telescopes or his observations. (4) Therefore this seems hardly a sufficient reason for Divini to answer Huygens, nor does his desire to point out 'certain errors' in Huygens' observations, as he states in Brevis Annotatio. (6) He had, after all, never pointed out errors in the observations of other astronomers.

The reason for Divini's decision to partake in a public response to Systema Saturnium is more likely to be found in the whole tone of Huygens' writing, which is somewhat arrogant and overbearing. Huygens argued that since no one had seen Saturn's moon before him, his telescopes necessarily had to be the best and throughout the book he maintained that if other observers had only had telescopes equal in quality to his, they would have come to the same conclusion as to the cause of Saturn's appearances as he had. Leaving aside for the moment the question of whether this was in fact true, this arrogance was offensive to men like Hevelius and Divini who had reputations for being good telescope makers. Thus, Hevelius wrote to Ismael Bouillau on this subject:

> ... Huygens attaches more faith to his own observations ... made with his most splendid tubes with which he sees the new Saturnial [star], which others are not able to see, clearly whenever he wishes, than to all others. For this reason he asserts that the rest of his observations are more correct than the rest of the observations of others. I reply that I similarly possess telescopes of various lengths and various types, not only with two lenses, but also with three and five and more, which lenses were made partly by my own labour and partly by that of others, especially by that most skillful Imperial Optician, Wiesel, who supplied me with a tube for 500 Polish florins ... You know that I have accurately observed, with the benefit of these various telescopes, that companion of Saturn several years ago now, ... or rather, I remember that I noticed it about Saturn ten years or more ago. But being too careless, I held it then to be a fixed star. Thus, on this point, I don't concede anything to him. (7)

It is not at all remarkable that Divini, whose livelihood came from the making of telescopes, was even more upset than was Hevelius, who was independently wealthy. In a letter to Prince Leopold de' Medici Divini wrote about Systema Saturnium:

> ... trovai ch'in qualche cosa troppo egli si sia fidato, e di se, e delli suoi occhiali. (8)

It seems therefore fair to conclude that this was Divini's real reason for letting himself be drawn into the controversy.

But it is questionable whether Divini would in fact have written anything publicly had it not been for the help of Honoré Fabri. Fabri apparently saw a welcome opportunity to air his own views about Saturn's appearances. Although the tracts, Brevis Annotatio in Systema Saturnium (Rome, 1660) and Pro Sua Annotatione in Systema Saturnium (Rome, 1661), indicated Divini as their author, it has been generally agreed ever since that time that they came in fact from the pen of Fabri. This is almost certainly correct, as Divini himself stated in a letter to Prince Leopold (written in Italian) that he was not versed in the Latin tongue and that Fabri had kindly translated the tract (Brevis Annotatio) into Latin for him. This letter was not even written by Divini, because the signature is in a hand different from the hand in which the rest of the letter was written. (9) It is difficult to imagine how one who could not even write his own letters to an important person could write a tract such as Brevis Annotatio or Pro Sua Annotatione, even in Italian. It also seems silly for Fabri to tell Divini about his theory, have Divini write it up in Italian, only to have Fabri translate it into Latin. It is much more likely that Divini supplied Fabri with some information, which Fabri wrote up and to which he then added his own theory. We shall ignore here the argument between Huygens and Fabri about their respective systems of Saturn, and concentrate on the more difficult controversy between Divini and Huygens regarding the quality of their telescopes.

But first a word should be said about Divini's credentials as a telescope maker, up to the time of the controversy. He started making telescopes some time in the 1640's, at the age of perhaps 35, and rapidly gained prominence in astronomical circles in Rome, from where his reputation spread. In 1649 he published a single sheet advertisement for his telescopes, containing a map of the full Moon, as well as figures of his observations of the new Moon, Venus, Jupiter, and Saturn. In the legend he mentions telescopes of up to 45 palmi (about 34 feet) and also makes mention of the use of a reticule in depicting the face of the Moon. This has led to some speculation on Divini's claim to the invention of the micrometer. (10) But apparently Divini's telescopes were not then as good as those of Johannes Wiesel, the Imperial Optician of Augsburg. Riccioli mentions in his Almagestum Novum of 1651 that although he had used telescopes by Galileo, Fontana, Torricelli, and Divini, none were as good as the 15 foot telescope sold to him by a Bavarian artisan - undoubtedly Wiesel. (11) It appears however that in subsequent years Divini's telescopes came to equal those of Wiesel, and even to surpass them. When Antonius Maria Schyrle de Rheita, the Capucin monk whose description of Wiesel's telescopes in his Oculus Enoch et Eliae of 1645 made this type of telescope famous, visited

Balthasar de Monconys in Lyons in the late 1650's, he examined an 18 foot Divini telescope purchased by Monconys and was so impressed by this telescope that he begged until Monconys made him a present of it. (12) It is, of course, true that Rheita made his own telescopes (he had made one for Monconys some time earlier (13)) and that these were probably not as good as those of Wiesel. But it is certain that Rheita was familiar with the quality of Wiesel's telescopes and could tell a superior telescope when he saw one.

During the 1650's Divini's telescopes were exported to various parts of Europe. As mentioned above, he had sent one to Monconys in Lyons, and when Sir Kenelm Digby left Italy he took no fewer than six Divini telescopes with him, according to Divini. (14) It was probably one of these that Digby gave to Gassendi in 1653 (15) and perhaps some of these also found their way into England. Divini also tells us that a certain Thomas Paggi (Thomas Page?), an English nobleman (16), took one of his telescopes with him when he left Rome. Thus, Divini's reputation was rising during the 1650's and Huygens himself described him as '<u>praestantissimus perspicillorum artifex</u>'. It is therefore fair to say that by 1659 his claim to being the best telescope maker in Europe was substantial and it is quite understandable that he was upset by Huygens' allegations about the telescopes of all others.

On the surface Huygens' claim for the superiority of his telescopes is well founded. <u>Systema Saturnium</u> is a carefully constructed tract. Huygens begins by describing his telescopes in some detail and discussing his observations of other planets and some fixed stars, before he launches into an elaborate description of his observations of Saturn's satellite and the determination of its period. It was difficult to dispute Huygens' discovery of the satellite after reading all this information. On the subject of the planet Saturn itself, Huygens states that he observed it just before it became solitary in 1656 (i.e. when the ring was edge-on) not with two detached globes flanking it (see fig. 2), but rather with a continuous streak of light projecting out from the central globe on each side (see fig. 3). (17) Furthermore, when Saturn was solitary in 1656, he saw a dark band on the face of the disc (see fig. 4) and throughout the period 1655 to 1659 he saw dark bands adjacent to the ring on the central disc (see fig. 5).

Now Divini had not discovered the satellite, although he had observed it after he had heard of its discovery, nor had he seen a dark band on Mars, such as Huygens claimed to have seen. (18) He had seen Saturn as in fig. 2 rather than as in fig. 3 and he had never seen any dark band on the planet's disc. Therefore, what conclusion could one come to other than that Divini's telescopes were inferior to those of Huygens? Furthermore, Huygens was right in his theory about Saturn's ring (with some exceptions however) and Divini and Fabri were proven wrong in their theory. Quite reasonably therefore, there was no particular reason to doubt Huygens' allegations concerning his telescopes.

It is impossible in the scope of this paper to prove conclusively that Divini's telescopes were the equals of Huygens' telescopes. Indeed, this can only be atteempted by a direct comparison of telescopes of the two men of comparable size and made roughly at the same time. But the evidence will be examined to see if it substantiates the claims made.

In the period under consideration, 1655-1660, Huygens had not yet invented the compound eye piece that still bears his name, although in the 23 foot telescope described in Systema Saturnium he employed two contiguous plano-convex lenses in his eye piece rather than a single lens. This configuration of the eye piece has in fact very little advantage over a single lens, so that this difference in configuration between Huygens' and Divini's telescopes can be neglected. Furthermore, most of the observations on which they disagreed were made in Huygens' case with a 12 foot telescope employing a single lens in the eye piece. The apertures of the objectives were almost identical and if there was a difference in the quality of the glass employed in making the lenses, Divini had the advantage because Italian glass was better than Dutch glass. Thus, if Huygens' telescopes were superior to Divini's, it had to be because his lenses were better ground and polished. Since Huygens started making his own telescopes early in 1655 (19) and discovered the satellite on March 25th of the same year (20), we are forced to conclude that if Huygens' telescopes were better, he acquired in less than three months greater skill in grinding and polishing lenses than Divini had acquired in about ten years of practice. Surely, this conolusion, although possible, must be greeted with skepticism.

During the period under consideration no direct comparison between Huygens and Divini telescopes was made. But in 1663 Balthasar de Monconys had a 10 foot Divini telescope with five lenses with him when he visited The Hague. This telesoope was compared to some of the telescopes made by the Huygens brothers and Constantijn Huygens wrote to his brother Christiaan (who was in Paris):

> Nous avons ... fait essay de sa lunette de Divinis, dont je trouve le principal verre fort bon, mais elle n'est pas exempte des defaults qu'ont toutes celle qui sont de cette longeur quand on s'en veut servir de jour. Dans les Oculaires aussi dont il se sert, on remarque touts les points du verre, puis le tuyau est si gros et si lourd que je ne voy rien d'extraordinaire à cette piece. (21)

Monconys relates the same incident as follows in his travel journal:

> Nous fismes comparaison de nos lunettes, ils trouverent mon objectif excellent, comme j'admiray la clarté, & la netteté de leurs, ce qui procede de la disposition, & proportion des deux oculaires [i.e. the Huygens eye piece] ... (22)

By this time Huygens had invented the eye piece named after him and this rendered his eye pieces superior to those of Divini. But it is evident from the above that Divini's objectives were just as good, if not better than those of Huygens. This telescope had been in Monconys' possession at least since early 1662 (23), so that if Huygens' expertise in grinding lenses had been greater than Divini's in 1655, by about 1661 Divini had caught up with Huygens' skill. It appears that at least Divini's objectives remained better than those of Huygens throughout the 1660's for in 1667 Huygens wrote:

> L'on n'a jamais parlè de faire venir icy Campani ou Divini, par ce que nous croions pouvoir faire aussi bien et mieux qu'eux pourveu qu'on nous fournisse du verre qui soit comme nous le souhaitons. (24)

Therefore, there is all the more reason to be somewhat skeptical about the supposed superiority of Huygens' telescopes between 1655 and 1660.

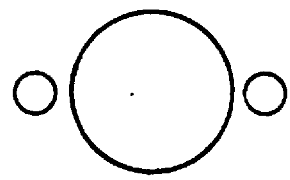
The problem can be approached from a different direction. In the above discussion we have neglected for the time being the role of the observer. An investigation of what was in fact seen by Huygens and Divini through their telescopes sheds further light on the problem. As may already have become apparent from the above quoted passage by Hevelius, the fact that Huygens discovered a satellite of Saturn does not necessarily mean that his telescopes were superior to all other telescopes. Hevelius'claims to have seen this body for a number of years before 1655 without realising that it was a satellite rather than a fixed star, and there is no reason to doubt his word. It had likewise been noticed in England around 1655 by Christopher Wren and Sir Paul Neile, but these men too held it to be a fixed star. (25) It seems thus that any excellent telescope of that time could have been used to discover this satellite and the reason why it was discovered by Huygens rather than by Wren or Hevelius or any one else, was that Huygens was looking for satellites, as he himself tells us in Systema Saturnium. (26) It is therefore probable that this satellite could also have been discovered with a Divini telescope. In fact, Divini claimed to have observed it between 30 June and 20 July 1657 (27), a claim that was dismissed by Huygens, because Divini indicated the wrong positions of the satellites for those particulars days. (28) Yet, on 18 July 1657 Nicolas Zucchi wrote to Riccioli from Rome:

> Saturn continues to be seen with his larger star separated from the two smaller ones. But with more powerful telescopes there seems to be observed [videtur observari] near him a star very much smaller than these, which moves above and below and to the right and left [of Saturn] and sometimes vanishes. But in order to establish whether it is perhaps various stars of the Firmament which happen to align with Saturn in those positions, great diligence and perseverance in observing is necessary. (29)

Thus, the satellite was observed in Rome at that time and there is no reason to deny Divini's claim.

The difference between the appearances seen by Huygens and those seen by Divini and all other observers, just before and after the 'solitary' appearance of 1656, is rather more difficult to deal with. Near its solitary appearances Saturn had always been seen 'tri-spherical', as in fig. 2. This was due to the fact that when the inclination of the ring was very small with respect to the Earth, as in fig. 6, the ring was so narrow near A and B that it could not be discerned with the poor telescopes made before the 1650's. Therefore the part of the ring near C appeared to be detached from the main body of Saturn and was rounded off because of the defects of these telescopes, just as the horns of Venus were foreshortened. In this form Saturn had been seen by Galileo in 1610 and again after the solitary appearance of 1612, and by Gassendi, Bouillau, Riccioli and Hevelius before and after the solitary appearance of 1642. Because of this appearance, it was often thought that the appearances of Saturn were somehow caused by two satellites which moved with some complicated unknown motion about the central globe. The same appearance was again seen before and after the solitary appearance of 1656 by every one except Huygens and Wren. Huygens saw the appearance shown in fig. 3. Wren, who was working on a theory about Saturn's appearances as well, was trying to determine whether or not the 'anses' were of constant length and he managed to affirm this in December 1657. (30) Obviously, in Wren's case, what the observer was looking for had great bearing on what he saw. Both Wren and Huygens were young men and perhaps less bound by authority in this matter than were the other astronomers. Riccioli, Bouillau, Gassendi (who died in 1655) and Hevelius belonged to an older generation. They had seen Saturn 'tri-spherical' on previous occasions and they were waiting for that appearance to return even before it occurred. An example from Gassendi's work will illustrate the role of expectation and authority in astronomical observations. Gassendi first looked at Saturn through a telescope in 1633. At that time he was not familiar with the 'handled' appearance, knowing only the 'tri-spherical' appearance. Thus, although in 1633 the ring was near its maximum inclination with respect to the Earth, Gassendi expected the planet to appear flanked by two small globes. The entry in his notebook reads:

> At about ten o'clock when I observed Saturn with the tube through some gaps in the clouds, he was rounded off like a silk-egg, or that from which the silk thread is drawn [i.e. a cocoon]... And indeed, on the preceding side of Saturn an ansa, or little appendage was seen rather confusedly, but on the side following [Saturn] the ansa was displayed entirely distinctly; and the whole was seen in this shape and magnitude.



> At some times the body of Saturn appeared round and not with rays embracing the anses on all sides and at other times it appeared rather confusedly, when the same anses had a symmetrical effusion all around them. (31)

Although the description is confused, one thing is clear: the figure drawn by Gassendi does not look like a 'silk-egg' in the least. If Gassendi could force himself to see a 'tri-spherical' appearance when the ring was near its most open position (admittedly with a very poor telescope) then it seems possible that he and his successors could see the planet 'tri-spherical' twenty years later, when the ring was nearly closed, with telescopes that were a lot better.

By far the most interesting aspect of this controversy is the problems of the shadows. First of all, it should be pointed out that the shadows were not easily seen. Wren wrote about the dark band across the central disc of Saturn:

> We observed the centre of Saturn to be girded with a certain zone, darker than the rest of the area of the disc and slightly narrower than Jupiter's belts ... And I should say that this zone consists of four spots, if I did not think myself deceived by the defects of my eyes or by my imagination (which nevertheless happens easily) in contemplating such a faint spectacle for too long, although even

> the belt itself (to say nothing of the spots) can be seen hardly or not at all, because of the dense mists of our island. (32)

But despite this difficulty, the shadow of the ring on the body (that is what it was, although no one realised this until sometime later) was seen by several observers besides Huygens. In Bologna it was observed by Grimaldi and Riccioli from 1655 until 1658. (33) Although Riccioli does not say anything about the telescopes he used, it is possible that the observations were made with a Divini telescope. Wren and Neile were first made aware of the existence of a dark band across the central disc of Saturn by William Balle in 1655. In 1659 John Wallis wrote to Huygens:

> The band [fascia], which you also claim to have seen, was first observed (as far as I know) several years ago by Mr. William Ball ... with a Roman telescope hardly more than twelve English feet long, (made by Eustachio Divini as it is said [ut aiunt]), in the year 1655: ... (34)

Thus, not only did Ball observe this shadow, he apparently did so with a Divini telescope of about 12 feet long! But were there Divini telescopes in England at this time? Divini himself tells us that Sir Kenelm Digby took six of his telescopes with him when he left Italy, and that Thomas Paggi also took one, as mentioned above. If Wallis' statement is correct as to the maker of this telescope, then by 1655 (and probably somewhat earlier, as communications between Italy and England were very slow) Divini made telescopes with which this shadow could be discerned. Yet, in 1660 he (or Fabri) wrote in Brevis Annotatio on the subject of this band: 'But this is also a pure fiction contrived by the author to prop up his ring.' (35)

Now Divini was at least partially correct in this opinion. From the beginning Huygens had conceived a thick ring rather than a very thin structure as Wren had postulated. Huygens considered a very thin ring, but had rejected it. (36) He firmly believed that the dark band which he saw on Saturn's disc in 1655 and 1656 was the dark outside edge of a thick ring. This edge was dark because it was either covered with a material that did not reflect light or was so smooth that it did not scatter light at all, so that reflected sunlight reached the eye of the observer from only one point on this edge. The editors of Huygens' Oeuvres Complètes have shown that in four of the figures in Systema Saturnium Huygens shows the dark band adjacent to the exterior edge of the ring (see fig. 5) whereas the relative positions of the Earth, the Sun and Saturn on the dates of the observations presented in these figures dictate that if a shadow was visible on those dates, it should have been seen adjacent to the interior edge of the ring. (37) The explanation given by the editors is that Huygens saw the contrast between the bright ring and one of the obscure equatorial zones of Saturn. But this explanation is not very satisfying. These equatorial zones are much more vague than the shadow of the ring on the body, and they were never seen by Huygens. Yet we are to believe that he could see the contrast between the bright outside edge of the ring and such a subtle equatorial zone, while he did not see the contrast between the bright inside edge of the ring and the darker shadow. It is much more reasonable to suppose that, guided by his hypothesis, Huygens looked for the dark exterior edge of his thick ring and managed to see it, that is, he managed to see something that wasn't there. In view of Wren's description of the shadow, quoted above, this seems much the more reasonable explanation.

If then Huygens, who in all likelihood was a more talented observer than Divini, could see things that did not exist, isn't it even more likely that Divini did not see things that he could in fact have seen through his telescopes? On 27 August 1660 Pierre Guisony wrote to Huygens:

> Je vous envoie les 2. observations de Saturne, qui furent fáites a Florence (si je ne me trompe) le mois de máy dernier, qui confirment parfaitement vótre pensée: voiant dernierement Eustachio je ne manquay pas de luy en parler, sachant qu'il les avoit eües [veües?], il ne put s'empecher de me dire que la prevention [pretention?] de vótre cercle en avoit donné à imaginer à ces Messieurs, & qu'aveq des meilleures lunetes il n'oberva rien icy semblable; quoique je sáy que la personne aveq qui il regardoit, luy dit plusieurs fois de voir sur le cors du planete l'ombre que la portion du cercle ŷ devoit produire. (38)

Furthermore, in the package of letters sent to Huygens by Prince Leopold, containing the reports by the members of the Accademia del Cimento on the ring-hypothesis, was an observation which had not been made before. On the 20th of August 1660 the shadow of the body of Saturn on the ring had been observed in Florence with a telescope of 18 braccia (see fig. 7). (39) Now this telescope was one of those taken by Digby on his departure from Rome, and, unknown to Divini, given by Digby to Arch Duke Ferdinand II. (40) This telescope had thus been made at the latest in the early 1650's! Quite clearly, other observers could see more through Divini's telescopes than Divini himself could. Huygens did not observe this shadow until 1664. (41) Thus, if it can be argued that because Divini did not see the shadow of the ring on the body, while Huygens did, therefore his telescopes were inferior to those of Huygens, then it can be argued just as plausibly that since Huygens did not see the shadow of the body on the ring, which was seen through a Divini telescope, therefore his telescopes were inferior to those of Divini.

But it is precisely the purpose of this paper to point out that such arguments are not to be trusted. Telescopes did not represent the phenomena discussed here with decisive clarity. These men were stretching their telescopes to the very limit of their capabilities, and at this limit the observations are better described as opinions than as facts. The role of the observer becomes crucial at this limit and examples of imagined observations are numerous. We must also not forget that these telescopes suffered from all the inherent defects caused by the imperfect geometry of the lenses and by the poor quality of the glass. But also, these telescopes were extremely difficult to manage. Observation through a tube of some 25 feet in length (and much longer in subsequent years) which flexed and fluttered in the breeze, had to be raised and lowered by means of ropes and pulleys, and had, of course, no clock drive to track celestial objects, must have been a task that required great skill and determination. To judge the relative merits of such telescopes on the basis of allegations made in a polemic is not very fruitful.

NOTES TO APPENDIX C

[1]This sheet is reproduced in G. Govi, 'Della invenzione del micrometro per gli strumenti astronomici', Bullettino di Bibliografia et di Storia delle Scienze matematiche e fisiche, XX (1887), facing p. 614.

[2]Christiaan Huygens, Systema Saturnium, The Hague, 1659, p. 37. I quote here from the French translation in Oeuvres Complètes de Christiaan Huygens, The Hague, 1888-1950 (hereafter O.C.) XV: p. 278.

[3]Eustachio Divini, Brevis Annotatio in Systema Saturnium ... una cum Christiani Hugenii Responso, The Hague, 1660, O.C., XV: pp. 409-411.

[4]O.C., XV: p. 311.

[5]O.C., XV: p. 411.

[6]O.C., XV: p. 405

[7]Hevelius to Bouillau, 9 December 1659, Bibliotheque Nationale MSS. 'Collection Boulliau' XXV (FF 13043), ff. 89v-90r.

[8]Divini to Prince Leopold, 10 July 1660, Biblioteca Nazionale (Florence) MSS. Gal. 276, f. 33r.

[9]Ibid.

[10]Govi, op. cit.

[11]G.B. Riccioli, Almagestum Novum, Bologna, 1651, p. 204.

[12]Divini, op. cit., p. 413.

[13]B. de Monconys, Journal des Voyages de Monsieur de Monconys, 3 vols, Lyons, 1664-1666, I: p. 117.

[14]Divini, op. cit., p. 407.

[15]P. Gassendi, Opera Omnia, Lyons, 1658, IV: pp. 474, 480.

[16]Perhaps Thomas Page (1613-1681), a secretary of the Duke of Ormond; J. Venn and J.A. Venn, Alumni Cantabrigiensès, part i (1922-1927, III: p. 294.

[17]Reproduced here from Systema Saturnium, O.C., XV: p. 239.

[18]O.C., XV: p. 235.

[19]Huygens to Hevelius, 8 March 1656, O.C., I: p. 388: 'Mihi anno demum praecedente telescopiorum ars innotuit, neque proprias observationes antiquiores habeo.'

[20]Huygens, De Saturni Luna Observatio Nova, The Hague, 1656, O.C., CV: p. 173.

[21]Constantijn Huygens (brother) to Christiaan Huygens, 10 August 1663, O.C., IV: pp. 392-393.

[22]Monconys, op. cit., II: p, 145.

[23]Christiaan Huygens to Lodewijk Huygens, 3 May 1662, O.C., IV: p. 125.

[24]Christiaan Huygens to Constantijn Huygens (brother), 14 October 1667, O.C., VI: p. 155.

[25]Wallis to Huygens, 1 January 1659, O.C., II: p. 306.

[26]O.C., XV: pp. 233-235.

[27]Divini, op. cit., p. 413.

[28]Huygens, Brevis Assertio Systematis Saturnii, The Hague, 1660, O.C., XV: pp. 443-447.

[29]Riccioli, Astronomiae Reformatae, Bologna, 1665, p. 365.

[30]Wren to Neile, 11 October 1661, O.C., III: p. 417.

[31]Gassendi, op. cit., IV: p. 142 (observation of 19 June 1633).

[32]C. Wren, De Corpore Saturni, first published in O.C., III: pp. 419-424. I quote here from my translation of this tract in 'Christopher Wren's De Corpore Saturni', Notes and Records Roy. Soc. Lond., XXIII (1968), p. 224

[33]Riccioli, Astronomiae Reformatae, p. 360.

[34]Wallis to Huygens, op. cit., p. 305.

[35]Divini, op. cit., p. 417.

[36]Systema Saturnium, O.C., XV: p. 319.

[37]O.C., XV: pp. 202-203.

[38]Guisony to Huygens, 27 August 1660, O.C., III: pp. 116-117.

[39]Prince Leopold to Huygens, 4 October 1660, O.C., III: p. 151; Borelli to Prince Leopold, Ibid., pp. 157-158.

[40]Fabri to Magalotti, 14 October 1660, Biblioteca Nazionale (Florence) MSS. Gal. 283, f. 111r: '... gaudeo, Eustachianum illud ultimum placuisse; immo audio, longum illud 18 cubitos Seren.mi magni Ducis, ab eodem Eustachio fabricatum fuisse, ut a Riccio vestro accepi; illud Digbaeus Roma discedens secum asportarat; quomodo vero deinde in manus Serenis.mi venerit, nescimus.'

[41]O.C., XV: p. 79.

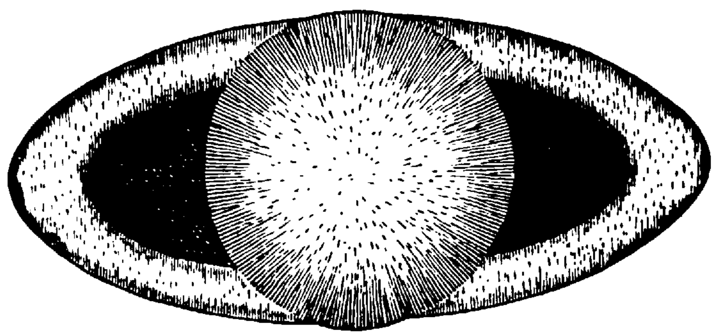


Fig. 1
Figure of Saturn published by Divini in 1649

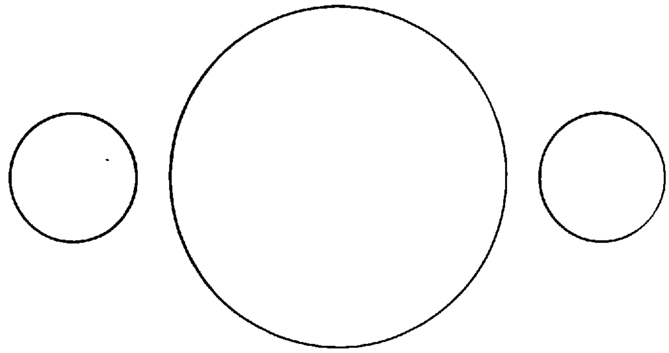


Fig. 2
'Tri-spherical' appearance of Saturn seen by Divini before and after the 'solitary' appearance of 1656

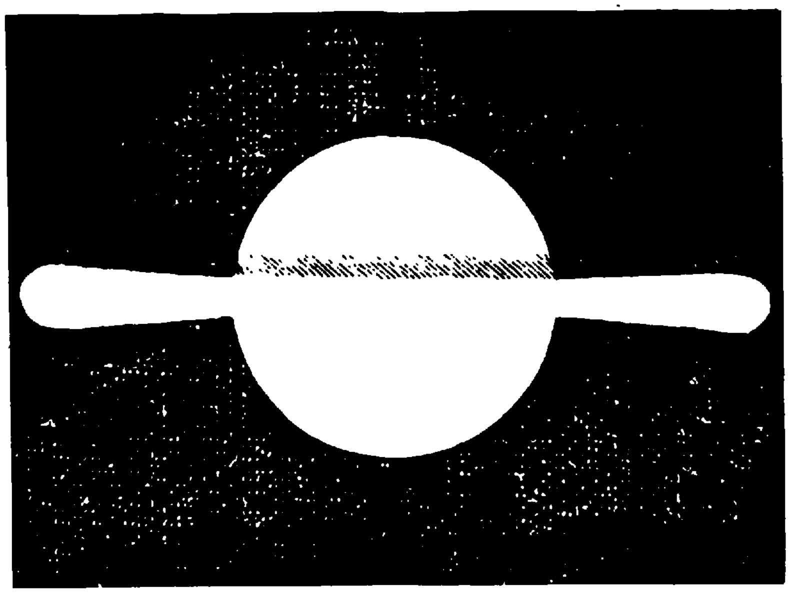


Fig. 3

Figure of Saturn seen by Huygens before the 'solitary'appearance of 1656

Fig. 4

Dark band on the disc of Saturn observed by Huygens in 1656

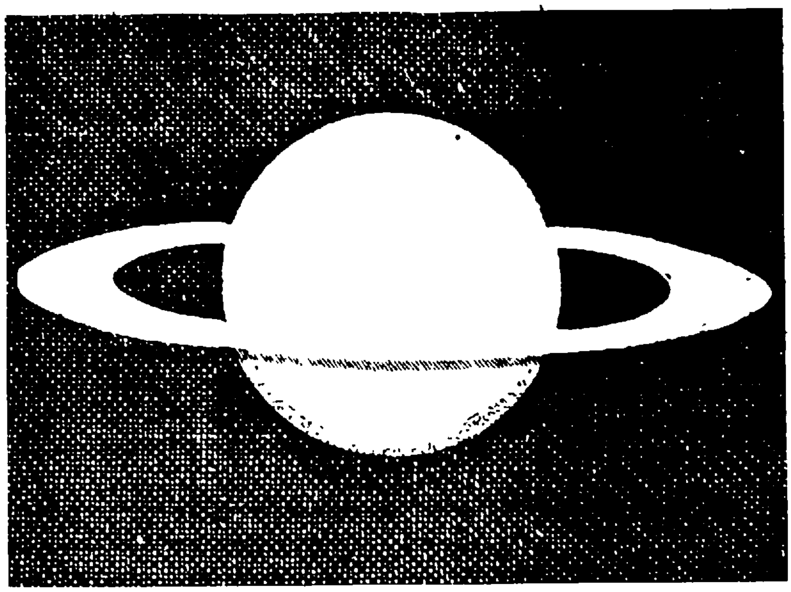


Fig. 5

Dark band adjacent to the exterior edge of the ring observed by Huygens in 1657

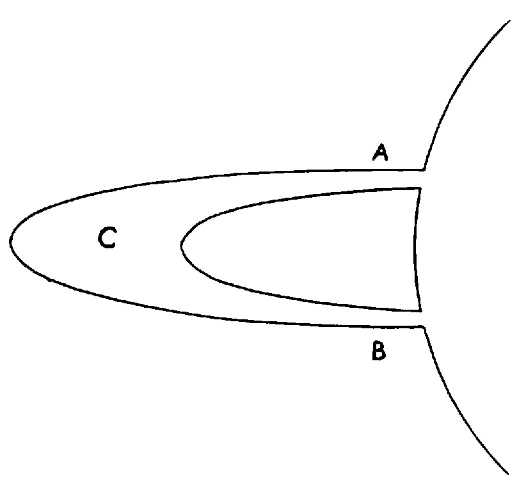


Fig. 6

Part of the ring when it is nearly in the same plane as they eye of the observer

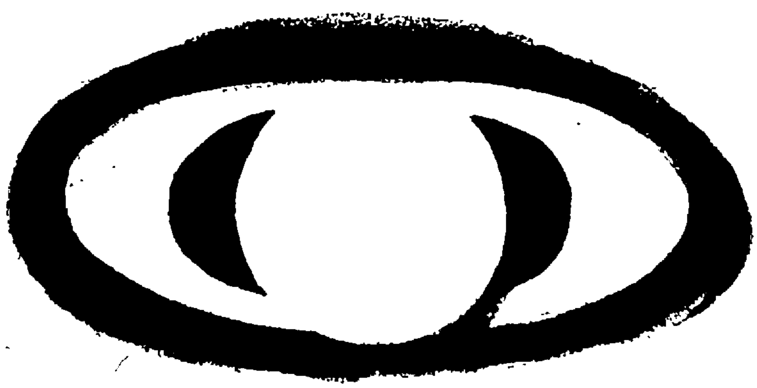


Fig. 7

Saturn as it was observed in Florence on
20 August 1660. Note the shadow
of the body on the ring

Appendix D

CHRISTOPHER WREN'S *DE CORPORE SATURNI*

BY

ALBERT VAN HELDEN

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CHRISTOPHER WREN'S *DE CORPORE SATURNI*

By Albert Van Helden

*Department of the History of Science and Technology,
Imperial College, London*

[*Plate 17*]

> We there discoursed . . . the Copernican Hypothesis, the Nature of Comets and new Stars, the Attendants on *Jupiter*, the Oval shape of *Saturn*, the Inequalities and Selenography of the *Moon*, the several Phases of *Venus* and *Mercury*, the Improvement of Telescopes, the grinding of Glasses for that purpose . . . (1)

THIS was written by John Wallis in 1678 in his *Defence of the Royal Society* and he was referring to the meetings held in London about 1645 by men interested in experimental philosophy. The 'Oval shape of Saturn' was a reference to what was then an important problem in astronomy: the explanation of the different appearances of Saturn. Among the men who were to become founding members of the Royal Society were a number who had an interest in this problem, John Wallis, Seth Ward, Dr Jonathan Goddard and Sir Paul Neile, who both kept operators at their houses for the grinding of lenses, John Wilkins, Laurence Rooke, William Balle, and Christopher Wren. Neile, Balle and Wren especially spent a great deal of time and effort on the problem in the 1650's, effort that resulted in Wren's hypothesis on Saturn, which is the subject of *De Corpore Saturni*.

The problem of Saturn went back to the year 1610. In July of that year, Galileo made the first telescopic observation of Saturn and saw not a single disc or globe as he expected, but a central globe flanked by two smaller ones. After the surprise had worn off and Galileo had become used to Saturn's tri-spherical appearance, he was treated to a second surprise when, in the autumn of 1612, he saw only the central globe. Had Saturn devoured his children? Galileo was, of course, looking at Saturn edgewise, or nearly edgewise. This phenomenon occurs twice during each period of Saturn when the Earth passes through the ring plane (2). Because of the ring's extreme thinness, even the largest telescopes of today are not powerful enough to show the ring when it is in this position, and Galileo's telescope which had shown the ring in 1610, when it was still far from this position, as two detached globes, was not nearly good enough to resolve the ring close to Saturn's 'equinoxes' (3).

As the ring slowly became more inclined with respect to the Earth, Galileo saw Saturn tri-spherical again, and, as the inclination approached its maximum, he received his third surprise: in August 1616 he saw, instead of two round detached globes, two '. . . half ellipses with two dark little triangles . . .' attached to the central globe on either side (4). The sketches made by Galileo of this appearance indicate that his telescopes were good enough to show the ring when it was at or near its most open position (5). But Galileo did not interpret what he saw as a ring. He died leaving the mystery of Saturn unsolved.

His immediate successors fared little better. Gassendi observed Saturn from 1633 until his death in 1655 and left a large collection of widely varying sketches and descriptions (6), but not a single idea or theory as to their explanation. It was, however, through the work of Gassendi and a few other astronomers such as Riccioli (7) and Hevelius (8) that by the middle of the 1650's the sequence of appearances as related to Saturn's orbital position was known—a necessary prerequisite for the formulation and testing of theories about Saturn.

By this time also, great improvements had been made in telescopes. Whereas Gassendi had used a Galilean telescope (given to him by Galileo) (9) —an instrument that in 1654 still showed the right 'ansula' to be smaller and closer to the main body than the left one (10)—the Keplerian telescope started displacing it in the 1640's, a process which had nearly been completed by the middle 1650's. Through the work of Francesco Fontana, Eustachio de Divinis, Hevelius, and later Campani, Huygens, Neile and Wren, this instrument had become vastly superior to Gassendi's telescope.

With the availability of sufficient data and improved telescopes, theories about the 'phases' of Saturn followed. Hevelius supposed the central body of Saturn to be ellipsoidal and to have two sickle-shaped appendages attached to it on either side. As Saturn turned in its epicycle or in its Keplerian orbit, according to taste, the different appearances were caused by this shape (11). Giovanni Battista Hodierna, a Sicilian astronomer, thought that Saturn was egg-shaped and had two dark spots, causing the various appearances by its rotation (12). Gilles Personne de Roberval, a French mathematician, believed that the appearances were caused by exhalations from a torrid zone (13). These theories left a great deal to be desired and within the next few years they were superseded.

When Christopher Wren came to Wadham College, Oxford, in 1649, he came to what was soon to be the most profitable place in England for one interested in astronomy. In 1648 John Wilkins became warden of Wadham. In 1649 Seth Ward and John Wallis became Savillian professors of astronomy and geometry respectively. Seth Ward also became a fellow commoner of Wadham College, as was Laurence Rooke from 1650 until his appointment as Gresham professor of astronomy. In 1651 Jonathan Goddard became warden of Merton College. There was thus an active community of astronomers, amateur and professional, in Oxford, a community in which Wren rapidly assumed an important position (14).

Observations were made from the '... slight observatory ...' built and equipped by Seth Ward in the tower of Wadham College (15). The telescopes of 6, 12 and 22 feet, mentioned by Wren in *De Corpore Saturni* (16) and by John Wallis in a letter to Christiaan Huygens in 1655 (17) were almost certainly the telescopes in this observatory. According to Wren, these telescopes were made by Sir Paul Neile, or rather by craftsmen under his supervision, at his house at White Waltham (18). Neile's early association with the Oxford astronomers is attested to by the reference to him in the preface of Ward's *In Ismaelis Bullialdi Astronomiae Philolaica Inquisitio Brevis* of 1653. It appears that Jonathan Goddard, who was also mentioned in the same preface, was replaced by Neile as provider of telescopes.

Both Wren and Wallis speak of a continuous series of observations of Saturn going back to 1649 (19). Since Wallis does not mention Wren in connexion with these observations, it is fair to assume that Wren was at best a contributor to this collection. Indeed, in *De Corpore Saturni*, Wren speaks of some figures, dating from the last four years, which he had depicted with the greatest care (20). This indicates that his active interest in Saturn started about 1654. In his letter to Neile of 1 October 1661, Wren speaks of wax models made by him and Neile in 'January 1655' in an effort to solve the problem of Saturn (21). Considering Wren's penchant for model-making and the usefulness of models in attempted solutions of this problem, this probably happened shortly after Wren took the problem under serious consideration. Therefore the start of Wren's work on Saturn most likely dates from the latter part of 1654. From Wren's statement about Balle's observations of the belt of Saturn in 1655, it appears that from the beginning Balle as well as Neile co-operated closely with Wren (22).

Wren's hypothesis was framed after Wren had become Gresham professor of astronomy. In the winter of 1656/7 Wren was at the house of Neile at White Waltham, probably observing Saturn with Neile's 35-foot telescope, when he got the clue that led him to his hypothesis:

> ... this kind of Saturne was long before hatched by your Influence at White Waltham, upon the Observation of December. 3 1657. when

first we had [an] apprehension that the Armes of ♄ kept their length, w^{ch} produced [this] Hypothesis . . . (23).

Wren first made two pasteboard models of his hypothesis and then a copper one (24), undoubtedly the one depicted in *De Corpore Saturni* (25), which was put on the 'Obeliske' erected at Gresham College to accommodate the 35-foot telescope given by Neile (26). This may be the same model that Huygens saw at the house of Sir Robert Moray during his visit to England in 1661 (27).

Wren discussed his hypothesis of Saturn in his astronomy lectures at Gresham College in the spring of 1658 and a number of the later Fellows of the Royal Society heard his discourses on this subject (28). Strangely enough, Wren thought that this was '. . . publication enough . . .' (29) and planned no treatise for publication until he was urged to put his thoughts on paper. He then planned an elaborate work, but was '. . . first . . . enjoyned [possibly by Rooke] to give that short & generall account of it . . .' (30). This short and general account, in Latin, was duly prepared by Wren. He called it *Christophori Wren Londini in Collegio Greshamensi Astronomiae professoris De Corpore Saturni ejusque Phasibus Hypothesis.* But it appears that he made no further effort to publish it. The tract is certainly not much beyond a first draft, as evidenced by the careless errors and by Wren's own testimony (31). The sole copy prepared by Wren was the only copy in existence until 1661 and it was apparently read by only one person, Laurence Rooke, the Gresham professor of Geometry (32). *De Corpore Saturni* would undoubtedly have remained unknown, had it not been for a coincidence.

In August 1661, Bernard Frenicle de Bessy, a French councillor and mathematician, wrote a letter to Sir Kenelm Digby, in which he set forth a theory of Saturn, which was very similar to Wren's theory (33). Digby read the letter at the meeting of the Royal Society of 4 September 1661, at which both Wren and Neile were present (34). Neile, who knew Wren's hypothesis, pointed out to the gathering that Wren had held a similar theory a few years ago. Wren, who was apparently reluctant to speak about his old theory, confirmed what Neile had said (35) and was requested to '. . . deliver a copy of his observations and hypothesis of Saturn to the amanuensis to be transmitted by Sir Kenelme Digby to Monsieur Frenicle' (36). Wren, against his will, sent the only existing copy of *De Corpore Saturni* to Sir Paul Neile, accompanied by the letter of 1 October 1661 (37). However, he did not give his consent to have copies made, and the copies sent to Digby, who had returned to Paris, and to Huygens in The Hague, were made without this consent (38). The latter copy was preserved in the Huygens collection in Leiden and printed in the *Oeuvres Complètes* (39). The copy in the Boyle

Papers (40) was probably also made at this time, as was the partial copy in the Hill Papers (41). The original tract found its way into the hands of William Jones, where it was discovered by John Ward (42), who made a copy of it (43). Another copy was made by Ted Stack in 1732 (44).

Wren's hypothesis supposes Saturn to be surrounded by what he calls a 'corona', whose outside and inside boundaries are two ellipses with the same minor axis, equal to the diameter of the planet. Thus it is a thin elliptical ring of variable width, which touches the planet at its narrowest point. (See figures on plate 17.) The planet and corona revolve about the major axis of the corona, once during each period of Saturn, thus presenting the corona edgewise twice and completely open twice during each period. By this configuration and motion, Wren could explain all the appearances of Saturn, observed up to 1658 except for some minor difficulties. It was therefore vastly superior to all previous theories about Saturn. Why then did it remain virtually unknown?

To answer this question we must go back a few years. In March 1655, Christiaan Huygens discovered a satellite of Saturn and in the winter of 1655-1656, he framed his ring-hypothesis (45). He disguised this hypothesis in an anagram which he included in his *De Saturni Luna Observatio Nova*, of March 1656, in which he announced the discovery of the satellite (46). For various reasons Huygens did not complete his *Systema Saturnium*, which gives a full explanation of the ring-hypothesis, until July 1659 (47). He did, however, reveal the hypothesis to some of his correspondents earlier. It was known in Paris shortly after Easter 1658 (48) but probably remained unknown to Wren until early in 1659, when Huygens informed Wallis (49). Thus, although Huygens' ring-hypothesis antedates Wren's hypothesis by almost two years, it remained unknown to Wren until after he had finished *De Corpore Saturni*. Upon hearing Huygens' hypothesis, Wren abandoned his own.

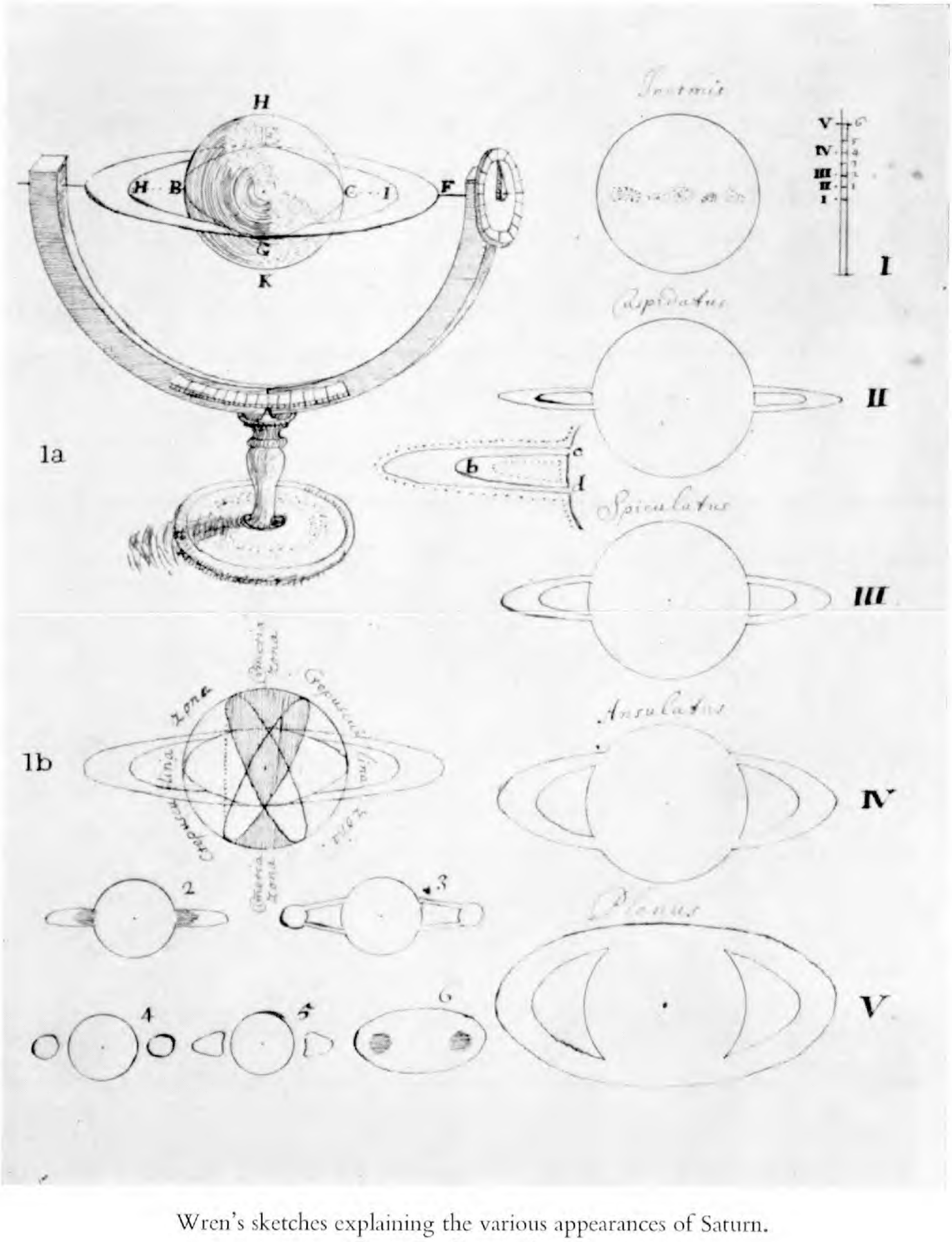
> . . . but when in a short while after, the Hypothesis of Hugenius was sent over in writing, I confesse I was so fond of the neatnesse of it, & the Natural Simplicity of the contrivance agreeing soe well with the physicall causes of the heavenly bodies, that I loved the Invention beyond my owne & though this be so much an equipollent with that of Hugenius, that I suppose future observations will never be able to determine which is the trewest . . . (50).

How good was Wren's hypothesis, and how 'equipollent' was it to Huygens' hypothesis? First we must correct an error made by Wren, no doubt due to the fact that *De Corpore Saturni* was little beyond a first draft. Wren states that the axis of rotation, the major axis of the elliptical corona, lies in the plane of the orbit. This is not correct. Wren knew perfectly well that the line of Saturn's 'anses' is inclined to the ecliptic and Saturn's orbital plane (51). With this correction, Wren's hypothesis explains the appearances of Saturn, as Wren knew them, satisfactorily. The major difficulty, which is immediately apparent to the modern reader of *De Corpore Saturni*, is the variable width of Wren's corona. But in 1658 this was not such an obvious difficulty. During the years of Wren's personal observations, 1654 to 1658, the ring-plane was in a very oblique position with respect to the Earth, passing through the Earth late in 1655 or early in 1656. Therefore, Wren had to rely on figures published by astronomers who had observed the open phase of the 1640's, and these figures are sufficiently conflicting to allow them to be interpreted to represent a corona of variable width (52). Thus, Wren's hypothesis could in 1658 reasonably be considered 'equipollent' to Huygens' hypothesis. Whether this was still true in 1661, when Wren made the statement, can be debated.

By 1661, the inclination of the ring was becoming great enough to suggest that Huygens' hypothesis was preferable to Wren's, and by the summer of 1662, the issue was settled (53). Furthermore, in the middle of the 1660's more subtle shadow effects, revealed by improved telescopes, showed that the ring passes in front of and behind the body of the planet, even in the most open position (54). Thus the planet could not possibly have the motion that Wren gave to it.

But Wren did not maintain his hypothesis until it had been proved wrong; he abandoned it in favour of Huygens' hypothesis as soon as he was informed about it. As his reason she cited the '... neatnesse ...' of the ring-hypothesis, and the fact that it agreed '... soe well with the physicall causes of the heavenly bodies ...' (55). In these aspects Huygens' hypothesis is clearly superior. The round ring is simpler than the elliptical corona and the lack of motion of the ring (neglecting the orbital motion that it shares with the planet) is simpler than the revolving or reciprocating motion of the corona. The different parts of the corona are attracted unequally by the planet and therefore the corona cannot be of a solid construction—an awkward problem. The ring, on the other hand, is balanced because all its parts are equally attracted. Furthermore, the fact that the ring-plane remains parallel to itself finds a perfect analogy in the equatorial plane of the Earth.

In one aspect Wren's hypothesis is more sophisticated than Huygens'. Huygens made his ring '... solid and permanent ...' (56) with appreciable thickness (57). This made it difficult to explain how the ring is invisible when viewed edgewise. Huygens therefore thought that the edge of the ring was



Wren's sketches explaining the various appearances of Saturn.
From *De Corpore Saturni.*

[*Facing page 219*

covered with some material which does not reflect light as well as does the surface of the rest of the ring (58). Wren, on the other hand, believed the corona to be so thin that it could be considered to be a mere surface. His parenthetic remark, that the corona was perhaps a few miles thick, is astonishingly close to modern estimates (59). But such a thin corona could not possibly support its own weight and therefore it could not be a solid structure. Thus Wren was of the opinion that it consisted of vaporous exhalations from a torrid zone. This structural explanation had been advanced by Roberval a few years earlier, in an hypothesis otherwise different from Wren's.

Wren's acceptance of Huygens' hypothesis, though he thought it equivalent to his own, is certainly a good example of what Summerson calls his '. . . disinterestedness, his passion for truth for its own sake and his perfect readiness to follow where it happened that he could not lead . . .' (60). In this case, it saved him from having to admit that his hypothesis was erroneous under pressure from new observations.

* * *

A printed text of *De Corpore Saturni* can be found in *Oeuvres Complètes de Christiaan Huygens* (vol. 3, pp. 419-425). I have compared this with the MSS. mentioned above (see notes 40, 41, 43, 44) without finding any significant discrepancies. The following translation is fairly literal and aims at reproducing Wren's style and terminology as closely as English permits (61).

The Hypothesis of Christopher Wren
Professor of Astronomy at Gresham College, London
Concerning the Body of Saturn and its Phases (62)

The incomparable Galileo, who was the first to direct a telescope to the sky—although the telescope had then only recently been invented and was not yet in all respects perfected—so overcame yielding nature, that all celestial mysteries were at once disclosed to him. And with the crystal sceptre he almost overcame not only the lonely multitude of the Milky Way, the crowd of nebulae, the earth-like Moon, horned Venus and the spotted Sun, but even triple-bodied Saturn. His successors are envious because they believe that there can scarcely be any new worlds left, about which they can boast, and believe that only to succeeding Lyncei is it granted to add to the discoveries of Galileo. And, indeed, it did not seem useless or inglorious (because it still remained to be done) to describe the lunar appearance more accurately, or to show the more than lunar fickleness of Saturn in a variety of figures, as the mathematicians improved the theory of dioptrics and craftsmen daily promoted the art of working big lenses. For which reason distinguished men of nations everywhere, even now, eagerly apply themselves to the production of longer telescopes. Saturn is proposed as the greatest test of skill. This is the target upon which they aim their artfully strengthened vision and they strive to bind this most deceitful star with the laws of a particular hypothesis. For Saturn alone stands apart from the pattern of the remaining celestial bodies, and shows so many discrepant phases, that hitherto it has been doubted whether it is a globe connected to two smaller globes or whether it is a spheroid provided with two conspicuous cavities or, if you wish, spots, or whether it represents a kind of vessel with handles on both sides, or finally, whether it is some other shape. For without motion and some rotation of the body, even ten different forms of the body would not suffice, although a single body, diversely rotated, could very well account for the observations worthy of consideration. On the other hand, it has not been possible thus far to devise one shape so flexible as to be in sufficient agreement with all observations taken indiscriminately. And certainly, because observers did not often use very long tubes and absolutely perfect lenses (of which there is need) and did not take good enough care to remove completely all superfluous light fringes from the aperture in the customary manner (63), or because they were unaccustomed to depict graphically on the spot just what they saw distinctly, it came about that they left us very disparate figures, so that if anyone chooses to construct an hypothesis which may agree accurately with all the sketches published lately by Galileo (64), Fontana (65), Gassendi (66), Riccioli (67), Hevelius (68), and others up till now, he wastes his time completely, for he impedes himself with so many contrary motions of the anses, that it is necessary either to give plastic wings or handles (according to taste) as attendants to a monstrous star, or to make it protean (69) and animate. Indeed, at certain times and intervals nothing will come out right and nothing agreeable to the uniform and beautiful harmony of natural motions is portrayed.

But those sketches are not therefore to be rejected as being altogether deceptive, because it cannot be that the telescope represents things that have no existence at all in nature. No one will deny that indeed things can appear otherwise than they really are, for the telescope has all the treachery of the naked eye and, in addition, those that generally arise from the imperfection of the instrument. But these are both things that cannot be concealed from the experienced observer and practised optician, so that he readily takes notice of them and substitutes genuine phenomena for erroneous ones, especially if he makes use of not one, but several telescopes at the same time.

And therefore, since it was granted to us to have the use of very well worked telescopes of 6, 12, 22 and even 35 feet long, together with a supply of all sorts of lenses of English manufacture, and to have at hand many observed appearances of Saturn in a continuous series from 1649 onward (some of which, from the last four years, we have depicted with the greatest care) we have not hesitated to unveil at last the hypothesis of Saturn which for a long time has been kept secret from learned men; especially lest the stars would seem to have granted to us the friendship of that very distinguished man, Sir Paul Neile, in vain. This is the man who, having hired the best workmen, ordered the making of these above mentioned celestial devices, and even greater ones, of 50 feet (70), in his own house, he himself supervising the work (by virtue of the remarkable strength of his judgment in mathematics). And not less sincerely does he rejoice to share his hospitality at the same place with his chosen astronomical friends; and I am also grateful for the gift of certain remarkable lenses and very many observations of Saturn.

Thus equipped, having made an attack on Saturn, I find it to be exactly spherical (71) and variegated with spots (although rather dark), and even to have poles, whose axis is positioned at right angles to the solar rays which lie in the plane of Saturn's orbit (72). Moreover, it may be supposed that a certain zone passes through the poles (like a colure) in the spherical surface of the star, in the plane of which colure is situated that elliptical corona, which, touching the globe at two points equidistant from the poles, represents the shape of handles. In the globe of Saturn, let *B* and *C* be the poles (73), the western and eastern ones respectively, and let *BECG* be that colure in whose plane the elliptical corona *DEFGHI* is circumscribed, touching the globe in opposite points, *E* and *G*, and being at a quadrant's distance from the poles. It is similar in shape to the space which is intercepted between two ellipses having the same centre and the same transverse axis; and the longer diameter of the corona is to the diameter of the globe as about 11 to 5, and the maximum width of the corona, *FI*, is to the same as 1 to 4. Its thickness is not sufficient to be seen in any way by the inhabitants of the Earth, and for this reason the corona may be taken as a mere surface. Accordingly we have concluded that the various appearances of the anses are brought about in this way.

Let the globe of Saturn, together with the corona, be rotated about the axis *DF* in consequence, once during one whole period of Saturn, which is, of course, twenty-nine and a half years, according to the rule that when Saturn is near aphelion in its orbit, the corona is at right angles to the plane of the orbit, so it is observed totally turned toward the sun and us. But then, with the turning of the globe, the corona is gradually closed (because we look at it obliquely) so that the anses become more and more narrow and finally are united. Then, with Saturn reaching about mean longitude, they are seen to suddenly disappear. At that time, the globe appears indeed solitary because both the sun and the eye are in the plane of the corona, which, because it has no sensible thickness—although it is perhaps a few miles thick—must escape the keenest of sight because of the great distance, thus leaving the globe utterly bare. However, after a few months, the cusps are seen to return (the better the telescope, the sooner this can be observed to happen) and then to broaden into anses, until near perihelion the corona again shines forth in full. And toward mean ascending longitude it disappears a second time and thence again grows continuously until the time when the planet has returned to aphelion, from where it started, at which point it becomes full as it was before. And thus, once in about every seven years, Saturn experiences all the variations of the anses, alternately increasing and decreasing, and in one period it becomes twice full and twice solitary, and goes through the remaining phases four times (74).

Thus, since the hypothesis is so simple and natural, depending solely on the rotation or inclination of the body, we can very easily project the appearance of Saturn at any given time orthographically. Therefore we have set out the five most distinctly different appearances with their own names; not, in fact, those which occur at equal intervals of time or anomaly (since the phases vary according to the ratio of sines, that is to say, more rapidly when the point of contact *G* is in the middle of the disc, more slowly when it is near the edges), but according to how many digits the point of contact, *E* or *G*, is removed on either side from the centre of Saturn, whose diameter we have divided into 12 digits.

1st. When the contact is at the centre, solitary Saturn is called *unarmed* [inermis].

2nd. When it is at the distance of one digit from the centre, either soon after the corona first reveals itself following the unarmed phase, or before that phase, when the corona is just disappearing from sight, Saturn is called *cusped* [cuspidatus].

3rd. When it is two digits distant and points now plainly cling to either side of the globe, and these points are seen to be cleft so that they are very similar to darts or iron arrow heads, Saturn is then called *dart-like* [spiculatus].

4th. When [the point of contact] has traversed four digits, and the darts are blunted and rounded out into a kind of handle, Saturn is called *handled* [ansulatus].

5th. Finally, when the point of contact is six digits from the centre and it is on the edge on either side, Saturn is called *encircled* [coronatus] or *full* [plenus].

With longer telescopes, of 20 or 30 feet, the planet will be seen precisely in the shapes that are shown in figures I, II, III, IV and V (75), except that the cusped phase is seen a little differently even with the most perfect instruments, from what is shown in the second figure. But that shape is seen in the telescope not because it is really like that, but because of diffuse light and weakness of vision, as, for instance, the new Moon spreads its image beyond the actual limits of the disc, so that the luminescence is seen to thrust out beyond the circumference of the dark part (as also happens to every white object placed against a black one). So, in the case of Saturn, the apparent shape gains a little around all its real edges, and makes the shape broader. Whence it comes about that (in figure II) the parts *bc* and *bd* come together more quickly than ought to happen at *b*, and the parts around *b* appear to be nearer to the body [of Saturn], because the narrow spaces made by the extremely acute ellipse *bcd* are wholly filled up by the neighbouring light of the cusps; so also the parts *c* and *d*, although luminous, escape from sight because of their thinness. For this reason, instead of appearing in the true cusped shape, Saturn is seen with its arms detached from its body (76). In the same way, if we look at Saturn with a modest telescope, when it is dart-like, it will be thought to be more like the shape in figure 3. Certainly, dullness of sight rounds off the light where the corona is broader and makes it appear brighter and spread into an isolated sphere, without defining it sharply. Where the corona is thinner, [the light] is reduced and thus it counterfeits that appearance of two lateral objects doubly connected with straps to the globe on either side (77). And the same rounding off of light is the reason why Saturn has sometimes been seen flanked by two lateral bodies either spherical or conical, when in reality it was cusped (78). If one employs a common telescope [to observe] Saturn when [it] is handled or full, it will not be adequate to distinguish those interior angles at the points where the corona is joined to the disc. Therefore, Saturn may be judged of an oval shape, stained with two black spots (79). Actually, we shall perhaps continue soon with a fuller account of this matter, and a more detailed examination of all figures and observations (80).

Perhaps the appearance of the belt on Saturn may substantiate this turning of the body (which we postulated in our hypothesis). Almost three years ago, the illustrious Mr Balle first saw this belt, and showed it to us at once (81). We observed the centre of Saturn to be girded with a certain zone, darker than the rest of the area of the disc and slightly narrower than Jupiter's belts (82). Saturn was then in the unarmed phase. Afterwards, with the planet cusped, the belt was seen to descend a little toward the northern quarter. Furthermore, the belt is none other than the colure of the globe which the corona touches, marked with a certain series of spots and conspicuous like a zone. And I should say that that zone consists of four spots, if I did not think myself deceived by the defect of my eyes or by my imagination (which nevertheless does happen easily) in contemplating such a faint spectacle for too long, although even the belt itself (to say nothing of the spots) can be seen hardly, or not at all, because of the dense mists of our island (83).

But besides, for the same reason, it is obvious that the hypothesis of the very renowned Hevelius (which was elegantly forged from the observations of Gassendi (84)), which supposes that the star is revolved about the line *HI* (85), at right angles to the orbit, does not agree well enough with observations. For the length of the corona remains the same to our view; it only becomes broader or narrower in the vertical [direction] by the spreading out of the anses. And the cusps don't separate gradually, but all at once, through lack of light, which could not happen unless the position of the axis is lengthwise. And he does not discuss the inclination of Saturn more happily (86). Our observations of the last years indeed contest [this discussion]. We think that the few years since the introduction of the new method of observing, during which we have watched zealously in order to establish this matter more certainly, are not yet enough to determine the period and limits of inclination correctly. For, if conjectures (perhaps not unfitting ones) may be employed, the axis *DF* may be supposed to reciprocate, within fixed limits not exceeding half a straight angle (87), according to the ratio of sines (that is to say, more slowly toward the ends, more rapidly in the middle). Then it is perhaps according to this rule that Saturn is always seen unarmed at the limits. Furthermore, whether these things are so by necessity or whether the maximum inclination is variable or not, our successors will investigate (88). We can learn from the decreasing (89) phases in the next four years, what the nature of the spots of the other hemisphere may be, or, if the disc shows little variation (which I suspect) it may be said that the corona goes through revolutions detached from the less mobile planet.

It remains for us to fix a definite period for this motion. But there is more to be said on this subject than is relevant to the present purpose. We don't advance a complete treatise on the appearance of Saturn, but we put forth a little dissertation to stake a preliminary claim. For which reason it will suffice to have indicated that the four places in which the cardinal phases, full and unarmed, happen, are not seen to coincide exactly with aphelion and perihelion and mean longitudes of Saturn. In the same way, too, the aphelion of the Earth is not repeated at the same time and the same place as the solstice. The proof of this is that Saturn appeared unarmed near the end of 1655, but nevertheless showed its cusps very plainly after the heliacal setting, but was not able to reach mean longitude before April 1657, at which time it appeared pointed. Having compared the last phases that preceded the solitary appearance with the first phases following it, we can say that the exact unarmed phase occurred about mid-February 1656. Accordingly, let this be the epoch whence the anomaly of the phases, which otherwise does not differ from the anomaly of the orbit of Saturn, takes its beginning. I leave tables and a method of predicting phases, however, for a more elaborate treatise, waiting in the mean time for better observations.

As regards the companion or moon of Saturn, which has very often been observed by me, I leave it completely to the most illustrious and most ingenious inventor, Christiaan Huygens, except that I shall add that its period of 13 days neglecting fractions (90) (as far as it could be observed up till now) has been confirmed by the observations of the illustrious Mr Balle as well as ours.

How the nature of this wonderful world of Saturn may be constituted, is difficult to guess at. No doubt the spots (almost like those which we have seen in Mars) reveal that the globe itself is opaque, but to believe that the anses are made of solid matter, like vast arches built on the globe, exceeds credibility; especially since they have no thickness by which such a great mass, many times exceeding the Earth's diameter in height, could be sustained. What then? Is the corona merely an appearance like the halo or the rainbow? But this is ruled out by the varying appearances, which variation is nevertheless linked to the motions of the star. Lastly, is it a fluid? Nothing is more likely, and I hardly know if anything more suitable can easily present itself; for since the belt follows the motion of the anses, what is rather to be said than that only this spotted zone emits vapours, the rest of the globe being miserably barren? From which it follows that the globe is not totally surrounded by an atmosphere but only by a vaporous corona, which, like a cloud, drinks in the splendour of the sun, and in turn gives back a visible glimmering brilliance. Therefore (if the harsh star, so far removed from the shining focus of the universe can give out any vital breath), indeed the inhabitants of Saturn have a very delightful spectacle of the corona. The corona illuminates even the hemisphere removed from the sun and from the other planets with a dusky light and warms it perpetually except for two unfortunate small areas at the edges around *H* and *K*, which nevertheless delight in the light of the sun alternately (91). Thus, while [the inhabitants] are enclosed in darkness for 15 years because of the sluggish rotation of the globe, the absence of light is somewhat compensated for, while intervals of time are marked out by Saturn's moon, a star that is very swift and rarely suffers eclipses.

The figure shown in the first position can represent a model of Saturn. For the planet, made of copper, which rotates about the axis *DF*, is supported by a semicircle connected to the movable pedestal, by which it is adjusted to the proper inclination as shown by a little tooth projecting from the pedestal and a scale, unequally divided in the manner indicated above, applied to [the semicircle]. To the semicircle, moreover, is fastened a circle divided into degrees of anomaly and, finally, to the axis is attached a pointer, which, when it is set to a suitable reading, so adjusts the globe that it represents the true phases of Saturn when regarded from a distance. Or, if it is turned toward the sun and the shadow of the instruments is caught upon paper, there is projected graphically a correct image of Saturn at a given time (92).

NOTES

(1) Douglas McKie, 'The Origins and Foundations of the Royal Society of London', *Notes and Records, Roy. Soc. Lond.* **15**, 12 (1960).

(2) In the seventeenth century the ring was also invisible when the ring-plane passed through the sun or between the Earth and the sun. However, modern telescopes do show the ring in these positions.

(3) The story of Galileo's observations of Saturn can be read in A. Favaro, 'Intorno alla Apparenza di Saturno osservata da Galileo Galilei nell'Agosto dell'Anno 1616', *Atti R. Ist. Veneto Sci.* (parte secunda), **60**, 415-432 (1900-1901).

(4) Galileo to Cesi, August 1616, *Opere di Galileo Galilei* (hereafter G.G.), **12**, p. 276.

(5) G.G. **6**, p. 361; **12**, p. 276, n. 1; **13**, p. 13.

(6) Gassendi, *Opera Omnia*, Lyon, 1658, **4**, passim.

(7) G. B. Riccioli, *Almagestum Novum*, Bononiae, 1651, pp. 487-488, 723-724.

(8) J. Hevelius, *Selenographia*, Danzig, 1647, pp. 41-44; *De Nativa Saturni Facie*, Danzig, 1656.

(9) Peiresc to Galileo, 17 April 1635, G.G. **16**, p. 28; Peiresc and Gassendi to Galileo, 24 February 1637, G.G. **16**, p. 34.

(10) Gassendi, *op. cit.* **4**, p. 479.

(11) J. Hevelius, *De Nativa Saturni Facie*, figs. opposite p. 12 and p. 30.

(12) G. B. Hodierna, *Protei Coelestis Vertigines seu Saturni Systema*, Panormi, 1657.

(13) Roberval to Huygens, 4 August 1656, *Oeuvres Complètes de Christiaan Huygens* (hereafter O.C.), The Hague, 1888-1950, **2**, pp. 474-475.

(14) R. T. Gunther, *Early Science at Oxford*, Oxford, 1937, **11**, pp. 90, 232, 263, 265; see also the biographical articles on the various men involved in *Notes and Records, Roy. Soc. Lond.* **15** (1960).
(15) Ward to Isham, 27 February 1652, *Notes and Records, Roy. Soc. Lond.* **7**, 70 (1950).
(16) p. 221.
(17) Wallis to Huygens, 22 March 1656, *O.C.* **1**, pp. 396–397.
(18) p. 221.
(19) p. 221, and Wallis to Huygens, 1 July 1655, *O.C.* **1**, pp. 235–238.
(20) p. 221.
(21) Wren to Neile, 1 October 1661, C. A. Ronan and Sir Harold Hartley, 'Sir Paul Neile FRS (1613–1686)', *Notes and Records, Roy. Soc. Lond.* **15**, 163 (1960); this article (pp. 159–165) describes Neile's role in the observations of Saturn.
(22) Balle's observations of Saturn are discussed in A. Armitage, 'William Ball, FRS (1627–1690)', *Notes and Records, Roy. Soc. Lond.* **15**, 167–172 (1960).
(23) Wren to Neile, *op. cit.* p. 163.
(24) *Ibid.* p. 163.
(25) fig. 1, p. 28 below.
(26) Wren to Neile, *op. cit.* p. 163.
(27) Huygens to Moray, 24 June 1661, *O.C.* **3**, p. 283.
(28) Wren to Neile, *op. cit.* p. 164.
(29) *Ibid.* p. 164.
(30) *Ibid.* p. 163.
(31) *Ibid.* p. 164.
(32) *Ibid.* p. 164.
(33) Frenicle to Digby, 31 August 1661, *O.C.* **3**, pp. 337–339.
(34) Moray to Huygens, 16 September 1661, *O.C.* **3**, pp. 321–322; 19 October 1661, *O.C.* **3**, pp. 368–369; 23 December 1661, *O.C.* **3**, pp. 425–426.
(35) *Ibid.* p. 368.
(36) T. Birch, *History of the Royal Society*, London, 1756, **1**, p. 43.
(37) Wren to Neile, *op. cit.* pp. 162–164.
(38) *O.C.* **3**, pp. 368, 425.
(39) *Ibid.* **3**, pp. 419–424.
(40) Royal Society MSS. Boyle Papers, **20**, pp. 19–31.
(41) 'A. Hill Philosophical Papers', British Museum, MS. Sloane 2903, ff. 102–103.
(42) C. Wren, Jr. to Ward, 3 February 1741 and 8 April 1742, British Museum. MS. Add. 6209, ff. 161–162.
(43) 'Miscellaneous Collections relating to Gresham College, Volume the first by I. Ward', British Museum, MS. Add. 6193, ff. 83–95.
(44) British Museum, MS. Sloane 243, ff. 144–150.
(45) *O.C.* **15**, p. 181.
(46) *Ibid.* **15**, pp. 172–177.
(47) *Ibid.* **15**, pp. 218–219.
(48) Chapelain to Huygens, 10 May 1658, *O.C.* **2**, pp. 173–176.
(49) Huygens to Wallis, 31 January 1659, *O.C.* **2**, p. 330.
(50) Wren to Neile, *op. cit.* p. 163.
(51) The inclination of the ring-plane to the orbital plane of Saturn is 26·7°; to the ecliptic it is 28·1°; A. F. O'D. Alexander, *The Planet Saturn*, London, 1962, p. 103.

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(52) F. Fontana, *Novae Coelestium, Terrestriumque Rerum Observationes*, Naples, 1646, pp. 131–142; Riccioli, *loc. cit.*; Hevelius, *Selenographia, loc. cit.*; *O.C.* **15**, figure facing the last page; Gassendi, *op. cit.* **4**, passim.

(53) Huygens to Moray, 9 June 1662, *O.C.* **4**, p. 151.

(54) G. Campani, *Ragguaglio di due nuove osservazioni*, Rome, 1664, fig. opposite p. 20; A. Auzout, *Lettre . . . sur le Ragguaglio . . . da . . . Campani*, Paris, 1665, 6–9; R. Hooke, *Phil. Trans.* **1**, 246–247 and fig. opposite p. 240 (1666).

(55) Wren to Neile, *op. cit.* p. 163.

(56) *O.C.* **15**, p. 299.

(57) *Ibid.*; note also that Huygens deliberately rejected a very thin ring: *O.C.* **15**, 318.

(58) *O.C.* **15**, p. 320.

(59) Alexander, *op. cit.* pp. 319–320; 359.

(60) J. Summerson, *Sir Christopher Wren*, London, 1953, p. 30.

(61) I wish to thank the staff of the library of the Royal Society for their help in this project and Professor A. Rupert Hall and Dr Marie Boas Hall for their help with the translation and for their many helpful comments.

(62) '*Christophori Wren Londini in Collegio Greshamensi Astronomiae professoris de Corpore Saturni ejusque Phasibus Hypothesis.*'

(63) These imperfections of telescopes indeed led to some very disparate figures in the case of Gassendi, *op. cit.* passim.

(64) Galileo, *Il Saggiatore*, Rome, 1623, see S. Drake and C. D. O'Malley, *The Controversy on the Comets of 1618*, Philadelphia, 1960, p. 324.

(65) Fontana, *loc. cit.*

(66) Gassendi, *loc. cit.*

(67) Riccioli, *loc. cit.*

(68) Hevelius, *Selenographia*, pp. 41–44; *De Nativa Saturni Facie, loc. cit.*

(69) In his inaugural lecture as Gresham professor of astronomy, Wren called Saturn '. . . a very Proteus . . .', *Parentalia*, p. 205.

(70) In 1664 Neile gave a 50-foot telescope to the Royal Society. It is possible that Wren added the mention of this instrument in 1661 before sending *De Corpore Saturni* to Neile.

(71) This is a reference to Hevelius's opinion that the body of Saturn was ellipsoid, presenting an *ellipse* flanked by two 'anses' in the open position and a single *circle* in the 'monospherical' position, see *De Nativa Saturni Facie*, fig. opposite p. 4. Actual observations and measurements of the oblateness of Jupiter and Saturn did not occur until after 1691, when Cassini noticed the oval shape of Jupiter, R. Grant, *History of Physical Astronomy*, London, 1966, p. 245.

(72) See fig. 1a, plate 17.

(73) Actually, Wren should have stated that the axis of rotation is inclined to the ecliptic, see p. 218 and n. 51 above.

(74) Because of the excentricity of Saturn's orbit, the 'closed' phases happen at intervals of about 15¾ and 13¾ years, Alexander, *op. cit.* p. 103.

(75) Plate 17.

(76) See figs. 4 and 5, plate 17.

(77) See fig. 3, plate 17.

(78) See figs. 4 and 5, plate 17.

(79) See fig. 6, plate 17; note also that it will therefore be impossible to distinguish the decreasing width of the corona near the body.

(80) p. 216.

(81) See A. Armitage, *op. cit.* p. 167.

(82) This is of course the shadow of the ring on the body of the planet.

(83) Huygens had seen the belt in March 1655, O.C. **15**, 238-239.

(84) Hevelius, *De Nativa Saturni Facie.*

(85) Read *HK*.

(86) Hevelius thought that the line of the 'anses' was roughly parallel to the ecliptic.

(87) '. . . semissem anguli recti . . .' Wren evidently means a reciprocating or rocking motion through 180°.

(88) Saturn's axis of rotation, which coincides with the axis of the ring, has a precessional movement with a period of more than 412,000 years, but the inclination of the ring does not vary appreciably, Alexander, *op. cit.* p. 103.

(89) Wren probably meant increasing rather than decreasing.

(90) Read 16 days; in the copy of *De Corpore Saturni* in O.C. this period is given correctly as 16 days neglecting fractions, in all other copies it is given as 13 days neglecting fractions.

(91) See fig. 1a, plate 17.

(92) Here Wren undoubtedly describes the procedures that he himself had used in checking his hypothesis.

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