—otherwise an error in the collimation of the former produces an appreciable error in the focus of the latter.

Our method was as follows: The collimator was, roughly, collimated by auto-collimation with a flat. A rapid prismatic camera was then focused for the violet on the roughly collimated slit. The prismatic camera was then turned on a star and a perfect focus was got for the violet by means of a number of test exposures. Finally, the prismatic camera was again lined up with the collimator on an iron arc, and the collimation of the collimator was varied until a perfect focus plate was obtained in the same wavelength as was used in the star photographs. We then had a perfectly collimated collimator; and since the collimator was a silver on glass mirror the collimation was correct for *all* wave-lengths. We then used it with an iron arc for focusing our spectrographs in the infra-red—which, even in the violet part of the spectrum, were so slow as to require over an hour for a single star exposure.

Our thanks are due to the members of the Lick Observatory expedition for their kindness and help; to Mr. Zering, of Camptonville, for his assistance in mounting our instruments and in the exposure of one of them; and lastly, to Mr. George Butler, of Baltimore, who helped us by making much of the mechanical parts of our apparatus.

The Discovery of the First Four Satellites of Jupiter.*

By J. H. JOHNSON, M.A., F.R.Hist.Soc.

Since its opening in the early years of the seventeenth century the controversy as to the validity of Simon Mayer's claim to have discovered the four chief satellites of Jupiter before or at least contemporaneously with Galileo has led to many expressions of opinion, most of them unfavourable to the German; but not until the beginning of the twentieth century does any attempt seem to have been made to settle the question in a scientific manner by a detailed examination of the evidence.

In March, 1610, Galileo published at Venice, in his *Sidereus Nuncius*, an account of his discovery, among other things, of the four great satellites of Jupiter. He gives in this a daily record of his observations, with diagrams to show the positions of the satellites at each observation and with estimates of their distances. His first observation was on January 7th, 1610, when he saw three

* Members of the B.A.A. may have noticed that in the current number of the *Handbook*, pp. 50, 51, the discovery of all Jupiter's great satellites is accredited to Galileo, and Simon Mayer's name is only mentioned in a footnote, although in an earlier number the discovery of Europa was assigned to him. For three centuries Mayer's claim to independent or even prior discovery of the satellites has been doubted, and he has been branded by more than one historian as an impostor; but our President, Major Levin, thought it possible that he was maligned, and he appealed to the Historical Section to investigate the point. The following paper, by our member, Mr. J. H. Johnson, is the response to this appeal.—M. A. EVERSHED, *Director, Historical Section.*

stars, two on the east of the planet and the other on the west. On the next evening all three were on the west. The ninth of January was cloudy, but on the tenth he could see only two, and similarly on the eleventh, though they were then further to the east. It was now that he came to the conclusion that they revolved around Jupiter. On the twelfth he once more saw three, and finally on the thirteenth he saw all four, confirming this on the fifteenth (the fourteenth was stormy).*

After the announcement of his discovery he continued to observe carefully the movements of the satellites, which he named the "pianeti Medicei," and to amass material for the determination of their periodic times and for calculating their future movements. In his *Discorso intorno alle cose che stanno in su l'acqua* (Florence, 1612) he says that he had completed this investigation in April, 1611, while in Rome, and he gives the times as follows: The first satellite (that nearest to Jupiter) makes a complete circuit in 1 day 18½ hours, nearly; the second, in 3 days 13⅓ hours; the third, in 7 days 4 hours; the fourth, in 16 days 18 hours.† At the same time he speaks of the necessity of continuing his labours in order, by means of more precise observations, to correct the errors in his tables of their movements.‡

In his third letter *Delle macchie del sole, of* 1st September, 1612 (published in 1613) he states that he has at length, after much trouble, succeeded in composing tables, and he gives diagrams, something like those now given in the *Nautical Almanac*, showing their exact positions for each night, sometimes for more than one hour of the same night, for ten weeks in the spring of 1613: he does not, however, publish tables from which their positions might be found for any other date.

There is of course no dispute as to the validity of these claims by Galileo. We are not obliged to rely only on his published account of his discoveries. In the National Edition of his works the Italian Government have published not only the text of his

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* *See* Grant's *History of Physical Astron.* for a fuller account based on the *Sidereus Nuncius*.

† Tables were caluclated by M. Berberich so that the true positions of Jupiter's satellites from 1610-1614 might be easily found for comparison: he also calculates their mean times of revolution about the planet for those years to have been:—

	day	hours	min.	sec.
I.	1	18	28	34
II.	3	13	17	42
III.	7	3	58	48
IV.	16	18	0	0

‡ Per simili precisioni non mi bastano le prime osservazioni, non solo per li brevi intervalli di tempi, ma perchè, non avendo io allora ritrovato modo di misurar con istrumento alcuno le distanze di luogo tra essi pianeti, notai tali interstizi con le semplici relazioni al diametro del corpo di Giove, prese, come diciamo, a occhio, le quali, benche non ammettano errore d'un minuto primo, non bastano però per la determinazione dell' esquisite grandezze delle sfere di esse stelle. Ma ora che ho trovato modo di prender tali misure senza errore anche di pochissimi secondi, continuerò l'osservazioni sino all' occultazion di Giove; le quali dovranno essere a bastanza per l'intera cognizione de' movimenti e delle grandezze de gli orbi di essi pianeti e di alcune altre conseguenze insieme.

books but also a complete photographic facsimile of the manuscript journal in which he set down his observations, and other facsimiles of important documents. The dispute concerns Mayer's claim to have discovered the same satellites.

Simon Mayer (Marius in its Latinised form) was born at Gunzenhausen in Bavaria in 1570. He studied astronomy under Tycho Brahe and Kepler and assisted in Tycho's observations for a little while before the latter's death in 1601, and then, after spending three years in Italy, he engaged in mathematical and astronomical work as court astronomer to Georg Frederick, Margrave of Brandenburg-Anspach. He is said to have first mentioned his discovery of four bodies revolving round Jupiter in his *Frankischer Kalendar oder Practica** of 1612, *but the full* description was given in his *Mundus Jovialis* which appeared at Nuremberg in February, 1614.

In the preface to this Mayer gives a detailed account of the circumstances which led to his discovery. He relates how at the autumn fair at Frankfurt in 1608 his friend and patron John Philip Fuchs met a Belgian who had a certain instrument "quo mediante remotissima quæque obiecta quasi proxima essent." They tried in vain to make one, but in the summer of 1609 they were able to obtain one from the Netherlands. From this time on, Mayer began to observe the heavens with this instrument, which he was sometimes allowed to take home with him, "præsertim circa finem Novembris, ubi pro more in meo observatorio considerabam astra. Tunc primum aspexi Jovem, qui versabatur in opposito Solis, et deprehendi stellulas exiguas, modo post, modo ante Jovem in linea recta cum Jove. Primum ratus sum illas esse ex numero illarum fixarum, quæ alias absque instrumento hoc cerni nequeunt, quales in via lactea, pleiadibus, hyadibus, Orione, aliisque in locis a me deprehendebantur. Cum autem Jupiter tum esset retrogradus, et ego nihilominus hanc stellarum concomitantiam viderem per Decembrem, primum valde admiratus sum, post vero paulatim in hanc descendi opinionem, videlicet quod stellæ hæ circa Jovem ferrentur, prout quinque solares planetæ ☿, ♀ ♂ ♃ et ♄ circa solem circumaguntur, itaque cœpi annotare observationes, quarum prima fuit die 29 Decembris, quando tres eiusmodi stellæ in linea recta a Jove versus occasum cernebantur. Hoc tempore quod ingenue fateor, credebam saltem tres eiusmodi stellas esse quæ Joven comitentur, cum aliquoties tres ordine collocatas eiusmodi stellas prope Jovem viderim. Interim etiam mittebantur e Venetiis duo vitra egregie polita, convexum et concavum, a clarissimo et prudentissimo viro Domino Johanne Baptista Lenccio, qui e Belgio post factam pacem reversus Venetias concesserat, et cui instrumentum hoc notissimum fuerat. Hæc vitra tubo ligneo coaptata fuerunt et a prius nominato nobilissimo maximeque strenuo viro mihi tradita, ut quid in astris stellisque prope Jovem præstarent experirer. Ab hoc itaque tempore usque in 12 Januarii diligentius attendebam his Jovialibus

* *See* a letter by Mr. W. T. Lynn in Vol. 32 of the *Observatory;* I have not been able to see a copy of the almanac in question. J. F. Weidler, *Historia Astronomiæ*, also gives quite a full account of Mayer.

sideribus, et deprehendi aliquo modo quatuor eiusmodi corpora esse, quæ Jovem sua circuitione spectarent. Tandem circa finem Februarii et initium Martii de certo numero horum siderum omnino confirmatus sum. A decimo tertio Januarii usque in 8 Februarii fui Halæ Suævorum et instrumentum domi reliqui, veritus ne in itinere damnum aliquod acciperet. Postquam igitur domum redii, ad consuetas observationes me accommodavi, et ut exactius et diligentius sidera Jovialia observare possem, ex singulari affectione erga hæc studia mathematica sæpius citatus celeberrimus et nobilissimus vir mihi plenam instrumenti copiam fecit. Ex hoc itaque tempore usque in præsens cum hoc instrumento et aliis postmodum constructis observationes continuavi."*

Mayer protests that he does not desire at all to diminish the glory of Galileo's discovery, but merely to vindicate his own claim to have discovered them independently " sub ipsissimum fere tempus, vel aliquanto citius quo Galilæus in Italia ea primum vidit." He goes further and expresses his obligation to Galileo's observations published in the *Sidereus Nuncius,* by which gaps in his own series had been filled, although he does not find them quite exact except as regards the relative positions of the satellites.

It should be noticed here that the dates given by Mayer are all according to the old style, for the Gregorian calendar was not yet in use in Germany: we must add ten days to each before we compare them with those of Galileo. Thus he first noticed the stars in the early part of December (N.S.), and his first dated observation, following his conviction that they were moving round Jupiter, was on 1610 January 8 (N.S.), thus corresponding with Galileo's second observation. He realised the fact that there were four satellites on January 22 (N.S.). Thus according to his own account he noticed the existence of the satellites about a month before Galileo, he was nine days later than Galileo in detecting the fourth satellite, but he preceded him by two or three days in recognising the revolution of the other three.

The first part of the *Mundus Jovialis* opens with an attempt, which with the knowledge at his command was not very successful, to assess the size of the Jovian system. He calculates that the diameter of the orbit of the outermost satellite is 28,000 German miles. Then follows his determination of the periodic times of the four satellites. The fourth (farthest from Jupiter), for which he suggests the name Saturnus Jovialis, makes a complete circuit in 16 days 18 hours 9 min. 15 sec.; the third (Jupiter Jovialis) in 7 days 3 hours 56 min. 34 sec.; the second (Venus Jovialis) in 3 days 13 hours 18 min.; and the fourth (Mercurius Jovialis) in 1 day 18 hours 28½ min.† Finally he suggests as alternative names Io, Europa, Ganymede and Calisto, the last named being the outermost.

In the second part he discusses certain details of the appearance and movements of the satellites and gives an account of

* For translation, *see* Appendix.

† It will be noticed that, if we accept M. Berberich's calculations (*see* p. 2, note 1) as the standard, Mayer's times for satellites I and II are more exact than those given by Galileo: in comparison with the mean synodic periods given in the NAUT. ALM., 1931, Mayer's time for IV also is slightly more accurate than Galileo's.

his difficulties in satisfactorily solving some of the problems of observation and calculation. Cassini (quoted by Weidler) says, in speaking of Galileo's doubt whether Mayer had ever really seen the satellites at all, " on n'en sauroit neantmoins douter si on examine la methode dont il dit qu'il s'est servi pour les observer, qui apparemment ne seroit pas tombée dans la pensée d'une personne qui ne l'eût pratiquée; les difficultés qui se rencontroient dans la pratique de ces observations y étant fort bien representées."

In the third part he gives diagrams to explain the movements of the satellites, and tables, with examples, drawn from his own observations, for their use, from which the positions of the satellites at any given time might be calculated. In giving tables he undoubtedly has the priority over Galileo.

In his *Il Saggiatore,* published at Rome in 1623, Galileo makes a violent attack on Mayer. He not only accuses the latter of plagiarism but goes so far as to assert " ch'egli non solamente non osservò le dette stelle avanti di mè, mà non le vide, ne anco sicuramente due anni dopo. E dico di più che molto probabilmente si può affirmare ch'ei non l'ha osservate giamai." He controverts Mayer's theory that the orbits of the satellites are inclined to the ecliptic, and citing that writer's statement that he had never seen the satellites disposed in a straight line except when they were at their greatest distance from Jupiter, he states that for four whole months, from February to mid-June, 1611, the four satellites were always in a straight line in all positions. Yet the facsimile of Galileo's own manuscript journal now available shows that he had himself on several occasions during that period observed and noted them as not being in a straight line.

Thus the matter stood for nearly three hundred years, with the majority of writers inclining to accept Galileo's own version of the affair and branding Mayer as an " impudent pretender." In 1900 a committee consisting of J. A. C. Oudemans, J. C. Kapteyn and E. F. van de Sande Bakhuyzen, examining a memoir by J. Klug submitted to the Société Hollandaise des Sciences in opposition to Mayer's claim, were led by their examination of the evidence to an entirely opposite conclusion, and in 1903 Oudemans, in collaboration with J. Bosscha, editor of the *Archives Néerlandaises des Sciences exactes et naturelles,* published in that journal a paper entitled *Galilée et Marius* summarising the committee's conclusion. As Klug, however, printed his memoir, Oudemans and Bosscha decided to give a fuller account of their examination of the evidence. Oudemans having died in the meanwhile, the work was completed by Bosscha, and appeared under his name in 1907.*

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* *Archives Néerl. des sciences,* ser II, tome XII. The remainder of the present paper is largely a résumé of Bosscha's results, which seem to have had little effect on subsequent accounts. Mr. Lynn drew attention to this rehabilitation of Mayer in a letter to the *Observatory* in 1909, Vol. 32. The *Enc. Brit.*, 11th edn., made no mention of Mayer in its text, although in its bibliography to the article on Galileo it cited the paper *Galilée et Marius* of 1903. The new *Enc. Brit.* copies this.

The case against Mayer may be looked at from three points of view; first, that he was incapable of having made the observations himself and had merely copied those published by Galileo; second, that he had compiled his tables from the data given by Galileo; thirdly, that, even allowing that he had made the observations, he had antedated them in order to claim priority over Galileo.

The last charge is one which it is quite impossible either to prove or to disprove, for we have not, as in Galileo's case, any manuscript evidence to supplement Mayer's account in the preface to his *Mundus Jovialis*. Our attitude will therefore depend entirely upon our estimate of Mayer's own trustworthiness. We are, however, far more likely to give credit to his own assertions in this respect if we can be satisfied that his observations were his own, that his tables were his own, and that he was himself a capable observer. To prove this was the aim of Oudemans and Bosscha.

The writings of Galileo from which Mayer might, before February, 1614, have borrowed information were the *Sidereus Nuncius* of 1610, the *Discorso* of 1612, giving the periodic times of the satellites, and the plates given in the third *Solar Letter* of 1613 showing positions of the satellites for the spring of that year.

We know now, since the publication of Galileo's own journal, that the value assigned by him to the diameter of Jupiter varied considerably at different times from four minutes of arc down to two minutes. He afterwards attained values of thirty to forty-six seconds, but they were not published. His measures of the distances of the satellites were obtained by estimating them in terms of the diameter of Jupiter and counting from the planet's edge, but he nowhere explained what this diameter was. At the time of the publication of the *Nuncius* he appears to have fixed upon a value of two minutes. Now Mayer tells us that his own estimate for the diameter of Jupiter was one minute. If, then, he had used this value for calculating from Galileo's data he could hardly have derived any very accurate information as to the radii of the orbits, their periodic times or their epochs.*

Galileo maintained that Mayer had copied the periods from him. But we have seen that Mayer's values are in some cases more accurate than those given by Galileo in 1612, and Klug's suggestion that Mayer may have calculated them from the plates given in the *Solar Letter* is disposed of by Bosscha, who by careful measurements from the original edition of these plates has calculated that the values which Mayer could have obtained from them would have been very much more inaccurate.

The differences between the tables of Marius and those of to-day come very largely from an error in the epochs, which seem to have been badly chosen by him, although we do not know which of his observations he took as a starting point. But his estimates of the satellites' diameters and the values assigned

* Galileo estimates the diameter of one of the satellites as 30″, or $\frac{1}{4}$ of Jupiter's diameter: Mayer estimates the diameter of Satellites I, II and IV as 1/12th that of Jupiter, the diameter of III as $\frac{1}{8}$ of Jupiter's.

by him to the radii of their orbits* agree more closely with modern measurements than do those of Galileo.

We have one piece of independent evidence as to Mayer's ability as an observer. There is in a letter by Odontius (J. K. Zahn) to Kepler a detailed account of an observation by Mayer of Jupiter on 1610 December 30 (not cited in the *Mundus Jovialis*) which shows that Mayer was able to determine the satellites' distances with more precision than is shown in Galileo's own observation at the same time, given in his journal. The authenticity of this observation has been verified by Bakhuyzen, who has identified a fixed star shown in Mayer's sketch as being in the same field. From this fact and from the superiority of Mayer's estimates of the diameters of Jupiter and of the satellites Bosscha considers it highly probable that Mayer's telescope was capable of better definition than those used by Galileo during these early observations. Even Galileo himself might have seen evidence in the *Mundus Jovialis* of independent observation by Mayer; for the latter describes the positions of the satellites as observed by him on 1613 February 7/17, and Galileo's own journal contains a sketch of his own observation on that night which agrees closely with it.

We now come to the question of Mayer's trustworthiness. First, it must be remembered that he cites as witness to the truth of his account his patron Fuchs, a man of undoubted eminence and credit. His account of the difficulties he encountered in his observations and calculations rings true, as Cassini remarked. Moreover, he never tries to claim undue accuracy. One circumstance which has been used to convict him of copying from Galileo really seems to redound to his credit. It is a remarkable coincidence that on 1610 January 8 neither Galileo nor Marius saw Satellite IV, although it was visible. But in fact, as Bosscha points out, Mayer's own tables made it visible on that occasion, and he might easily have scored a point by claiming to have seen it. The explanation seems to be that on that night Satellite IV, although visible, was at such a distance from Jupiter (more than ten minutes) that it would have been outside the field of their telescopes, and was moreover lost against the background of a considerable group of stars, which would easily prevent its being noticed unless it were already known to be there. I do not think that anyone reading without prejudice Mayer's own account of his work could fail to be impressed with the studied moderation of his claims and with the way in which he frequently acknowledges his indebtedness to Galileo's observations. If the man were really the "impudent pretender" he has been called, it seems strange that he should not have made a more thorough job of it.

Our Dutch friends seem to have clearly shown that Mayer was a competent observer and computer and that his tables, the first to be published, while not accurate in accordance with modern knowledge, were by no means the clumsy fabrications they have

* The most accurate estimates of Galileo for the radii of the orbits are, in semi-diameters of Jupiter:—I, 5·7; II, 9; III, 14; IV, 25. Mayer gives I, 6; II, 10; III, 16; IV, 26. Bessel's measures give I, 5·94; II, 9·46; III, 15·1; IV, 26·5.

been assumed to be. There seems a strong presumption that he was an honest man and that his claims were not exaggerated. If we admit them we must recognise that he was the first both to notice the existence of the satellites and to detect their revolution; but that Galileo was the first to observe their full number.

APPENDIX.

Translation of Mayer's account of his discovery.

" especially about the end of November, when as usual I watched the stars in my observatory. Then for the first time I looked at Jupiter, which was in opposition, and I noticed some small stars, now following, now preceding Jupiter, and in a straight line with it. At first I thought them to be some of those fixed stars such as cannot be seen without a telescope, and such as I had noticed in the Milky Way, the Pleiades, the Hyades, Orion and elsewhere. Since, however, Jupiter was then retrograde and I nevertheless saw these stars accompany him throughout December, I was at first greatly astonished, but gradually formed the opinion that they were moving round Jupiter just as the five solar planets are round the sun. I therefore began to set down my observations, of which the first was on the 29th December, when three stars of this kind were visible in a straight line to the west of Jupiter. I must freely confess that at this time I believed that there were only three of these stars accompanying Jupiter, since I had on several occasions seen three arranged in a row near him. Meanwhile there arrived from Venice two lenses excellently polished, convex and concave, from John Baptist Lenccius, who after the peace had returned from Belgium to Venice, and who was well acquainted with this instrument. These lenses were set in a wooden tube and were handed over to me by my patron in order that I might try what they would show in the stars near Jupiter. Accordingly from that time to 12th January I carefully watched these stars near Jupiter and I found that there were four of these bodies revolving round Jupiter. I was finally satisfied as to their number about the end of February or the beginning of March. From the 13th January to 8th February I was at Hall in Swabia, and left my telescope at home lest it should be damaged on the journey. After returning home I resumed my usual observations, and in order that I might observe these stars about Jupiter more exactly and diligently my patron gave me the full use of the instrument. From that time, therefore, down to the present, I have continued my observations with this instrument and with others constructed later."