schern vor, und eröffnete dabei eine Subscription auf diese Instrumente, die den Unterzeichnern nach der Ordnung ihrer Unterschrift abgeliefert werden sollten. Ich würde dieser Einrichtung zufolge noch einige Zeit auf ein dialytisches Fernrohr haben warten müssen, wenn nicht (wie Herr Baron *v. Jacquin* mir meldet) ein hoher Gönner in Wien, den er nicht nennt, die Güte gehabt hätte, mir seine frühere Nummer abzutreten, wofür ich hiemit meinen ergebensten Dank öffentlich abzustatten nicht ermangele. Auf diese Art bin ich schon seit ein paar Monaten in Besitz eines solchen Instruments gekommen, und glaube es nicht länger aufschieben zu dürfen wenigstens eine kurze Notiz darüber zu geben.

Mein Fernrohr hat, wenn das schwächste astronomische Ocular angeschraubt ist, nur eine Länge von 22¼ Par. Zoll. Die Oeffnung ist 25⅓ Par. Linien. Es hat 4 Vergrößserungen, zwei terrestrische, und zwei astronomische, welche mit einem Dynameter bestimmt folgende Werthe haben:

terrestrisches Ocular	Nr. 1.	53
	Nr. 2.	76
astronomisches Ocular	Nr. I.	60
	Nr. II.	86

Bei diesen Messungen ist nicht die Oeffnung des Objectivs als Dividend gebraucht, welches, wenn Blendung im Fernrohr ist, eine zu starke Vergrößserung geben würde, sondern das Objectiv mit einem ausgeschnittenen Quadrat von bekannten Dimensionen bedeckt, und mit dem Dynameter die Seite des kleinen dadurch auf dem Oculare gebildeten Quadrats gemessen.

Die Würkungen dieses kleinen Instruments sind aufserordentlich. Es ist schwer mehr Schärfe der Bilder zu finden. Zum Beweise seiner Praecision brauche ich nur anzuführen, dafs es ε Bootis scharf gesondert zeigt, und die Lichtstärke (wobei man die geringe Oeffnung nicht vergessen mufs), erhellt daraus, dafs geübte Augen (Herr Staatsrath *v. Struve* und ich) dadurch selbst den lichtschwachen Begleiter des Polaris erkennen. Der ebengenannte trefliche Astronom, der es bei mir sah, vereinigte sich mit mir in aufrichtiger Bewunderung der Leistungen des talentvollen Künstlers. Wir glauben beide kein Fernrohr von gleichen Dimensionen gesehen zu haben, das mit ihm verglichen werden könnte. Der Preis ist 140 fl. Wien. Währ.

S.

On the Rotation of Venus.

By the Reverend *T. J. Hussey*.

(Beschlufs.)

Such are *Bianchini's* observations nor doth it appear to me that there is much more conclusive evidence of any phaenomenon than of the rotation of Venus in 24 days and 8 hours. Thus stands the case; a practized astronomer of the very highest character associating other gentlemen with himself and employing the finest instruments then known is led to suppose from his own observations that Venus describes round her axis an arc of 15° daily: to satisfy himself on this head he and they again examine the planet under the most favourable circumstances and having placed the threads of a micrometer in contact with the edges of the spots of Venus found that after the lapse of 50 minutes the edges of the spots and the micrometer threads were still in contact, where had the planet revolved in 23 hours, however much the angle of 15° $\pm$ it would then have described might have been foreshortened it would still have been appreciable. A diagram having been made of Venus, and its accuracy ascertained by all the parties present, the observations were suspended for about two hours and a half, but again resumed for half an hour and upon the former diagram of Venus being compared by the same persons with the appearance the planet then offered the identity of the spots was immediately recognised, as also that their position was the same with regard to the threads of the micrometer. (I shall not enter into a discussion of the observations from which the exact period of rotation was inferred, they speak for themselves.) Now to compare results thus obtained with inferences drawn from observations made with the comparatively imperfect instruments in use 60 years before, observations distrusted by him who made them and disregarded by his contemporaries, measurements with a micrometer with estimations formed without one to compare the results thus deduced, is as discreditable to him who makes the comparison as insulting to the public to whom it is addressed.

Again the form of the spots of Venus traced by *Bianchini* much resembles what was seen by *Herschel* June 19. 1780 and engraven in the Phil. Trans. for 1793, while between either of these and what *Cassini* has given there is not the slightest resemblance. An inspection of the plates would prove this, and I will corroborate my opinion by the authority of the above mentioned *Melchior à Briga* mathematical professor at Florence. *Figurae macularum quas Cassinus exhibet longe diversae sunt ab iis, quas in globo à*

te descripto, praesul doctissime, ad momentum mihi ostendisti. Leit. p. 5.

But further in the space of 50 minutes on one occasion and thirty minutes on another during which the observations were in the very strictest sense of the word continuous, for either *Bianchini* himself or some one of his friends was constantly watching the planet (which was absolutely free from tremor) and delineating it, the micrometer was unable to detect any rotatory motion, whereas *J. Cassini* speaking of a spot said to be observed by his father relates *au lever du soleil cette tache étoit distante de la corne meridionale, de la quatrième partie du diamètre de Venus, et elle en parut éloignée de deux cinquièmes lorsque le soleil fut à la hauteur de* 4 *degrés où il se trouva* 24 *minutes après son lever. Ast. Vol.* 1. *p.* 515. The rapid motion asserted in the one case is utterly irreconcileable with the very slow motion observed in the other, and to point out which statement is to be preferred, would be superfluous and impertinent.

What then are the younger *Cassinis* proceedings? Having expressed himself in the highest terms of *Bianchini* whose authority he cannot controvert, then having misquoted him as to the direction of the pole of Venus and the hemisphere in which it was found, he presumes to reconcile the observations of *Bianchini* with those of his father that is to say, observations made with the most scrupulous exactness and of which all the minutiae are detailed, are to be reconciled with observations scarcely deserving the name, for in this light I must repeat they were looked upon by him who made them and by his contemporaries, made with inferior instruments and respecting which all is vague if not contradictory, for, be it remarked, if the rotation of Venus be deduced from the supposed rapid motion *Cassini* detected and not from a comparison of the observations made on successive days, the result is totally different and the time of rotation very much lefs. Where as from the first series of *Bianchinis* observations he deduced the direction of the pole of Venus, determined the time of the year when it would be visible from the earth, and actually saw the spot revolve around it at that determined time.

Je suppose avec lui, says *J. Cassini*, qu'à 5^h 45^m il a paru trois taches dans le disque de Venus, telles qu'elles sont marquées dans la figure pour le 26 Février, et que trois heures après, on en a vu aussi trois-à peu-pres de la même figure et dans la même situation sur ce disque. Mais il faut remarquer que selon lui, son observation a été interrompue depuis 6^h 15^m jusqu'à 8^h 40^m, par des édifices qui empéchoient de voir Venus, et que dans cet intervalle, qui est de près de 2^h 30^m, il n'a pû observer les taches, ni par conséquent le mouvement qu'elles ont pû avoir.

Comme dans l'hypothèse de la révolution de Venus en 23 heures les taches ont dû dans l'espace de 5 heures qui se sont écoulées entre la description des taches, avoir un mouvement de 47 degrés, comme M. *Bianchini* en convient; il se peut fort bien faire que par le mouvement apparent de ces taches, qui étoit alors du nord vers le sud, la tache *E* qui étoit dans la partie méridionale de Venus, se soit approchée de la corne inférieure où elle a cessé de paraître, pendant que la tache *F*, qui étoit au centre a pris sa place, et que celle qui étoit en *G*, dans la partie septentrionale, étant parvenue au centre en *F*, il ait reparu une nouvelle tache dans la partie septentrionale au point *G*; de sorte qu'à 8^h 45^m, temps de la seconde observation, on ait vû trois taches à peu-près au même endroit où étoient les précédentes, et de la même figure qu'à 5^h 45^m. Car la tache qui étoit au milieu, étant parvenue en *E*, a dû par la raison d'optique, diminuer de largeur, et paroître à peu-près semblable à celle dont elle a pris la place, pendant que par la même raison la tache *G*, qui étoit vers la partie méridionale, a augmenté de grandeur apparente en s'approchant du centre. Cela se peut voir aisément par l'inspection de la figure de M. *Bianchini*, où ayant tiré par le centre de chaque tache, des lignes perpendiculaires à la section de Venus jusqu'à la circonférence, l'on trouve qu'il y a entr'elles une arc d'environ 45 degrés, et que le milieu de la tache méridionale a dû être sorti de Venus dans l'espace de 3 heures, supposant sa révolution de 23 heures, pendant que la tache *F* a succédé à la tache *E*, et la tache *G* à la tache *F*. A l'égard de la nouvelle tache, que nous supposons être survenue à la place de la tache *G*, il y a tout lieu d'admettre cette supposition, si l'on considère les figures de M. *Bianchini*, où l'on voit qu'en divers jours, plusieurs taches de la même forme se sont succedées les unes aux autres, et qu'il y en a eu une qui a dû paroître dans le disque de Venus, immédiatement après la tache *G*, quoiqu'à une distance un peu plus grande que la révolution ne la demande.

L'observation du 26 Février de l'année 1726, ne prouve donc rien de décisif, comme l'a cru M. *Bianchini*, contre la révolution de Venus autour de son axe, qui avoit été trouvée de 22 heures. Astronomie Vol. 1. p. 519. 520.

A more disingenuous, sophistical statement was never palmed upon unsuspecting confidence. All the circumstances which determine the value of *Bianchini's* observations are carefully omitted and tant pis pour les faits when they differ from what is supposed necessary to vindicate the honour of *J. D. Cassini*. Not to insist upon more minute points, which will be obvious enough on a close examination, I would remark:

That the observations were continuous for 50 and 30 minutes during which time, as already mentioned, no motion of the spots could be detected by a micrometer, a fact which is incompatible with the supposition of such a translation of the spots as *J. Cassini* requires;

That, the several observers could not be mistaking in the appearance of the planet is clear from their having compared the drawing of her taken at one time with the spots exhibited $2^h\ 50^m$ afterwards;

That, the spots shifted in the manner supposed by *J. Cassini* is utterly improbable when we consider that the distances of them from the micrometer threads were unchanged;

That, they did not so alter is evident, for si l'on considère les figures de M. *Bianchini*, as *J. Cassini* suggests, it will be found that if the spot 5 had passed and the spot 6 had taken its place and 7 replaced 6 and 1 7, three spots would indeed still appear where three were left but instead of a large spot between two considerably smaller one smaller would have been seen between two considerably larger, a difference which must have been noted, especially as the correctnefs of the diagram was ascertained by many persons.

The conclusions then which it appears to me we are authorised to deduce are these:

1. From *Cassini* I and II [1]) and *Maraldi* I who employed large lenses of long focus at Paris and *Herschel* [2]) who made use of large reflecting telescopes in England not having been able to distinguish satisfactorily the spots of Venus, it may be inferred that the atmosphere in these latitudes is unsuited for such observations.

2. From *Herschel* having employed instruments of greater power than those of *Schroeter* [3]) without obtaining similar results it is highly probable that *Schroeters* assertions relative to Venus are unworthy of credit.

3. From *J. D. Cassini* not having laid any great strefs upon his own observations of the spots of Venus [4]), from his contemporaries having beheld them in the same light as himself [5]), from the loose manner in which the observations were made, the instrument being of inferior power and imperfectly mounted [6]), distances being estimated by the eye not measured with a micrometer [4]), the spots being indistinctly seen and in general only suspected [4]), from the suspicious manner in which a deduction from these same observations was circulated [7]), from the silence which for more than 40 years *J. D. Cassini* maintained on the subject [8]), and as facts cannot be changed by reasoning upon them after a lapse of 80 years [9]), J conclude that any value assigned by the son to his fathers observations is fictitious.

4. From the younger *Cassini* having been informed by *Bianchini* himself of his observations at the time they were made [10]) and of his inferences from them, and not having attempted to deduce from his fathers observations any deductions in opposition thereto until eleven years after the death of *Bianchini*, which took place in 1729, and from his having garbled and misrepresented the statements of that astronomer in order to substantiate these deductions, I conclude that *J. Cassini* was well aware of the badnefs of the cause he undertook to advocate, and likewise that he was disposed to support it by all means.

5. From the observations of *J. D. Cassini* being such as are detailed in Nr. 3, and as they cannot be reconciled to those of *Bianchini* except, by the means specified in Nr. 4 I conclude that the statements of one of these astronomers must be rejected.

6. From the circumstances under which the observations of *Bianchini* were made and from the minutenefs with which they are detailed, from their correctnefs having been ascertained by several bystanders, from the precautions adopted, from the superiority of the instruments employed and the measures being micrometrical, from the high and unsullied character of *Bianchini*, above all from his candour, I conclude that we are fully justified in considering *J. D. Cassinis* observations of Venus as spurious and those of *Bianchini* as genuine.

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1) *Lalande* Astronomie Vol. III. p, 436. *Cassini* Astronomie Vol. I. p. 527.

2) Philosophical Transactions 1793.

3) Philosoph. Trans. 1793.

4) Mémoires de l'Academie. Vol. X. p. 467. *Lalande* Aetronomie III. p. 435. *Cassini* Astronomie Vol. I. p. 513.

5) *Melchior à Brig*â, Lett. p. 2 in Hesp. et Phosph. nova Phaen. *Bianchini* p. 58. 59. *Keill* does not even notice *Cassinis* determination of the period of rotation of Venus, at least I can find no notice of it in his astronomy published at Oxford in 1718.

6) Hesp. et Phosp. nova Phaenomena p. 58.

7) Gran Giornal di Europa part. V. Ann. 1667. cit. à *Melchior à Brig*â.

8) Hesp. et Phosp. n. Phaen. p. 59.

9) *J. D. Cassinis* letter to Petit was first published in the Journal des Savans for 1667. *J. D. Cassinis* Astronomie appeared in 1740.

10) *Cassini* Astronomie Vol. I. p. 526.

7. Consequently that the period of rotation of Venus is twenty four days and eight hours, may be regarded as a close approximation to the truth.

8. From Venus and her spots when seen through a large lens of long focus in the climate of Italy resembling the appearance of the moon and her spots when seen by the naked eye, we have the instrument and locality pointed out for future investigations of these phaenomena.

I shall terminate this paper by briefly noticing a subject which again attracted my attention while examining the authors whose names occur in the preceding pages.

From the supposed imprisonment of *Galileo* in the dungeons of the Inquisition, which *Tiraboschi* *) has clearly and satisfactorily proved to be a fiction, from the retractation of this great man and his subsequent exile from Rome which the same author has shown to have depended upon other causes than the philosopher simply advocating the Copernican system, from the denunciations of some zealous bigots forming the tribunal of the Roman Inquisition being mistaken for a formal decision of the Church of Rome, it has been inferred that that Church condemns the Copernican system as heretical and false.

Truth requires it should be stated that for nearly two centuries before the time of *Galileo* the *Copernican* system, as it was subsequently designated, was favoured and its promulgation encouraged by the Popes, Cardinals and Bishops at Rome.

Nicholas of Cusa who first revived the Pythagorean doctrine of the earths revolution round the sun was nominated Cardinal in 1448 by Pope *Nicholas* 5. By Popes *Callistus III* and *Pius II* he was most highly esteemed and honoured and his works were first printed at Rome in 1502. At the repeated instances of Cardinal *Nicholas Schonberg* and *Tidemannus Gisius*, Bishop of Culm, *Copernicus* who was then a canon of Wurms, was induced to publish his great work de revolutionibus orbium celestium, which in 1543 issued from the Nuremberg press under the auspices of Pope *Paul III*, previous to which *Celio Callagnini* under the patronage of Cardinal *Ippolito d'Este*, who persuaded James *Ziegler* to settle at Rome, had published his tract quod coelum stet terra autem moveatur, and in 1533 Pope *Clement VII* presented *John Albert Widmanstad* with a splendid Greek manuscript of *Alexander Aphrodisius* de sensu et sensibili, still preserved in the Electoral Library of Monaco, for having in the Vatican gardens explained the Copernican System to him, Cardinals *Franciotto Orsini*, and *Giovanni Salviati*, *Giampietro Grassi*, Bishop of Viterbo, and *Matteo Coste*, his, the Popes, private Physician.

I may add that while *J. D. Cassini* was *le devot Italien qui n'ose se déclarer Copernicien et qui revient sans cesse au Système de Ptolémée*, (*Delambre Astronomie Moderne Tom. II. p.* 784) *Bianchini* whose observations demonstrated the Copernican system while his writings maintained its truth, was „Sanctissimi Domini Papae Praelatus Domesticus."

Hayes. July 1832.

Hussey.

*) *Tiraboschi* Storia della Letteratura Italiana. Appendice al Capo II. del Libro II. Vol. 8. Sui primi Promotori del Sistema *Copernicano* e sulla Condanna del *Galileo* e del Sistema *Copernicano*, from which two memoirs all these particulars are taken.

Nachschrift des Herausgebers.

Herr Doctor *Olbers* hat mich ersucht am Ende dieses Aufsatzes die Herren Astronomen in Italien aufzufordern, die von *Bianchini* mit *Campani*schen Fernröhren gemachten Beobachtungen der Venus mit *Fraunhofer*schen zu wiederhohlen. Die Luft dort wird noch ebenso durchsichtig wie zu *Bianchinis* Zeiten seyn, und wir würden also, abgesehen von der aus diesen Beobachtungen gehofften Bestätigung oder Widerlegung der *Bianchini*schen Umdrehungszeit, auf jeden Fall dadurch in den Stand gesetzt werden die Leistungen der jetzigen achromatischen Fernröhre mit denen der früheren einfachen Objective an einem delicaten Gegenstande vergleichen zu können.

Der Abdruck dieses Aufsatzes ist etwas verspätet, weil ich den Herrn Verfasser zu manchen Aenderungen in der Form zu bewegen wünschte, was mir indessen nur bei einigen gelang. *S.*

Schreiben des Herrn *v. Steinheil* an den Herausgeber.

München 1833. Octbr. 12.

Ich erinnere mich nicht, ob ich Ihnen schon angezeigt habe, dafs ich mir in meinem Garten in München eine Sternwarte erbaut habe, deren Ausstattung mit Instrumenten, die ich gröfstentheils bey mir in meiner Privatwerkstätte ausführen liefs, jetzt so gut als vollendet ist. Vielleicht ist es Ihnen nicht unangenehm hierüber ein Näheres zu erfahren, da die