Cartography, Geodesy, and the Heliocentric Theory: Yves Simonin's Unpublished Papers*

Abstract: Yves Simonin, a rather obscure professor of hydrography in Bayonne, submitted five scientific papers to the Paris Academy of Sciences between 1738 and 1740, which only survive in the original manuscript versions. The topics Simonin deals with in these texts are essentially three: the rectification of navigation charts of the Southern Sea, the shape of the Earth, and the heliocentric theory. Far from acknowledging Simonin's contribution to the ongoing academic debate as a valuable one, the institution systematically rejected his work. In this paper, I first provide a critical analysis of Simonin's manuscripts. As I argue, their originality lies in the adoption of the perspective of the practitioner of and expert in navigational techniques. I then investigate the reasons behind the Academy's negative reception of Simonin's papers, arguing that the rejection was partly due to the cultural policy of the institution, and partly to the mechanism for monitoring the quality of scientific research.

Keywords: Yves Simonin; Paris Academy of Sciences; shape of the Earth; navigation; heliocentric theory; Isaac Newton.

The early 18th century debate on the shape of the Earth has been approached from various angles, from the history of mathematics to the history of philosophy, mostly focusing on the contributions provided by members of the Paris Academy of Sciences.[1] Despite the lack of scholarly interest in the role that minor figures from outside the Academy played in the debate, the consideration of unorthodox or marginal actors might significantly improve the available knowledge about the question of the shape of the Earth in 18th century France. As a matter of fact, the inclusion of such actors in the narrative makes it possible to see how the debate went far beyond the walls of the Academy, and the ways in which a wider public appropriated and reshaped the arguments first formulated and discussed in the restricted circle of the academicians. Whereas the part played by the salons, the learned circles, and the literary press in this process of appropriation and reshaping

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[1] There are many valuable contributions to the study of the debate, including Bodenmann (2018); Brown (1976); Greenberg (1995) and (1996); Passeron (1994); Shank (2008); Terrall (1992) and (2002).

has been investigated,[2] the role of technicians and practitioners has rather been neglected. Technicians and practitioners represent a particularly interesting case study as they embody a particular form of expertise, alternative to the expertise of theoretical scientists, but nevertheless important in the making of modern science.[3]

The figure of Yves Simonin, hydrography professor in Bayonne until 1739, is largely unknown. Simonin was the first of a dynasty of hydrography professors, including his son Jean-François, and his grandson Pierre.[4] As reported in a paper sent to the Academy, Simonin had been studying geometry and astronomy for forty-five years, while practising navigation: the experience in technical schools and shipyards on the Atlantic Ocean was completed by decades of long-distance trips to India, China and the Pacific Ocean (at least four).[5] Starting from 1738, Simonin wrote several scientific pieces and sent them off to the Academy, trying to join the debate on the shape of the Earth and receive acknowledgment for his scientific work. Almost all of Simonin's writings are still unpublished, and preserved in the original manuscript version in the archives of the Paris Academy of Sciences, with the sole exception of the *Mémoire contre la mesure conjecturale de l'étendue de la mer du sud par M. d'Anville* (1738), which Louis-Charles-Joseph de Manne inserted in the 1834 edition of Jean Baptiste Bourguignon d'Anville's collected works.[6]

The topics Simonin deals with in the papers sent to the Academy are essentially three: the rectification of navigational charts of the Southern Sea,[7] the shape of the Earth calculated on the basis of the measuring operations conducted in the Far East and in Europe, and the heliocentric theory. Whereas the first two questions are widely discussed in the scientific community of the 1730s, leading scientists of the 18th century mostly considered uncontroversial, if not trivial, the question of the alternative between the heliocentric and geocentric cosmological models. Interestingly, Simonin establishes a tight link between these three topics, and uses the conclusions he reaches on cartography and geodesy as anti-Copernican and anti-

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[2] These aspects are approached in Terrall (1992) and (2002).
[3] Hilaire-Pérez (2007) and (2016); Roberts, Schaffer and Dear (2007); Shapin (1989) and (1994); Shapin and Schaffer (1985).
[4] Duo (2002, p. 413); Larrieu (2016, p. 32).
[5] Archives of the Academy of Sciences (AAS), *Pochette de séance* of February 1738: Simonin (1738, f. 1r), also edited in d'Anville (1834, p. 587). For the quotation, see footnote 33.
[6] d'Anville (1834, pp. 586-600). Simonin's paper is followed by d'Anville's reply (pp. 601-618).
[7] In the 18th century, the lemma "Southern Sea" refers to a broad marine region to the south of Panama's latitude. Simonin uses the expression to indicate the breadth of the Pacific Ocean between Peru and the Philippines.

Newtonian arguments, opposing to the heliocentric and attractionist philosophy – which for Simonin, as we shall see, are one and the same thing – the intellectual heritage of a long tradition, running from Aristotle to Descartes.

In the first section, I will present the main events of the academic debate on the shape of the Earth in the first half of the 18th century. This introductory account is necessary to understand Simonin's position with respect to the ongoing discussion, and the intrinsic relevance of his scientific work, which I will assess in section two. As previously mentioned, Simonin goes far beyond the questions posed by cartography and geodesy, and connects them to the question of the possibility of the Earth's motion around the Sun. In the third section, I will analyse the reasons and legitimacy of this conceptual passage. How can the accurate determination of geographical distances and the arguments in favour of a perfectly spherical Earth demonstrate its stillness at the centre of the universe, and therefore refute the Copernican theory? Why does Simonin bind together these seemingly unrelated questions? The last section of the paper will then be devoted to the reaction of the Academy to Simonin's papers. The systematic rejection of Simonin's *mémoires* raises the question of the control over the quality of scientific research exercised by the institution. Does the Academy reject Simonin's contributions because of their scientific irrelevance, or rather because the hypotheses Simonin puts forward are too eccentric with respect to the ongoing debate in the Academy? The case study of Simonin points at the broader question of the patterns of scientific discussion in the institution, and of the interventionist role of the Academy in regulating debates, and silencing dissonant voices.

The Shape of the Earth and the Paris Academy

In the second half of the 17th century, the French government realized the importance of having an accurate map of the kingdom, which would not only allow for better control of the territory and improvement of commercial routes, but also contribute to establishing the exact measure of the terrestrial meridian, thus displaying the excellence of France in attaining a much coveted result.[8] Under the auspices of

[8] On the political relevance of cartography and related practices, see Petto (2007) and (2015).

Colbert, Jean Picard inaugurated the operations for mapping the French territory in 1668. Picard and his team measured a degree of the meridian arc joining Sourdon, near Amiens, and Malvoisine (in the municipality of Champcueil), in the south-eastern suburbs of Paris, to draw up a map of the area around the capital. The success of Picard's operations, and the need to integrate his results with further data, pushed the French authorities to fund a series of more systematic measurements in the following years. Starting from 1683, academician and first director of the Observatory Gian Domenico Cassini led new operations that consisted in measuring a meridian arc longer than Picard's, joining Perpignan, near the Pyrenees, to Dunkirk, on the northern shore. The problem with Picard's measurements was not their accuracy, but rather – as Jacques Cassini (Gian Domenico's son) puts it – that they concerned "only a very small portion of the Earth […] with respect to its circumference."[9] For the sake of a general map of France, therefore, a longer arc was to be measured: "A very accurate map of the surroundings of Paris had been made, […] but these particular researches were not enough to form a map of France, on the accuracy of which one could rely."[10] The measurement of the Perpignan-Dunkirk meridian arc was achieved in 1713, and described in various papers presented at the Academy. Due to his death in 1712, Gian Domenico Cassini did not author any of these; his son Jacques was in fact responsible for the organization of the results in a consistent scientific discourse.

In the most systematic account of the operations, namely the treatise *De la grandeur et de la figure de la Terre*, published as a supplement to the annual collection of the *Mémoires* of 1718 (out in 1720), Jacques Cassini presents the details of the operations, including the techniques and the tools employed in measuring the meridian arc. Interestingly, in the second part of the text, Cassini, from the inequality of the degrees of the French meridian, deduces the shape that the Earth must have. The difference between the Dunkirk-Paris and Paris-Perpignan distance proves that

> Except for the inequalities caused by the presence of mountains, the surface [of the Earth] must have the shape of an ellipsis elongated towards the poles, such that if divided in degrees by perpendicular lines drawn on its surface, the

[9] Cassini (1733, p. 390).
[10] Cassini (1733, p. 391).

> degrees are smaller when approaching, and larger when moving away from the poles.[11]

The results of Cassini's measurements, however, were far from being uncontroversial. Not only because they were at odds with certain empirical evidence, but also because a strong alternative hypothesis, based on less systematic experimental data, but on highly reliable physical concepts, had been formulated. As for the empirical evidence, this goes back to Jean Richer's expedition to Cayenne, French Guiana, in 1672-73. Richer discovered that the length of the seconds pendulum, namely the length of a pendulum whose period is precisely one second, was shorter in Cayenne than at Paris.[12] In physical terms, Richer's discovery implies that gravity is less strong near the equator than at northernmost latitudes. Relying on the Cayenne pendulum experiment, Newton, in the third book of the *Principia* (propositions 18-20), formulates a hypothesis on the shape of the Earth, which comes as a corollary of his general theory of attraction. The Earth, according to Newton, must be slightly flattened at the poles, for the combined effect of attraction and of the daily rotation around its axis.

> If it were not for the daily circular motion of the planets, then, because the gravity of their parts is equal on all sides, they would have to assume a spherical figure. Because of that circular motion it comes about that those parts, by receding from the axis, endeavour to ascend in the region of the equator. […] If our earth were not a little higher around the equator than at the poles, the seas would subside at the poles and, by ascending in the region of the equator, would flood everything there.[13]

The behaviour of the pendulum is easily accounted for, as the centripetal force is less efficient where bodies are farther from the centre of gravity. As Newton puts it in proposition 20:

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[11] Cassini (1733, p. 238).
[12] On Richer's expedition to Cayenne, and its scientific relevance, see Olmsted (1942).
[13] Newton (1999, p. 821).

> Weights that are in any other regions whatever, anywhere on the whole surface of the earth, are inversely as the distances of those places from the centre; and therefore, on the hypothesis that the earth is a spheroid, the proportion of those weights is given. From this the following theorem is deduced: the increase of weight in going from the equator to the poles is very nearly as the versed sine of twice the latitude […]. And the arcs of degrees of latitude on a meridian are increased in about the same ratio.[14]

Newton's hypothesis is incompatible with Cassini's, if nothing else, for Newton's statement that degrees of latitude should increase going towards the poles, and decrease going towards the equator: that which Cassini's experimental results contradicted.

As the theory of attraction started to be discussed in the Academy, the early advocates of Newtonian physics in France, such as Pierre-Louis Moreau de Maupertuis, insisted on the centrality of the question of the shape of the Earth in establishing the superiority of Newton's natural philosophy over Descartes's.[15] In fact, some Cartesians working at the Academy, most notably Jean-Jacques Dortous de Mairan, had written papers arguing for the consistency between Cassini's elongated-Earth hypothesis and the cosmology of vortices.[16] The academic debate on the shape of the Earth of the 1720s and 30s, therefore, concerned not only the techniques and the tools, mathematical and material, for solving the geodetic question, but also the cosmological principles in which the hypotheses on the shape of the Earth were framed, namely planetary vortices and universal gravitation.

The persistence of intellectual disagreement within the Academy, and the practical necessity to go on with the mapping of France, pushed the French institutions to set up two scientific expeditions to measure one meridian arc across the north pole and one close to the equator. The comparison between degrees of the meridian at different latitudes would have been a reliable test to tell which geodetic hypothesis was

[14] Newton (1999, pp. 826-827).

[15] See, for instance, the first chapter of Maupertuis's *Discours sur les différentes figures des astres*, published in 1732 (Maupertuis, 1768a, pp. 81-89).

[16] The reference is to Mairan's paper *Recherches géométriques sur la diminution des degrés terrestres en allant de l'équateur vers les pôles*, published in the *Mémoires de l'Académie Royale des Sciences* in 1720 (Mairan, 1720). For an in-depth study of Mairan's paper, see Greenberg (1995, pp. 15-51).

correct.[17] A first contingent of scientists led by Charles-Marie de La Condamine left for Ecuador in 1735, but came back only in 1745, too late to play a significant role in the controversy.[18] A second expedition directed by Maupertuis left for Finland in 1736, and completed the operations in less than one year. The results emerging from Maupertuis's measurements spoke out: the degree of the meridian arc measured across the north pole is greater than the degree measured in France. The Earth therefore, as Newton predicted, must be flattened at the poles. As Maupertuis stresses in the report of the expedition to the Academy:

> Our degree […] differs by 950 toises[19] from the measure it should have according to the measures Cassini proposed in his book *De la grandeur et de la figure de la Terre*; […] from where it is clear that *the Earth is significantly flattened toward the poles*.[20]

The result of the expedition to Finland, however, did not persuade all academicians to embrace the flattened-Earth hypothesis, let alone Newtonian celestial mechanics. The attacks of the Cassini faction on the Newtonians became all the more rough, as they insisted on alleged experimental fallacies in the operations conducted at the north pole: the results of the measurements were distorted in consequence of the bad faith of Maupertuis and his fellows, who presupposed the validity of the flattened-Earth hypothesis, and constructed an artificial confirmation of it.[21]

The persistence of disagreement explains the way in which the story evolved. In 1739, César-François Cassini de Thury (Gian Domenico's grandson) got the ministry's approval to start a new set of verification measurements in France. Such operations were required in order to confirm the results of the Finnish expedition with

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[17] As Newton's disciple John Theophilus Desaguliers points out in the 1724 *Dissertation concerning the figure of the Earth*, which circulated widely amongst the Paris academicians, "If any consequences of this kind [on the shape of the Earth] could be drawn from actual measuring, a degree of latitude should be measured at the equator, and a degree of longitude likewise measured there; and a degree of very northerly, as for example, a whole degree might be actually measured upon the Baltic sea, when frozen, in the latitude of sixty degrees" (Desaguliers, 1724, p. 209).
[18] On La Condamine's expedition, see Safier (2008).
[19] One toise is equivalent to 0.3266 metres, or 0.3572 yards.
[20] Maupertuis (1768b, pp. 167-168).
[21] This argument was first brought up by Johann Bernoulli I, in a letter sent to Maupertuis before his departure for Finland (8 May 1735): "Do the observers have any inclination for either theory? For if they sympathize with the flattened Earth, they will find it flattened; if, on the contrary, they are fond of the idea of the elongated Earth, they will certainly confirm its elongation. It is a short step from the flattened to the elongated spheroid, so that it is easy to be mistaken, if one wants to be mistaken" (Basel UB, SIGN: L Ia 662, Nr.35, f. 2v).

the measure of a meridian arc at more southerly latitudes,[22] carried out with more modern instruments than those used by Gian Domenico and Jacques Cassini, which everybody now considered old-fashioned and unreliable. In the final account of the measurements, *La méridienne de l'Observatoire Royal De Paris* (1744), Thury constructs a narrative aimed at toning down any disagreement on the question of the shape of the Earth, proving that a conciliation of all positions is possible. Thury refers to Maupertuis's expedition to Finland, and summarizes its results, but does not present it as decisive in settling the question once and for all. Maupertuis's measurements are said to be more accurate than Picard's and Cassini's, mostly because of the modern instruments available to him.[23] By means of instruments similar to those Maupertuis employed,[24] Thury was able to correct Gian Domenico and Jacques's results, and to show that "the degrees of the meridian get smaller while approaching the equator, and that therefore the Earth has a flattened shape."[25] Thury never quotes Newton, but repeatedly insists on the continuity between his work and Picard's and Cassini's operations, so as to reduce the hostility between Descartes's and Newton's upholders in the Academy. However, Thury's attempt to tone down the disagreement on the shape of the Earth, and indirectly on cosmological principles, was not totally successful, as critiques and personal attacks kept on being published even after 1740.[26]

Simonin on Cartography and Geodesy

[22] As La Condamine's expedition was not yet back from Ecuador.

[23] For the measurements in Finland, Maupertuis employed a sector constructed by the English instrument maker George Graham, which was much more accurate than the sector used by the Cassinis in France. A few years before (1725-28), with the same sector, James Bradley had discovered the aberration of light.

[24] Thury, however, only used French-crafted instruments: "We judged therefore that the bigger [the instruments] the better, which is true, other things being equal; the size of these instruments, however, was a drawback […]. The best option was to have quadrants of a smaller radius, whose accuracy was the same as that of bigger instruments. This we obtained by applying a micrometer to the lenses of these instruments, as suggested by the Chevalier de Louville […]. We also made other changes to make the employ [of the quadrant] easier, many of which had been invented by Mr Langlois […]" (Thury, 1744, p. 8).

[25] Thury (1744, p. 25).

[26] Maupertuis played an important part in the anti-Cassini satirical propaganda throughout the 1740s. On this point, see Badinter (1999, pp. 144-153); Beeson (1992, pp. 116-134); Terrall (2002, pp. 130-172).

Simonin entered the debate fairly late with respect to the chronology I have presented. At the beginning of the *Mémoire contre la mesure conjecturale de l'étendue de la mer du Sud par M. D'Anville* (1738), Simonin stresses that he has already presented another paper one year before, of which however there is no trace in the archives.

> In the paper I sent you one year ago against Cassini and Newton on the shape of the Earth, I argued that, to meet the zeal of the Royal Academy, those who work in the sciences must communicate to you their thoughts on anything they consider worthy of your attention.[27]

Simonin's first available work is thus the critique of d'Anville's *Mesure conjecturale de la Terre sur l'Équateur, en conséquence de l'étendue de la mer du Sud* (1736). D'Anville is a central figure in the development of cartography and geography in the 18th century, especially for his valuable contributions in mapping several regions of Asia, Africa and Latin America.[28] In the *Mesure conjecturale*, d'Anville rectifies the widely accepted measurement of the equator by making it smaller by 440 leagues.[29] The rectification is based on a new calculus of the distance between Lima and Manila, by means of astronomical observations, and verified by the determination of the latitude of other sites, such as Peking (Beijing). d'Anville also refers to the work Cassini has carried out in France, and to the elongated-Earth hypothesis:

> We will quantify these latitudes [of Manila and Lima] in toises, following the value of the degrees in Cassini's hypothesis of the measure of the Earth […]. If the inequality [of the meridian degrees] can increase or decrease, it always

[27] Simonin (1738, f. 1r), also in d'Anville (1834, pp. 586-587): "J'eus l'honneur de vous dire, dans le mémoire que je vous adressai il y a un an, contre les sentiments de MM. Cassini et Newton sur la figure de la Terre, que ceux qui travaillent sur les sciences doivent, pour répondre au zèle de l'Académie Royale, vous communiquer leurs recherches sur tout ce qu'ils trouvent digne de votre attention."

[28] D'Anville was appointed a member of the *Académie des inscriptions et belles-lettres* in 1754, and of the Academy of Sciences in 1773. On d'Anville's cartographical and geographical activity, see Ferreira Furtado (2011), (2013) and (2017).

[29] 1 league corresponds to 3.452 miles or 5.556 kilometres. 440 leagues are therefore 1519.03 miles or 2444.64 kilometres.

> remains consistent with the initial hypothesis, according to which the degrees of latitude are wider towards the equator than at the poles.[30]

The connection between d'Anville's new calculations on the dimensions of the Southern Sea, the shortening of the equator resulting from these calculations, and Cassini's elongated-Earth hypothesis, provides ground for Simonin's critique.

Simonin believes that the results of d'Anville's inquiry are nothing but the consequence of his theoretical bias in favour of Cassini's elongated-Earth hypothesis:

> I am surprised that, *after making so many combinations to show that the degrees of the equator are smaller than the degrees of the meridian*, he [d'Anville] did not think of combining the different observations of longitude concerning Lima and Manila.[31]

To refute d'Anville's new measurements of the Pacific Ocean, and the resulting modification of cartographical practices, Simonin employs two different arguments. The first is drawn from navigational practice, and the high reliability of old charts. Simonin focuses on an example drawn from his personal experience to make the general point. When seafarers need to go through the Sunda Strait to reach China or Batavia (now Jakarta), the knowledge derived from practice, and the indication in Pieter Goos's maps of the Indian and Pacific Ocean,[32] provide enough information to find the right way through the strait. Back in 1698, Simonin was embarked on a ship and had to find the Sunda Strait; as his fellow navigators and he did not trust the available knowledge (the old charts and the knowledge derived from practice), they followed some Jesuits' advice, and corrected the position of the Strait on their charts. The result of this emendation, however, was far from successful: "On that basis we established our route and missed the Sunda Strait for 200 leagues, that which caused one year of delay in our travel, and all the dangers one can go through without

[30] d'Anville (1834, p. 574).
[31] Simonin (1738, f. 2v), also in d'Anville (1834, p. 593): "Je suis surpris qu'ayant fait tant de combinaisons pour démontrer que les degrés de l'Équateur était plus petits que ceux du méridien, il ne se soit pas avisé de combiner aussi les différentes observations de longitude qui regardent Lima et Manille." My emphasis.
[32] Pieter Goos, a famous Dutch cartographer of the 17th century, is the only reference explicitly quoted in Simonin's paper.

dying."[33] As the case of the 1698 navigation through the Sunda Strait shows, the knowledge of navigators and the existing maps of the Southern Sea needed no integration or emendation, as they worked perfectly well in practice. d'Anville's emendation is therefore unnecessary, if not dangerous – as the Jesuits' corrections had been for Simonin and his fellows.

The second argument, *ad hominem*, which Simonin presents, insists on d'Anville's lack of direct experience of traveling to South East Asia. In establishing new charts or correcting the existing ones, d'Anville would rely on indirect knowledge of the places, and on the astronomical observations made by scientists traveling to South America and the Philippines. Simonin, on the contrary, has direct experience of the Southern Sea, and of navigational routes in the Pacific Ocean:

> For 45 years I have been continuously practising geometry, astronomy and navigation; […] for over 20 years, [I have been] in the practice of long-distance trips to India, China and to the Southern Sea […]. Mr d'Anville bases his calculus of Lima's longitude solely on two observations, that of Peralta, of 302 degrees 11 minutes, and that of Alexandre Durand, of 300 degrees 51 minutes […].[34]

Simonin's scientific expertise does not consist in the bookish culture of a learned man or an academician, but in the experience and the know-how of a practitioner. First-hand knowledge, acquired through direct experience and through practice, is presented as better evidence than second-hand notions, learnt through reports (however detailed they might be).

From the discussion on navigation and cartography, Simonin draws interesting conclusions about geodesy. In 1738, after Maupertuis's return from Finland, and while the debate with Cassini on the alternatives between the flattened and the

[33] Simonin (1738, f. 1v), also in d'Anville (1834, p. 591): "J'en puis rapporter une preuve sensible et funeste de l'année 1698, sur le vaisseau de roi l'Amphitrite, allant du Cap de Bonne-Espérance à Batavia: les PP. Jésuites, embarqués sur ce vaisseau, nous ayant persuadé que le détroit de la Sonde était de 200 lieues plus à l'occident que les cartes ne le marquaient, nous dirigeâmes notre route en conséquence et nous manquâmes le détroit de la sonde de 200 lieues, ce qui causa une année de retardement au voyage, et tous les dangers que l'on peut essayer sans périr."

[34] Simonin (1738, f. 1r-2r), also in d'Anville (1834, pp. 587-590): "Depuis quarante-cinq ans je suis dans l'exercice continuel de la géométrie, de l'astronomie et de la navigation; […] pendant plus de vingt ans, dans la pratique des voyages de long cours aux Indes, à la Chine et à la mer du Sud […]. M. d'Anville ne nous propose la longitude de Lima que sous deux observations, celle de Peralta, de 302 degrés 11 minutes, et celle d'Alexandre Durand, de 300 degrés 51 minutes […]."

elongated Earth gets increasingly heated, Simonin defends the most classical hypothesis of a perfectly spherical Earth. Interestingly, d'Anville and Simonin give specular readings of the conceptual relationship between the generally accepted measurement of the equator and the hypothesis of the perfectly spherical Earth. d'Anville holds that the generally accepted measurement of the equator (which he considers fallacious) is based on the false belief that the Earth is perfectly spherical; once Cassini has proved this geodetic hypothesis to be wrong, then the measurement of the equator should be emended too. Simonin, calling upon his direct experience, claims that charts and practices modified on the basis of geodetic models alternative to the perfectly-spherical Earth are highly unreliable, if not dangerous. The Earth, therefore, must have the shape that is implied by the charts and practices which work best, that is, it must be perfectly spherical.

> By virtue of these very natural reasons,[35] navigators can be persuaded of the perfect roundness of the Earth, and have the same confidence in globes and charts that have been produced based on this idea; otherwise they could never establish any fixed point which was not doubtful, and which would have troublesome consequences.[36]

While being critical of both the elongated and flattened-Earth hypotheses, Simonin is not equally critical of Cassini and of Newton. In the paper submitted to the Academy in February 1739 *sur la nécessité de déterminer la véritable figure de la Terre et contre les sentiments du système de Newton*, Simonin, while criticizing Cassini's deductions of the shape of the Earth, praises the accuracy of the measurements of the French meridian. Simonin commends Cassini's work as it provided "the perfect knowledge of the famous meridian line from Perpignan to Dunkirk, so accurately measured by the most learned men that it is impossible to demand anything better."[37] The measurements that navigators elaborated empirically,

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[35] The arguments against d'Anville's rectification of the equator presented above.

[36] Simonin (1738, f. 3v), also in d'Anville (1834, p. 596): "Les navigateurs peuvent donc encore par toutes ces raisons bien naturelles, se persuader de plus en plus de la parfaite rondeur de la Terre, et se conduire avec la même assurance par les globes et les cartes qui ont été dresses dans cette idée; autrement ils ne pourraient jamais se fixer aucun point qui ne fut dans le doute et d'une conséquence très fâcheuse."

[37] AAS, *Pochette de séance* of February 1739: Simonin (1739, f. 1v): "La parfaite connaissance de la fameuse ligne méridienne de Perpignan à Dunkerque, mesurée par les plus savants d'une manière si précise qu'il n'est pas possible d'espérer rien de mieux."

during their travels all over the world, are in fact "confirmed in all possible regards by Cassini's just observations."[38] Simonin's appraisal of Cassini's experimental results is rooted in a similarity of approaches between his own geographical practice and Cassini's cartographical method. In fact, both Simonin and Cassini gave priority to direct experience and a consolidated practical expertise over abstract mathematical reasoning backed by no work in the field.[39]

The only problem with Cassini's work concerns the conclusions he draws on the shape of the Earth, which is but the result of a miscalculation of the position of Dunkirk, and of its distance from Paris.

> […] although they [Cassini's observations] display a difference of 137 toises between the length of the meridian degree from Paris to Collioure and from Paris to Dunkirk, I have proved that if one takes correctly the weighted mean between the three latitudes that he [Cassini] gave us of Dunkirk, one would find that the degrees from Paris to Collioure have at most a ten-toises difference than those from Paris to Dunkirk. This shows that the degrees measured on this famous meridian line are all the same size, from where one should infer the perfect roundness of the Earth.[40]

Simonin claims that, if correctly interpreted, Cassini's results confirm the hypothesis of a perfectly spherical Earth. Simonin's major polemical target is in fact not Cassini, but Newton. As I will argue in section three, Simonin does not merely reject Newton's flattened-Earth hypothesis, but the very principles of Newtonian natural

[38] Simonin (1739, f. 1v): "Avant cette Royale entreprise et cette heureuse connaissance d'une ligne de près de 9 degrés de longueur, nous étions par différentes mesures incertaines sur la grandeur et figure de la Terre, mais celle de Monsieur Picard adoptée comme une merveille de savants et des navigateurs et confirmée dans toute l'apparence possible par les justes observations de Monsieur Cassini […]."

[39] This highlights a difference with respect to the Newtonians participating in the debate (Maupertuis, Clairaut), who devoted much effort to mathematical elaborations. In particular, they discussed problems aimed at finding the best mathematical method to calculate the shape of the Earth – and this by means of infinitesimal calculus. As Mary Terrall (2002, pp. 93-94) nicely puts it, "Cassini's book on the size and shape of the earth contained some geometrical diagrams and demonstrations, and many tables of observations, but no equations. Maupertuis and Clairaut, on the other hand, directed much of their work toward showcasing mathematical methods, often by considering various observational programs."

[40] Simonin (1739, f. 1v): "[…] encore qu'ils aient trouvé 137 toises de différence entre la longueur des degrés de Paris à Collioure et de Paris à Dunkerque, j'ai démontré qu'en prenant comme il se devait la moyenne proportionnelle entre les trois latitudes qu'il nous a donné de Dunkerque, il se trouve que les degrés de Paris à Collioure sont égaux à dix toises près à ceux de Paris à Dunkerque, ce qui fait connaître que les degrés mesurés sur cette fameuse ligne sont tous de même grandeur, d'où on doit conclure la parfaite rondeur de la Terre."

philosophy. Simonin's anti-Newtonian polemic is probably motivated by the sensation of Maupertuis's impending success in the academic dispute, and the correlative urgency to find a way to prove the groundlessness of Newtonian natural philosophy. Contrary to other academicians, however, Simonin does not contrast Newton with arguments having a Malebranchist or Leibnizian inspiration,[41] but goes back to more traditional sources – from Aristotle to Descartes.

Before examining the details of Simonin's critique of Newton, and the place that the discussion of the heliocentric theory holds in it, I will draw some conclusions on the most relevant aspects of Simonin's contribution to the academic debate on cartography and geodesy.

In the first place, Simonin makes a link between the determinations of longitude and latitude. The debate on the shape of the Earth, which the French academicians were discussing from the perspective of longitude measurements (carried out in France and in Finland), is therefore integrated with evidence coming from latitude measurements at the antipodes. Simonin's reference to the measurement of latitude is meaningful because it represents a possible verification of the accuracy of longitude measurements. Moreover, the inclusion in the debate of data concerning the Pacific Ocean makes the dispute no longer Europe-centred, but global.

Secondly, Simonin emphasizes the tight connection between the determination of the shape of the Earth and the practice of navigation. As d'Anville remarks in his answer to Simonin's critique (*Réponse de M. d'Anville au mémoire envoyé à l'Académie par M. Simonin…*), the geodetic question as discussed in the Paris Academy seems to have no relevant consequences for navigation and geography: "It seems to me that the difference between Cassini's and Newton's systems does not considerably affect navigation, and does not make any difference in geography either […]."[42] The insistence on the positive effects of the solution of the geodetic dilemma for navigational practice, however, is fairly common in the writings of the protagonists in the controversy, who rhetorically stress this point to stress the practical utility of geodetic discussions. As Maupertuis writes in the report on the voyage to Finland presented to the Academy in 1738, namely the *Relation du voyage fait par ordre du Roi au cercle polaire, pour déterminer la figure de la Terre*:

[41] The use of concepts and arguments borrowed from the Malebranchist and Leibnizian traditions was a fairly common anti-Newtonian strategy in the works of the Paris academicians: see Guerlac (1981); Hankins (1970); Shank (2003), (2004) and (2008); Shea (1988).
[42] d'Anville (1834, p. 602).

> But if the navigator cannot understand how useful it would be for him that the shape of the Earth be accurately determined, it is not the certainty he has on all other things that prevents him to grasp the relevance of this question, but rather all the information he is missing. […] If it should happen one day […] that all other elements of navigation are perfected, the one which will remain the most important will still be the exact determination of the shape of the Earth.[43]

The originality of Simonin's argument lies in the fact that the joint discussion of navigational practice and the shape of the Earth leads him to argue for an option that none of his contemporaries would support, namely that of a perfectly spherical Earth.

Thirdly, Simonin brings to the debate the perspective of the practitioner. Most academicians evaluated the results of the measuring operations, and the cosmological systems of Descartes and Newton, from a purely theoretical perspective. Simonin, to the contrary, discusses the problems at stake by means of the knowledge he has acquired in the field. He insists on the primacy of direct experience and on the higher reliability of knowledge acquired through doing than the knowledge derived from the mere study of other people's experiences. Simonin also seems to depreciate abstract reasoning – mathematical, and more generally theoretical speculation – preferring the immediate and concrete nature of practice.

Simonin against Newton (and Copernicus)

In the paper Simonin presented to the Academy in February 1739, he tries to prove that not only Newton's hypothesis on the shape of the Earth is fallacious, but that the overall system of attraction is groundless, if not dangerous for the progress of science. In Simonin's view, Newton's argument for a flattened Earth is based on four pillars, which are criticized one by one, with the ultimate goal of proving that "the system of attraction is in fact only grounded in imagination."[44] The first objection is a more general one, and is meant to undermine the very conceptual basis of Newton's

[43] Maupertuis (1768b, pp. 83-83).
[44] Simonin (1739, f. 2v): "On verra que le système de la gravitation n'en a d'autre [de fondement] que celui de l'imagination."

physics. Simonin argues that there is no need to introduce any principle of attraction to account for bodies falling towards the centre of the Earth. This phenomenon can easily be explained through basic mechanical concepts:

> A falling body receives at every moment an impulsion or increment of force that is occasioned by the parts of matter which the falling body travels through […] from where we see that the acceleration of motion that occurs when a body falls is caused by a natural principle, which has nothing in common with attraction or gravitation.[45]

Simonin criticizes the idea of understanding the acceleration towards the centre by means of the principle of attraction, as the notion of impulsion is enough to make sense of the behaviour of falling bodies. Simonin's preference for impulsion, however, does not imply a complete adherence to the mechanical worldview, as the discussion of the next point makes clear.

The second pillar of Newton's flattened-Earth theory is the interpretation of the behaviour of pendulums at different latitudes. Newton's mistake would have been, in this case, to consider the behaviour of pendulums as relevant information on the gravity of bodies. Simonin objects that, since the pendulum does not approach the centre of the Earth, but rather has an oscillatory movement, it must be considered as not subject to gravity. Consequently, the difference in the period of pendulums at different latitudes "cannot but derive from the different qualities of air in which it [the pendulum] oscillates faster or slower."[46] In so arguing, Simonin not only goes against Newton, but also against a long list of modern scientists (Galilei, Mersenne, Huygens) who considered the study of pendulums important in the physical understanding of gravity.

The third objection against Newton is based on the comparison between different measurements of meridian arcs. Simonin mentions the measurement of the London-York distance carried out by Richard Norwoord in the 1630s, which Newton quotes in

[45] Simonin (1739, f. 2v): "Un corps qui descend reçoit à chaque instant des impulsions ou augmentation de force qui lui sont occasionné par les parties de la matière qu'il déplace […] d'où l'on voit que l'accélération du mouvement qui se fait à la descente des corps vient d'un principe naturel qui n'a rien de commun avec l'attraction ou la gravitation."

[46] Simonin (1739, f. 3r): "La différence des oscillations du pendule d'un lieu à un autre ne pouvant être attribuée aux lois de la pesanteur ne peuvent donc venir que des différentes qualités de l'air dans lequel il fait ses oscillations plus ou moins vite."

the *Principia*, and Maupertuis's operations in Finland, as they would both support Newton's flattened-Earth hypothesis. Norwood's and Maupertuis's results, however, are incompatible, as they entail different quantities of the Earth's flattening: "this contradiction […] would be enough to conclude that Newton is wrong on the shape of the Earth."[47]

With the fourth and last argument, Simonin not only criticizes Newton's flattened-Earth hypothesis, but also brings into the discussion the question of the Earth's motion around the Sun. According to Simonin, Newton would have grounded his geodetic theory on the alleged observation that the sea is deeper on the equatorial coasts than anywhere else.[48] In Newton's view, this hydrographical phenomenon would not only be proof that the Earth is flattened, but also that it revolves around the Sun, as Newton attributes to one and the same cause the Earth's revolution and its flattened shape (the action of a central force on material masses moving inertially). Simonin holds that if one could demonstrate that the seas are not deeper at the equator than at other latitudes, then one would have paved the way to proving that the Earth is not flattened, but also that it does not revolve around the Sun. Once again, Simonin provides an empirical demonstration of his point, drawn from his navigational practice: in his travels to the Pacific Ocean, he has found that the sea is often less deep there than offshore France, which clearly shows that Newton's assumption is erroneous.

The objection from the depth of the seas is not the only one Simonin provides against Newton's cosmology. In subsequent papers,[49] Simonin formulates direct arguments in contrast to the heliocentric theory, and to demonstrate the stability of the Earth. In the 18th-century campaign against Newton's natural philosophy, the association of his name with that of Copernicus is fairly uncommon.[50] This makes

[47] Simonin (1739, f. 3v): "Cette contradiction sans ce qui vient d'être démontré suffirait pour conclure que Newton s'est trompé sur la figure de la Terre."

[48] Simonin refers to proposition 18 of the third book of the *Principia*, where Newton states that "if our Earth were not a little higher around the equator than at the poles, the seas would subside at the poles and, by ascending in the region of the equator, would flood everything there" (Newton, 1999, p. 821).

[49] AAS, *Pochette de séance* of March 1740: *Mémoire contre les sentiments de Newton sur la figure et le mouvement de la Terre* (Simonin, 1740a); AAS, *Pochette de séance* of April 1740: *Mémoire présenté à Messieurs de l'Académie Royale des Sciences à Paris par le Sieur Simonin ancien professeur d'hydrographie, pour soutenir la stabilité de la Terre contre le sentiment de Copernic* (Simonin, 1740b); AAS, *Pochette de séance* of November 1740: *Mémoire présenté a Messieurs de l'Académie Royale des Sciences pour prouver la stabilité de la Terre et la fausseté des prétendus mouvements des corps célestes d'Occident en Orient,* (Simonin, 1740c).

[50] To the best of my knowledge, the association between Newton and Copernicus is never brought up in any other academic papers relating to the question of the shape of the Earth.

Simonin's case study exceptional in the panorama of the "Newton wars" in France.[51] Amongst the arguments proposed against the heliocentric theory, two seem particularly relevant to our concerns. The first draws on the conjectural nature of astronomy, and insists on the primacy of first-hand experience in the investigation of natural phenomena. The second expounds the conceptual foundations of Simonin's physical and astronomical thought, and casts light on the influence of traditional scientific positions on Simonin's approach.

The starting point of the first argument is the question of the distance between the Earth and the Sun. The Copernicans, according to Simonin, would take the Earth to be much further from the Sun than it actually is; in their hands, the wide Earth-Sun distance would then become an argument against the possibility of the motion of the Sun, and therefore a proof of the heliocentric system.[52]

> The great distance they [the Copernicans] pretend to introduce as the only means to give an allure to their system is neither as great nor as certain as they would like to persuade us, since all astronomers before Copernicus believed as I do that we are not so far from the Sun.[53]

Simonin, however, contends that the idea of a great distance between the Earth and the Sun is not grounded on any convincing experimental proofs. As a matter of fact, it is impossible to establish the actual distance of the Sun from the Earth because of the parallax: illustrious astronomers, from Tycho Brahe to Jacques Cassini, have calculated the Sun-Earth distance, coming to very different results, thus displaying the impossibility of knowing the true distance between the two bodies. Insofar as we have no certain knowledge of the position of planets in space, we should stick to the

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[51] I borrow the lemma "Newton wars" from Shank (2008).

[52] To the best of my knowledge, Copernicus proposes to broaden the dimensions of the Solar System, and leaves open the question whether the universe is finite or infinite, but does not increase the distance between the Earth and the Sun to use it as a proof of his theory. Thus in the *Commentariolus* (fourth postulate): "The ratio of the Earth's distance from the Sun to the height of the firmament is so much smaller than the ratio of the Earth's radius to its distance from the Sun that the distance between the Earth and the Sun is imperceptible in comparison with the loftiness of the firmament" (Copernicus, 1985, p. 81). See also the *De revolutionibus*, bk1, par. 6 and 10.

[53] Simonin (1740b, f. 2r): "La grande distance qu'ils affectent de persuader comme le seul moyen de colorer leur système, n'est ni aussi grande ni aussi assurée qu'ils veulent nous le persuader, puisque tous les astronomes avant Copernic étaient dans la même croyance que je suis sur le peu d'éloignement de nous au Soleil."

clearest data of our first-hand experience, which clearly privileges the stillness of the Earth at the centre of the world.

Simonin, however, is also concerned with the expansion of the boundaries of the cosmos, which is certainly present in the Copernican tradition (if not in Copernicus's own work). He finds particularly dangerous the suggestion that a plurality of worlds might actually exist. As several partisans of the heliocentric system have claimed, the expansion of the boundaries of the cosmos allows for the possibility that far away stars might be other suns, with other sets of planets revolving around them.[54] Such conjectures on the indefinite extension of the cosmos, and on the plurality of worlds, were regarded as problematic especially from a religious perspective, as they overthrow the privileged position of the Earth and mankind in the structure of the universe. In his papers, Simonin repeatedly insists on the opposition between the Copernican theory and Catholicism. Copernicus is presented as "this famous innovator, contemporary of Luther, and probably of intelligence with him on religious matters."[55] Likewise, the geocentric hypothesis is presented as "the most wondrous and less dangerous for the soul and for religion."[56] The steadiness of the Earth at the centre of the (finite) cosmos is stated in the Holy Scriptures, where "to establish the belief in the stability of Earth [it is said] that God has grounded it on its own solidity, and that it will never be shaken, that is to say that it will never occupy another place than the centre of all matter."[57]

In passage quoted last, Simonin hints at the place where the Earth would be designed to lie, namely the centre of the material world. This brings us to the second direct argument Simonin formulates to prove geocentrism, which concerns the conceptual foundations of his physical and astronomical thought. Simonin's general assumption is that matter can have only one centre towards which all of its parts tend, in proportion to their weight. In the structure of the world, material elements are

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[54] On this philosophical tradition, running from Bruno to Fontenelle, see Del Prete (1998).

[55] Simonin (1740a, f. 1v): "Copernic ce fameux novateur contemporain de Luther et peut être d'intelligence contre la Religion."

[56] Simonin (1740a, f. 2v): "S'il est vrai qu'on ne puisse jamais philosopher que par hypothèse, pourquoi ne s'en pas tenir à celle qui paraît la plus merveilleuse et la moins dangereuse à l'esprit et à la religion."

[57] Simonin (1740b, f. 3v): "Car Messieurs qui a-t-il de plus clair à la raison que ce que dit le St. Esprit pour établir la croyance de la stabilité de la Terre que Dieu l'a fondée sur sa propre solidité et qu'elle ne sera jamais ébranlée, c'est-à-dire qu'elle n'aura jamais d'autre lieu que le centre de toute la matière." For the Biblical reference, see Psalm 24: "The earth is the Lord's, and the fullness thereof; the world, and they that dwell therein. For he hath founded it upon the seas, and established it upon the floods."

disposed according to their different weights, the lighter the higher, the heavier the lower: fire, for instance, "insofar as it is the lightest, is located in the highest place."[58] Following this argument, if the Earth was to move in space, it would have to be carried around by some heavier matter, which is impossible. The firmament is indeed made of a solid but ethereal and diaphanous element, thin enough to let the sunlight pass through it.[59] As is clear, the conceptual grounds of Simonin's physics and astronomy are Aristotelian: according to him, the heavens are incorruptible, and the elemental composition of the terrestrial world is qualitatively different from that of the skies. Moreover, Simonin overlooks all the classical objections against the Aristotelian-Ptolemaic cosmos, such as the telescopic observations of the sunspots and the irregularities of the lunar surface, the phases of Venus, and so forth.

Simonin's work on general physics and astronomy seems thus to be a revival of traditional ideas. Simonin, however, is original in linking the polemic against Newton to the critique of the heliocentric theory: he takes up arguments from the mechanical philosophy of his day[60] and frames them within a more traditional worldview, of a clear Aristotelian inspiration. This makes his approach not only anti-Newtonian but also, and more generally, anti-modern. Also, the central place of religious ideas in Simonin's papers is an important element, as it marks a difference with respect to the style of mainstream academic papers.[61]

Having expounded the contents of Simonin's unpublished papers, I will devote the last section of this contribution to the analysis of the Academy's reaction to Simonin's work. The systematic rejection of his works raises the question why this rather eccentric (and certainly minor) figure was excluded from the academic debate: are Simonin's arguments scientifically too weak, or is the methodology he follows too

[58] Simonin (1740b, f. 1v): "De manière que si plusieurs corps de même grandeur et figure se trouvent dans l'étendue avoir plus de pesanteur les uns que les autres, celui qui est le plus pesant se place au bas et les autres chacun au lieu qui convient a la proportion de leur légèreté, le feu comme le plus léger se trouve placé au dessus."
[59] Simonin (1740b, f. 1v): "De ce principe si la Terre était supportée hors de son centre, il faudrait que la matière qui la supporterait fût beaucoup plus pesante, ce que nous ne pouvons concevoir, en ce qu'une matière plus pesante placée dans l'étendue du firmament qui doit être lumineux, serait plus condensée, plus opaque et entièrement opposée a l'effet de la lumière propre à éclairer toute l'étendue qui se trouverait séparée de la Terre."
[60] In so doing, Simonin is part of a large group of authors who, without being *stricto sensu* Cartesians, take up the vocabulary of mechanical physics to undermine the conceptual validity of Newtonian attraction. On such debates, see Shank (2008).
[61] Simonin's anti-modernism and his religious attitude could suggest his proximity to the 18th century Jesuit tradition. However, there is no evidence of an actual connection between Simonin and the Society, as the Bayonne hydrography school, unlike other French hydrography schools, was not held by the Jesuits (Lamy, 2006, p. 96).

obsolete and non-canonical, or rather is a technician and practitioner considered inadmissible in the restricted circle of the learned savants?

Simonin and the Institution

In the paper *Pour soutenir la stabilité de la Terre contre le sentiment de Copernic* (1740b), Simonin insists on the fact that he has communicated the results of his research to no institution other than the Paris Academy, and seeks the Academy's approval to present his work also to the French universities, as the arguments against the heliocentric theory might be relevant to the defence of religion.[62] Likewise, in the very last text submitted to the institution in November 1740, Simonin affirms that he has completed a new astronomical model, and asks whether he should transmit it to the Academy, or directly to the King:

> This, Sirs, is the great discovery[63] I intend to communicate to you as soon as you will tell me whether I should send the astronomical plan to the King or to the Academy […].[64]

These examples show that Simonin profoundly respects the Academy as a research institution, and wishes to take part in its activities on a more official and regular basis. Not only does Simonin aim to acquire intellectual prestige, which the status of academician would grant him, but he is also deeply convinced of the originality and soundness – scientific, practical, and religious – of his ideas.

The Academy's reaction to Simonin's work, however, is far from encouraging. Simonin's papers were discussed in academic sessions, and the *procès-verbaux* give

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[62] Simonin (1740b, f. 4r): "C'est Messieurs ce que mon zèle le plus respectueux et le plus discret pour ne communiquer à personne ce que j'ai l'honneur de vous dire, m'a suscité de vous représenter; Il est vrai que comme l'Église fait un grand intérêt dans la contradiction du faux système de Copernic, je voudrais fort communiquer mes deux derniers mémoires à les Universités."

[63] Simonin (1740c, f. 1v): "Ainsi Messieurs si j'ai l'honneur de vous démontrer clair comme la lumière que les corps célestes n'ont point d'autre mouvement que celui d'Orient en Occident, je démontrerai la fausseté des prétendus mouvements d'Occident en Orient et de tout généralement ce qui est supposé de faire dans l'astronomie par leur moyen. Le mouvement de la Terre que Copernic nous assure se faire par deux de ces mouvements, sera donc évidemment faux puisque ces prétendus mouvements n'existent pas."

[64] Simonin (1740c, f. 1v): "Voilà Messieurs la grande découverte que je me propose de vous communiquer si tôt que vous m'aurez fait connaître si c'est au Roi ou à l'Académie que je dois présenter le plan astronomique […]."

interesting information about the academicians' opinion of Simonin's writings.[65] On February 26th, 1738, Simonin's *Mémoire contre la mesure conjecturale de l'étendue de la mer du sud par Monsieur Danville* is discussed. Dortous de Mairan and François Nicole had been asked to read the paper and give an overall judgment on the quality of Simonin's work. The report does not dismiss Simonin's practical expertise as irrelevant to the ongoing discussion on the shape of the Earth, nor it criticizes his argument as ill-grounded. Dortous de Mairan and Nicole, however, state that the practical operations described by Simonin are insufficient to settle the geodesic question: what is missing, although not explicitly stated, seems to be a discussion of the mathematical and physical aspects of the question.

> As the Academy has never thought that proofs drawn from the measure of routes and the breadth of seas, made by travellers and seafarers, are sufficient to deduce the oblong or flattened shape of the Earth, we do not think that Simonin's proofs, which rely on the same principle, are enough to prove the Earth's sphericity.[66]

The reviewers suggest therefore that Simonin and d'Anville discuss privately of the technical details of their measures: "We leave Mr Simonin and Mr d'Anville discuss the validity of their conjectures."[67]

On February 25th, 1739, Simonin's *Mémoire sur la nécessité de déterminer la véritable figure de la Terre et contre les sentiments du système de Newton* is analysed. The reveiwers are now Clairaut and Thury, and their judgment on Simonin's argument is strongly negative. Moreover, the report focuses almost exclusively on Simonin's critiques of Newton.

> He [Simonin] attacks the experiments made on the length of the pendulum, not as good geometers would do, since these experiments alone cannot determine what is the shape of the Earth, but because this instrument, he argues, cannot prove the diminution of gravity going from the poles to the equator. […] This

[65] The *procès-verbaux* are the reports of the academic sessions, compiled from 1667 onwards. They are preserved in the AAS, but also available on the Gallica database (gallica.bnf.fr).
[66] *Procès-verbaux des séances de l'Académie Royale des Sciences* (PV), 26 February 1738, unnumbered page.
[67] PV, 26 February 1738, unnumbered page.

sample of his theory allows us not to report the other things of this kind he says.[68]

Clairaut and Thury's report mentions *en passant* the arguments for the perfect roundness of the Earth drawn from Simonin's practice of navigation. On this point, they only comment that, "concerning the practical operations to determine the figure of the Earth, it is no better than the theory,"[69] but make no specific objections against Simonin's practical and technical arguments.

It is interesting to note that scientists from very different backgrounds, as Clairaut and Thury are, agree on judging Simonin's paper negatively, at least on the theoretical side. As a matter of fact, their report does not deny that Newton's theses can be subject to criticism; the report, however, does state that Simonin's critique is not good enough to meet the Academy's standards ("he attacks the experiments […] not as good geometers would do").[70]

The academicians' appraisal of Simonin's research becomes even more critical the next year, when the first paper against Newton and Copernicus is reviewed by mathematician Charles Étienne Louis Camus – who had taken part in Maupertuis's expedition to Lapland, and sided with the Newtonian faction – and by astronomer Giovanni Domenico Maraldi – the nephew of Giacomo Filippo Maraldi, and a supporter of the Cassini faction. In a report dated April 9th, 1740, Camus and Maraldi quote some excepts of Simonin's work on the heliocentric theory, and pronounce a devastating final judgment: "All these propositions are so false and unreasonable, that they do not deserve an answer."[71] Once again, scientists from different backgrounds and with opposing theoretical preferences agree on judging Simonin's work negatively. After Camus and Maraldi's report, there is no further record of academic discussion of the last two papers that Simonin had submitted to the institution in July and November of the same year.

Several members of the Academy, who had different – if not incompatible – intellectual perspectives and were on opposite sides in the current academic disputes, seemed to be in complete agreement on rejecting Simonin's papers. Which are the

[68] PV, 25 February 1739, unnumbered page.
[69] PV, 25 February 1739, unnumbered page.
[70] See the passage quoted above.
[71] PV, 9 April 1740, unnumbered page.

reasons behind this rejection? In what follows, I will put forward two hypotheses trying to account for the Academy's negative reaction to Simonin's papers.

The first motivation pertains to the timing of Simonin's intervention in the debate. As previously recalled, the controversy on the shape of the Earth became very heated on Maupertuis's coming back from Finland in 1737. Whereas the supporters of the flattened-Earth theory had enthusiastically declared the success of Newton's natural philosophy as a consequence of the results of Maupertuis's new measurements in Finland, the partisans of Cassini tried to foster a rhetoric of conciliation to save their faction from discredit.[72] Simonin came forward precisely at this stage of the debate (his first paper dates back to 1738). On the one hand, Simonin strongly criticizes Newton: it seems therefore likely that his work could not be regarded sympathetically by the supporters of the flattened-Earth theory, and of Newton's natural philosophy. On the other hand, while agreeing with Simonin on some critical aspects of Newtonian natural philosophy, Cassini and his faction could also be perplexed by a violent attack on their opponents, if one considers that, precisely at that time, they were deploying a strategy of reconciliation. Furthermore, Simonin openly sided with the elongated-Earth hypothesis, while the Cassini party was abandoning this option in favour of a flattened Earth.[73] In sum, although the scarce evidence left does not provide an explicit confirmation of this reading, it is not surprising that, if one considers the stage of the debate when Simonin submitted his work to the Academy, none of the factions were sympathetic to him, albeit on different grounds.

The main reason for the Academy's rejection of Simonin's work, however, is connected with the poorness of his theoretical knowledge of physics. While the academic reports do not reject Simonin's technical and practical arguments, but only denounce their insufficiency when used to prove a geodetic hypothesis, they explicitly dismiss his critique of Newton's (and Copernicus's) ideas. In fact, for any reader acquainted with the contents of the *Principia*, several objections raised by Simonin could have appeared problematic.

Let us consider for instance Simonin's objection from the depth of the sea at different latitudes (Simonin, 1739, f. 3v). Simonin argues that Newton's flattened-

[72] On the Newtonian perspective, see Beeson (1992, pp. 116-134) and Badinter (1999, pp. 71-186). On the stategy deployed by the members of the Cassini faction, see Thury (1744).

[73] As I have already mentioned in a previous section, in the *Méridienne de l'Observatoire Royal De Paris* Thury acknowledges that "the degrees of the meridian gets smaller while approaching the equator, and that therefore the Earth has a flattened shape" (1744, p. 25).

Earth theory relies on the alleged observation that the seas are deeper at the equator than at the poles. As far as the grounds of of Simonin's critique of Newton are concerned, the textual evidence for Simonin's objection comes from proposition 18 of the third book of the *Principia*, where Newton states that "if our Earth were not a little higher around the equator than at the poles, the seas would subside at the poles and, by ascending in the region of the equator, would flood everything there."[74] Newton, however, is not arguing here that the sea is deeper at the equator and that, therefore, the Earth is flattened at the poles. Rather, Newton claims that, by way of analogy with the shape of other celestial bodies (Jupiter) and in consequence of the daily rotation around its axis, the Earth is to be considered flattened at the poles. If the Earth was not flattened, the fluid on the surface of the Earth, "because of that circular motion [...] by receding from the axis, [would] endeavour to ascend in the region of the equator,"[75] thus flooding all the regions there. As this is not the case, the flattened-Earth hypothesis is further confirmed. The depth of the seas in different places on the Earth is therefore incidental to the main argument, which has more to do with the physical principles of Newtonian mechanics, and astronomical data.

This example suggests that Newton's demonstrations are in fact more sophisticated than Simonin makes them appear in his papers. This might therefore explain why the academicians were disappointed by the quality of Simonin's work, and conducted to dismiss it as "bad science," in spite of the possible interest of the technical and practical details contained in his writings. Ultimately, Simonin's lack of an in-depth knowledge of physics, as much as his unfamiliarity with mathematics, made it impossible for him to be received in the Academy as a relevant interlocutor.[76]

Conclusion

I have presented a wide selection[77] of the manuscript papers of the rather obscure hydrograph and navigator Yves Simonin. After sketching the relevant context, I

[74] Newton (1999, p. 821).
[75] Newton (1999, p. 821).
[76] On the Academy's mechanism to control the quality of its publications, see McClellan (2003). On the role of mathematical expertise in shaping the 18th century scientific community, see Gingras (2001) and (2003).
[77] Besides the manuscripts in the AAS, two other Yves Simonin manuscripts exist. In the University Library of the VU Amsterdam (Heijting, Catalogus hss UBVU, 568) one of Simonin's manuscript

expounded the contents of the papers and provided a critical analysis of the main arguments to be found therein. I then investigated the reasons for the negative reception of Simonin's work in the Academy, from both the institutional and the intellectual perspective.

The contribution Simonin makes to the debate on the shape of the Earth is that of a technician and practitioner: he uses references to first-hand experience and the practice of navigation to criticize mainstream arguments, while arguing on the same basis for more traditional hypotheses (the perfect roundness of the Earth). If considered from this perspective, Simonin's contribution is very original in the context of the academic debate on the shape of the Earth in the 1730s, where the reference to navigation is in general purely rhetorical, and the accent is rather put on mathematical and cosmological stakes. Simonin's practical approach, however, does not only lead to traditional ideas on the Earth's shape, but also on the structure and functioning of the Solar System: he is a convinced advocate of the geocentric theory, and a fierce critic of Copernicus and Newton. As several passages clearly show, Simonin's physical worldview is in fact deeply influenced by the Aristotelian tradition.

The traditional inspiration of Simonin's arguments, together with the poor knowledge of advanced physics, partly explains the Academy's rejection of his work. The other cause of Simonin's negative reception is connected to the evolution of the academic debate on the shape of the Earth, which was coming, after 1738, to a pacification: in this sense, the endorsement of a highly polemical and dissonant voice such as Simonin's would probably have contributed to reigniting the dispute.

Independently of the intrinsic scientific interest of the texts, the analysis of a case study as the one offered here is useful to see how academic debates are received and reshaped outside the institution, involving actors with different backgrounds and expertise. The understanding of the dynamics of scientific discussion within the Academy is thus completed and clarified by the analysis of the contributions of the actors working outside it, but interacting with it in manifold ways. In this sense, studies adopting the perspective of the "excluded," such as the one conducted here, can contribute to further clarify the origins and development of the 18th century

letters (1 folio) and the transcription of another of Simonin's letters by Joseph Nicolas Delisle (1 folio) – both addressed to Jean Baptiste Charles Bouvet de Lozier – are preserved. These letters rehearse the contents of the paper submitted to the Academy in 1738 (Simonin, 1738).

scientific community, as the result of the establishment of strict rules for participating in the scientific discussion, which becomes accessible only to a restricted number of experts.

Manuscripts

Archives of the Paris Academy of Sciences

- Pochettes de séances:

  February 1738 (Simonin 1738): *Mémoire présenté à Messieurs de l'Académie Royale des Sciences par le Sr Simonin Hydrographe à Bayonne, contre la mesure conjecturale de l'étendue de la mer du sud par Monsieur Danville géographe ordinaire du Roi*.

  February 1739 (Simonin 1739): *Mémoire présenté à Messieurs de l'Académie Royale des Sciences sur la nécessité de déterminer la véritable figure de la Terre et contre les sentiments du système de Newton*.

  March 1740 (Simonin 1740a): *Mémoire contre les sentiments de Newton sur la figure et le mouvement de la Terre*.

  April 1740 (Simonin 1740b): *Mémoire présenté à Messieurs de l'Académie Royale des Sciences à Paris par le Sieur Simonin ancien professeur d'hydrographie, pour soutenir la stabilité de la Terre contre le sentiment de Copernic*.

  November 1740 (Simonin 1740c): *Mémoire présenté a Messieurs de l'Académie Royale des Sciences pour prouver la stabilité de la Terre et la fausseté des prétendus mouvements des corps célestes d'Occident en Orient*.

- Procès-verbaux des séances de l'Académie Royale des Sciences: 1739, 1740.

Basel University Library, Bernoulli edition. Korrespondenz zwischen Johann I Bernoulli und Maupertuis.

University Library of the Vrije Universiteit Amsterdam. Heijting, Catalogus hss UBVU, 568.

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