DOI: 10.1111/1600-0498.12351

ARTICLE

Centaurus WILEY

Cartography, geodesy, and the heliocentric theory: Yves Simonin's unpublished papers

Marco Storni

Department of Philosophy and Cultural Heritage, Ca'Foscari University of Venice, Venezia, Italy

Correspondence
Marco Storni, Department of Philosophy and Cultural Heritage, Ca'Foscari University of Venice, Venezia, Italy.
Email: marco.storni@gmail.com

Funding information
Horizon 2020 Framework Programme, Grant/Award Number: GA n. 725883

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 a practitioner of and expert in navigational techniques. I then investigate the reasons behind the Academy's negative reception of Simonin's papers, casting light on the expertise required to participate in academic debates in the mid-18th century, and on the limits set by the institution to the possibility of external intervention.

KEYWORDS

heliocentric theory, Isaac Newton, navigation, Paris Academy of Sciences, shape of the Earth, Yves Simonin

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] So far, however, the part played by minor figures from outside the Academy in the history of the debate has rather been neglected. Such figures are worth considering as they embody forms of knowledge different from the mathematical or natural-philosophical expertise of the academicians. Furthermore, analysis of the academic reception of their contributions is useful in understanding the dynamics of participation in academic debates for nonstructured authors, as well as the limits set by the institution to the possibility of external interventions.

While the parts played by the salons, the learned circles, and the literary press in the process of appropriating and reshaping the question of the shape of the Earth outside the Academy have been investigated, the role of practitioners such as seafarers, cartographers, and hydrographers has been largely overlooked.[2] Among these practitioners, the figure of Yves Simonin, hydrography professor in Bayonne until 1739, is particularly relevant, though still largely unknown. Simonin was the first in a dynasty of hydrography professors, including his son Jean-François and his grandson Pierre.[3] All of them worked at the Bayonne hydrography school, which had been established in 1676 at the request of the local authorities.[4] Unlike most hydrography schools founded in France in the 17th and 18th centuries (in Nantes, Montpellier, Marseille, Toulouse, and so on), the Bayonne school was not run by the Jesuits.[5] The hydrography schools had an eminently practical function, namely, to provide students with some knowledge of navigational techniques, together with basic notions of astronomy and mathematics.[6] As reported in a paper sent to the Academy, Simonin had been studying geometry and astronomy for 45 years, while practicing navigation; his experience in technical schools and shipyards on the Atlantic Ocean was rounded out with decades of long-distance trips to India, China, and the Pacific Ocean (of which he underwent at least four).[7] Starting in 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, preserved only in the original manuscript versions 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.[8]

Simonin deals with essentially three topics in the papers he sent to the Academy: the rectification of navigational charts of the Southern Sea, the shape of the Earth calculated on the basis of measuring operations conducted in the Far East and in Europe, and the heliocentric theory.[9] While the first two questions were 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-Newtonian arguments, placing in opposition 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 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, as well as the intrinsic relevance of his scientific work, which I assess in Section 2. 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 analyze the reasons

[1]There are many valuable contributions to the study of the debate, including Bodenmann (2018); Brown (1976); Greenberg (1995; 1996); Passeron (1994); Shank (2008); Terrall (1992; 2002).
[2]On the cultural aspects of the debate on the shape of the Earth, see Terrall (1992; 2002).
[3]Duo (2002, p. 413); Larrieu (2016, p. 32).
[4]Duo (2002, p. 412).
[5]Lamy (2006, p. 96).
[6]Russo (1986, p. 420).
[7]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*, fol. 1r, Pochette de séance of February 1738, Archives of the Academy of Sciences, Paris, France (hereafter AAS); hereafter, this document will be referred to as Simonin (1738). It is also edited in d'Anville (1834, p. 587).
[8]d'Anville (1834, pp. 586–600). Simonin's paper is followed by d'Anville's reply (pp. 601–618).
[9]In the 18th century, the term "Southern Sea" referred 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.

for and legitimacy of this conceptual passage. How can the accurate determination of geographical distances and the arguments in favor of a perfectly spherical Earth demonstrate its stillness at the center of the universe, and thereby refute the Copernican theory? Why does Simonin bind together these seemingly unrelated questions? The last section of the paper is devoted to the Academy's reaction to Simonin's papers. The systematic rejection of his *mémoires* invites a reflection on the requirements to participate in academic debates, in terms of the expertise demanded of those seeking to intervene in discussions on natural-philosophical and mathematical problems. Additionally, the case of Simonin casts light on the importance attributed to some scientific beliefs, including the heliocentric theory, which any author had to hold in order to be considered a qualified interlocutor by the Academy.

1 | 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 this coveted result.[10] Under the auspices of Colbert, Jean Picard inaugurated the operations for mapping 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 in 1683, Gian Domenico Cassini, an academician and first director of the Observatory, led new operations that consisted of measuring a longer meridian arc 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." For a general map of France, therefore, a longer arc needed 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."[11] 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, 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 (released in 1720), Jacques Cassini presents the details of the operations, including the techniques and tools employed in measuring the meridian arc. Interestingly, in the second part of the text, Cassini deduces the shape that the Earth must have from the inequality between the degrees of the French meridian. The difference between the Dunkirk–Paris and Paris–Perpignan distances 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 degrees are smaller when approaching, and larger when moving away from the poles.[12]

[10]On the political relevance of cartography and related practices, see Petto (2007; 2015).
[11]Cassini (1733, pp. 390, 391).
[12]Cassini (1733, p. 238).

The results of Cassini's measurements, however, were far from 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 employing highly reliable physical concepts, had been formulated. The empirical evidence itself went back to Jean Richer's expedition to Cayenne, French Guiana, in 1672–1673. Richer discovered that the length of the seconds pendulum—that is, the length of a pendulum whose period is precisely 1 s—was shorter in Cayenne than in Paris.[13] In physical terms, Richer's discovery implied that gravity is less strong near the equator than at the northernmost latitudes. Relying on the Cayenne pendulum experiment, Newton, in the third book of the *Principia* (Propositions 18–20), formulates a hypothesis on the Earth's shape, which comes as a corollary of his general theory of attraction. The Earth, according to Newton, must be slightly flattened at the poles, with 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, endeavor 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.[14]

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

> 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 center; 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.[15]

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

As the theory of attraction began to be discussed in the Academy, early advocates of Newtonian physics in France, such as Pierre-Louis Moreau de Maupertuis, insisted that the question of the Earth's shape was central to establishing the superiority of Newton's natural philosophy over that of Descartes.[16] 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.[17] The academic debate on the shape of the Earth in the 1720s and 1730s, therefore, concerned not only the techniques and tools, mathematical and material, for solving the geodetic question, but also the cosmological principles according to which hypotheses on the shape of the Earth were framed, namely planetary vortices and universal gravitation.

The persistence of intellectual disagreement within the Academy, as well as the practical necessity of continuing with the mapping of France, impelled the French institutions to set up two scientific expeditions to measure one meridian arc across the north pole and one close to the equator. A comparison of degrees of the meridian at different

[13]On Richer's expedition to Cayenne and its scientific relevance, see Olmsted (1942).
[14]Newton (1999, p. 821).
[15]Newton (1999, pp. 826–827).
[16]See, for instance, the first chapter of Maupertuis's *Discours sur les différentes figures des astres*, originally published in 1732: Maupertuis (1768a, pp. 81–89).
[17]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).

latitudes would have been a reliable test to tell which geodetic hypothesis was correct.[18] A first contingent of scientists led by Charles-Marie de La Condamine left for Ecuador in 1735, but only returned in 1745, too late to play a significant role in the controversy.[19] A second expedition directed by Maupertuis left for Finland in 1736, and completed the operations in less than a year. The results that emerged from Maupertuis's measurements were clear: 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 his report to the Academy:

> Our degree … differs by 950 toises 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 results of the expedition to Finland, however, did not persuade all academicians to embrace the flattened-Earth hypothesis, let alone Newtonian celestial mechanics. The Cassini faction's attacks on the Newtonians became all the more rough, as they insistently 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) obtained 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 the measure of a meridian arc at more southerly latitudes (as La Condamine's expedition was not yet back from Ecuador), 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.[22] With instruments similar to those that Maupertuis employed, Thury was able to correct Gian Domenico's 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."[23] 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 the respective supporters of Descartes and Newton in the Academy. However, Thury's attempt to

[18]As Newton's disciple John Theophilus Desaguliers (1724, p, 209) points out in his *Dissertation Concerning the Figure of the Earth*, which circulated widely among 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."

[19]On La Condamine's expedition, see Safier (2008).

[20]Maupertuis (1768b, pp. 167–168). Emphasis in the original text. One toise is equivalent to 0.3266 m or 0.3572 yards.

[21]This argument was first brought up by Johann Bernoulli I, in a letter sent to Maupertuis before his departure for Finland: "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." Johann I Bernuolli to Maupertius [Letter] (1835, May 8), fol. 2v, SIGN: L Ia 662, Nr.35, Basel University Library, Basel, Switzerland.

[22]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 earlier (1725–1728), with the same sector, James Bradley had discovered the aberration of light.

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

tone down the disagreement on the shape of the Earth, and indirectly that on cosmological principles, was not totally successful, as critiques and personal attacks continued to be published even after 1740.[24]

2 | SIMONIN ON CARTOGRAPHY AND GEODESY

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 had already presented another paper 1 year earlier, 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.[25]

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).[26] D'Anville was 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.[27] In the *Mesure conjecturale*, d'Anville rectifies the widely accepted measurement of the equator by reducing it by 440 leagues.[28] This rectification is based on a new calculus of the distance between Lima and Manila using astronomical observations, and verified by determining the latitude of other sites, such as Peking (Beijing). D'Anville also refers to the work Cassini 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 remains consistent with the initial hypothesis, according to which the degrees of latitude are wider towards the equator than at the poles.[29]

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 favor of Cassini's elongated-Earth hypothesis:

[24]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).

[25]Simonin (1738, fol. 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.”

[26]Simonin's contribution to the debate is represented by five papers in total. The first was submitted to the Academy in February 1738 and is entitled *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* (Simonin, 1738). The second, presented in February 1739, is the *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* (1739), Pochette de séance of February 1739, AAS; hereafter, this document will be referred to as Simonin (1739). Simonin then submitted three other papers in 1740: *Mémoire contre les sentiments de Newton sur la figure et le mouvement de la Terre* (1740), Pochette de séance of March 1740, AAS; *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* (1740), Pochette de séance of April 1740, AAS—hereafter, this document will be referred to as *Pour soutenir la stabilité de la Terre* (1740); and *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* (1740), Pochette de séance of November 1740, AAS.

[27]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; 2017).

[28]One league corresponds to 3.452 miles or 5.556 km; 440 leagues are therefore 1,519.03 miles or 2,444.64 km.

[29]d'Anville (1834, p. 574).

> 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.[30]

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 his personal experience as a seafarer and the high reliability of old charts. When seafarers need to go through the Sunda Strait to reach China or Batavia (Jakarta), the knowledge derived from practice and the indications in Pieter Goos's maps of the Indian and Pacific Oceans provide enough information to find the correct route through the strait.[31] Back in 1698, Simonin had embarked on a ship with the goal of finding 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' astronomical 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 dying."[32] As this 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—just as the Jesuits' corrections had been for Simonin and his fellows.

The second argument that Simonin presents focuses 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 who did travel to South America and the Philippines. Simonin, on the contrary, had direct experience of the Southern Sea and of navigational routes in the Pacific Ocean:

> For 45 years I have been continuously practicing 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.[33]

Simonin's scientific expertise does not consist of the bookish culture of a learned man or an academician, but of the experience and know-how of a practitioner. First-hand knowledge, acquired through direct experience and practice, is presented as better evidence than second-hand notions obtained from 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 elongated Earth was getting increasingly heated, Simonin defended the most classical hypothesis of a perfectly spherical Earth. Interestingly, both 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

[30]Simonin (1738, fol. 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.

[31]Pieter Goos, a famous Dutch cartographer of the 17th century, is the only reference explicitly quoted in Simonin's paper.

[32]Simonin (1738, fol. 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."

[33]Simonin (1738, fols. 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."

based on the false belief that the Earth is perfectly spherical; once Cassini had proven this geodetic hypothesis to be wrong, then the measurement of the equator should be emended too. Simonin, drawing on 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 that work best, that is, it must be perfectly spherical.

> By virtue of these very natural reasons [the arguments against d'Anville's rectification of the equator presented above], 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.[34]

While he is critical of both the elongated- and flattened-Earth hypotheses, Simonin is not equally critical of both Cassini and 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 on the shape of the Earth, praises the accuracy of the measurements of the French meridian. Simonin commends Cassini's work for providing "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."[35] The measurements that navigators elaborated empirically, during their travels all over the world, are in fact "confirmed in all possible regards by Cassini's just observations."[36] Simonin's appraisal of Cassini's experimental results is rooted in the 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 not backed by work in the field.[37]

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 which one should infer the perfect roundness of the Earth.[38]

[34]Simonin (1738, fol. 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."

[35]Simonin (1739, fol. 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."

[36]Simonin (1739, fol. 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."

[37]This highlights a difference from 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."

[38]Simonin (1739, fol. 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."

Simonin claims that, if correctly interpreted, Cassini's results confirm the hypothesis of a perfectly spherical Earth. Simonin's major polemical target is not in fact Cassini, but Newton. As I argue in the next section, Simonin does not merely reject Newton's flattened-Earth hypothesis, but the very principles of Newtonian natural philosophy. Simonin's anti-Newtonian polemic was probably motivated by his sense of Maupertuis's impending success in the academic dispute, and the corresponding urgency of finding a way to prove that Newtonian natural philosophy was groundless. Contrary to other academicians, however, Simonin does not contrast Newton with arguments inspired by Malebranche or Leibniz, but goes back to more traditional sources, from Aristotle to Descartes.[39]

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 deductions on the Earth's shape that were formulated on the basis of longitude measurements alone.

Secondly, Simonin emphasizes the close connection between determining 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.”[40] 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 underline the practical utility of geodetic discussions. As Maupertuis writes in the report on the voyage to Finland presented to the Academy in 1738, *Relation du voyage fait par ordre du Roi au cercle polaire, pour déterminer la figure de la Terre*:

> 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.[41]

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 seafarer and expert in navigation techniques. As part of a long tradition of artisanal knowledge seeking recognition and epistemic autonomy vis-à-vis more theoretical forms of knowledge, Simonin insists on the primacy of direct experience and on the higher reliability of knowledge acquired through doing over that derived from the mere study of other people's experiences.[42] Simonin also seems to depreciate abstract reasoning—mathematical, and more generally theoretical speculation—preferring the immediate and concrete nature of practice.

[39]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; 2008); Shea (1988).
[40]d'Anville (1834, p. 602).
[41]Maupertuis (1768b, pp. 83–84).
[42]On the early modern tradition of artisanal knowledge and the importance of bodily experience, see Smith (2004); Roberts, Schaffer, & Dear (2007). For a specific contribution on the interactions between the spheres of navigational and cosmographical knowledge in the early modern age, see Almeida (2019).

3 | SIMONIN AGAINST NEWTON (AND COPERNICUS)

In the paper Simonin presented to the Academy in February 1739, he tries to prove not only that 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 he criticizes in turn, with the ultimate goal of proving that "the system of attraction is in fact only grounded in imagination."[43] The first objection is a more general one, and is meant to undermine the very conceptual basis of Newton's physics. Simonin argues that there is no need to introduce any principle of attraction to account for bodies falling towards the center 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 which 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.[44]

Simonin criticizes the idea of understanding the acceleration towards the center by means of the principle of attraction, as the notion of impulsion is enough to make sense of the behavior 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 behavior of pendulums at different latitudes. Newton's mistake would have been, in this case, to consider the behavior of pendulums as relevant information on the gravity of bodies. Simonin objects that, since the pendulum does not approach the center 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."[45] 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 Norwood in the 1630s, which Newton quotes in 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."[46]

With the fourth and last argument, Simonin not only criticizes Newton's flattened-Earth hypothesis, but also brings into the discussion the question of 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.[47] 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 the Earth's revolution and its flattened shape (the action of a central force on material masses moving inertially) to one and the same cause. Simonin holds that if one

[43]Simonin (1739, fol. 2v): "On verra que le système de la gravitation n'en a d'autre [de fondement] que celui de l'imagination."

[44]Simonin (1739, fol. 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."

[45]Simonin (1739, fol. 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."

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

[47]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).

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 off the coast of France, which clearly shows that Newton's assumption is erroneous.

The objection about the seas' depths is not the only one Simonin provides against Newton's cosmology. In three subsequent papers, all published in 1740, Simonin formulates direct arguments to contradict 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 was fairly uncommon.[48] This makes Simonin's case study exceptional in the panorama of the "Newton wars" in France.[49] Among the arguments proposed against the heliocentric theory, two seem particularly relevant in understanding Simonin's astronomical and philosophical perspective. 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.[50]

> 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.[51]

Simonin, however, contends that the idea of a great distance between the Earth and the Sun is not grounded in any convincing experimental proofs. In 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 and come to very different results, demonstrating the impossibility of knowing the true distance between the two bodies. Insofar as there is no certain knowledge of the position of planets in space, Simonin feels it is best to stick to the clearest data of first-hand experience, which clearly privileges the stillness of the Earth at the center 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, expanding the boundaries of the cosmos allows for the possibility that faraway stars might be other suns, with other sets of planets revolving around them.[52] 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 overthrew the privileged position of the Earth and mankind in the structure of the universe. In his papers, Simonin repeatedly insists on the

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

[49]I borrow the term "Newton wars" from Shank (2008).

[50]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 *De revolutionibus*, Book 1, Chapters 6, 10 (Copernicus, 1978, pp. 13-14, 18-22).

[51]*Pour soutenir la stabilité de la Terre* (1740, fol. 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."

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

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." Likewise, the geocentric hypothesis is presented as "the most wondrous and less dangerous for the soul and for religion."[53] The steadiness of the Earth at the center 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 center of all matter."[54]

In this last passage, Simonin hints at the place where the Earth would be designed to lie, at the center 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 center towards which all of its parts tend, in proportion to their weight. In the structure of the world, material elements are 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."[55] Following this argument, if the Earth were 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.[56] 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.[57]

Simonin's work on general physics and astronomy thus seems 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 and frames them within a more traditional worldview, with a clear Aristotelian inspiration.[58] This makes his approach not only anti-Newtonian, but also, and more generally, anti-modern. Also, the centrality 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.[59]

Having expounded the contents of Simonin's unpublished papers, I will devote the last section of this contribution to an analysis of the Academy's reaction to Simonin's work. The systematic rejection of his works raises the question of why he was excluded from the academic debate: were Simonin's arguments too weak scientifically, or was the methodology he employed too obsolete and non-canonical, or rather was a seafarer considered inadmissible to the restricted circle of the learned savants?

[53]Simonin (1740), *Mémoire contre les sentiments de Newton sur la figure et le mouvement de la Terre*, Pochette de séance of March 1740, AAS: "Copernic ce fameux novateur contemporain de Luther et peut être d'intelligence contre la Religion" (fol. 1v); "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" (fol. 2v).

[54]*Pour soutenir la stabilité de la Terre* (1740, fol. 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."

[55]*Pour soutenir la stabilité de la Terre* (1740, fol. 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 à la proportion de leur légèreté, le feu comme le plus léger se trouve placé au-dessus."

[56]*Pour soutenir la stabilité de la Terre* (1740, fol. 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 à l'effet de la lumière propre à éclairer toute l'étendue qui se trouverait séparée de la Terre."

[57]Simonin's Aristotelianism reflects an approach that still survived in natural-philosophical teachings in French universities until the 18th century. As in Simonin's case, Aristotelian notions were there combined with ideas from other traditions, most notably the Cartesian. See Brockliss (1987) and, on Jesuit colleges, Romano (2010).

[58]In so doing, Simonin is part of a heterogeneous 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).

[59]There are no available sources from which to learn more about Simonin's religious beliefs. Simonin's anti-modernism and his religious attitude could suggest a proximity to the 18th-century Jesuit tradition. However, there is no evidence of an actual connection between Simonin and the Society, since, as I have stressed above, the Bayonne hydrography school, unlike other French hydrography schools, was not run by the Jesuits (Lamy, 2006, p. 96).

4 | SIMONIN AND THE INSTITUTION

In the paper *Pour soutenir la stabilité de la Terre contre le sentiment de Copernic* (1740), 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 defense of religion.[60] 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 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."[61]

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

The Academy's reaction to Simonin's work, however, was far from encouraging. Simonin's papers were discussed in academic sessions, and the *procès-verbaux* give interesting information about the academicians' opinion on Simonin's writings.[62] On February 26, 1738, Simonin's *Mémoire contre la mesure conjecturale de l'étendue de la mer du sud par Monsieur Danville* was discussed. Jean-Jacques Dortous de Mairan, a Cartesian natural philosopher, and François Nicole, a skilled mathematician interested in algebra and infinitesimal calculus, 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 does it criticize his argument as poorly grounded. Mairan and Nicole, however, affirm 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 travelers 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.[63]

At this stage, Simonin's work had not been rejected as inconsistent, but it was not considered relevant to the ongoing debate on the shape of the Earth. Since, according to the reviewers, the subject of the memoir is eminently

---

[60] *Pour soutenir la stabilité de la Terre* (1740, fol. 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."

[61] Simonin (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*, Pochette de séance of November 1740, AAS: "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" (fol. 1v). In the same paper, Simonin provides a description of his "great discovery": "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" (fol. 1v).

[62] 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).

[63] *Procès-verbaux des séances de l'Académie Royale des Sciences* (hereafter PV) (1738, Feb. 26), AAS. The 1738 collection of *procès-verbaux* is not available online and is only consultable at the Archives of the Paris Academy of Sciences.

of a technical nature, they suggest that Simonin and d'Anville discuss the technical details of their measures privately: “We leave Mr Simonin and Mr d'Anville discuss the validity of their conjectures.”[64]

On February 25, 1739, Simonin's *Mémoire sur la nécessité de déterminer la véritable figure de la Terre et contre les sentiments du système de Newton* was analyzed. The reviewers on this occasion were Newtonian mathematician Alexis-Claude Clairaut and Gian Domenico Cassini's grandson César-François Cassini de Thury. Their judgment on Simonin's argument was strongly negative. Significantly, their 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 sample of his theory allows us not to report the other things of this kind he says.[65]

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,” but make no specific objections to Simonin's practical and technical arguments.[66]

It is interesting to note that scientists from very different backgrounds, as Clairaut and Thury are, agree on a negative judgment of Simonin's paper, at least from a theoretical perspective. In fact, their report does not deny that Newton's theses can be subject to criticism; the report, however, states that Simonin does not criticize Newton “as good geometers would do.” The reviewers seem to imply that, when Simonin ventures into natural philosophy—as shown by his critique of the pendulum experiments—his arguments are flawed, and therefore unacceptable in the context of academic debate.

The academicians' appraisal of Simonin's research became even more critical the next year, when the first paper against Newton and Copernicus was 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 9, 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.”[67] This report marks a discontinuity from the preceding evaluations of Simonin's work by the Academy readers. Simonin's argument is dismissed as utterly unreasonable, and he is no longer regarded as a relevant interlocutor. As a matter of fact, 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.

The rejection of Simonin's work is an interesting case to reflect on the boundaries set by the Academy to participation in ongoing debates around 1740, but also on the ideas that the Academy considered inadmissible in a scientific discussion, which would have led to ostracism from the community. With regard to the first point, all academicians compiling the reports seemed to agree that, when discussing natural-philosophical problems, Simonin ventured outside his domain of expertise. In fact, reviewers did not pronounce any negative judgment on Simonin's arguments drawn from navigation and personal experience, but disapproved his conclusions on the shape of the Earth and his critique of Newton's and Copernicus's theories.

That the academicians considered it problematic for someone with an expertise in navigation and hydrography to venture into natural-philosophical speculations is further confirmed by coeval reports on other papers sent to the

[64]PV (1738, Feb. 26), AAS.
[65]PV (1739, Feb. 25), AAS (Also: gallica.bnf.fr/ark:/12148/bpt6k55734f).
[66]PV (1739, Feb. 25), AAS.
[67]PV (1740, Apr. 9), AAS (Also: gallica.bnf.fr/ark:/12148/bpt6k55735s).

Academy in which systematic theories were deduced from navigational data. One example is Louis-François Delisle de La Drevetière's *Mémoire où l'on fait voir la facilité qu'il y a de faire l'épreuve de ma découverte des longitudes* (1740). In their report on this paper, astronomers Jacques Cassini and Jean-Paul Grandjean de Fouchy stated that “despite all the reasons he [La Drevetière] adduces to establish this system, we do not find it convincing.” Moreover, the theoretical nature of La Drevetière's proposal entailed that “navigation cannot derive any utility from his work.”[68] Conversely, merely technical proposals such as Philippe Buache's *Carte des terres australes* (1739) were favorably received as they were considered theoretically unbiased, and were declared useful to the public.[69]

These considerations suggest that Simonin was excluded from the academic controversy on the shape of the Earth because he was considered unfit to contribute in a debate outside his field of expertise. The Academy might have estimated Simonin's contribution as worthy of consideration (and publication) if he had focused only on navigational techniques and empirical measures. In all likelihood, the negative judgment and consequent rejection of Simonin's work came from his presumption in participating in a discussion that required an expertise in natural philosophy and mathematics, which he—based on what is stated in the academic reports—was deemed not to possess.

A second aspect of Simonin's rejection worth investigating is the escalation in the Academy's reaction to his work. Whereas Simonin's contributions were initially criticized for their natural-philosophical contents, but did not question his knowledge of navigational techniques or his experience as a seafarer, by 1740 his work was utterly dismissed, leading to his ostracism from the academic community. It would seem that the academicians gave up on constructive criticism as soon as Simonin advanced positions that were perceived as totally absurd, and which therefore did not require any reply. The unreasonableness that the Academy attributed to Simonin is most likely related to his rejection of the heliocentric theory.

Indeed, in the mid-18th century, the Copernican view was no longer controversial in French scientific culture. As Jean le Rond d'Alembert stresses in the article *Copernic* of the *Encyclopédie* (1754), “in France we support the Copernican system without any fear, and we are persuaded … that this system is not contrary to faith. … Nowadays there is no knowledgeable and good faith astronomer who could possibly casts doubt on it.”[70] It is clear that, for the Paris academicians, the rejection of the heliocentric theory amounted to a complete scientific delegitimization. At the moment Simonin challenged such a widely shared belief and argued for the reintroduction of the geocentric model, the possibility of a dialogue between him and the institution faded away.

The analysis of the escalation in the Academy's responses to Simonin's papers suggests that, beyond the importance of expertise, there were some more general criteria determining who could take part in academic debates. These criteria corresponded to the acceptance of certain basic beliefs, such as the heliocentric theory, that might define a scientist as, in d'Alembert's words, knowledgeable and in good faith.

5 | CONCLUSION

I have presented a wide selection of the manuscript papers of the hydrographer and navigator Yves Simonin.[71] After sketching the relevant context, I 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.

[68]PV (1740, Jul. 23), AAS.

[69]PV (1739, Sept. 5). AAS. The report, signed by de Fouchy and naturalist Georges-Louis Leclerc de Buffon, states that Buache's *Carte des terres australes* only provided information about “the new observations made to the south of the Cape of Good Hope.” The work, according to the reviewers, therefore deserved “to be communicated to the public, which will hopefully receive it positively.”

[70]d'Alembert (1754, p, 174b). Although this text was published a decade after the *affaire* Simonin, the perception of the Copernican theory was most likely the same in the 1740s as at the time d'Alembert was writing.

[71]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 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.

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, when references to navigation were in general purely rhetorical and participants instead stressed the mathematical and cosmological stakes. Simonin's practical approach, however, leads not only 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.

Simonin's attempt to participate in a debate on natural-philosophical questions, such as the one on the shape of the Earth, while being judged by the Academy to lack a relevant expertise, explains the rejection of his papers from 1738 to 1740. The utter dismissal of his work after this date coincides with his formulation of critiques against the heliocentric theory and defense of the geocentric view. At this stage, the academicians no longer considered Simonin to be a relevant interlocutor, as he supported ideas that were perceived as totally absurd in the academic circle.

Independently of the intrinsic scientific interest of the texts, the analysis of a case study such as is offered here is useful in revealing how academic debates were received and reshaped outside the institution, involving actors with different backgrounds and expertise. An understanding of the dynamics of scientific discussion within the Academy is thus completed and clarified by an analysis of the contributions of actors working outside it, trying to interact with it in various ways. In this sense, studies focusing on the "excluded," such as the one conducted here, can contribute to further clarifying the origins and development of the 18th-century scientific community as the result of the establishment of strict rules for participating in the scientific discussion, which thus became accessible only to a limited number of experts.

ACKNOWLEDGEMENTS

This article is part of a project that has received funding from the European Union's Horizon 2020 Research and Innovation Programme (GA n. 725883 ERC–EarlyModernCosmology). I thank the archives of the Paris Academy of Sciences for providing access to the manuscripts. I am grateful to the editors of *Centaurus* and the anonymous referees for their extensive feedback and constructive criticism.

ORCID

Marco Storni https://orcid.org/0000-0003-2500-7607

REFERENCES

Almeida, B. (2019). Transmitting nautical and cosmographical knowledge in the 16th and 17th centuries: The case of Pedro Nunes. *Centaurus*, *60*(3), 216–229.

Badinter, E. (1999). *Les passions intellectuelles. I. Désirs de gloire (1735–1751)*. Paris, France: Fayard.

Beeson, D. (1992). *Maupertuis: An intellectual biography*. Oxford, England: The Voltaire Foundation.

Bodenmann, S. (2018). Empiricism as a rhetoric of legitimation: Maupertuis and the shape of the earth. In S. Bodenmann & A.-L. Rey (Eds.), *What does it mean to be an empiricist? Empiricisms in eighteenth century sciences* (pp. 87–119). Cham, Switzerland: Springer.

Brockliss, L. W. B. (1987). *French higher education in the seventeenth and eighteenth centuries: A cultural history*. Oxford, England: Clarendon Press.

Brown, H. (1976). *Science and the human comedy: Natural philosophy in French literature from Rabelais to Maupertuis*. Toronto, Canada: University of Toronto Press.

Cassini, J. (1733). De la carte de la France, et de la perpendiculaire à la méridienne de Paris. *Mémoires de l'Académie Royale des Sciences*, 389–405.

Copernicus, N. (1985). In P. Czartoryski (Ed.), *Minor works*. London, England: Macmillan.

Copernicus, N., Dobrzycki, J. & Rosen, E. (1978). *Nicholas Copernicus on the Revolutions*, (vol. 2). London, England: Macmillan.

d'Alembert, J. L. R. (1754). Copernic, système ou hypothèse de Copernic. In D. Diderot & J. Le R. d'Alembert (Eds.), *Encyclopédie ou Dictionnaire raisonné des sciences, des arts et des métiers* (Vol. 4, pp. 173–174). Paris, France: Briasson et al.

d'Anville, J. B. B. (1834). *Œuvres* (Vol. 1). Paris, France: Imprimerie Royale.

Del Prete, A. (1998). *Universo infinito e pluralità dei mondi. Teorie cosmologiche in età moderna*. Napoli, Italy: La Città del Sole.

Desaguliers, J. T. (1724). A dissertation concerning the figure of the earth. *Philosophical Transactions*, *33*, 201–222.

Duo, G. (2002). L'enseignement de la science nautique en Labourd au XVIII^{e} siècle. *Zainak*, *21*, 411–418.

Ferreira Furtado, J. (2011). Guerra, diplomacia e mapas: a Guerra da Sucessão Espanhola, o Tratado de Utrecht ea América portuguesa na cartografia de D'Anville. *Topoi: Revista de História*, *12*(23), 66–83.

Ferreira Furtado, J. (2013). Mapping mythical and imaginary places in d'Anville's *Carte de l'Amérique meridionale*. *Journal of Earth Science and Engineering*, *3*, 714–721.

Ferreira Furtado, J. (2017). Evolving ideas: J. B. d'Anville's maps of Southern Africa, 1725–1749. Imago Mundi. *The International Journal for the History of Cartography*, *69*(2), 202–215.

Greenberg, J. L. (1995). *The problem of the Earth's shape from Newton to Clairaut. The rise of mathematical science in eighteenth-century Paris and the fall of "normal" science*. Cambridge, England: Cambridge University Press.

Greenberg, J. L. (1996). Isaac Newton and the problem of the Earth's shape. *Archive for History of Exact Sciences*, *49*(4), 371–391.

Guerlac, H. (1981). *Newton on the continent*. Ithaca, NY: Cornell University Press.

Hankins, T. (1970). *Jean d'Alembert: Science and the Enlightenment*. Oxford, England: Clarendon Press.

Lamy, J. (2006). Le problème des longitudes en mer dans les traités d'hydrographie des Jésuites aux XVII^{e} et XVIII^{e} siècles. Choix méthodologiques et pratiques instrumentales. *Histoire & Mesure*, *21*(2), 95–120.

Larrieu, P.-Y. (2016). Luttes juridiques pour la tutelle des écoles d'hydrographie, à l'occasion de l'expulsion des Jésuites, en particulier dans les villes de La Rochelle, Nantes, Rouen et Bayonne (1760–1785). *Cahiers François Viète*, *2*(8–9), 13–35.

Mairan, J.-J. D. d. (1720). Recherches géométriques sur la diminution des degrés terrestres en allant de l'équateur vers les pôles. *Mémoires de l'Académie Royale des Sciences*, 231–276.

Maupertuis, P.-L. M. d. (1768a). *Œuvres* (Vol. 1). Lyon, France: Bruyset.

Maupertuis, P.-L. M. d. (1768b). *Œuvres* (Vol. 3). Lyon, France: Bruyset.

Newton, I. (1999). In I. B. Cohen & A. Whitman (Eds.), *The Principia: Mathematical principles of natural philosophy*. Berkeley, CA: University of California Press.

Olmsted, J. W. (1942). The scientific expedition of Jean Richer to Cayenne (1672–1673). *Isis*, *34*(2), 117–128.

Passeron, I. (1994). *Clairaut et la figure de la Terre au dix-huitième siècle: Cristallisation d'un nouveau style autour d'une pratique physico-mathématique* (Doctoral dissertation, University of Paris 7 Diderot, Paris, France).

Petto, C. M. (2007). *When France was king of cartography: The patronage and production of maps in early modern France*. Lanham, MD: Lexington.

Petto, C. M. (2015). *Mapping and charting in early modern England and France: Power, patronage, and production*. Lanham, MD: Lexington.

Roberts, L., Schaffer, S., & Dear, P. (2007). *The mindful hand: Inquiry and invention from the late Renaissance to early industrialisation*. Chicago, IL: The University of Chicago Press.

Romano, A. (2010). Les jésuites et la science moderne. Contribution à l'analyse de l'antijésuitisme scientifique des Lumières. In P.-A. Fabre & C. Maire (Eds.), *Les antijésuites. Discours, figures et lieux de l'antijésuitisme à l'époque moderne* (pp. 329–349). Rennes, France: Presses universitaires de Rennes.

Russo, F. (1986). L'hydrographie en France aux XVII^{e} et XVIII^{e} siècles. Écoles et ouvrages d'enseignement. In R. Taton (Ed.), *Enseignement et diffusion des sciences en France au XVIII^{e} siècle* (pp. 419–440). Paris, France: Hermann.

Safier, N. (2008). *Measuring the New World: Enlightenment science and South America*. Chicago, IL: The University of Chicago Press.

Shank, J. B. (2003). On the alleged Cartesianism of Fontenelle. *Archives Internationales d'Histoire des Sciences*, *53*(150–151), 139–156.

Shank, J. B. (2004). There was no such thing as the "Newtonian Revolution," and the French instituted it: Eighteenth-century mechanics in France before Maupertuis. *Early Science and Medicine*, *9*, 257–292.

Shank, J. B. (2008). *The Newton wars and the beginning of the French Enlightenment*. Chicago, IL: The University of Chicago Press.

Shea, W. R. (1988). The unfinished revolution: Johann Bernoulli (1667–1748) and the debate between the Cartesians and the Newtonians. In W. R. Shea (Ed.), *Revolutions in science* (pp. 70–92). Canton, MA: Science History Publications.

Smith, P. H. (2004). *The body of the artisan: Art and experience in the Scientific Revolution*. Chicago, IL: The University of Chicago Press.

Terrall, M. (1992). Representing the Earth's shape: The polemics surrounding Maupertuis's expedition to Lapland. *Isis*, *83*(2), 218–237.

Terrall, M. (2002). *The man who flattened the earth: Maupertuis and the sciences in the Enlightenment*. Chicago, IL: The University of Chicago Press.

Thury, C.-F. C. d. (1744). *La méridienne de l'Observatoire Royal de Paris, vérifiée dans toute l'étendue du royaume par de nouvelles observations. Pour en déduire la vraie grandeur des degrés de la Terre, tant en longitude qu'en latitude, et pour y assujettir toutes les opérations géométriques faites par ordre du Roi, pour lever une carte générale de la France*. Paris, France: Guerin.

How to cite this article: Storni M. Cartography, geodesy, and the heliocentric theory: Yves Simonin's unpublished papers. *Centaurus*. 2021;63:192–209. https://doi.org/10.1111/1600-0498.12351