Huygens's 1688 Report to the Directors of the Dutch East India Company on the Measurement of Longitude at Sea and the Evidence it Offered Against Universal Gravity

Abstract

When Christiaan Huygens prepared the 1686/1687 expedition to the Cape of Good Hope on which his pendulum clocks were to be tested for their usefulness in measuring longitude at sea, he also gave instructions to Thomas Helder to perform experiments with the seconds-pendulum. This was prompted by Jean Richer's 1672 finding that a seconds-pendulum is 1 1/4 lines (2.8 mm) shorter in Cayenne than in Paris. Unfortunately, Helder died on the voyage, and no data from the seconds-pendulum ever reached Huygens. He nevertheless did receive data from his clocks on the return-voyage from the Cape of Good Hope to Texel. When he first calculated the ship's course according to these data, it appeared to have gone straight through Ireland. He then tried introducing a correction to the data, based on an idea he had previously entertained as a possible explanation of Richer's finding: he corrected the observed time to compensate for a reduction in the effect of gravity from the Poles to the Equator resulting purely from the Earth's rotation. His newly calculated course convinced him that this rotational effect is the sole source of any variations in gravity with latitude. This paper examines Huygens's corrections to the data and his reasoning from the new course to the conclusion that nothing else causes a variation in gravity. In the process, we show that Huygens had cogent empirical reasons to reject Isaac Newton's theory of universal gravity, which predicted a somewhat larger variation in gravity.

Introduction

Christiaan Huygens published his *Discourse on the Cause of Gravity* as an addendum to his *Treatise on Light* in 1690.[1] The *Discourse* consists in part of an old work, "On the Cause of Gravity," that he had first presented to the French Academy of Sciences

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[1] Both the *Treatise* and the *Discourse* have recently been reissued in Christiaan Huygens's *Traité de la lumière*, with an Introduction by Michel Blay (Paris: Dunod, 1992). We have used the version of the *Discourse* as found on pp. 443-488 of Vol. 21 of *Oeuvres Complètes de Christiaan Huygens* publiées par la Société Hollandaises des Sciences, 22 vols. (The Hague: Martinus Nijhoff, 1888-1950), (hereafter *OCCH*); and English translations from "Huygens's *Discourse on the Cause of Gravity*," by Karen Bailey, with notes and commentary by Karen Bailey and George E. Smith, forthcoming in *A Measure in Evidence: Huygens's Determination of the Strength of Gravity*, ed. George E. Smith.

in 1669, a brief middle section that he says he wrote in 1686 or 1687, and an "Addition" that he wrote after he had read Newton's *Principia*.[2] The first paragraph of this Addition refers not only to Newton's *Principia,* but also to his own Report of 1688 to the Directors of the Dutch East India Company on the measurement of longitude at sea[3] (hereafter, the Report).

ADDITION

> Some time after I had finished writing the preceding, I received and examined the journal of the voyage, which, by order of the Directors of the Dutch East India Company, had been made with our pendulum clocks as far as the Cape of Good Hope. Since then I have also read the very scholarly work of M. Newton, entitled *Philosophiae Naturalis principia Mathematica*. Both provided me with material to extend this Discourse further. First, concerning the different lengths of the pendulums in different regions, which he has also addressed, I believe I have from the average of these clocks a clear confirmation not only of this effect of the motion of the Earth but also of the measure of these lengths, which agrees very well with the calculation that I have just given. For, having corrected and adjusted, according to this calculation, the longitudes that were measured with the clocks on the return from the Cape of Good Hope to Texel in Holland (because going they were not of service), I have found that the route of the vessel was much better marked on the map than it was without this correction; so much so that arriving at this port, there was not 5 or 6 leagues of error in the longitude thus adjusted. This presupposes that the aforementioned Cape had been well surveyed by the Jesuit Fathers when they passed by there on the way to Siam in the year 1685, and that it is located some 18 degrees more to the East than Paris, which I know moreover to be scarcely far from the truth. The detail of this whole matter is deduced in full in the Report that I have made to said Honorable Directors concerning this voyage of the pendulums. After this report had

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[2] See comments of the <u>*OCCH*</u>'s Editors in the footnotes of pp. 462 & 466 of Volume 21; see also, p. 206 of Roberto De A. Martins's "Huygens reaction to Newton's gravitational theory," in *Renaissance and Revolution: Humanists, scholars, craftsmen and natural philosophers in early modern Europe*, ed. J.V. Field and Frank A. J. L. James (Cambridge: Cambridge University Press, 1993) pp. 202-213.

[3] "Report on the Measurement of Longitude by my Clocks on the Voyage from the Cape of Good Hope to Texel in the Year 1687," <u>*OCCH*</u>, Vol. 9, No. 2519, 24 April 1688, pp. 272-291; and English translations from "Huygens's 1688 Report to the Directors of the Dutch East India Company," by Eric Schliesser, with notes and commentary by Eric Schliesser and George E. Smith, forthcoming in George E. Smith, op. cit Note 1. This version of the Report is based on a manuscript copy in Huygens's hand in the Huygens Archives ("Hug.45 aan Oost-Indische" in the Library of the University of Leiden). It differs from the one originally sent to the Dutch East India Company by incorporating the small arithmetical changes in the table to which Burchard de Volder called attention when he reviewed the original version; some of these changes in the table were incorporated in the text of the manuscript copy as well, but others were not.

> been examined by knowledgeable persons, it pleased them to direct us to conduct a second trial in order to be assured by several experiments of the soundness of this discovery. We will see what the success of this other voyage will be, and particularly what the variation of the pendulums is, it being certain that, in order to know the variation well, these clocks, by their acceleration and deceleration, give an average more reliable than actually measuring the length of the seconds-pendulum in different countries. Meanwhile, because experience in the trial of which I have been speaking is so well in accord with what I have found by reasoning, I trust enough in this to want to continue this speculation, considering first what the figure of the Earth is, since, as has been said, it is not spherical.[4]

The most celebrated topic of this paragraph, both then and now, is the determination of longitude at sea. Huygens's claim of accuracy to within 5 or 6 leagues amounts to roughly 30 kilometers, or 27 minutes of arc at Texel.[5] A decade and a half later, the full prize of £20,000 legislated in the British Longitude Act of 1714 was for a method of determining longitude at sea to within 30 minutes of arc, which amounts to roughly 55 kilometers at the Equator, with lesser prizes of £15,000 for 45 minutes and £10,000 for one degree.[6] Huygens's paragraph was thus announcing at least the prospect of a much sought, major advance in determining longitude at sea.

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[4] See OCCH, Vol. 21, p. 466f. The reference to the "speculation" on the non-sphericity of the Earth is to material in the *Discourse* immediately preceding the Addition, ending the middle section that Huygens says he wrote before receiving the *Principia*. In this section, he first calculates the variation in (observed) surface gravity that would result from rotation of the Earth alone. He then notes that the superposition of the centrifugal force on gravity toward the center of the Earth would result in a small, but detectable 5 minute 54 second angular shift of observed gravity off the radius extending from the Earth's center to the surface. He finally concludes from the fact that no such angular shift has been revealed by plumb lines that, if the rotation of the Earth causes a variation in observed surface gravity, then the level surface of the Earth -- i.e. the seas -- must not be perpendicular to the radius extending from the Earth's center, or in other words the Earth must be oblate, and not perfectly spherical.

[5] The *league* did not have a single established length at the time. It is here being assumed to be 3 (modern) nautical miles.

[6] See *The Quest for Longitude*, the Proceedings of the Longitude Symposium held at Harvard University, Cambridge Massachusetts, November 4-6, 1993, ed. William J. H. Andrewes (Cambridge: Harvard University Press, 1996), *passim*, or Dava Sobel's widely read *Longitude* (New York: Walker and Company, 1995), p. 53ff. In the 17th century the Spanish, Dutch, Venetian, and French governments all offered large sums of money to anyone who could devise a method for finding longitude at sea to the accuracy needed for safe

From the point of view of the present paper, however, the most important feature of this paragraph is Huygens's claim that the Report not only confirms the rotation of the Earth -- this by showing that the rotation necessitates a correction to his clocks at sea -- but also establishes the precise amount the seconds-pendulum has to be shortened at different latitudes. Huygens had two claims at stake in the voyage discussed in the 1688 Report: first, that his clocks could be used to determine longitude at sea; and, second, that the seconds-pendulum provides a universal, invariant standard of length.[7] In effect, the voyage discussed in the Report had shown him that the second of these claims requires modification because the Earth's rotation reduces the net effect of gravity at the Equator versus the Poles, thereby requiring the seconds-pendulum to be shorter at the Equator. This is the first major conclusion from the Report. The second is that the Earth's rotation is the ***only*** source of variation in gravity, so that by taking this rotation into account the precise length of the seconds-pendulum at each latitude can be calculated once and for all, given the length at Paris.

Strangely, the Report was never published, nor was it translated from Dutch into French or Latin. We have found no evidence that Huygens tried to circulate the document among his scientific peers, although it was reviewed by two Dutch contemporaries, Johannes Hudde and Burchard de Volder.[8] This raises the suspicion that the evidence of the Report may not have been so compelling as the paragraph quoted

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navigation; see C.A. Davids's *Zeewezen en Wetenschap* (Amsterdam: De Bataafsche Leeuw, 1985), especially pp. 129-131 for more details.

[7] See, for instance, Huygens's *Horologium Oscillatorium*, OCCH, Vol. 18, pp. 349-353, especially Proposition XXV, or the English translation by Richard J. Blackwell, *Christiaan Huygens'* The Pendulum Clock Concerning the Motion of Pendula as Applied to Clocks (Ames: The Iowa State University Press, 1986), pp. 167-172. Also see, pages 69, 94, 154-155, 157, 200, and 220 of Joella Yoder's *Unrolling Time: Christiaan Huygens and the Mathematization of Nature* (Cambridge: Cambridge University Press, 1988).

[8] Hudde was a prominent mathematician and at the time Mayor of Amsterdam, as well as a Director of the Dutch East India Company. He admitted that he did not have time to review Huygens's Report carefully. See his letter to Huygens of 30 April 1688, No. 2521, OCCH, Vol. 9, p. 294. De Volder, Professor of Mathematics and Philosophy at the University of Leiden, formally reviewed the report for the Dutch East India Company. For his review, dated 22 July 1689, see Letter No. 2547 (Appendix to No. 2546) "B. De Volder aux Directeurs de la Compagnie des Indies," in OCCH, Vol. 9, pp. 339-343. A copy of de Volder's review can be found in Hudde's personal archives at the Algemeen Rijksarchief (1.10.48 no. 44) in The Hague.

from the *Discourse* suggests. The questions we will be examining here are 1) what was Huygens's evidence that the Earth's rotation and it alone affects the length of the seconds-pendulum?; and 2) how good was this evidence?

To appreciate the importance of these questions, one needs to view Newton's theory of gravity in the manner Huygens and his contemporaries viewed it, namely as consisting of two parts: (1) *inverse-square celestial gravity*, i.e. gravitation toward celestial bodies; and (2) *universal gravity*, i.e., gravitation of every particle of matter toward every other. As the subsequent portions of the Addition to the *Discourse* make clear, Huygens thought that Newton had established the former, at least to the extent of establishing that the planets and their satellites are retained in their orbits by inverse-square gravity toward their respective central bodies.[9] By contrast, Huygens thought that Newton had done nothing toward establishing universal gravity, which he regarded as untenable on its face because of its incompatibility with the "mechanical philosophy." Throughout the *Principia*, however, Newton had shown himself willing to allow empirical considerations to override the constraints on theorizing imposed by the mechanical philosophy. Hence, invoking the mechanical philosophy was a question-begging way to respond to Newton. A more appropriate way was to demonstrate the absence of empirical evidence in support of universal gravity. Newton proposed one specific piece of empirical evidence for universal gravity in the *Principia*: assuming that the Earth is oblate, and not perfectly spherical, the Earth's rotation is *not* by itself sufficient to account for the extent to which the seconds-pendulum is shorter at the Equator. The appropriate way for Huygens to respond to Newton was accordingly to show that the Earth's rotation by itself *is* sufficient to account for the contrasting lengths of the seconds-pendulum.

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[9] Huygens granted without qualification (one-body) inverse-square gravity toward central celestial bodies. He held back from granting (two-body) interactive gravity between orbiting and central bodies and (multiple-body) interactive gravity among orbiting bodies, though he seemed prepared to grant either or both of these should empirical evidence be adduced to confirm them. (See *Discourse*, OCCH, Vol. 21, pp. 471-473.)

More specifically, before reading the *Principia* Huygens had concluded, in conjunction with his vortex model for the mechanism of gravity, that surface gravity itself would be the same everywhere on the Earth in the absence of centrifugal effects arising from the Earth's rotation.[10] Centrifugal effects, he then concluded, produce both a variation in effective surface gravity with latitude and sufficient oblateness of the Earth for the net gravitational tendency always to be perpendicular to the surface of the sea. On Newton's theory of universal gravity, by contrast, the gravitational force at the Earth's surface results from the net effect of inverse-square forces directed toward the individual particles of matter forming the Earth. In the case of a spherical Earth, this implies an inverse-square variation of gravity with radius above the surface, and a linear variation of gravity with radius below the surface. If, however, the Earth was formed from a rotating body of fluid, universal gravity implies that the Earth is oblate and effective surface gravity varies with latitude from the combination of two effects -- centrifugal effects, just as in the view of Huygens, but also the effect on the gravitational forces at the surface arising from their composition out of inverse-square forces directed toward individual particles of matter in a non-spherical Earth! Huygens must have been taken aback when he encountered these consequences of universal gravity in Propositions 19 and 20 of Book 3 of the *Principia*, for these two propositions directly challenged conclusions he had reached before receiving his copy of the *Principia*, conclusions that had nothing to do with worries over action at a distance.[11] The variation of effective surface gravity with latitude implied by Newton's theory is significantly larger than on his theory, and, as he quickly determined, the implied oblateness (assuming a uniformly dense Earth) is two and a half times greater than on his

[10] At least at sea level; Huygens does not address the question whether gravity varies with altitude in the pre-*Principia* version of the *Discourse*.

[11] The text expounding these two propositions was more guarded in the first edition of the *Principia* than in the two later editions, when more data had become available indicating a larger effect than from the Earth's rotation alone; see Isaac Newton, *The Principia: Mathematical Principles of Natural Philosophy*, tr. I. Bernard Cohen and Anne Whitman (Berkeley: University of California Press, 1999), pp. 821-832.

theory.[12] No data were then available on the difference between the equatorial and polar radii of the Earth, but Huygens thought that his data from the voyage were sufficient to refute the greater variation in surface gravity with latitude implied by Newton's theory.

The fact that Huygens turned out to be wrong about this does not reflect badly on him, for he understood the danger of limited data as well as anyone. Rather, it illustrates the difficulty in developing evidence from limited data when relatively few complementary data are available that can help protect against being misled.

Measuring the Length of a Seconds-Pendulum

The Report contains a discussion of the performance of Huygens's experimental sea clocks on a voyage commissioned by the Dutch East India Company. In 1686 two of Huygens's pendulum clocks were placed aboard a Company ship in order to test their accuracy in measuring longitude at sea.[13] Three men, Thomas Helder assisted by Johannes de Graaff and the clockmaker Willem van der Dussen,[14] were to accompany the clocks from Texel to the Cape of Good Hope and back. Huygens's Report is based on data collected by de Graaff and the ship's mariners during their return voyage on the ship

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[12] See OCCH, Vol. 21, pp. 467-471.

[13] The clocks left Texel in May 1686 on the ship, Huis te Zilverstein, probably under the command of Karsten de Gilde. The Zilverstein sailed beyond the Cape of Good Hope to the Dutch East Indies. [*Dutch-Asiatic Shipping in the 17th and 18th Centuries, Vol. II: Outward-bound voyages from the Netherlands to Asia and the Cape (1595-1794)*, ed. J. R. Bruijn, F. S. Gaastra, *et al.*, (The Hague: Martinus Nijhoff, 1979, pp. 222-223).] Helder and Huygens's other assistant, Johannes de Graaff, disembarked at the Cape with the clocks at the end of September 1686, where they waited for the arrival of the Dutch East India return-fleet of the following year. The Wapen van Alcmaer had lost its captain, the chief mariner, and several of its crew to an unknown illness raging off the coast of the Cape. Apparently Helder held the rank of chief mariner, and he was asked to captain the Alcmaer back to Texel. This, it was thought, would enhance Helder's opportunity to analyze the clocks and check them on their usefulness in finding longitude. This information comes from a letter, dated 18 April 1687, that can be found at the Algemeen Rijksarchief, The Hague, V. O. C. Archief, 4023, folio 42ff. We thank dr. F. S. Gaastra for locating this letter and calling our attention to it.

[14] See, for instance, Letter No. 2488, "Christiaan Huygens à A. de Graef," 3 October 1687, OCCH, Vol. 9, pp. 222-223; the Editors of OCCH mistakenly suggest in note 1 to Letter No. 2488, that de Graaff's first name is Isaak, but they correct this in note 1 of Letter No. 2516, 24 April 1688, "Christiaan Huygens à Abraham de Graaf," OCCH, Vol. 9, p. 266. Abraham was Johaness de Graaff's father. Huygens often addresses them spelling their name 'Graef'—typical of variants in spelling of the time. We have chosen to use 'Graaff' except where we cite Huygens's letters and manuscripts, where we have followed the Editors of OCCH.

Alcmaer.[15] After Helder died early in the return voyage, many of his notes disappeared, denying Huygens some potentially crucial further evidence. The handwritten Report was sent to the Dutch East India Company on 24 April 1688.

As Huygens had remarked in a letter of 1 May 1687 to Philippe de la Hire, while the Alcmaer was still at sea and a few weeks before he received his copy of Newton's *Principia*, the question of the length of the seconds-pendulum was quite confusing at the time.[16] As Table 1 shows, Huygens, Richer, Jean Picard, and Varin,[17] Jean des Hayes, and Guillaume de Glos had all obtained virtually the same value for the length of the seconds-pendulum in Paris, within 1/10th of a line.[18] Thus, we can surmise a level of accuracy in measuring the length of the pendulum of about a tenth of a line -- something that would have been difficult to achieve with the naked eye, but was possible using minor magnification. The good agreement between the acceleration of gravity implied by the measurements in Paris and our modern measured value adds support for this level of accuracy.

TABLE 1 AROUND HERE

In 1672 Richer had found that the seconds-pendulum had to be shortened by 1 and 1/4 lines in Cayenne, just north of the Equator.[19] Ten years later Varin *et al.* found that

[15] For a discussion of the clocks Huygens designed for maritime use, including those on the Alcmaer, see J.H. Leopold's "The Longitude Timekeepers of Christiaan Huygens," in *The Quest for Longitude*, op. cit. Note 6, pp. 101-114.

[16] See letter No. 2455, "Christiaan Huygens à Ph. de la Hire," OCCH, Vol. 9, pp. 130-133. For an English translation of this letter, see "Huygens's *Discourse on the Cause of Gravity*," op. cit. Note 1.

[17] We have been unable to discover Varin's first name.

[18] A line is 1/12 of a Paris inch; we have used 2.25575 millimeters as its equivalent in modern units, corresponding to Eric Aiton's value of 2.7069 cm per Paris inch given in *The Vortex Theory of Planetary Motions* (London: Macdonald, 1972), p. 89.

[19] See Jean Richer, "Observations astronomiques et physiques faites en l'isle de Caïenne," originally published in 1679 and reprinted in *Mémoires de L'Académie Royale des Sciences Depuis 1666 jusqu'à 1699*, Paris, 1729, Volume VII, part 1, pp. 233-326; see especially, p. 320. For an English commentary, see John W. Olmsted's account, "The Scientific Expedition of Jean Richer to Cayenne (1672-1673)," pp. 117-128 in *ISIS* 34 (1942); see also, p. 253 of Michael Mahoney's "Christiaan Huygens: The Measurement of Time and

TABLE 1: MEASURED LENGTHS OF THE SECONDS-PENDULUM AS OF 1684

	LOCATION	LATITUDE	LENGTH (Paris units)	IMPLIED g (cm/sec^2)
HUYGENS	Paris	48°50′	3ft 8 1/2 lines	(980.7)
RICHER	Paris	48°50′	3ft 8 3/5 lines	(980.9)
	Cayenne	4°55′	$\Delta\ell = 1$ 1/4 lines	(978.1)
VARIN et al	Paris	48°50′	3ft 8 5/9 lines	(980.7)
	Goree	14°40′	$\Delta\ell = 2$ lines	(976.4)
	Guadaloupe	15°00′	$\Delta\ell = 2$ 1/18 lines	(976.3)
PICARD	Paris	48°50′	3ft 8 1/2 lines	(980.7)
	Uraniborg	55°54′	$\Delta\ell = 0$ lines	(980.7)
	Cape Cete	43°24′	$\Delta\ell = 0$ lines	(980.7)
MOUTON	Lyons	45°47′	3ft 6 3/10 lines	(975.8)

NB Modern measured g at Paris = 980.970 cm/sec^2

Modern average g at Equator = 978.032 cm/sec^2

the seconds-pendulum had to be shortened by 2 lines in Goree, just off Cape Verde, ten degrees further north from the Equator than Cayenne, and they found an even larger correction was needed in Guadeloupe.[20] In the early 1670s, by contrast, Picard had found no need to change the length of the seconds-pendulum from Uraniborg to Cape Cete on the Mediterranean coast south of Paris. This, together with the clock measurements he made while in Lyons, led him to reject Gabriel Mouton's period for a one-foot pendulum in Lyons and its implication that the seconds-pendulum was noticeably shorter in Lyons than in Paris.[21]

Huygens had reasons to find these measured lengths of the seconds-pendulum confusing even beyond their conflict with one another. From his previous work on centrifugal force he had calculated that the centrifugal effect of the Earth's rotation at the Equator is 1/289th of the force of gravity. From this, he could readily calculate how much the seconds-pendulum had to be shortened at every latitude if the centrifugal effect of the Earth's rotation, and only it, was altering local effective gravity.[22] In particular, as Table 2 indicates, rotation alone would necessitate a shortening of the seconds-pendulum at Cayenne by 4/10 of a line less than Richer had found, and a shortening at Goree of almost 1 and 1/4 lines less than Varin *et al.* had found. By contrast, the seconds-pendulum would have been 1/3 of a line longer at Uraniborg than at Cape Cete, something Picard should have been able to detect. Admittedly, all of these differences in

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Longitude at Sea," pp. 234-270 of *Studies on Christiaan Huygens; Invited Papers from the symposium on the Life and Work of Christiaan Huygens, Amsterdam, 22-25 August 1979,* edited by H. J. M. Bos *et al.* (Lisse: Swets & Zeitlinger: 1980).

20 See Varin, Des Hayes, and de Glos, "Observations astronomiques faites au Cap Verd, en Afrique, et aux Isles de l'Amerique," reprinted in *Mémoires de L'Académie Royale des Sciences Depuis 1666 jusqu'à 1699,* Paris, 1729, Volume VII, Part II, pp. 431-459. It should be noted that Varin *et al.*'s measurements of latitude were not all that good, raising some questions about how careful they were. The latitudes given by the other authors listed in Table 1 do not always agree with one another, nor with the values listed in the table.

21 See Picard's "Observations Astronomiques Faites en Divers Endroits du Royaume," reprinted in *Mémoires de L'Académie Royale des Sciences Depuis 1666 jusqu'à 1699,* Paris, 1729, Volume VII, Part I, pp. 329-347, especially pp. 346-347; see also his "Voyage d'Uranibourg, ou Observations Astronomiques faites en Dannemarck," *ibid.*, pp. 193-233, especially p. 208.

22 See the Report No. 2519 in OCCH, Vol. 9, p. 275 or the *Discourse* in Vol. 21, pp. 462-464.

length were impressively small to be attaching great significance to. Huygens must have given some thought to dismissing them as resulting from careless measurement.[23]

TABLE 2 AROUND HERE

Huygens had another reason to distrust the measurements. He knew that the arc of the seconds-pendulum has to be kept quite small in order for it to provide a meaningful measure of the strength of local gravity. When proposing the seconds-pendulum as a universal standard of length in his *Horologium Oscillatorium* of 1673, he specifically warned that the length will not be correctly determined if the total pendulum arc exceeds 5 or 6 degrees.[24] Huygens had no way of determining just how sensitive the apparent length of the seconds-pendulum is to the total arc. But we can readily calculate it, thanks to work of Euler and Lagrange in the 18th century.[25] The total arc has to be kept below 5 degrees to prevent the apparent lengthening from exceeding a tenth of a line. Moreover, the difference between a 4 degree arc and a 10 degree arc is 0.35 lines, almost the entire difference between Richer's measured shortening and the effects of rotation alone. In his brief paragraph on the subject Richer tells us that he had kept the arc very small, but not how small.[26] Furthermore, an excess arc length could easily explain the notably greater

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[23] In the Report, after noting Richer's measurement and the possibility of attributing it to the rotation of the Earth, Huygens remarks, "because other observations made in various regions regarding this unequal length of pendulums did not turn out that well according to expectations, I previously had the thought that maybe this effect of the rotation of the Earth was nullified by some other natural cause, or it was rendered irregular." Moreover, given Richer's careless handling of Huygens's clocks on a voyage in 1670, Huygens had some reason to question Richer's competence. Huygens was surely speaking of Richer when he attributed failures of his clocks at sea to "negligence" in *Horologium Oscillatorium* (p. 116 in OCCH, Vol. 18, and p. 28 in Blackwell's English translation, op. cit. Note 7). For a summary of the 1670 voyage and Huygens's anger in response to it, see Mahoney, op. cit. Note 19, p. 353, and Leopold, op. cit. Note 15, p. 106.

[24] *Ibid.*, Part IV, Proposition XXV, p. 349 in OCCH, Vol. 18, and p. 167 in Blackwell's translation, op. cit. Note 7.

[25] See p. 531ff of Roger Cooke's "Elliptic integrals and functions," pp. 529-539, of *Companion Encyclopedia of the History and Philosophy of the Mathematical Sciences*, ed. by I. Grattan-Guinnesss (London & New York, Routledge, 1994), and for the formula for the effect of arc length, p. 1085 of Paul Foulkes's "The pendulum: Theory, and its use in clocks," *ibid*, pp. 1082-1088.

[26] Richer, op. cit. Note 19, p. 320.

TABLE 2: MEASURED VARIATIONS IN GRAVITY

VERSUS THE VARIATIONS INFERRED BY HUYGENS

	LATITUDE	MEASURED $\Delta \ell$	MEASURED % CHANGE	FROM ROTATION ALONE $\Delta \ell$	FROM ROTATION ALONE % CHANGE
Richer					
Paris-	48°50′	1.25 lines	0.284	0.852 lines	0.194
Cayenne	4°55′	(2.82 mm)		(1.92 mm)	
Varin et al					
Paris-	48°50′	2.00 lines	0.456	0.774 lines	0.176
Goree	14°40′	(4.51 mm)		(1.75 mm)	
Picard					
Uraniborg-	55°54′	0.00 lines	0.000	0.325 lines	0.074
Cape Cete	43°24′	(0.00 mm)		(0.73 mm)	

changes in length found by Varin *et al.*[27] So, Huygens had every reason to want new pendulum length measurements to be made at various locations. As he remarked in the middle section of the *Discourse*, the measurements that had been made before 1686 should be looked on as providing only rough initial data. Specifically, Huygens remarks,

> But we cannot entirely trust these first observations, the occurrence of which we do not see as conspicuous in any way – and we can trust still less, given what I believe, in those that are said to have been made in Guadeloupe, where the shortening of the Paris pendulum had been found to be two lines. We must hope that in time we will be informed exactly of these different lengths, at the Equator as well as in other regions; and certainly it is something that well deserves being researched with care, even if it would only be to correct, according to this theory, the motions of the pendulum clocks, in order to make them serve as a measure of longitudes at sea.[28]

It should come as no surprise, then, that when the 1686 expedition was being prepared, Huygens not only wrote elaborate and precise instructions for the installation and handling of his clocks,[29] but he also gave detailed instructions to Helder for measuring the length of the seconds-pendulum along the course of the voyage.[30] Huygens explicitly instructed Helder not to let the arc of the pendulum become larger than 2 or 3 thumbs -- i.e. 2 or 3 Rhenish inches, which amount to arcs of 3 and 4.5 degrees respectively. This would have kept any discrepancy in the length of the seconds-pendulum below 0.085 lines. As Huygens realized, the proposed seconds-pendulum measurements could in principle have done more than just supply him with a set of data to compare with the earlier measurements. Suppose gravity did turn out to vary systematically with latitude in a way different from the centrifugal effects of the Earth's rotation alone. Then Huygens could have used the measured values of gravity to devise a

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[27] A total included arc around 15 degrees would explain the error in Varin's findings.

[28] Huygens, *Discourse*, OCCH, Vol. 21, p. 464.

[29] See Letter No. 2423, 23 April 1686, "Christiaan Huygens à Thomas Helder," OCCH, Vol. 9, pp. 55-76.

[30] See Letter No. 2520, undated 1686, "Christiaan Huygens à Thomas Helder," OCCH, Vol. 9, pp. 292-293; see also, the Report, OCCH, Vol. 9, pp. 275-276. For an English translation of Huygens's instructions to Helder on the seconds-pendulum, see "Huygens's Report to the Dutch East India Company," op. cit. Note 3.

set of empirical corrections according to latitude that would have allowed his clocks still to be used for determining longitude at sea -- even should gravity vary with latitude in a way that he could not calculate from theory alone.

If Helder performed any experiments with the seconds-pendulum during the voyage, Huygens never received data from them.[31] Helder died shortly after the departure from the Cape of Good Hope, and although Huygens requested Helder's notes,[32] we have no indication he received all of them. The only useful data Huygens received were on the performance of the clocks on the return-voyage from the Cape to Texel.[33] Fortunately, de Graaff took over the observation of the clocks shortly after Helder died and monitored the clocks all the way back to Texel, where the Alcmaer arrived on August 15, 1687.

The 1688 Report

De Graaff's journal and the logbook of the Alcmaer's mariners provided Huygens with enough material to compare the course of the ship as determined by the mariners with both the course as determined from the longitude implied by his clocks and the course implied by his clocks after they had been corrected to account for the effects of the Earth's rotation. The table Huygens presents in the Report, the first page of which is shown in Figure 1, has ten columns. Column I gives the date. The first day de Graaff used the clocks to determine longitude was May 10. Column II gives the latitude as

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FIGURE 1 AROUND HERE

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[31] The Editors of <u>OCCH</u> speculated in note 1 of No. 2520, <u>OCCH</u>, Vol. 9, p. 292, that Helder may never have received the instructions on the seconds-pendulum, but Huygens seems to have thought he had given them to him, see the Report, pp. 275-276 .

[32] See <u>OCCH</u>, Vol. 9, letter No. 2488, 3 October 1687, "Christiaan Huygens à A. de Graef," pp. 222-223, and the Report, pp. 287-291.

[33] Helder's journal indicated that mishaps with the clocks had occurred on the leg between Texel and the Cape rendering any data useless; see pp. 287-291 of the Report.

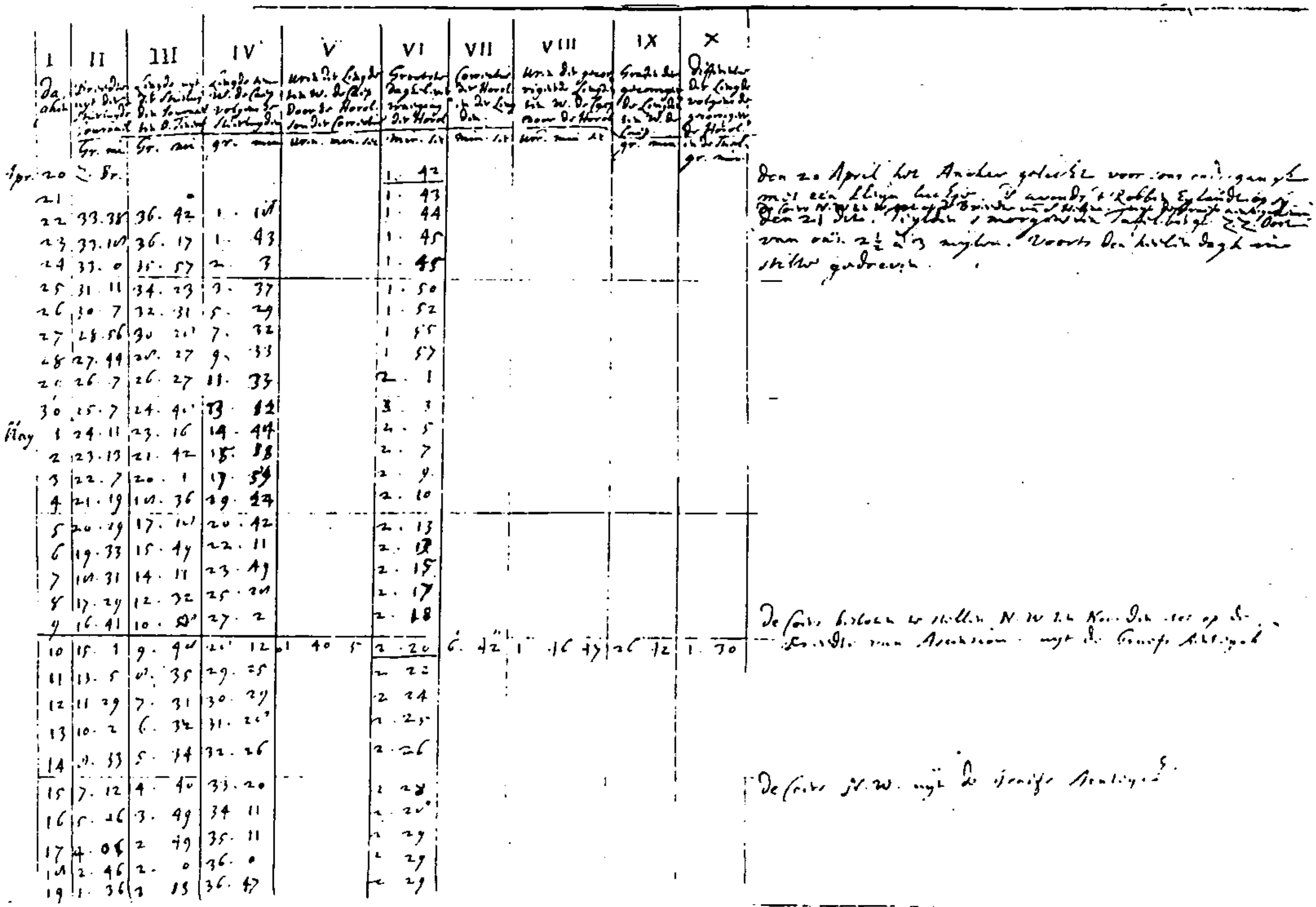

FIGURE 1: THE FIRST PAGE OF THE TABLE

OF HUYGENS'S CALCULATIONS IN THE REPORT

determined by the ship's mariners. Column III gives their estimates of the ship's longitude, restated in Column IV as longitude west of the Cape of Good Hope. Column V shows the longitude of the ship according to the clocks in hours west of the Cape as recorded by de Graaff. The numbers in the remaining columns are from Huygens's calculations after the voyage. Column VI shows the theoretical amount the clocks were losing each day, also known as the largest daily delay for the indicated latitude, versus the time they would keep at the North and South Poles -- this as a consequence of the lessening of effective gravity from the Earth's rotation. Column VII then gives the net time the clocks have theoretically fallen behind what they would have shown had they remained at the Cape. This column is calculated by adding all of the largest daily delays in Column VI and then subtracting for each day the corresponding largest daily delay at the Cape. Thus, on May 10, from the Earth's rotation alone the clock theoretically would have fallen behind by 6 minutes and 42 seconds versus what it would have shown at the Cape. Adding this amount to the longitude as indicated by the clock gives the corrected longitude, in hours west of the Cape, as shown in Column VIII, and in degrees, as shown in Column IX. Column X then gives the difference between Columns IV and Columns IX, that is, the difference between the mariners' course and the course as calculated from the clocks after the correction for the Earth's rotation is included.

Huygens's complete table, with text translated into English, is given at the end of this paper. The mariners gave their estimate of the position for each of the 117 days the Alcmaer was at sea. Longitude could be determined by means of the clocks only on days when the Sun was visible -- from Huygens's instructions, both at sunrise and sunset.[34]

[34] See Huygens, "Instructions Concerning the Use of Pendulum-Watches, for finding the Longitude at Sea," *Philosophical Transactions of the Royal Society* No. 47, Vol. 4 (1669), pp. 937-953; the key instruction (p. 949) reads as follows: "At the Rising and Setting of the Sun, when it is half above the Horizon, marke the time of the day, which the Watches, then shew; and though you have in the mean time sayl'd on, it is not considerable. Then reckon by the Watches, what time is elaps'd between them, and add the half thereof to the time of the Rising, and you shall have the time by the Watches, when the Sun was at South; to which is to be added the Aequation of the present day by the Table. And if this together makes 12. hours, then was the Ship at Noon under the same Meridian, where the Watches were set with the Sun. But if the summe be more than 12, then was she at Noon under a more Westerly Meridian; and if less, then under a

Presumably because of this, de Graaff entered the longitude from the clocks on only 21 of the 117 days. As the table shows, sometimes the spread between one of these 21 days and the next exceeded a week -- in particular, from April 20 to May 10, from May 10 to May 27, and from July 8 to July 24. The uncorrected and the corrected courses as determined from the clocks were consequently a good deal more sparsely specified than the course estimated by the mariners.

Huygens laid these three courses out on a map accompanying the report, shown in Figure 2.[35] The course lying farthest to the west in the Atlantic is the one estimated by

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FIGURE 2 AROUND HERE

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the mariners. The course a little to the east of it is the one determined by the clock after the correction for the Earth's rotation had been included. And the course lying farthest to the east is the one determined by the clock without the correction. This uncorrected course goes straight through the middle of Ireland. This alone was strong evidence that some correction to the clock was needed.

The comparison between the mariners' course and the course with the correction to the clock, as shown on the map, is reasonably good along the entire northward leg of the voyage. In the Report Huygens remarked that "the differences between the mariners and the corrected clocks are usually about 1 or 2 degrees, and always less than 3 degrees. And it should amaze no one that the mariners' reckoning would be 3 degrees off the true longitude on such a long voyage, because of the uncertainty in their guesses, from unknown currents and the ship's falling behind, as well as from its uncertain advance-

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more Easterly; and that by as many times 15 degrees, as the Summ exceeds or comes short hours of 12: as the Calculation thereof hath been already delivered."

35 The map shown in the figure is a facsimile of the one accompanying the Report in _OCCH_. It contains a number of anomalies that raise questions about whether it is a facsimile of the map Huygens originally sent to the Dutch East India Company with the Report. Some of these anomalies are noted in passing below. The map is discussed in detail in Appendix 1 of this paper.

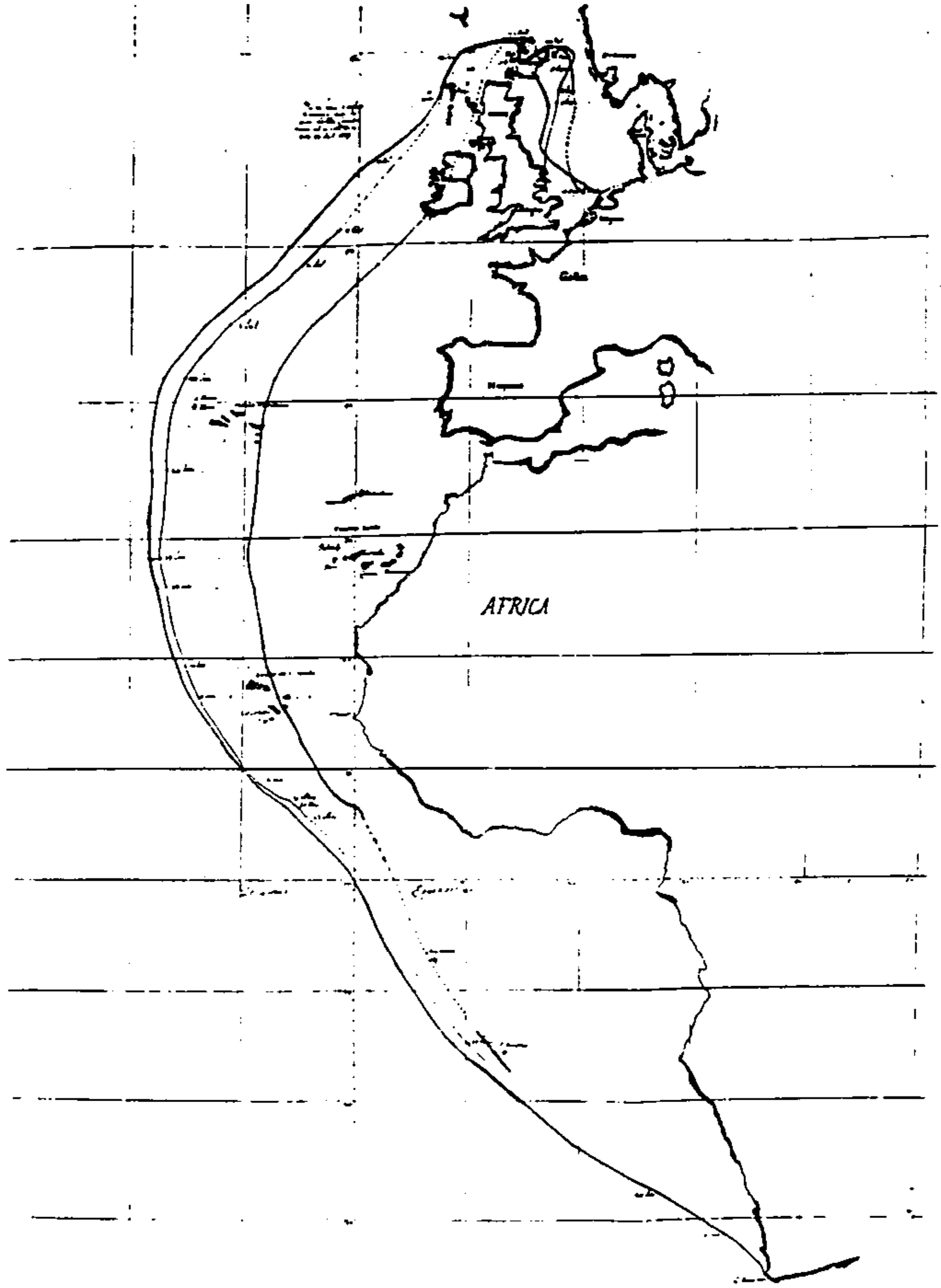

FIGURE 2: HUYGENS'S MAP COMPARING HIS UNCORRECTED AND CORRECTED COURSES WITH THE MARINERS' COURSE

ment."[36] Huygens might have added that the sea-trials of his clocks in the 1660s had shown conclusively that the mariners' course was not necessarily more accurate than one based on his clocks.[37] So, the discrepancies shown in the Atlantic need not have been cause for great concern.

The same cannot be said so readily, however, of the discrepancies in the southward leg from the tip of Scotland down to Texel. The remark quoted above notwithstanding, the discrepancy for August 5 listed in the table is 3 degrees 17 minutes, and all of the discrepancies from August 1 on are on the high side. Huygens never comments on this in the Report. Instead, he turns to an independently determined value of the difference in longitude between the Cape and Texel. In 1685 Father Guy Tachard, a Jesuit, had established the longitude of the Cape of Good Hope, relative to Paris, by comparing the time of an eclipse of the innermost moon of Jupiter with the time predicted by Gian Domenico Cassini's tables.[38] Huygens combined Tachard's value with the value Giambattista Riccioli had given in his *Geography* for the difference in longitude between Paris and Texel to reach the conclusion that Texel is 14 degrees 25 minutes west of the Cape.[39] The mariners' course, by contrast, located Texel more than 15 degrees west of the Cape. This then gave him a basis for challenging the map the mariners used on the

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[36] OCCH, Vol. 9, p. 285f. The largest discrepancy listed for the northern leg in the Table, 3 degrees and 24 minutes on June 8, does not appear on the map. In the Report Huygens points out that Clock B, the motion of which was the more suspect of the two, was used only for this day and May 10, and he then argues that the much closer agreement for June 2 and 10 provides strong grounds for ignoring this one discrepancy. Other than this exception, Huygens's claim about the discrepancies holds true for the northern leg. Still, one should keep in mind that, under the assumption that the mariners' course was correct, each degree of discrepancy amounted to a 4 minute total error in the uncorrected clock.

[37] See p. 252ff. of Mahoney op. cit. Note 19.

[38] See pp. 49-59 of Father Guy Tachard's *A Relation of the Voyage to Siam Performed by Six Jesuits, sent by the French King, to the Indies and China, in the year, 1685. With their Astrological Observations, and their Remarks of Natural Philosophy, Geography, Hydrography, and History.* (London: A. Churchil, 1688). Reprinted (facsimile) by White Orchid Press, Bangkok, Thailand, 1981. The original *Voyage de Siam des Pères Jésuits, Envoyes par le Roy aux Indes & a la Chine* was published in 1686 in Paris, and summarized with commentary in *Mémoires de L'Académie Royale des Sciences Depuis 1666 jusqu'à 1699*, Paris, 1729, Volume VII, Part II, pp. 607-853; see especially pp. 611-613.

[39] See, the Report, OCCH, Vol. 9, pp. 273-274. In Riccioli's *Geographiae et Hydrographiae reformatae libri duodecim quorum argumentum sequens pagina explicabit* (Bolagna, 1661) longitudes were based on lunar eclipses.

Alcmaer, *implicitly* suggesting that the mariner's course for the last leg of the voyage could be ignored insofar as it undoubtedly reflected incorrect longitudes shown on their map for the tip of Scotland and the islands nearby.

More importantly, the comparison of this independently established difference in longitude between the Cape and Texel and the difference obtained from the clocks along the voyage gave Huygens his strongest evidence for concluding that the ***only*** variation of gravity is from the Earth's rotation.[40] At the end of the 117 day voyage, with the correction to the clock accumulating the entire way, the calculated location of Texel was only 17 arc-minutes to the east of the location Huygens had inferred from Tachard's observations.[41] This is a mere 19 km. In terms of deviations in the clocks from unaccounted-for sources it amounts to only 68 seconds accumulated loss over 117 days. This was Huygens's primary grounds for claiming that, once his clocks were corrected to account for the Earth's rotation, they could be used to determine longitude at sea accurately. This conclusion he summarizes for the benefit of the Directors of the Dutch East India Company in the opening paragraph of the Report:

> ...I can bring very good news concerning this invention, for I have found that by using the aforementioned clocks the longitudes between the Cape of Good Hope and Texel have on the whole been measured very well, and the total longitude between these two places [has been measured] so perfectly that it only deviates by 5 or 6 miles, which I admit I have seen with exceptional satisfaction, it being a certain proof of the possibility of this very-long-sought-after affair.[42]

An Assessment of Huygens's Evidence

How good was Huygens's evidence for his two basic claims? The course in the Atlantic, as well as the good agreement for Texel, provide compelling evidence that some

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[40] See *ibid.*, Vol. 9, p. 287.

[41] The final entry for Huygens's corrected course in the original report, prior to de Volder's corrections, was 14 degrees 1 minute, giving a 25 instead of 17 minute discrepancy.

[42] OCCH, Vol. 9, p. 272f. The miles Huygens refers to are German nautical miles, 7404 meters; his statement of the discrepancy here includes allowance for the fact that Texel, though visible, was still a few miles to the east of the Alcmaer on August 15.

correction to the clocks is needed -- or, equivalently, that the length of the seconds-pendulum is not invariant with latitude. But the comparison between the mariners' course and the course based on the corrected clock is not enough to show that the only correction needed is the one for the Earth's rotation. The mariners' course, which was based on dead-reckoning "guesses" (to use Huygens's term), is not that reliable. Therefore, the comparison at Texel must provide the principal evidence that the only correction needed is for the Earth's rotation.

This evidence for rotation alone is not as strong as it might have been. As Table 3 shows, the amount by which the clocks needed correcting kept increasing until June 22, when the Alcmaer passed 35 degrees north latitude, finally reaching a latitude more north of the Equator than the Cape is south of it. From that day on the amount by which the clocks needed correcting kept diminishing, until it reached zero around August 2, after

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TABLE 3 AROUND HERE

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which the correction changed in sign. Hence, the total correction at Texel was only about 1/3 of the maximum correction. As a consequence, any corrections to the clock needed beyond the one for the Earth's rotation were somewhat masked at Texel versus what they would have been around June 22, when the correction for the Earth's rotation was more than 32 minutes, or 8 degrees in longitude. In other words, a successful comparison between an independently established longitude and a longitude based on the corrected clocks in the Azores, not far north of where the Alcmaer was on June 22, would have provided more telling evidence that the only correction needed is for the Earth's rotation. But the Alcmaer did not stop in the Azores.

There is another approach to assessing how good Huygens's evidence was that the only effect on local gravity is from the Earth's rotation. It was entirely within Huygens's reach to recalculate the corrections to the clock based on Richer's measured length of the

TABLE 3: HUYGENS'S TABLE

OF CORRECTIONS TO THE CLOCKS

DATE	LATITUDE	CUMULATIVE CORRECTION TO CLOCKS	CORRESPONDING CORRECTION TO LONGITUDE
20 Apr	34° 30′ S	0 m 0 s	0° 0′ 0″
10 May	15° 1′ S	6 m 42 s	1° 40′ 30″ W
27	5° 39′ N	19 m 41 s	4° 55′ 15″ W
30	6° 47′	21 m 59 s	5° 29′ 45″ W
31	7° 12′	22 m 45 s	5° 41′ 15″ W
2 Jun	9° 10′	24 m 14 s	6° 3′ 30″ W
8	16° 42′	28 m 11 s	7° 2′ 45″ W
10	19° 26′	29 m 17 s	7° 15′ 0″ W
16	26° 7′	31 m 43 s	7° 55′ 45″ W
18	28° 25′	32 m 14 s	8° 3′ 30″ W
22	34° 55′	32 m 32 s	8° 8′ 0″ W
27	41° 38′	31 m 40 s	7° 55′ 0″ W
3 Jul	45° 23′	29 m 26 s	7° 21′ 30″ W
6	49° 0′	27 m 42 s	6° 55′ 30″ W
8	50° 28′	26 m 19 s	6° 34′ 45″ W
24	59° 30′	10 m 21 s	2° 35′ 15″ W
29	60° 19′	5 m 3 s	1° 15′ 45″ W
1 Aug	59° 52′	1 m 54 s	0° 28′ 30″ W
5	59° 12′	- 2 m 17 s	0° 34′ 15″ E
8	58° 11′	- 5 m 20 s	1° 20′ 0″ E
9	57° 33′	- 6 m 19 s	1° 34′ 45″ E
15	53° 0′	-11 m 34 s	2° 53′ 30″ E

seconds-pendulum at Cayenne, Varin's measured length at Goree, or, for that matter, Newton's theoretical prediction of the variation of gravity with latitude, inferred from his theory of universal gravity. Huygens could easily have calculated the largest daily delay of the clock from the Pole to the Equator on any of these bases.[43] He had used 2 minutes 30 seconds in the table in the Report, having rounded from the theoretical value of 2 minutes 29.35 seconds. Based on Richer's measurement at Cayenne, the delay would have been 3 minutes 38 seconds; based on Varin's Goree measurement, it would have been 6 minutes 31 seconds; and based on the first edition of Newton's *Principia*, 3 minutes 7 seconds. Given the method Huygens used in calculating the local daily delays in his table, which involved a minor simplification,[44] all he had to do was to scale the cumulative corrections to the clocks at each point where longitude was found along the course to obtain new corrections corresponding to these other assumptions.[45] Specifically, a course based on Richer's measurement could be determined simply by scaling all of the corrections in Table 3 by a factor of 1.455; on Varin's measure, by a factor 2.607; and on Newton's theory, by a factor of 1.247. We do not know whether Huygens took the trouble to do these calculations, but he certainly could have; and even if he didn't, it would have been trifling for him to have estimated the magnitudes of the differences in his head.

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[43] Even lesser figures than Huygens could have easily done the calculations since Newton had indicated how to do them in Proposition 20 of Book 3 of the *Principia.*

[44] The largest daily delays listed in Column VI of Huygens's table were obtained from a second table in the Report that gives largest daily delays versus latitude. He derived these values by multiplying the largest daily delay from the Poles to the Equator by the square of the cosine of the latitude. This is tantamount to approximating local effective gravity, which deviates from the radial by a few minutes of arc in the middle latitudes, by its radial component. Newton adopts the same approximation in Proposition 20 of Book 3 of the *Principia*. The resulting error in the largest daily delay never exceeds 1/8 of a second.

[45] In other words, all Huygens needed to do was to scale the corrections in Column VII of the calculation table in the Report, repeated in Table 3 above. No further cumulative calculations were needed. Hence, it would have taken him only a couple of minutes to obtain the contrast between his corrections and the alternatives to it at Texel, or at any other single point along the voyage. (For the mathematical basis of this and other calculations in the paper, see Huygens's *Discourse on the Cause of Gravity*, cited in note 1.)

A course based on Varin's measurement is the one far to the west on the map shown in Figure 3. It lies even farther to the west of the mariners' estimated course than Huygens's original uncorrected course lay to the east. However inaccurate the mariners' estimated course may have been, it was very unlikely to have been this bad.

FIGURE 3 AROUND HERE

Moreover, a course based on Varin's measure would have placed Texel only 9 degrees 30 minutes west of the Cape -- almost 5 degrees further east than Huygens's calculated course had located it. This is more than 329 kilometers to the east of the location of Texel based on Tachard's observations. Hence, this calculation would have given Huygens decisive grounds for rejecting any systematic corrections to the clocks based on Varin's measurement of the length of the seconds-pendulum in Goree. If the lengths measured by Varin *et al.* in Goree and Guadeloupe were correct, gravity must vary locally in wild ways not reflected by the clocks during the return voyage from the Cape.

As Figure 4 shows, the evidence is less decisive in the case of a course recalculated on the basis of Richer's Cayenne measurement. A course based on Richer's

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FIGURE 4 AROUND HERE

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measurement, shown just west of the mariners' course at the Equator on the map, is relatively close to the mariners'. In some places it is a little farther to the west of the mariners' course than Huygens's course is to the east of it, but at several places a course based on Richer's measure is even closer to the mariners' course than Huygens's course is. So, the course in the Atlantic does not provide any compelling reason for rejecting Richer's measurement of the seconds-pendulum in Cayenne.

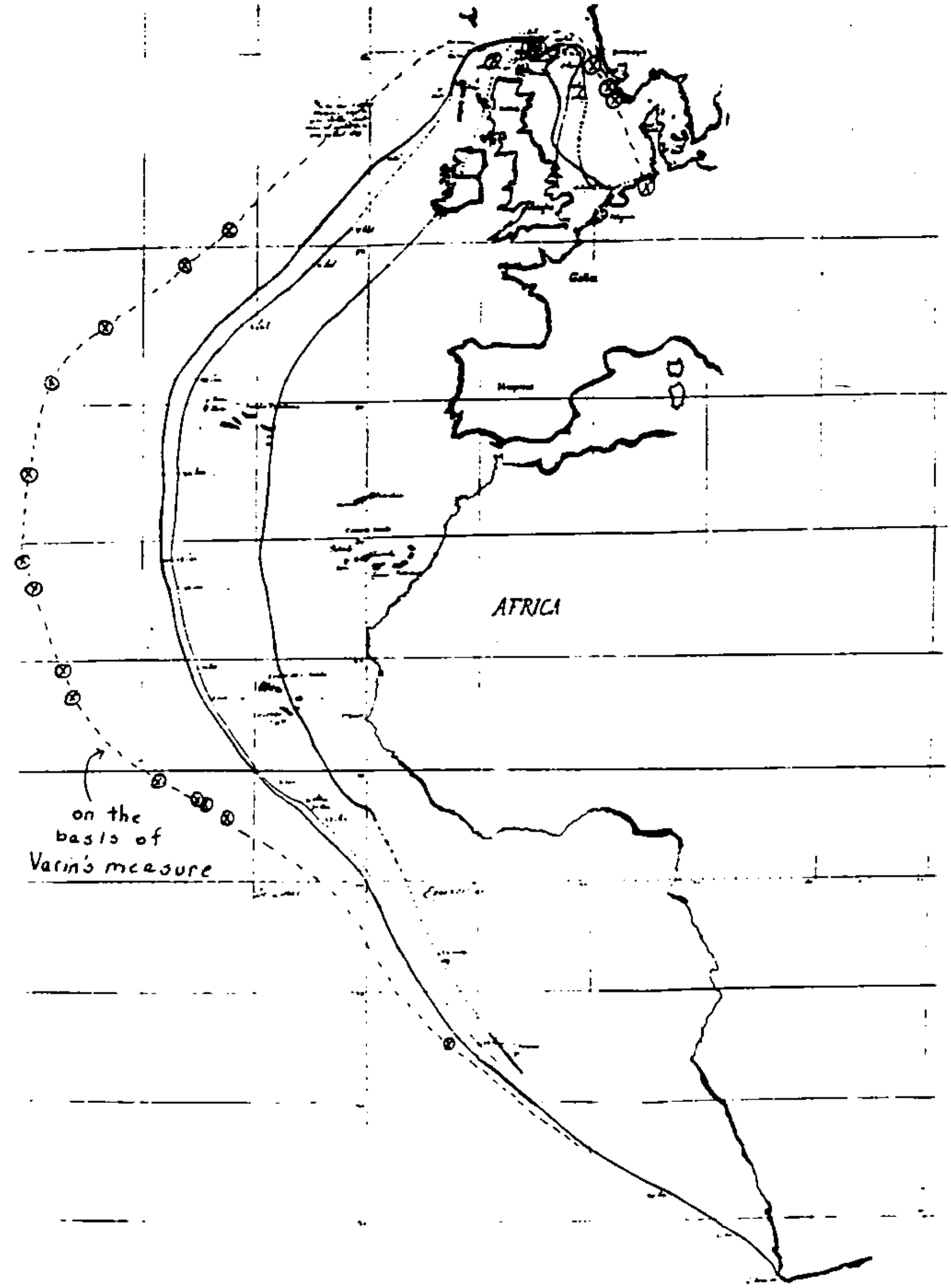

FIGURE 3: THE COURSE USING CORRECTIONS BASED ON VARIN'S MEASUREMENT IN GOREE

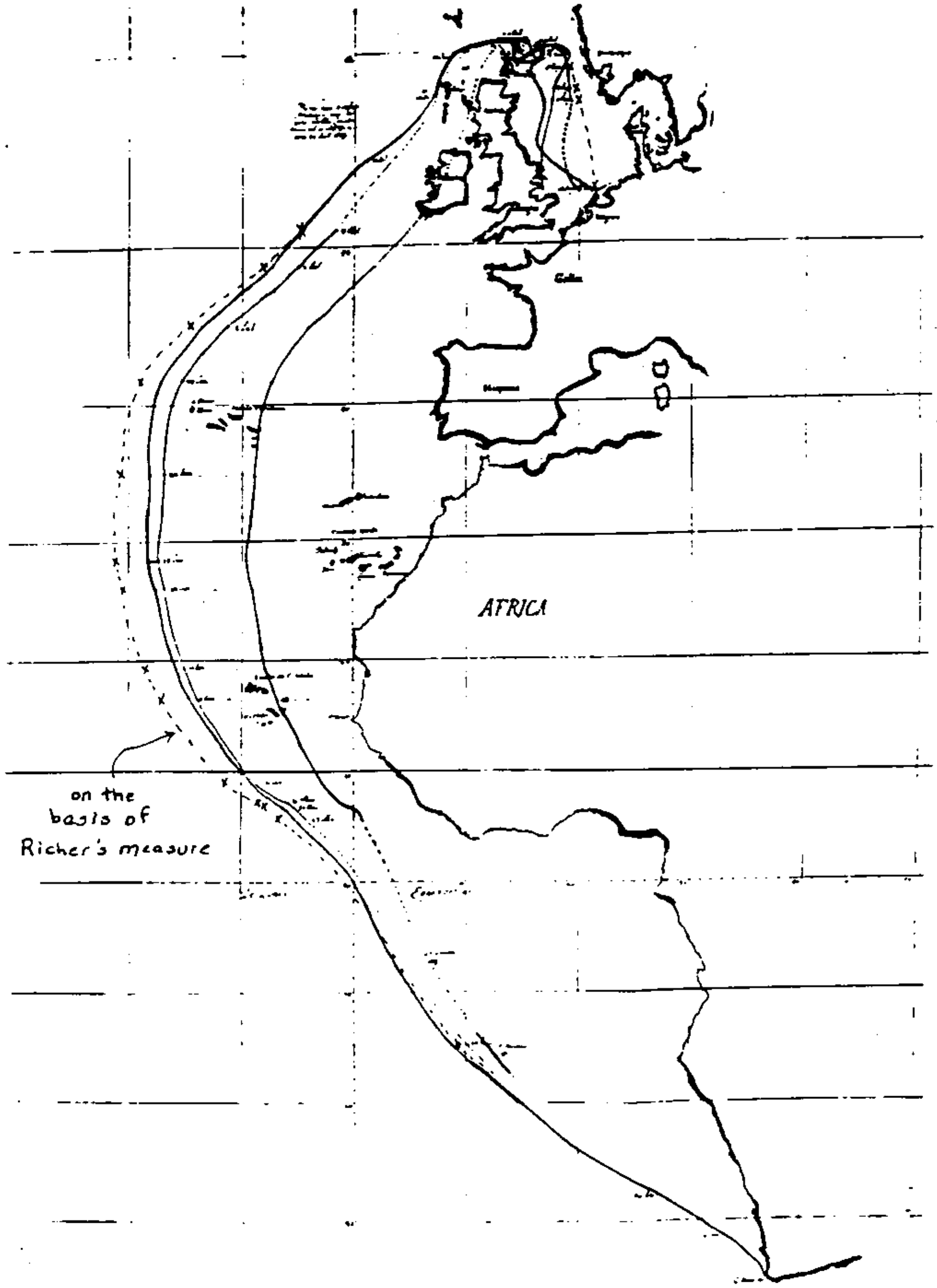

FIGURE 4: THE COURSE USING CORRECTIONS BASED ON RICHER'S MEASUREMENT IN CAYENNE

The comparison at Texel, however, seems more decisive. Correcting the course on the basis of Richer's measurement places Texel 12 degrees 50 minutes west of the Cape of Good Hope, 1 degree 35 minutes to the east of the location Huygens had inferred for it from Tachard's observations. This amounts to a discrepancy of 106 kilometers, nearly 6 times that of Huygens's corrected course. So, this calculation could have given Huygens strong grounds for thinking that Richer's difference of 1 and 1/4 lines in the length of the seconds-pendulum in Cayenne was excessive.

Finally, since Huygens had read the relevant parts of the *Principia* when he wrote the 1688 Report,[46] he had some reason for wanting to compare the correction based on rotation alone with a correction based on Newton's theory of universal gravity. In the first edition of the *Principia*, Newton gives the amount the seconds-pendulum has to be shortened in Cayenne and Goree on the basis of his theory.[47] Table 4 compares these values with Huygens's. The amounts in both cases are larger than the correction from rotation alone, but not so much larger as those implied by the Richer and Varin measurements. Specifically, Newton's theory gives a value for the shortening of the seconds-

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TABLE 4 AROUND HERE

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pendulum about midway between Richer's measurement and the value Huygens obtained on the basis of rotation alone.[48]

The question is what the course from the Cape of Good Hope to Texel would have looked like using Newton's value for the maximum daily delay from the Pole to the

[46] See Letter No. 2517, "Christiaan Huygens à J. Hudde," 24 April 1688, OCCH, Vol. 9, pp. 267-268.

[47] The amounts the seconds-pendulum has to be shortened that Newton gives in the *Principia* presuppose that the Earth is a uniformly dense oblate spheroid. As Newton pointed out in Book 3 Proposition 20 in all editions of the *Principia* -- and he especially emphasized in the first edition -- if the measured $\Delta\ell$'s are greater than the calculated values, then his theory of gravity implies that the Earth must be more dense toward the center than toward the surface.

[48] Newton does not mention the Picard comparison between Uraniborg and Cape Cete, but he would have had even more reason than Huygens to wonder why Picard saw no effect.

TABLE 4: HUYGENS'S THEORY VERSUS NEWTON'S

	LATITUDE	HUYGENS $\Delta \ell$	HUYGENS % CHANGE	NEWTON $\Delta \ell$	NEWTON % CHANGE
Paris- Cayenne	48°50′ 4°55′	0.852 lines (1.92 mm)	0.194	1.068 lines (2.41 mm)	0.242
Paris- Goree	48°50′ 14°40′	0.744 lines (1.75 mm)	0.176	0.972 lines (2.19 mm)	0.221
Pole - Equator	Maximum Daily Delay	2 m 30 s		3 m 7 s	

NB I.e., rotation alone versus universal gravity for uniformly dense Earth (as in the 1st edition of Newton's Principia)

Equator of 3 minutes and 7 seconds instead of Huygens's value of 2 minutes 30 seconds. In general, as Figure 5 shows, the leg of the Alcmaer's course in the Atlantic, based on Newton's theory, lies even closer to the mariners' course than Huygens's corrected course

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FIGURE 5 AROUND HERE

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does. So, a comparison of this part of the course would have given Huygens no grounds for claiming that a correction based on rotation alone was more accurate than a correction based on Newton's theory, which included not only the effects of rotation, but also a variation in local gravity from the non-sphericity of the Earth, as implied by the law of universal gravity.[49]

The comparison at Texel, however, did provide Huygens with grounds for claiming that the correction based on rotation alone is more accurate than one based on Newton's theory. A calculated course based on Newton's theory locates Texel at 13 degrees 26 minutes west of the Cape of Good Hope, which is 59 minutes to the east of its location based on Tachard's observations. This is almost 66 kilometers east, as compared to Huygens's value of 19 kilometers. So, Newton's theory seems to be entailing too large a correction, and hence the amounts by which he is saying that the seconds-pendulum has to be shortened at Cayenne and at Goree are too great.

We can now reconstruct the argument Huygens could have offered for claiming that the only mechanism causing the length of the seconds-pendulum to vary is the Earth's rotation. A course based on the hypothesis of rotation alone placed Texel very near the location that Huygens had independently established on the basis of Tachard's observations. Huygens could have easily calculated courses based on hypotheses corresponding to Richer's and Varin's measurements and Newton's theory. Indeed, he would not have

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[49] More precisely, this law applied to a rotating fluid Earth, as in Book 3, Propositions 19 and 20.

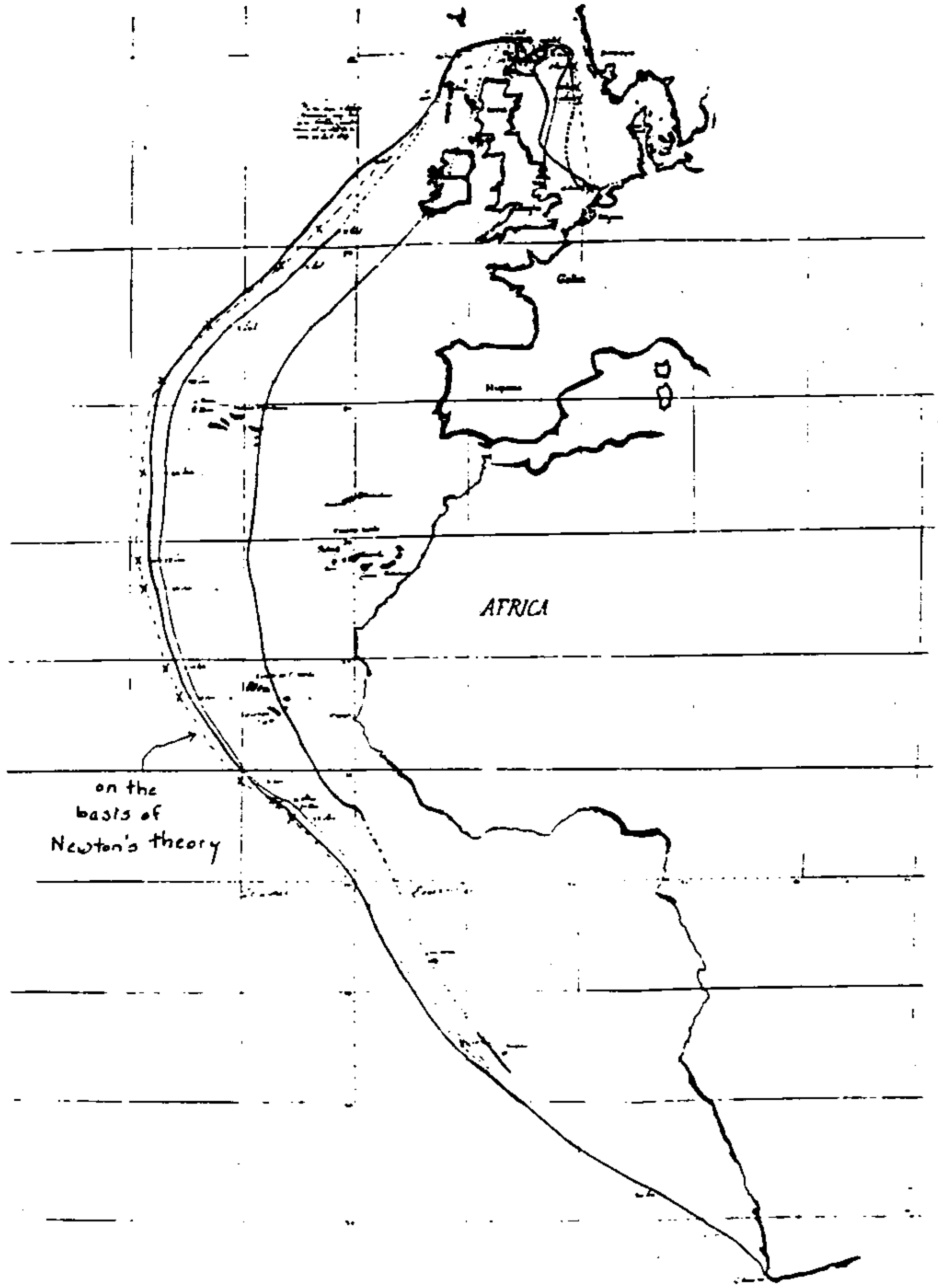

FIGURE 5: THE COURSE USING CORRECTIONS BASED ON NEWTON'S THEORY OF GRAVITY

had to perform the calculations to see the qualitative contrast shown in Table 5. If he, or anyone else, did the calculations, they would have found that each of these alternative courses would place Texel significantly farther to the east of the location established with

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TABLE 5 AROUND HERE

Tachard's measurement. Thus the calculations would have shown that any correction greater in magnitude than the one for rotation alone would have produced an excess discrepancy. All the evidence available to Huygens accordingly favored rotation alone over these alternatives to it. Given the questions about the measurements made by Richer and Varin *et al.*, not to mention those concerning Newton's theory, Huygens could have argued that the evidence from the voyage should take precedence. As he said in the first paragraph of the Addition to the *Discourse*, the cumulative effects of the alternative corrections over the 117 days of the voyage should have had the virtue of amplifying the small differences among the competing claims about the length of the seconds-pendulum.

Table 5 overstates the argument in one respect, but understates it in another. By the mariners' estimate, the Alcmaer was still 3 (German) nautical miles to the west of Texel on August 15 -- i.e. 22.2 km or 20 minutes of arc. Strictly speaking, these numbers should be added to the values listed in the table. Doing so makes the agreement in the case of the correction based on rotation alone a little less impressive: the discrepancy in longitude becomes 37 minutes, which amounts to 41.2 km. This corresponds to a 2 minute 28 second cumulative loss in the clock from extraneous sources over the 117 days of the voyage, still an encouragingly small number. More important, adding 22.2 km and 20 minutes of arc to the other rows in the table further increases the discrepancies for the three other approaches to correcting the clocks. In other words, if the fact that the Alcmaer was still 3 miles to the west of Texel on August 15 is taken into account, then the case against Varin, Richer and Newton becomes stronger still.

TABLE 5: DISCREPANCIES IN THE LONGITUDE OF TEXEL BASED ON CORRECTIONS TO THE LONGITUDES FROM HUYGENS'S CLOCKS AND THE VALUE BASED ON TACHARD'S LONGITUDE OF THE CAPE

WITH CORRECTIONS TO THE ALCMAER'S COURSE	DISCREPANCY IN LONGITUDE	DISCREPANCY IN KM
Based on rotation alone	0° 17′ E	19.0
Based on Varin's $\Delta\ell$ in Goree	4° 55′ E	329.7
Based on Richer's $\Delta\ell$	1° 35′ E	106.2
Based on Newton's theory	0° 59′ E	65.9

The conclusion we are reaching about the evidence available to Huygens is more important than it may at first seem. A close reading of Newton's *Principia* shows that the only clear empirical contrast it was offering between inverse-square gravity among celestial bodies and universal gravity among all particles of matter was the variation of surface gravity with latitude, coupled with the extent of the oblateness of the Earth.[50] Newton's theoretical derivation of the lengths of the seconds-pendulum in Cayenne and Goree in the first edition of the *Principia* presupposed universal gravity. Huygens was prepared to accept inverse-square celestial gravity on the basis of the *Principia*, but he had well known ***philosophical*** objections to universal gravity.[51] The lengths of the seconds-pendulum measured in Cayenne and Goree, however, could have been offered as empirical evidence for universal gravity. Newton, indeed, was inviting just such an interpretation in Proposition 20 of Book 3 of the *Principia*. But the voyage from the Cape of Good Hope to Texel was providing Huygens grounds for discounting these measured lengths. As he remarked in the first paragraph of the Addition to the *Discourse*, the voyage was showing that the observed variations in the length of the seconds-pendulum are evidence only for the rotation of the Earth. The voyage thus provided an ***empirical*** basis for challenging any claim to there being evidence for universal, over and above celestial, gravity. As our reconstruction suggests, Huygens did not have to appeal to philosophical arguments alone in rejecting it.

It may well have been this evidence that Leibniz had in mind when he wrote Conti in November or December of 1715: "I am strongly in favor of the experimental philosophy but M. Newton is departing very far from it when he claims that all matter is

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50 See George E. Smith, "Huygens's Empirical Challenge to Universal Gravity," forthcoming in George E. Smith op. cit. Note 1.

51 Huygens's philosophical objections to universal gravity are summarized in the *Discourse*, OCCH, Vol. 21, p. 471. In the immediately following discussion on the next page, and then again on p. 476, Huygens grants the full force of Newton's orbital analysis and moon test in verifying "the hypothesis" that the planets and their satellites are retained in their orbits by inverse-square centripetal forces that are one in kind with terrestrial gravity.

heavy (or that every part of matter attracts every other part) which is certainly not proved by experiments, as M. Huygens has properly decided..."[52] From *Oeuvres Complètes* we know that in some of his letters Huygens called attention to this evidence, even after the publication of the *Discourse*.[53] One of the correspondents in question was Leibniz.[54] Perhaps, then, from Leibniz's point of view, as well as Huygens's, the only empirical evidence that Newton had offered to support the step from inverse-square celestial gravity to universal gravity had been shown by the voyage not to be evidence at all.

Huygens's Own Assessment of the Evidence

The argument developed in the Report itself is more modest than the preceding reconstruction. Its sole announced conclusion, as summarized in the earlier quotation from the first paragraph of the Report, is that Huygens's clocks can be used to determine longitude at sea. The argument for this conclusion is developed in two steps. First, Huygens discusses the difference in longitude between the Cape and Texel, concluding:

> The true longitude, then, between the Cape and Texel is 14 degrees and 25 minutes; if the clocks yield the same or nearly the same longitude, then this is a demonstration of the soundness of this invention.[55]

Next, he describes the table, emphasizing that the correction he is introducing is principled, and not contrived. He then concludes:

> One sees here then how perfectly the clocks have measured the longitude between these two places, because on August 15, just before putting in at Texel, this longitude of the clocks had been 56 min. 34 sec. of time, which equals 14 degrees 8 and 1/2 minutes. Thus, the difference is only 16 and 1/2 minutes, which is about a quarter of a degree, which at the parallel of

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[52] See *Correspondance Leibniz-Clarke: présentée d'après les manuscrits originaux des bibliothèques de Hanovre et de Londres*, ed. Andre Robinet (Paris: Presses Universitaires de France, 1957), p. 43. We thank Daniel Garber and Roger Ariew for suggestions about this translation.

[53] See letter No 2617 of 2 September 1690, "Christiaan Huygens à D. Papin," OCCH, Vol. 9, pp. 482-487, for example.

[54] See letter No 2744 of 15 March 1692, "Christiaan Huygens à G. W. Leibniz," OCCH, Vol. 10, pp. 268-270.

[55] OCCH, Vol. 9, p. 274.

> Texel equals only 2 1/2 miles. Or if one adds to it the 3 miles to the west where the place of this observation was estimated, then the difference amounts to 5 and 1/2 miles, which one should consider small in light of such a long voyage.[56]

Although he does mention in passing the possibility of establishing the rotation of the Earth and the correct lengths of the seconds-pendulum, no conclusion about either of these is announced in the Report. The reasoning he presents clearly implies that gravity varies exactly in accord with the rotation of the Earth, but this too is never stated as such.[57] The Report focuses narrowly on the practical issue of concern to the Directors, and not on such "scientific" issues. Newton is not mentioned in the Report, but there is such a reference in the cover letter to Hudde, where Huygens remarks that the *Principia* "posits several hypotheses that I cannot approve of, and that lead to different conclusions than my reckonings give."[58]

The sole place where the scientific conclusions from the voyage were publicly announced was the first paragraph of the Addition to the *Discourse*, quoted at the beginning of this paper:

> ... as for the different lengths of the pendulums in different regions, which he [Newton] has also addressed, I believe to have, by the average of these clocks, a clear confirmation not only of the effect of the motion of the Earth but also of the measure of these lengths, which agrees very well with the calculation I have just given. For, having corrected and adjusted, following this calculation, the longitudes that were measured by the clocks on

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56 *Ibid.*, p. 285. (The original longitude numbers here have been overwritten in the handwritten text.) Huygens does not always include mention of the additional discrepency associated with the three nautical miles between the Alcmaer and the coast of Texel.

57 That is, in accord with the rotation of a spherical Earth. Throughout his calculations of the effect of the rotation of the Earth on the variation of effective gravity with latitude Huygens implicitly assumes a spherical Earth. In the Addition to the *Discourse* Huygens concludes that, assuming surface gravity in the absence of rotation is uniform, the rotational effect entails that the Earth must have an oblateness of 1/578, in contrast with Newton's oblateness of 3/689 of the first edition of the *Principia*. Factoring in the effect of this oblateness would alter Huygens's 1/289 at most to 1/288.5 and the maximum daily delay from 2 min. 30 sec. to 2 min. 29.5 seconds, both rounding to the original numbers. Huygens's calculation of this is reproduced in OCCH, Vol. 21, p. 396; according to the Editors of OCCH this calculation, which is surrounded by material related to the Report and the Addition to the *Discourse*, probably dates from November or early December 1687.

58 Letter No. 2517, 24 April 1688, "Christiaan Huygens à J. Hudde," OCCH, Vol. 9, p. 267. For an English translation of this letter, see "Huygens's Report to the Dutch East India Company," op. cit. Note 3.

> the return from the Cape of Good Hope to Texel in Holland (because going they were not of service), I have found that the route of the vessel was much better marked on the map than it would have been without this correction; so much so that arriving at this port there was not 5 or 6 leagues of error in the longitude thus adjusted....[59]

With minor qualifications,[60] this summary of the argument is accurate as far as it goes. Yet clearly it loses much of the detail. Whether then or now, anyone reading the *Discourse* alone, without access to the Report, will be unable to see the full force of the evidence the voyage had supplied Huygens.

One respect in which the summary in the *Discourse* falls short is that it understates the strength of the evidence that the clocks had lost time systematically in the first phase of the voyage, yet had largely regained it before turning south toward Texel. As such, the *Discourse* understates the grounds for concluding that some sort of correction of the clocks was needed. The map makes this most clear. On the one hand, the uncorrected course runs through the middle of Ireland and comes very near to the east coast of Scotland, which the true course surely did not. On the other, while the uncorrected course differs from the mariners' course by large amounts on the northern leg, the differences exhibit the pattern they should if the discrepancy in the time kept by the clocks was varying roughly as the square of the cosine of the latitude; and, more importantly, the two courses come back together north of Scotland just as they should. Huygens calls attention to both of these features in the Report,[61] but no mention of either of them occurs anywhere in the *Discourse*.

The other respect in which the summary of the argument in the *Discourse* falls short is that it understates the grounds Huygens had for rejecting any greater variation in gravity with latitude than that resulting from the Earth's rotation. The table in the Report, along with the description of how it was generated, makes clear first that the correction in

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59 Op. cit. Note 4.

60 Two qualifications seem needed: (1) de Graef did not employ an average of the clocks; and (2) 5 or 6 leagues may be a little less than the 5 or 5 and 1/2 German nautical miles Huygens gave in the Report.

61 See <u>OCCH</u>, Vol. 9, p. 274f.

the longitude from the clocks at Texel is of substantial magnitude and second that it is proportional to the largest daily delay from the Poles to the Equator. Given these two, any significantly larger value for this delay than Huygens's, and hence any greater variation in gravity with latitude, has to yield an unacceptably larger error in the longitude at Texel beyond Huygens's 16 and 1/2 minutes of arc. The core of this part of the argument lies in the details of the numbers.

In short, to see the full force of the evidence from the voyage, readers needed both the summary of the argument in the *Discourse* and the Report, or at least the map and the table from it. And even then they had to do some reasoning beyond that explicitly stated in either work in order to see the extent to which the evidence tended to override the measurements made by Richer and Varin *et al.* and to exclude Newton's alternative. This need for additional reasoning raises a question whether Huygens himself saw the full force of the argument for excluding any variation in gravity beyond that caused by the Earth's rotation. Maybe our reconstruction of the argument has reached beyond what Huygens himself saw. Maybe all he had in mind was that the data from the voyage were consistent with the claim that the Earth's rotation is the only cause, and -- pending evidence from subsequent voyages -- this, together with the philosophical objections to Newton's alternative and the anomalies in the measured variations in the length of the seconds-pendulum, made Huygens's account highly probable.

The clearest indication that Huygens saw the argument against Richer, Varin *et al.*, and Newton would be if he had generated tables based on their largest daily delays -- i.e. tables of the sort we had to generate in order to find the alternative courses shown in Figures 3 through 5 above. We have not been able to find any such tables in his notebooks. As we commented earlier, however, this is scarcely decisive, for the exclusionary force of the evidence ultimately depends on Texel alone, and he could easily have estimated the larger discrepancies in longitude for Texel in his head. His notebooks do reveal one pertinent calculation. While preparing the Report, Huygens definitely did calculate the shortening of the pendulum from Paris to Cayenne from rotation alone and compared it with Richer's value, finding a difference of 5/12 of a line.[62]

Moreover, an argument presented later in the Addition to the *Discourse* provides a strong indication that he did see the exclusionary force of the evidence from the voyage. Having rejected Newton's universal gravity, but having accepted inverse-square celestial gravity, Huygens considers what the eccentricity of the Earth and the variation in gravity would be if the inverse-square rule holds not only over celestial distances, but also at the surface of the Earth and below. He concludes that the radius to the Poles would be 1/578 shorter than the radius to the Equator and, owing to this eccentricity alone, the seconds-pendulum would have to be shorter at the Equator by a factor of 1/289:

> This is nearly the same difference that is produced for daily motion or centrifugal force. Thus a clock with the same length pendulum would run slower at the Equator than at the pole by twice what it would be slowed by the motion of the Earth; and so this daily difference at the Equator would be about 5 minutes. And at the other parallels, it would everywhere be more than twice what it was previously. But I strongly doubt that experience confirms this large of a variation, since I have observed, in the voyage that I mentioned that the first equation alone suffices, and it [this great variation] would give more than twice too great a difference around the middle of the course between the route of the vessel calculated with the pendulum and the route estimated by the Mariners.[63]

Since Huygens saw this, he surely must have seen at least the qualitative argument against Richer, Varin *et al.*, and Newton.

De Volder and perhaps Hudde were in a position to have seen the full force of the evidence from the voyage, though we know of no documents confirming that either ever did so. The interesting question, however, is whether anyone outside of Holland had an inkling at the time of how much more forceful the argument was than the summary in the

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62 Folio 160 of Workbook F, called "Hug.1" in the Huygens Archives.

63 <u>OCCH</u>, Vol. 21, p. 475f. Huygens adds: "And, to provide the reason why the second variation would not occur, I say that it would not be strange if the gravity near the surface of the Earth did not follow in precisely the same way as in the higher regions." Huygens expressly calls Papin's attention to this passage in the *Discourse* in his letter to Papin op. cit. Note 53.

Discourse indicated. None of the remarks Huygens made in his surviving correspondence on the subject mentioned either the uncorrected course's passing through Ireland or the numbers in the table. Huygens did, however, visit England in the summer of 1689. At a meeting of the Royal Society on June 12, with Newton present, he "gave an account that he himself was now publishing a Treatise concerning the Cause of Gravity, and another about Refractions giving amongst other things the reasons of the double refracting Island Chrystall."[64] A month later, on July 10, he, Newton, and Fatio de Duillier were together for 7 hours during a coach ride from Hampton Court to London.[65] How much he said about the voyage of the Alcmaer on these occasions is unclear, but the contrast between his view that only the rotation of the Earth causes a lessening of gravity at the Equator and Newton's view that there is an additional factor was almost certainly discussed.[66]

Joella Yoder has called our attention to a single sheet from a memorandum by David Gregory that gives reasons for thinking that Huygens did go beyond the Addition

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[64] Journal Book, 12 June 1689. This talk and the subsequent appearance of the *Treatise* and the *Discourse* in February 1690 provoked Hooke into giving two lectures before the Royal Society in February of 1690, the first in response to the *Treatise* and the second, to the *Discourse* (see A. R. Hall, "Two Unpublished Lectures of Robert Hooke," *Isis*, Vol. 42 (1951), pp. 219-230). In the second of these he said that the Addition "is concerning those proprietys of Gravity which I myself Discovered and shewed to this Society many years since, which of late Mr Newton has done me the favour to print and Publish as his own Inventions. And Particularly that of the Ovall figure of the earth was read by me to this Society about 27 years since upon the occasion of the Carrying the Pendulum Clocks to Sea And at two other times since, though I had the Ill fortune not to be heard" (Hall, p. 224). Hooke went on to discuss both the opening paragraph of the Addition, mentioning the voyage and Tachard's finding that the Cape is 18 degrees west of Paris (Hall, p. 226) and Huygens's argument that an inverse-square rule below the surface of the Earth must produce an excessive reduction of gravity at the Equator versus the experience during the voyage (*ibid.*). Nothing Hooke said, however, shows any indication that he had information about the voyage beyond what is presented in the Addition to the *Discourse*.

[65] See <u>OCCH</u>, Vol. 9, p. 333, n. 1. Joella Yoder has informed us of a passage in the diary of Christiaan Huygens's brother, Constantijn, which suggests that Newton also visited Christiaan at Constantijn's place at Hampton Court the day before the coach ride; see *Journalen van Constantijn Huygens, den Zoon*, reprinted in deel. 1 van "de Handschriften van de Koninklijke Akademie van Wetenschappen te Amsterdam" in the series, *Werken van het Historisch Genootschap*, No. 23, (Utrecht, Keminck & Zn, 1876-1888). At the time Constantijn was Secretary to William III, King of England.

[66] See, for example, Letter No. 2853, 9 April 1694, "N. Fatio de Duillier à [De Beyrie] (for Leibniz)," <u>OCCH</u>, Vol. 10, pp. 605-608, in which Fatio stresses the empirical contrast between Huygens and Newton. Earlier, in a letter of 21 April 1690, Fatio had told Huygens that Newton had assured him that he understood all the points made in the Addition (Letter No. 2582, "N. Fatio de Duillier à Christiaan Huygens," <u>OCCH</u>, Vol. 9, pp. 407-412).

in discussing the voyage of the Alcmaer at some point during his trip to England.[67] The sheet, dated 11 November 1691, contains a sequence of brief notes concerning different items Huygens had worked on. The passage of interest to us occurs in the middle of this sequence:[68]

> By observations of a ship from the Cape of Bonne Esperance to the Texel on board which was a two of these Clocks, the course of the ship was on the coast of Ireland on the supposition the weight was the same in all parts of the earth or the pendulys vibration in equal times, but if the the [sic] other hypothesis of the less weight at the Equator be true the course will be (as it was) by the north Scotland but both systems bring the ship to Texel. Huygens makes only one cause of the different gravity from the greater vis centrifuga in the greater parallels. But Newton adds another the different centripetation according to the different distances on the surface not exactly spherical.

The remark about the course going through Ireland shows that Gregory had information beyond the *Discourse*. The claim that both the uncorrected and corrected courses bring the ship to Texel shows that he did not get his information from the Report itself, either directly or indirectly. Also, neither the *Discourse* nor the Report contains a statement of the contrast between Huygens and Newton as accurate as Gregory's; thus Gregory appears to have had a better grasp of how the evidence from the voyage was posing a challenge to universal gravity than he could have obtained from reading either of these two.

We do not know how Gregory obtained this information about the voyage. Since he did not begin working closely with Newton on the proposed second edition of the *Principia* until 1694, Newton himself was probably not the source.[69] Fatio is a better

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[67] This sheet is among the handful of Gregory papers held by the Royal Society: MS 247 [Gregory Papers], f. 72r. The rest of the memorandum appears to be lost.

[68] The sheet starts in the middle of a sentence about Constantijn Huygens's 160 foot telescope, then makes a point about the mechanism of Christiaan Huygens's clocks, referring to the *Horologioum Oscillatorium*, followed by the remarks on the voyage quoted above, and ending with a discussion of Christiaan Huygens's Automoton Planetarii.

[69] Gregory resumed correspondence with Newton in August 1691, but nothing in their letters of 1691, nor in the memoranda accompanying some of them, bears in any way on Huygens. See *The Correspondence of Isaac Newton*, Vol. III, 1688-1694, ed. H. W. Turnbull (Cambridge: Cambridge at the University Press, 1961), pp. 165-184.

candidate, for Gregory was in contact with him at the time of the memorandum.[70] Regardless, the memorandum shows that at the time there was more awareness in Newton's circle of the empirical challenge the voyage of the Alcmaer had posed to universal gravity than could have been gained just from reading the *Discourse*. The ultimate source of this added awareness almost had to be Huygens himself.

As the opening paragraph of the Addition announces, the Report led the Directors of the Dutch East India Company "to direct us to make a second test in order to be assured by several experiments of the soundness of this discovery." Huygens, however, seems to have had little or no doubt about what the 1687 voyage had shown. What he says earlier in that paragraph appears to have been an entirely candid statement of his assessment of the situation: "I believe to have ... a clear confirmation not only of the effect of the motion of the Earth but also of these lengths." Moreover, the argument that he took to be providing this clear confirmation was at least close to our reconstruction of it. In particular, he definitely thought that an argument based on the cumulative time gained or lost by his pendulum clocks at different latitudes would ultimately be decisive.[71]

The "Weak Links" in the Argument

The Directors of the Dutch East India Company would have been foolish not to insist on a second sea-trial. The issue of concern to them was whether Huygens's clocks could be used to determine longitude at sea. The fact that the clocks had done so during the voyage of the Alcmaer -- taking for granted that it was a fact -- was not enough to re-

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70 In a memorandum of 28 December 1691 Gregory remarks, "Mr Fatio designs a new edition of Mr Newtons book in folio wherin among a great many notes and elucidations, in the preface he will explain gravity acting as Mr Newton shews it doth, from the rectilinear motion of particles the aggregate all which is but a given quantity of matter Dispersed in a given space." *Ibid.*, p. 191.

71 Curiously, Huygens never mentions the fact that Richer first detected a difference in Cayenne when he realized that the pendulum clocks were losing on average 2 minutes and 28 seconds per day (Richer, op. cit. Note 19, p. 280ff). By the way, the 2 minute 28 second number first appears in the second edition of Newton's *Principia*, and not in Richer's report. It is, in fact, the value one obtains by averaging the discrepancies between the length of the sidereal day and Richer's reported times of the meridional passing of various stars from one day to the next over the course of several weeks before he recalibrated his clock.

solve this issue. The seas might have been exceptionally tranquil during the voyage of the Alcmaer, and in rougher seas the clocks were not going to work nearly so well. This was less of a worry, however, in the case of the principal issue of scientific concern, whether the Earth's rotation and it alone causes a variation in gravity. Poor performance by the clocks during subsequent voyages in rougher seas would not necessarily discredit the evidence from the voyage of the Alcmaer. This voyage by itself could still be taken to have provided strong grounds for Huygens's scientific conclusions so long as one could discount the possibility that the evidence from it was illusory, a fortuitous by-product of some of the more tenuous premises entering the argument. Accordingly, so far as the scientific issue was concerned, the chief thing Huygens could have hoped to gain from a second sea-trial was evidence bolstering the weak links in the argument. In the two preceding sections of the paper we have been taking the argument at face value. Now we need to examine it more critically, focusing on its weakest links.

One potentially weak link was the assumption, largely tacit, that the clocks were keeping accurate time during the voyage, save for the effects of variations in gravity. Two possibilities needed to be ruled out. First, if the clocks were constantly gaining and losing significant amounts of time from day to day during the voyage, then the comparatively small error at Texel might well be a mere accident, with no evidential import. Second, if the clocks were biased either from the outset or they had become so during the voyage, then the comparatively small error in Texel was likely the product of two systematic errors canceling one another, the one in the clocks and the other in Huygens's corrections for the variation in gravity, so that the small discrepancy provided no evidence for Huygens's corrections in preference to others.[72] The only counter to these possibilities that Huygens mentions in the Report is the generally good agreement

[72] Huygens expressly acknowledges only the second of these possibilities in the Report: first, when he dismisses the observation using Clock B on June 8 (OCCH, Vol. 9, p. 286); and again in the comments at the end of the Report when he points out that rough seas can cause the lead of the pendulum to drop slightly on its rod (*ibid.* p. 288), because of which the clocks were far more prone to developing a bias during a voyage causing them to lose time than one causing them to gain.

between the mariners' course and his corrected course throughout most of the voyage. This agreement scarcely closed the issue, however. The mariners' course was not altogether trustworthy to begin with, and the data allowed Huygens's course to be compared with it on fewer than one-fifth of the days at sea. Moreover, the corrected course was the one being compared, and the schedule of corrections was itself one of the elements at issue. Hence, relying on this comparison to discount the possibility that the clocks were biased or were fluctuating significantly from day to day suffered from the shortcoming lawyers call "building inferences on inferences." Good agreement with the mariners' course on August 3, when the cumulative correction was 0, would have provided a response to this last worry. Unfortunately, however, the comparison for August 3 is exceptionally bad, for reasons Huygens was apparently prepared to attribute to errors in the mariners' course.[73]

The only other source Huygens could have turned to for evidence that the clocks were behaving well was to compare the longitudes implied by them with independently determined longitudes for landmarks sighted along the voyage. Data from the clocks was available for two such landmarks -- Fulo Island on July 29 and the southern tip of the Shetlands on August 1. These two have the virtue that the cumulative corrections for the clocks on these days were comparatively small. As Huygens points out in the Report, however, the voyage itself raised serious questions about what the correct longitudes of these two are: the map of Dirk Rembrandtz,[74] which he was using, located Fulo 3 degrees west of the location indicated by the corrected clock, and the map of the mariners apparently located it almost 3 degrees to the east of this location.[75] Therefore, until the longitudes of these sighted landmarks were better established, the only evidence Huygens could turn to in order to assess whether the clocks were behaving well was the generally

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[73] As pointed out on p. 14 above, Huygens never says anything about this large discrepancy.

[74] This map, which dates from the late 1650s, is discussed in the appendix to this paper.

[75] *OCCH*, Vol. 9, p. 287. The manuscript indicates that the passage on Fulo and Rembrandtz's map was extensively rewritten in the Report.

good agreement between his corrected course and the mariners' course. Huygens had good reason to emphasize the need for better substantiated maps at the end of the Report, for in their absence he could not do more to eliminate the possibility that the small discrepancy at Texel was a mere coincidence.

From our modern "omniscient" position, knowing the true longitudes of Fulo, the Shetlands, and Texel, as well as the true variation of gravity with latitude, we can assess how well Clock A was in fact doing near the end of the voyage. On July 29 it was around 2 minutes behind what it should have been; on August 1, more than 4 and 1/2 minutes behind; and on August 15, around 5 and 1/4 minutes.[76] Both the large jump between July 29 and August 1 and the magnitude of the true discrepancy at the end of the voyage are disconcerting. Although Huygens had no way of realizing it, his assumption that Clock A had behaved reasonably well during the voyage was mistaken.

The other potentially weak link in the argument was Huygens's newly inferred value of 14 degrees 25 minutes for the difference in longitude between the Cape and Texel. The *Discourse* mentions only Tachard's 18 degree difference between the Cape and Paris, suggesting that, once it was established, the longitude from the Cape to Texel followed unproblematically; the *Discourse* never mentions Riccioli or the 3 degree 35 minute difference between Paris and Texel that the Report singles out as coming from page 378 of his *Geography*.[77] An error in the value he had obtained from Riccioli,

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76 These numbers presuppose that Fulo Island is 20 degrees 33 minutes west of the Cape; the southern tip of the Shetlands, 19 degrees 46 minutes; Texel, 13 degrees 43 minutes; and the true variation in gravity is 4 percent greater than implied by Richer's measurement. The numbers are rough because they include allowances for the mariners' estimates of the distances from the ship to the landmarks. The numbers are predicated on reference values for the modern longitude at the Cape of 18 degrees 28 minutes, where 1 minute of arc amounts to 1.53 kilometers, and 4 degrees 45 minutes at Texel, where 1 minute amounts to 1.11 kilometers. The modern official values are 18 degrees 26 minutes for the tip of the Cape and 4 degrees 51 minutes for Texel. Our choices put our reference location for the Cape some 3 kilometers east of the tip, on the northern shore of the bay, and our reference location for Texel 6.6 kilometers to the west of the official modern value (because the old port was located on the west, not the east, side of the island).

77 In Huygens's handwritten copy of the Report, 'p. 378' is mentioned only as a note in the left-hand margin. In fact, Riccioli's page 378 does not mention Texel. It discusses eclipses observed at Paris and Amsterdam and the longitudes they implied. In a subsequent long list, Riccioli gives the longitude of Amsterdam as 27 degrees 55 minutes E (p. 403), the longitude of Paris as 24 degrees 30 minutes E (p. 419), and the longitude of Texel as 28 degrees 5 minutes E (p. 423) -- with 0 longitude 2 degrees west of

however, would have jeopardized Huygens's argument no less than an error in the value he had obtained from Tachard.

In fact, both were wrong. The correct value for the longitude from the Cape to Paris is 16 degrees 7 minutes and not 18 degrees, and from Paris to Texel, 2 degrees 24 minutes and not 3 degrees 35 minutes. The 1 degree 53 minute error in Tachard's value is offset to a considerable extent by the 1 degree 11 minute error in the value Huygens used for Paris to Texel. Hence, the correct value of the longitude of Texel -- 13 degrees 43 minutes west of the Cape -- is in fact merely 42 minutes less than the 14 degree 25 minute value Huygens used in the Report.

In the review of the Report that he wrote for the Dutch East India Company, de Volder expressed a doubt about Huygens's 14 degrees 25 minutes.[78] He first decided that Huygens's value of 3 degrees 35 minutes had come from assuming that Amsterdam is 3 degrees 52 minutes east of Paris and Texel is 17 minutes west of Amsterdam.[79] He then challenged the value from Paris to Amsterdam, calling attention to La Hire's more recent finding of 2 degrees 32 or 33 minutes. La Hire's value would have placed Texel 15 degrees 44 minutes west of the Cape according to de Volder's numbers, and 15 degrees 17 minutes west of the Cape according to the numbers Huygens had taken from Riccioli -- either way thoroughly undermining Huygens's argument.

Tenerife. Huygens calculated his value of 3 degrees 35 minutes from these longitudes for Paris and Texel ("Hug.1" --i.e. Workbook F-- folio 163 in the Huygens Archives), but noted the value for Amsterdam as well a little later (folio 168).

[78] Letter No. 2547, "B. De Volder aux Directeurs de la Compagnie des Indies," 22 July 1689, OCCH, Vol. 9, pp. 339-343. (An English translation of De Volder's letter accompanies the translation of Huygens's Report in George E. Smith op. cit. Note 3.) The Directors of the Dutch East India Company forwarded de Volder's review to Huygens on 9 September 1689. De Volder also expressed a worry about the effects of variations in temperature on the performance of the clocks. Unacceptable effects of changes in temperature that he could find no way to compensate for had led Huygens to abandon his efforts on a spring clock in the early 1680s (see Leopold, op. cit. Note 15, pp. 108-109). But he never believed that the effect on pendulum clocks was that severe. In fact, a 22 Deg C (40 Deg F) temperature change produces a 16 second per day change in the pendulum motion if the thread is bronze, copper, or iron. This corresponds closely to the value Newton gave for the difference between winter and summer in the third edition (1726) of the *Principia* (Book 3, Proposition 20).

[79] *Ibid.*, p. 341. The 17 minute number was common in Holland at the time. The correct numbers are 2 degrees 33 minutes from Paris to Amsterdam and 9 minutes (west) from Amsterdam to Texel.

Huygens was not in a position to dismiss de Volder's challenge out of hand. He had had a difficult time coming up with a reasonably unobjectionable map while he was reviewing de Graaff's data from the voyage, leading him to comment in the Report on "how much difference and [how many] errors are found in maps up until now, and how little they can be trusted."[80] Furthermore, the map that accompanies the Report in the *Oeuvres Complètes* has Texel around 2 degrees 35 minutes west of Paris, not 3 degrees 35 minutes, as he says in the Report.[81] If the European portion of this map was taken from Rembrandtz's map, as he says in the text of the Report, then Rembrandtz's map was giving Huygens added reason to take de Volder's challenge seriously. Concern over Riccioli's value may be part of the explanation for why Huygens never published the full argument from the voyage. Even more to the point, it may explain why Huygens was so specific in giving Tachard's value of 18 degrees for the difference in longitude between the Cape and Paris in the *Discourse*, yet neglected to mention Riccioli there, and never gave any value for the difference in longitude between the Cape and Texel, the number his argument was actually predicated on. Had he done so, those in the French Academy would have noticed a difficulty right away.

We have been unable to find any place where Huygens expressly acknowledged the issue de Volder raised. He did not mention it in his letter to the Directors on de Volder's review, in his subsequent correspondence on the subject with de Volder, nor in his more extensive correspondence with La Hire.[82] Still, in the months following his receipt of de Volder's review, he did show increased interest in the satellites of Jupiter and Tachard's measurement. Tachard had been given tables and ephemerides for these

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80 OCCH, Vol. 9, p. 287.

81 This map has Paris around 18 degrees west of the Cape, conforming with Tachard's value, and consequently Texel around 15 degrees 20 minutes west of the Cape, not 14 degrees 25 minutes as Huygens says in the Report. These and other curious features of this map are discussed in detail in Appendix 1 to this paper. Regardless of what else may be said about it, the handwriting on it does appear to be Huygens's.

82 See Huygens's letter to the Directors of 10 May 1690 (OCCH, Vol. 9, Letter 2588, p. 418), his letters to de Volder of 24 March and 19 April 1693 (Volume 10, Letters Nos. 2798 and 2799, p. 433ff and Letters Nos. 2802 and 2803, p. 442f), and his letters to La Hire (Volumes 9 and 10, *passim*).

satellites by "Members of the Academy."[83] These were surely not Cassini's tables of 1668, for Ole Römer had exposed a systematic discrepancy in those tables in 1676 (which he and others, though not Cassini, attributed to the finite speed of light).[84] Cassini had not yet published new tables in 1685, nor did he publish ephemerides for 1685 in the *Journal de Sçavans* (nor anywhere else prominent). Huygens knew from La Hire that Cassini had been updating his predictions of the eclipses of these satellites;[85] in the Report, he remarks that "these tables are calculated by him every year and tested by observation, so as to increase certainty."[86] Nevertheless, Huygens himself appears not to have had copies of either the tables or the ephemerides given to Tachard. In December of 1689 he informed his brother, Constantijn, that he had received John Flamsteed's ephemerides for the satellites of Jupiter.[87] Shortly thereafter, three weeks before the *Discourse* appeared in print, he inquired of La Hire whether Tachard's 18 degree value required a minor correction, reducing it to 17 and 1/2 degrees.[88]

Tachard had in fact introduced a small error when he used the leading edge and trailing edge of the Sun, rather than its midpoint, to determine the time of sunrise and

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[83] "They gave us the Tables of the *Satellites* of *Jupiter*, which have been made with so great labour, and which at present serve for determining the Longitudes of Places." Tachard, op. cit. Note 38, p. 6.

[84] The ideal way of determining the difference in longitude between two places was by simultaneous observations of the immersion or emersion of the innermost satellite of Jupiter at the two locations, for the differences in the local solar times of the observations then gave the difference in longitude directly. Efforts were made at the Paris Observatory to make such observations regularly in order to expedite comparisons with ones made elsewhere. When only a single observation was available, tables predicting the time of the eclipses (e.g. at Paris) had to be used. These tables were subject to two important sources of error besides possible inaccuracies in the orbit of the satellite: (1) speed of light effects, as discovered by Römer; and (2) the yet to be determined, slowly progressing vagaries in the orbit of Jupiter arising from the gravitational action of Saturn, vagaries that were not sorted out until Laplace's discovery of the "Great Inequality" in the mid 1780s. Up-to-date tables incorporating corrections for the speed of light effects were therefore required to achieve accurate determinations of longitude in the absence of simultaneous observations. See Suzanne Débarbat and Curtis Wilson, "The Galilean satellites of Jupiter from Galileo to Cassini, Römer and Bradley," *The General History of Astronomy*, Vol. 2A, *Planetary astronomy from the Renaissance to the rise of astrophysics, Part A*, ed. R. Taton and C. Wilson (Cambridge: Cambridge University Press, 1989), pp. 144-157,

[85] Letter No. 2462, "Ph. de la Hire à Christiaan Huygens," 1 June 1687, OCCH, Vol. 9, p. 163.

[86] OCCH, Vol. 9, p. 274.

[87] *Ibid.*, Letter No. 2555, "Christiaan Huygens, à Constantyn Huygens, frère," 23 December 1689, p. 354. Flamsteed had published ephemerides for the satellites of Jupiter in *Philosophical Transactions of the Royal Society* for the years 1684, 1685, 1686, and 1687.

[88] *Ibid.*, Letter No. 2557, "Christiaan Huygens à Ph. de la Hire," 18 January 1690, p. 357.

sunset on 4 June 1685. Father Thomas Goüye corrected this error in the 1692-93 review of Tachard's observation, changing the adjusted time of the observation at the Cape from 9 hours, 37 minutes, 40 seconds to 9 hours, 36 minutes, 38 seconds and the consequent longitude from the Cape to Paris to 17 degrees 44 minutes.[89] Whether Huygens became aware of Goüye's correction before he died in 1695 is unclear. But this correction does raise questions about what conclusions members of the Academy would have reached had they conducted a thorough critical review of Huygens's evidence following the publication of Goüye's notes.

By the 1690s the most worrisome source of potential error in Tachard's value was clearly the ephemerides of Jupiter's satellites that he had used. If Huygens, or anyone else, took the trouble to use Flamsteed's ephemerides in order to check Cassini's value for the time of the emersion of the satellite in Paris on 4 June 1685, they obtained not 8 hours 25 minutes 40 seconds, but 8 hours 28 minutes 20 seconds. With the Flamsteed value, the longitude from the Cape to Paris implied by Tachard's observations drops to 17 degrees 4 and 1/2 minutes.[90] Huygens learned from two letters from La Hire in the summer of 1690 that Cassini was developing new tables for the satellites of Jupiter that were to incorporate a newly revised orbit for the planet.[91] These tables finally appeared in 1693, and Halley issued a version for London, with commentary, in 1694.[92] If Huygens, or

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89 "Observations physiques et mathematique, pour servir a la perfection de l'astronomie et de la geographie, Envoyées de Siam à l'Académie Royale des Sciences à Paris, par les Peres Jésuites Francois qui vont à la Chine en qualité de Mathématiciens dy Roy, avec les reflexions de Messieurs de l'Académie, & quelques Notes du P. Goüye," *Mémoires de L'Académie Royale des Sciences Depuis 1666 jusqu'à 1699*, Vol. VII, Part II, pp. 607-813, especially pp. 611-613.

90 For the Cassini value, see *ibid.* and Tachard, op. cit. Note 38; for the Flamsteed value, see "A Letter from the learned Mr. John Flamsteed Astron. Reg. concerning the Eclipses of Jupiters Satellit's for the year following 1685. with a Catalogue of them, and informations concerning its use," *Philosophical Transactions of the Royal Society*, Vol. 14, No. 165, 1684, pp. 760-765. (June 4 is May 25 in Flamsteed's ephemerides, and 9 minutes 20 seconds have to be added to his values to give the time in Paris.)

91 Letter No. 2589, 11 May 1690, "Ph. de la Hire à Christiaan Huygens," OCCH, Vol. 9, p. 421, and Letter No. 2616, 30 August 1690, "Ph. de la Hire à Christiaan Huygens," OCCH, Vol. 9, p. 481.

92 Cassini, "Les Hypothéses & les Tables des Satellites de Jupiter, reformées sur les nouvelles Observations," *Mémoires de L'Académie Royale des Sciences Depuis 1666 jusqu'à 1699*, Paris, 1729, Volume VIII, pp. 315-505; and Halley, "Monsieur Cassini his New and Exact Tables for the Eclipses of the First

anyone else, had bothered to compare the value from Cassini that Tachard had used for June 4 with the value from his new tables, they would have obtained not 8 hours 25 minutes, 40 seconds, but 8 hours, 30 minutes, 13 seconds. With this value the longitude from the Cape to Paris implied by Tachard's observations drops to 16 degrees 36 minutes, and the longitude from the Cape to Texel, using La Hire's value for the longitude from Paris to Amsterdam, becomes 14 degrees 20 minutes. In other words, a properly conducted critical re-examination of the matter in 1694 or 1695 would have concluded that Huygens's original value of 14 degrees 25 minutes for the Cape to Texel was at worst a little on the high side! Huygens's argument would have emerged intact. Exposing the error in it would not have been feasible even with the best data available within the Academy at the time Huygens died.

More curiously still, even the modern value of 13 degrees 43 minutes for the Cape to Texel does no real harm to Huygens's argument. With this value, the August 15 entry for the corrected course would place the Alcmaer 25 arc-minutes to the west of Texel. But the Alcmaer ***was*** west of Texel that day -- by the mariners' estimate, 3 (German) nautical miles west. Table 6 lists the discrepancies between the corrected courses and the modern value for the longitude of Texel in two ways: first, using Texel itself as reference point, as we did in Table 5 earlier, and then using 22.2 km west of Texel as reference point. Using Texel as reference point, Huygens still has an argument that a correction

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TABLE 6 AROUND HERE

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based on rotation alone will suffice; but his prior argument against a correction based on Newton's theory, or even one based on Richer's measurement, has lost much of its force. Using the point west of Texel as reference point, by contrast, his argument against

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Satellite, reduced to the Julian Stile, and Meridian of London," *Philosophical Transactions of the Royal Society*, Vol. 18, No. 214, 1694, pp. 237-256.

TABLE 6: A COMPARISON OF THE DISCREPANCIES IN THE LONGITUDE OF TEXEL USING THE MODERN VALUE FOR TEXEL

	USING TEXEL AS THE POINT OF REFERENCE		USING 22.2 KM WEST AS THE POINT OF REFERENCE*	
	DISCREPANCY		DISCREPANCY	
WITH CORRECTIONS TO THE ALCMAER'S COURSE	LONGITUDE	KM	LONGITUDE	KM
Based on Huygens	0° 25′ W	27.9	0° 5′ W	5.7
Based on Varin's $\Delta\ell$	4° 13′ E	282.2	4° 33′ E	304.4
Based on Richer's $\Delta\ell$	0° 53′ E	59.1	1° 13′ E	81.3
Based on Newton	0° 17′ E	19.0	0° 37′ E	41.2

* The estimated 15 August location of the Alcmaer offshore of Texel

Newton and Richer becomes only slightly weaker than it was from the numbers in Table 5 above; and the agreement between the independently established longitude and his corrected course becomes nothing short of spectacular -- a discrepancy of only 5 minutes of arc west of where the Alcmaer was estimated to have been. In other words, if Huygens had had the true longitude from the Cape to Texel to begin with, he could have presented an even stronger argument than in the Report.

Needless to say, the conclusion Huygens reached from the voyage about the variation of gravity with latitude was wrong. The proper correction to the ship-board clocks for the daily delay of the clock from the Pole to the Equator is around 2 percent greater than the one implied by Richer's measurement, and hence slightly less than 1.5 times greater than from rotation alone. The systematically misleading element in the evidence, however, was not Huygens's mistaken value for the longitude from the Cape to Texel. The error lay in the fact that the uncorrected clock was behind by around 5 and 1/4 minutes on the day when the Alcmaer reached Texel, placing the ship east of where it should have. The 11 minute 34 second (negative) cumulative correction for the clock for this day, based on rotation alone, was 5 minutes and 34 seconds smaller in magnitude than it should have been, yielding a smaller eastward correction than it should have. The two discrepant numbers offset one another nearly perfectly.

The Dutch East India Company's second sea-trial of Huygens's clocks, carried out from 1690 to 1692, was not so successful as the first. Nevertheless, Huygens did not see the results from it as grounds for suspecting that he might have been systematically misled by vagaries in the clock in the first trial. Near the end of his "Explication and Notes on the Journal of Jo. de Graef [sic]," reviewing the second-trial, he concluded, "I have shown, then, that the clocks have either performed successfully, or were not enabled to do so."[93]

The error in the first trial was difficult to expose because three separate items were uncertain at the time: (1) how well the clocks were performing at sea, save for the effect of variations in gravity; (2) the proper correction for variations in gravity; and (3) the true longitudes of landmarks sighted during sea-trials. Sea-trials of Huygens's clocks were going to expose the inadequacy of the correction based on rotation alone only if they included several sighted landmarks, the longitudes of which had been independently and reliably established. Huygens's recommendation to the Directors of the Dutch East India Company that major efforts be undertaken to determine longitudes for numerous locations, using eclipses of the innermost satellite of Jupiter, was thus right on target.[94] Nonetheless, the second sea-trial did not include such landmarks.[95] Worse, it was not even designed to include them.

The best hope Huygens or anyone else at the time had for exposing the misleading element in the first sea-trial lay in the August 1 sighting of the southern tip of the Shetland Islands. Huygens's correction to the clock for August 1 was only 1 minute 54 seconds (to the west), implying that the Alcmaer was 18 degrees 32 minutes west of the Cape. This was 1 degree 25 minutes farther east than the Alcmaer in fact was. Consequently, if the true longitude of this landmark had become known, there would have been no escaping the conclusion that the clock was behind by more than 4 minutes on this day. That would not have exposed the inadequacy of the correction based on rotation alone. But it would have undercut the assumption that the clock was correct,

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93 "Verklaeringh en aenmerckingen op het Journal van Jo. de Graef en 't geen ontrent de Horlogien is voorgevallen in de laetste proeve der Lengdevindingh Anno 1690, 1691, 1692," *OCCH*, Vol. 18, p. 649. A discussion of Huygens's reaction to the second sea-trial can be found in Appendix 2.

94 The Report, *OCCH*, Vol. 9, p. 290f.

95 The leg of the journey Huygens used in assessing the results was from St. Jago (now São Tiago) in the Cape Verde Islands to the Cape. As we discuss in Appendix 2 below, the correct difference in longitude between these two was very much at issue in this assessment.

save for the effects of variations in gravity, on August 15, and Huygens's argument would then have lost most of its force.

Conclusion

Given the information Huygens did have, he had a clear basis for making the claims he put forward in the first paragraph of the Addition to the *Discourse*. His argument would have been stronger still if the data had provided corroborating comparisons between his corrected course and independently established longitudes at several sites along the voyage. Still, the close agreement at Texel, after 117 days, was no small thing. At the very least it provided Huygens with strong *prima facie* grounds for his conclusion that the only variation in surface gravity results from the Earth's rotation. It was surely enough to put the burden of proof on anyone claiming that the variation in gravity was larger than this.

To Huygens's credit, he did attempt to obtain a second, complementary set of data on the voyage of the Alcmaer. He cannot be held responsible for the failure to obtain the requested measurements of the length of the seconds-pendulum. One can only conjecture about what conclusions he would have drawn had he received such measurements. On the one hand, he would have been faced with the evidence from the corrected course and, on the other, the evidence from the measurements of the seconds-pendulum. If accurate, the latter would have supported Richer's measurement -- and hence, indirectly, Newton's theory, for, as Newton had emphasized in the first edition of the *Principia*, any larger increment in the length of the seconds-pendulum than he had initially predicted was simply an indication that the Earth, instead of being uniformly dense, is more dense toward its center than at its surface. Maybe Huygens would have discounted the measured variations in the length of the seconds-pendulum as too small to be reliable, or maybe he would have started worrying a good deal more than he did about the possibility that he was being misled by the close agreement at Texel. As matters stood, the evidence he had did not present him with any such problem. He will not be the last great scientist to be deceived by limited data.[96]

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1 August 2000

[96] We would like to acknowledge Dirk de Vries and Silvia Vermetten-Compaan of the DOUSA at the library of the University of Leiden, and Jan Werner of the map collection of the Library of the University of Amsterdam, as well as the various archivists at the libraries of the Tropenmuseum and the Scheepvaartmuseum in Amsterdam, the Newberry Library in Chicago, and the Burndy Library at The Dibner Institute for the History of Science and Technology in Cambridge for their assistance in researching this paper. Special thanks are due to Joella Yoder, who not only provided much encouragement, but who was also generous in sharing her expertise and her computerized catalogue, without which the Huygens Archives would have been far less accessible to us, and also to J. H. Leopold of the British Museum who helped us greatly in dealing with the clocks and provided detailed commentary on Schliesser's translation of the Report. Finally, we were fortunate to be able to query H. Floris Cohen, I. Bernard Cohen, Anne van Helden, Rob van Gent, Mordechai Feingold, Peter van der Krogt, Guy Picolet, Piet Steenbakkers, Alice Stroup, and Curtis Wilson; we received helpful comments from Michael Mahoney, Karen Bailey, and Miles Jackson. Of course, any errors in the paper should be attributed solely to its authors.

	I. Days.	II. Latitude from the mariners' journal.	III. Longitude E. of Tenerife from the mariners' journal.	IV. Longitude W. of the Cape according to the mariners.	V. Hours of longitude W. of the Cape by the clocks without correction.	VI. Largest daily delay of the clock.	VII.[97] Correction of the clocks and the longitudes.	VIII. Hours of corrected longitude W. of the Cape by the clocks.	IX. Degrees of corrected longitudes W. of the Cape.	X. Difference in longitude between the corrected clocks and the mariners.	
		deg. min.	deg. min.	deg. min.	h. m. s.	min. sec.	min. sec.	h. m. s.	deg. min.	deg. min.	
A P R I L	20	[of] southern latitude.				1, 42					On April 20 the anchor was raised before sundown with little wind. In the evening Robben Island [was] passed. The course was NW. by W. until the latitude of St. Helena. From De Graef's notes.
	21					1, 43					On the 21st ditto. took bearings from the Tafelberg 2 and 1/2 or 3 miles SS. East of us. Otherwise the whole day we floated without wind.
	22	33, 38	36, 42	1, 18		1, 44					
	23	33, 18	36, 17	1, 43		1, 45					
	24	33, 0	35, 57	2, 3		1, 45					
	25	31, 11	34, 23	3, 37		1, 50					
	26	30, 7	32, 31	5, 29		1, 52					
	27	28, 56	30, 28	7, 32		1, 55					
	28	27, 44	28, 27	9, 33		1, 57					
	29	26, 7	26, 27	11, 33		2, 1					
	30	25, 7	24, 48	13, 12		2, 3					
M	1	24, 11	23, 16	14, 44		2, 5					
A	2	23, 13	21, 42	15, 18		2, 7					
Y	3	22, 7	20, 1	17, 59		2, 9					
	4	21, 19	18, 36	19, 24		2, 10					
	5	20, 29	17, 18	20, 42		2, 13					
	6	19, 33	15, 49	22, 11		2, 13					
	7	18, 31	14, 11	23, 49		2, 15					
	8	17, 20	12, 32	25, 28		2, 17					Decided to take a NW. course north of the latitude of Ascension. From De Graef's notes.
	9	16, 41	10, 58	27, 2		2, 18					
	10	15, 1	9, 48	28, 12	1, 40, 5	2, 20	6, 42	1, 46, 47	26, 42	1, 30	
	11	13, 5	8, 35	29, 25		2, 22					
	12	11, 29	7, 31	30, 29		2, 24					
	13	10, 2	6, 32	31, 28		2, 25					
	14	8, 33	5, 34	32, 26		2, 26					
	15	7, 12	4, 40	33, 20		2, 28					The course is NW. From De Graef's notes.
	16	5, 46	3, 49	34, 11		2, 28					
	17	4, 01	2, 49	35, 11		2, 29					
	18	2, 46	2, 0	36, 0		2, 29					
	19	1, 36	1, 13	36, 47		2, 29					

[97] That is, VII gives the total amount of time the clock has fallen behind (or gained) since the day of departure, which is added to (or subtracted from) V to give VIII, the corrected longitude in hours.

	I Days.	II Latitude from the mariners' journal.	III Longitude E. of Tenerife from the mariners' journal.	IV Longitude W. of the Cape according to the mariners.	V Hours of longitude W. of the Cape by the clocks without correction.	VI Largest daily delay of the clocks.	VII Correction of the clocks and of the longitudes.	VIII Hours of corrected longitude W. of the Cape by the clocks.	IX Degrees of corrected longitudes W. of the Cape.	X Difference in longitude between the corrected clocks and mariners.
		deg. min.	deg. min.	deg. min.	h. m. sec.	min. sec.	min. sec.	h. m. sec.	deg. min.	deg. min.
		S. Lat.								
	20	0, 59	0, 40	37, 20		2, 30				
M		N. Lat.								
A	21	0, 15	359, 56	38, 4		2, 30				
Y	22	1, 29	358, 59	39, 1		2, 30				
	23	2, 24	358, 3	39, 57		2, 29				
	24	3, 56	357, 15	40, 45		2, 29				
	25	4, 12	356, 51	40, 59		2, 29				
	26	5, 3	356, 31	41, 29		2, 28				
	27	5, 39	355, 47	42, 4	2, 24, 55	2, 28	19, 41	2, 44, 36	41, 9	0, 55
	28	6, 11	355, 29	42, 13		2, 28				
	29	6, 43	354, 50	42, 31		2, 28				
	30	6, 47	354, 4	43, 10	2, 27, 29	2, 28	21, 59	2, 49, 28	42, 22	0, 48
	31	7, 12	353, 13	43, 56	2, 28, 27	2, 28	22, 45	2, 51, 12	41, 48[98]	1, 8
J	1	8, 4		44, 47		2, 27				
U	2	9, 10	352, 13	45, 47	2, 38, 19	2, 26	24, 14	3, 2, 33	45, 38	0, 9
N	3	10, 15	351, 6	46, 54		2, 25				
E	4	11, 30	349, 54	48, 6		2, 23				
	5	12, 38	348, 45	49, 15		2, 22				
	6	14, 4	347, 32	50, 28		2, 21				
	7	15, 22	346, 55	51, 5		2, 20				

NB. My miscalculation in the 7th column and those [errors] which follow from it in the following columns, has been corrected from this point according to the reckoning of Mr. de Volder.[99]

	I	II	III	IV	V	VI	VII	VIII	IX	X
	8	16, 42	346, 22	51, 38	2, 44, 44	2, 18	28, 11	3, 12, 55	48, 14	3, 24
	9	18, 3	345, 46	52, 14		2, 16				
	10	19, 26	345, 13	52, 47	2, 58, 51	2, 14	29, 17	3, 28, 8	52, 2	0, 45
	11	20, 36	344, 51	53, 9		2, 12				
	12	21, 42	344, 30	53, 30		2, 10				
	13	22, 45	344, 7	53, 53		2, 7				
	14	23, 54	344, 0	54, 0		2, 5				
	15	25, 0	343, 53	54, 7		2, 3				
	16	26, 7	343, 32	54, 28	3, 3, 22	2, 1	31, 43	3, 35, 5	53, 46	0, 42
	17	27, 9	343, 1	54, 59		1, 59				
	18	28, 25	342, 44	55, 16	3, 6, 8	1, 56	32, 14	3, 38, 22	54, 35	0, 41

[98] 41, 48 is an error in both *OCCH* and the manuscripts; the correct number is 42, 48.

[99] See footnote 78 for bibliographical information on de Volder's corrections and footnote 108 for further details on the corrections.

	I. Days.	II. Longitude from the mariners' journal.	III. Longitude E. of Tenerife from the mariners' journal.	IV. Longitude W. of the Cape according to the mariners.	V. Hours of longitude W. of the Cape by the clocks without correction.	VI. Largest daily delay of the clocks.	VII. Correction of the clock and of the longitudes.	VIII. Hours of corrected longitude W. of the Cape by the clocks.	IX. Degrees of corrected longitude W. of the Cape.	X. Difference of the longitude between the corrected clocks and the mariners.	
		deg. min. N. Lat.	deg. min.	deg. min.	h. m. sec.	min. sec.	min. sec.	h. m. sec.	deg. min.	deg. min.	
[J	19	29, 58	342, 35	55, 25		1, 52					
U	20	31, 29	342, 35	55, 25		1, 49					
N	21	33, 20	342, 42	55, 18		1, 44					
E]	22	34, 55	343, 4	54, 56	3, 2, 28	1, 41	32, 32	3, 35, 0	53, 45	1, 11	
	23	36, 21	343, 41	54, 19		1, 37					
	24	37, 15	343, 4	54, 56		1, 35					
	25	38, 9	343, 4	54, 56		1, 33					
	26	39, 43	343, 4	54, 56		1, 29					
	27	41, 38	344, 7	53, 53	2, 57, 5	1, 24	31, 40	3, 28, 45	52, 11	1, 42	
	28	41, 4	344, 53	53, 7		1, 25					
	29	41, 34	345, 21	52, 39		1, 24					
	30	42, 39	346, 19	51, 41		1, 21					
J	1	43, 44	347, 19	50, 41		1, 18					
U	2	44, 19	347, 51	50, 9		1, 17					
L	3	45, 23	348, 46	49, 14	2, 44, 20	1, 13	29, 26	3, 13, 46	48, 26	0, 48	
Y	4	46, 26	350, 16	47, 44		1, 11					
	5	47, 48	352, 17	45, 43		1, 7					
	6	49, 0	353, 47	44, 13	2, 20, 20	1, 4	27, 42	2, 48, 2	42, 0	2, 13	
	7	49, 44	354, 48	43, 12		1, 2					
	8	50, 28	356, 34	41, 26	2, 8, 1	0, 59	26, 19	2, 34, 20	38, 35	2, 51	
	9	52, 14	358, 37	39, 23		0, 56					From July 9 until the 24th neither sun nor moon were visible etc. From de Graef's notes on the difference between the clocks.
	10	53, 19	359, 56	38, 4		0, 53					
	11	54, 44	2, 16	35, 44		0, 50					
	12	56, 17	5, 0	32, 0		0, 46					
	13	57, 9	6, 48	31, 12		0, 44					
	14	57, 59	8, 8	29, 52		0, 42					July 14 the mean longitude of all the ships was 358 deg. and 21 min.[100]
	15	59, 2	8, 24	29, 36		0, 40					
	16	59, 44	9, 47	28, 13		0, 38					
	17	60, 19	10, 21	27, 39		0, 37					

[100] That is, the longitude of all the ships (at most 13) forming the fleet on the return journey from the Cape.

	I. Days.	II. Latitude from the mariners' journal.	III. Longitude E. of Tenerife from the mariners' journal.	IV. Longitude W. of the Cape according to the mariners.	V. Hours of longitude W. of the Cape by the clocks without correction.	VI. Largest daily delay of the clocks.	VII. Correction of the clocks and of the longitudes.	VIII. Hours of corrected longitude W. of the Cape by the clocks.	IX. Degrees Of corrected longitudes W. of the Cape.	X. Difference of longitude between corrected clocks and mariners.	
		deg. min. N. lat.	deg. min.	deg. min.	h. mi. sec.	min. sec.	min. sec.	h. m. sec.	deg. min.	deg. min.	
[J	18	60, 8	11, 48	26, 12		0, 38					
U	19	60, 8	13, 52	24, 8		0, 38					
L	20	60, 28	14, 52	23, 8		0, 37					
Y]	21	59, 50	14, 36	23, 4		0, 38					
	22	59, 28	14, 36	23, 24		0, 39					
	23	59, 30	14, 55	23, 5		0, 39	To add				
	24	59, 30	15, 18	22, 42	1, 20, 14	0, 39	10, 21	1, 30, 35	22, 39	0, 03[101]	
	25	59, 22	15, 26	22, 34		0, 39					
	26	59, 19	15, 36	22, 24		0, 39					
	27	59, 59	16, 9	21, 51		0, 38					
	28	59, 42	16, 32	21, 28		0, 39					
	29	60, 19	17, 43	20, 17	1, 15, 22	0, 37	5, 3	1, 20, 25	20, 6	0, 11	The morning of July 29, Fulo Island is in sight about 5 or 5 and 1/4 miles east to the south.
	30	59, 54				0, 39					The evening of July 30 the same island is 2 miles E. to the N. of us.
	31	59, 53				0, 39					The morning of the 31st the Orcades [are] 4 miles S. to the W.; in the afternoon Fairhil is 2 miles SE. to the E.
A	1	59, 52	17, 40	20, 20	1, 12, 16	0, 39	1, 54	1, 14, 10	18, 32	1, 48	Aug. 1 the Southern corner of Hitlandt is 2 miles to NE.
U	2	59, 44	17, 40	20, 20		0, 39					
G	3	59, 50	17, 49	20, 11		0, 38					
U	4	59, 4	16, 45	21, 15		0, 40	To Subtract				
S	5	59, 12	16, 40	21, 20	1, 14, 30	0, 40	2, 17	1, 12, 13	18, 3	3, 17	
T	6	58, 53	16, 58	21, 2		0, 40					
	7	58, 41	17, 35	20, 25		0, 41					
	8	58, 11	17, 35	20, 25	1, 17, 25	0, 42	5, 20	1, 12, 5	18, 1	2, 24	
	9	57, 33	17, 52	20, 8	1, 17, 58	0, 43	6, 19	1, 11, 39	17, 55	2, 13	
	10	56, 48	17, 52	20, 8		0, 44					
	11	56, 28	16, 53	21, 7		0, 46					
	12	55, 29	17, 25	20, 35		0, 48					
	13	54, 22	18, 42	19, 18		0, 51					
	14	52, 57				0, 54					Aug. 14 Texel is in sight at 4 o'clock in the late-afternoon.
	15	53, 0		15, 30	1, 8, 8	0, 54	11, 34	0, 56, 34	14, 8	1, 22	Aug 15. Yesterday Kyckduyn was 3 miles east of us; we let ourselves float until the ebb had receded and we put in at Texel.

[101] This number is 0,13 in <u>OCCH</u>, but the manuscripts correctly read 0,03.

Appendix 1: Huygens's Maps

The large map included with the Report in *Oeuvres Complètes*, reproduced in Figure 2 above, contains several curiosities and one remarkable anomaly.[102] The anomaly raises serious questions about whether this map could possibly be the one that actually accompanied the Report to the Directors. Before turning to it, however, we should review the curiosities, for they add to these questions while underscoring how much room for confusion there was at the time about the longitudes of all of the locales pertinent to Huygens's argument.

In the Report Huygens describes the map accompanying the copy sent to the Dutch East India Company as one derived from a "chart of Europe with advancing degrees made by Dirck Rembrandts van Nierop."[103] More specifically, he says that the top part of his map is a copy of this map, but in the bottom portion, below 27 degrees N, he has adjusted the location of Africa so that the Cape of Good Hope is 14 degrees 25 minutes east of Texel. He adds that he has retained Rembrandtz's longitude from Tenerife to Texel, namely 22 degrees, so that the Cape is 36 degrees 25 minutes east of Tenerife, in contrast to 38 degrees on the map used by the mariners in their course estimate and 41 degrees on "ordinary" maps.[104] (The meridian through Tenerife, an island in the Canary Islands just off the coast of Africa, was widely used for 0 longitude at the time; it is in fact 35 degrees 3 minutes east of the Cape.) He then plotted all three courses using the Cape as the reference point for longitude. The first sighted landmark along the voyage was Fulo Island, which Huygens says Rembrandtz's map located around 8 degrees west of Texel, some 3 degrees farther west than Huygens's corrected course implied.[105] (It is in fact 6 degrees 50 minutes west of Texel.)

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102 This map was contained in the back of Vol. 9 in the original edition of OCCH, and in Volume 18 in the more recent softcover edition.
103 OCCH, Vol. 9, p. 283. (In most other places in the Report, the name used is "Rembrandtz" -- the son of Rembrandt.)
104 *Ibid.*
105 *Ibid.*, p. 286.

By contrast, the map in *Oeuvres Complètes* has Texel 15 degrees 18 minutes west the Cape, 53 minutes further west than Huygens says in the Report.[106] On it the Cape is located 36 degrees 50 minutes, and Texel 21 degrees 32 minutes, east of Tenerife. Although Paris is not marked on the map, we have estimated its location from features of the coast of France to be 17 degrees 55 minutes west of the Cape, very close to Tachard's value of 18 degrees. Using this value, Texel is 2 degrees 37 minutes west of Paris, which contrasts with the Riccioli value of 3 degrees 35 minutes that Huygens uses in the text of the Report, yet is close to the La Hire value of 2 degrees 32 or 33 minutes de Volder mentions in his review of the Report. The geography of the map in *Oeuvres Complètes* thus differs from that of the one described in the Report.

What Huygens most likely did in constructing the map reproduced in *Oeuvres Complètes* was to take Rembrandtz's map for Europe and adjust Africa so that the longitude between the Cape and Paris conformed with Tachard's value -- this in contrast to adjusting Africa so that the longitude between the Cape and Texel conformed with the value he had obtained by combining Tachard's value with Riccioli's. Texel is 21 1/2 degrees east of Tenerife, and Fulo 14 degrees east, on Rembrandtz's map.[107]

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[106] These numbers have been obtained by careful scaling from the 76x48 cm map in OCCH. One minute of arc of longitude on this map is 0.1 mm, so that the scaled longitudes cannot be exact to the minute. Our numbers appear to be accurate to within around ±3 minutes. The author of the map had to have worked with at best comparable levels of accuracy, so that the total difference between our scaled numbers in the text and his intentions can surely be as much as 6 minutes, if not more.

[107] Wassende graade Paskaart Vertoonende all Zeekusten van Europa De geheele Middelandsche zee, als oockten Noordwesten, en Noordoosten soo veer als ons tot noch toe bekent is, geteeckent door D. Rembrandtz van Nierop, T'AMSTERDAM by Pieter Goos, op't Water in de vergulde Zee-Spiegel, ca. 1658. A copy of this map can be found in the map collection of the University Library of Amsterdam: Kaartenzl. L.K. VI 4, uit de collectie van het Koninklijk Nederlands Aardrijkskundig Gennootschap. We have dated this map first on the way it fits into Goos's series of charting maps from around 1660 and second on the basis of a notary agreement of July 1658 between Goos and Rembrandtz van Nierop on the terms of the sale of a map (see M.M. Kleerkoper & W.P. van Stockum Jr. *De Boekhandel te Amsterdam vnl. in de 17e Eeuw*. Den Haag, Martinus Nijhoff, 1914-1916, pp. 1275-1276; we were led to to this source by C. Koeman's *Atlantes Neerlandici*, Vol. 4, Amsterdam, Theatrum Orbis Terrarum, 1970, p. 192); the cited map is the only one by Rembrandtz van Nierop that we have found mention of in the literature. For more details, see Eric Schliesser's "Van Nierops paskaart van Europa in een rapport van Christiaan Huygens," *Caert-thresoor*, 16 January 1997, no. 4, pp. 93-95.

Far more disconcerting is the anomalous feature of the map in *Oeuvres Complètes*. As is evident in Figure 6, a blow-up of the last leg of the voyage on it, this map has the August 15 point on Huygens's corrected course to the *west* of the August 15

FIGURE 6 AROUND HERE

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point on the mariners' estimated course. Both the table and the text of the Report have it to the *east*! In other words, the map in *Oeuvres Complètes* contains a glaring error for August 15, an error that is totally at odds with the argument developed in the text of the Report. If the map in *Oeuvres Complètes* is a facsimile of the one Huygens enclosed with the copy of the Report sent to the Directors of the Dutch East India Company, as the editors say it is, then Huygens committed a remarkably clumsy blunder that no one, including de Volder, noticed at the time. Given the other differences noted above, it seems to us far more likely that the map in *Oeuvres Complètes* is not the map that accompanied the Report to the Dutch East India Company.

According to the table, the mariners' course has the Alcmaer at 15 degrees 30 minutes west of the Cape on August 15; the uncorrected course from the clocks, at 17 degrees 2 minutes; and the corrected course, at 14 degrees 8 minutes (14 degrees 1 minute before de Volder's correction[108]). According to the map in *Oeuvres Complètes*, the mariners' course again has the ship at 15 degrees 30 minutes on August 15, while the corrected course has it at 17 degrees 2 minutes and the uncorrected course at 20 degrees 12 minutes. All the other points on all three courses shown on the map in *Oeuvres Complètes* conform with the values listed in the table with one exception: the kink in the courses based on the clocks implied by the table for June 8, which Huygens attributes in the

108 De Volder's correction amounted to either 7 or 8 minutes of arc (to the west) at each point from June 8 on. This difference is on the edge of being too small to detect via scaling on the map in *Oeuvres Complètes*. The entries on the map through August 9 nevertheless appear to us to conform better with the table before de Volder's correction than with the final table. For more details see the "Introduction" to the translation of "Huygens's 1688 Report to the Directors of the Dutch East India Company," (op. cit. Note 3).

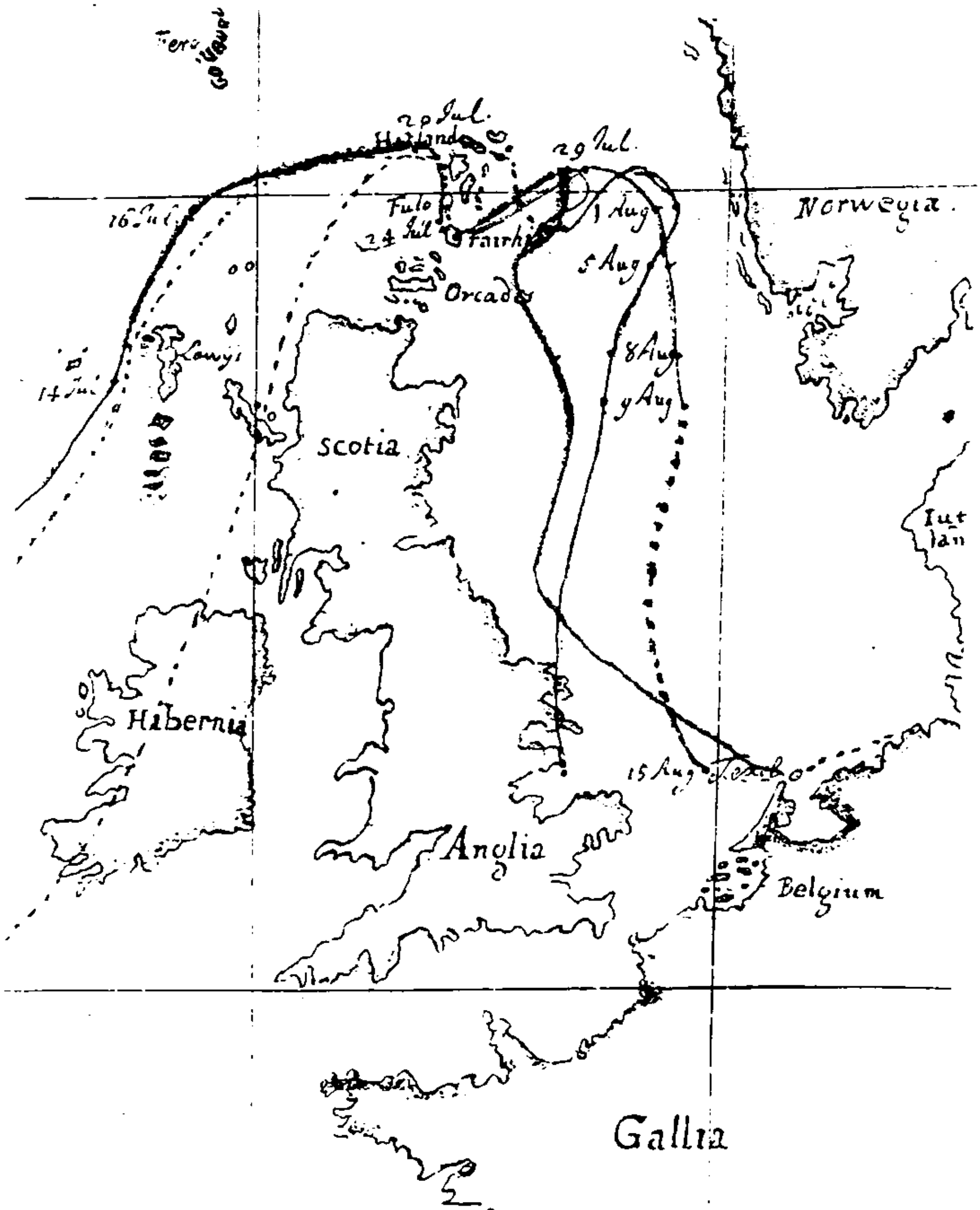

FIGURE 6: THE FINAL LEG OF THE VOYAGE

ACCORDING TO THE MAP IN *OEUVRES COMPLÈTES*

Report to the use of Clock B instead of Clock A on this day, has been smoothed out. So, the only correction needed to bring the courses in line with the table is for August 15. Figure 7 shows the last leg of the map once this correction has been made.

FIGURE 7 AROUND HERE

How could anyone have made such an egregious error? We have found only one plausible explanation. Prior to de Volder's correction, the difference between the mariners' and the corrected course listed in Column X of the table for August 15 was 1 degree 29 minute. Suppose the location of the mariners' course for August 15 was plotted on the map first, and then this difference was used to find the location of the corrected course on the map for that day. Then the error on the map would result if the location of the corrected course was mistakenly marked off to the west, rather than to the east. Using numbers, with which such a mistake would be far more readily noticed, this would amount to mistakenly adding 1 degree 29 minutes to 15 degrees 30 minutes instead of subtracting it; that is, this mistake would locate the final point on the corrected course at 16 degrees 59 minutes west of the Cape, within 3 minutes of the location we scaled off the map. Furthermore, if the final point on the uncorrected course were then located on the map by marking off 3 degrees 1 minute to the west of this final point on the corrected course, the result would be 20 degrees west of the Cape, very close to the 20 degrees 12 minutes we obtained from scaling this point on the map. So, the mistake for August 15 could have resulted if the final locations for the courses based on the clocks were determined by using a compass to mark off the differences in longitude specified in the table from the final point of the mariners' course in two consecutive steps, and the first step was absent-mindedly taken in the wrong direction. So long as the longitudes themselves were not being considered, but only the incremental differences among the three courses, this mistake might go unnoticed. As soon as attention turned to the longitudes,

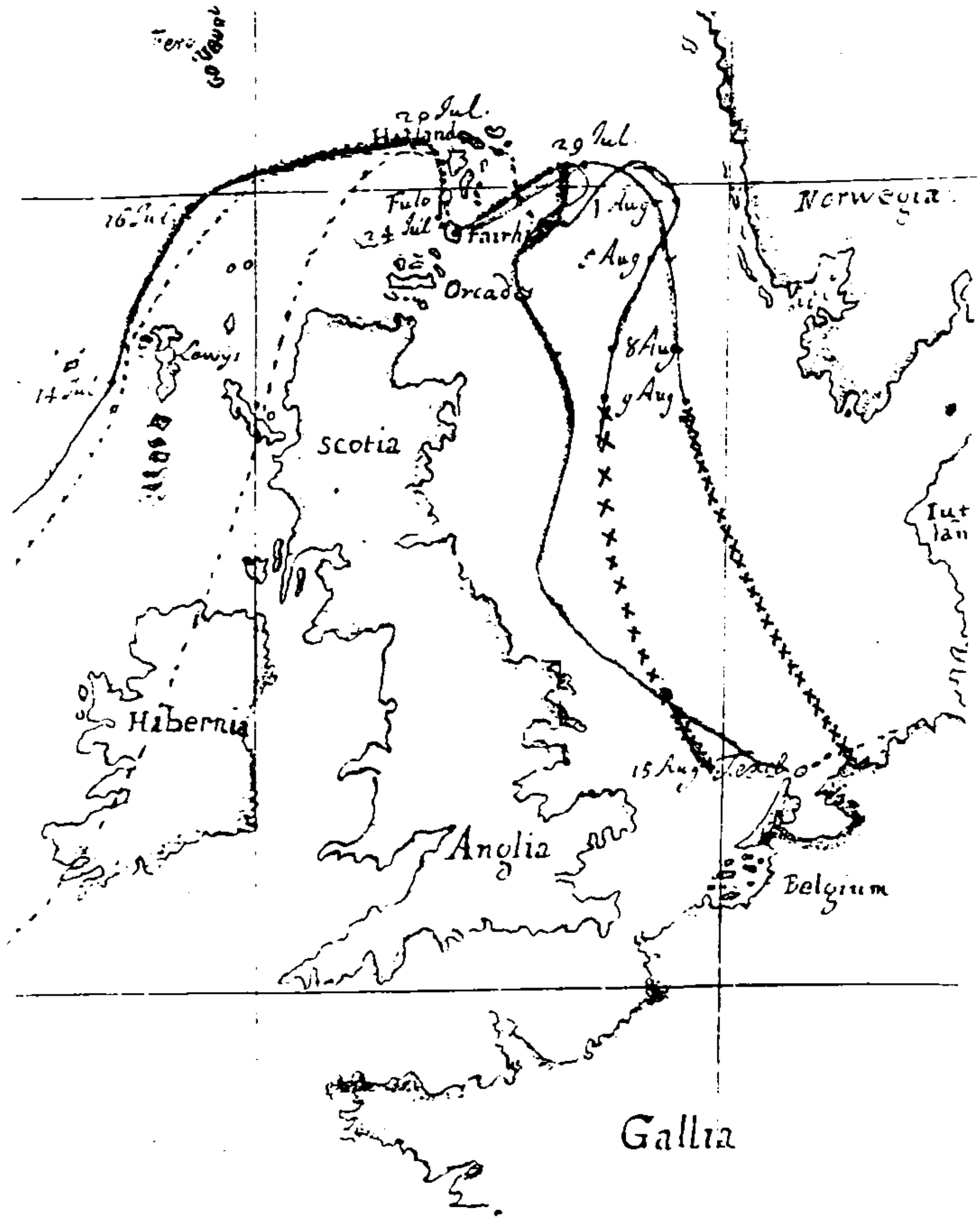

FIGURE 7: THE FINAL LEG OF THE VOYAGE

ACCORDING TO THE TABLE AND THE REPORT

however, the mistake would have become glaring. Consequently, assuming Huygens was responsible for this map, it undoubtedly preceded his formulating the reasoning presented in the Report.

From both the Archives and the Report, we know that Huygens tried several different maps before arriving at the one he sent to the Directors of the Dutch East India Company. One sheet in the part of the Archives containing the manuscript of the Report includes three separate sketches of Fulo and the Shetland Islands, changing their shapes and relative locations. This part of the Archives also contains another map displaying the three courses, shown in Figure 8.[109] On this map the Cape is 41 degrees 40 minutes east of Tenerife, and Texel is roughly 18 degrees 20 minutes west of the Cape and 3 degrees 35 minutes east of Paris. The geography of the land-masses is markedly dissimilar to

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FIGURE 8 AROUND HERE

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that of the map reproduced in Figure 2, especially the coastline of Africa. Also, the three courses are more different from those in Figure 2 than a first glance might suggest. For example, over the last leg of the voyage one of the three courses lies far to the east, ending near Denmark. It is probably the corrected course, since on August 15 it lies very near to 3 degrees east of the course that sits in the middle in the North Sea -- corresponding to the 3 degrees 1 minute calculated correction in arc -- and the western-most course in the North Sea closely resembles the mariners' uncorrected course on the other map. If so, then the mistake that was made in the map in *Oeuvres Complètes* was not made when drawing this map.

A final point to notice about the courses in Figure 8 is how large the discrepancies are between the corrected course and the mariners' course in the Atlantic. Discrepancies

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[109] This map is reproduced in <u>*OCCH*</u>, Vol. 18, p. 640. The original, as well as the sketches of Fulo and the Shetlands, can be found in folder 'Hug 45 aan Oostindische' in the Huygens Archives in Leiden.

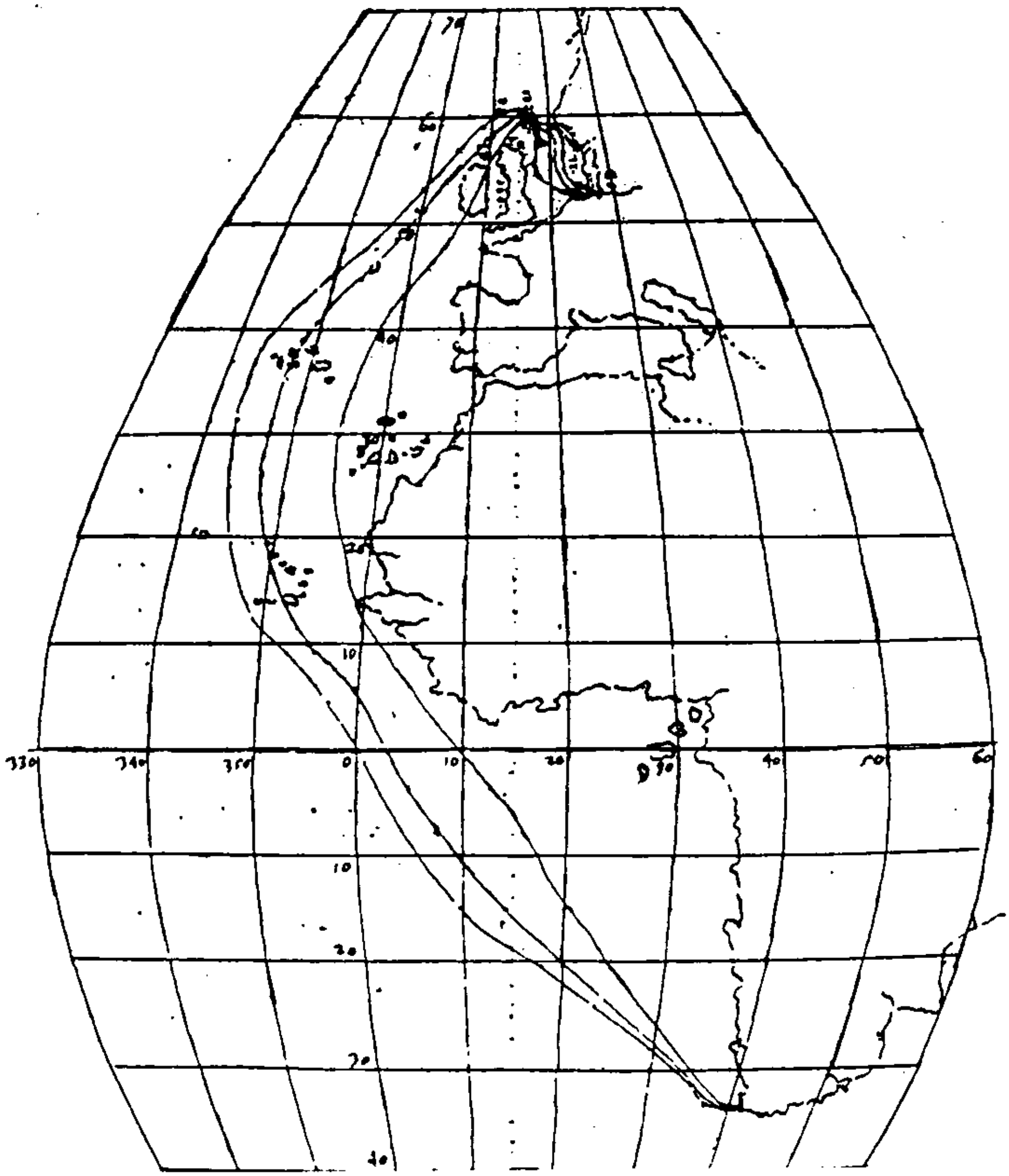

FIGURE 8: THE THREE COURSES DRAWN

ON A DIFFERENT ("ORDINARY") MAP

in excess of 3 degrees are common, and at some points the gap between the two courses appears to be 5 degrees or more. The explanation for the contrast between these large discrepancies and the smaller ones in the table in the Report and in Figure 2 can be found in the calculations on which the courses in Figure 8 were based.[110] These calculations presupposed that the Cape is 41 degrees east of Tenerife; and instead of transforming the longitudes of the mariners' course, which were based on Tenerife as reference point, to ones with the Cape as reference point, the longitudes from the clocks were transformed to ones with Tenerife as reference point. By contrast, in calculating the table in the Report and the courses in Figure 2, Huygens presupposed that the mariners' map had the Cape located not 41 degrees, but 38 degrees east of Tenerife; and he used this number to transform the longitudes in Column III for the course they had estimated to the ones in Column IV with the Cape as reference point. Consequently, the differences between the mariners' course and the corrected course both in Figure 8 and in the calculations in the workbook include an extra 3 degree increment that Huygens subsequently eliminated. This is strong evidence that the map in Figure 8 was prepared early in his analysis of the data from the voyage, most likely before he began experimenting with modified maps.

From what he says in the Report and the cover letter to Hudde,[111] Huygens himself never saw the map employed by the mariners aboard the Alcmaer. His conclusion that the Cape was located 38 degrees east of Tenerife on this map was undoubtedly inferred from the longitudes estimated by the mariners on the first few days of the voyage. The first longitude listed in Column III, two days after setting sail, is 36 degrees 42 minutes. Notice also how the mariners' course appears to begin a few degrees west of the Cape in Figure 8.

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[110] In Workbook F in the Huygens Archives, pp. 167 & 168. So far as we have been able to determine, these are the only extant calculations of courses from the voyage of the Alcmaer. Whether other calculations were done on pages missing from the workbooks -- there clearly are pages missing -- or on pages not in the workbooks to begin with remains an open question.

[111] OCCH, Vol. 9, No. 2519, p. 287 and No. 2517, p. 268.

Both Figure 8 and the reproduction in *Oeuvres Complètes* from which it is taken fail to show the rough outlines of a presumably earlier projection of the world underneath the map. This outline is visible with the naked eye if one examines the original document carefully. This suggests that there may have been a map on which Huygens tried drawing the courses even before the one in Figure 8, as well as maps between that one and the one in Figure 2 and, if we are correct about the latter, one or more after it. We have looked for such other maps, but with no success.

Appendix 2: The Second Sea-Trial[112]

The second sea-trial of Huygens's marine pendulum clocks that de Volder recommended to the Dutch East India Company was carried out in the years immediately following the publication of the *Discourse*, with two clocks leaving from Texel on the Brandenburg[113] for the Cape in December 1690, and returning on the Spierdijck in October 1692.[114] This time de Graaff, who had been made an employee of the Dutch East India Company, was in charge from the start.[115] At best, the trial was less than an unqualified success. De Graaff's report was thoroughly negative, indicating that the combination of the clocks and Huygens's corrections yielded calculated longitudes that veered far off those estimated by the mariners. Even after Huygens had demonstrated that de Graaff's conclusions were based on blunders in misapplying the corrections and that the results, once the blunders were removed, were not all that bad, neither de Volder nor the Dutch East India Company viewed the second trial as corroborating Huygens's claims from the first. In the aftermath of the trial Huygens's own efforts shifted away from pendulum clocks at sea to his "Perfect Marine Balance." All of this raises the question whether the results of the second-trial gave Huygens reason to question whether his correction for non-uniform gravity based on the Earth's rotation alone was sufficient.[116]

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112 This appendix has benefited from extended discussion with John Leopold.

113 The Brandenburg was under command of Captain Evert Verbrugge, see Letter No. 2651, 26 December 1690, "Christiaan Huygens à J. de Graaff," OCCH, Vol. 9, p. 583, and Letter No. 2718, 17 December 1691, "Abraham de Graaff à Christiaan Huygens," OCCH, Vol. 10, p. 206.

114 See Editors's footnote 9 to Letter No 2767, 9 October 1692, "Christiaan Huygens à Ph. de la Hire," OCCH, Vol. 10, p. 323, and see p. 648 of Huygens's commentary on de Graaff's report, OCCH, Vol. 18.

115 See a document taken from the minutes of the Directors of the Dutch East India Company, dated 11 December 1690, OCCH, Vol. 18, p. 545. De Graaff was assisted by a certain Pieter van Laar and by a clockmaker Gilles Meybos. (See Letter No. 2642, "Christiaan Huygens à J. Hudde," dated 14 December 1690, OCCH, Vol. 9, p. 567-568; Letter No. 2649, "A de Graaff à Christiaan Huygens," dated 24 December 1690, OCCH, Vol. 9, p. 581; and Letter No. 2789, "J. de Graaff à Christiaan Huygens," dated 14 February 1693, OCCH, Vol. 10, p. 396-398.)

116 In single brief paragraphs on the second sea-trial, both Michael Mahoney (op. cit. Note 19, p. 259) and John Leopold (op. cit. Note 15, p. 110f) conclude that the results failed to substantiate the promise from the first trial that the clocks offered means for determining longitude at sea. But neither Mahoney nor Leopold discuss whether the second trial in any way undermined Huygens's corrections for non-uniform gravity.

No actions were taken on the second trial to obtain an independent check of Huygens's correction. No seconds-pendulum measurements were made, nor did the trial include land-based measurements of longitude along the course of the voyage. Huygens's recommendation at the end of the Report -- "it would still be very helpful if one investigated the true longitude at some important places with regard to the Meridian of Texel or Amsterdam, by observing the satellites of Jupiter" -- was simply ignored.[117]

Worse, the trial itself was plagued by mishaps. One of the clocks behaved erratically from the outset because of a defective or broken spring, rendering data from it useless.[118] Bad weather prevented the clocks from being calibrated after they were installed on the Brandenburg, so that no meaningful trial of them could be carried out on the leg of the voyage from Texel to St. Jago in the Cape Verde Islands.[119] Hence, the outbound voyage yielded data only from one of the clocks on the leg from St. Jago to the Cape. The return voyage of the clocks was delayed a year because de Graaff became ill at the Cape.[120] (Unfortunately, de Graaff had not been instructed to make observations of the eclipses of the satellites of Jupiter, which could have been used to check Tachard's measurement of the Cape's longitude upon his return to Holland.) Finally, de Graaff failed to fixture the clocks properly when he installed them on the Spierdijck at the Cape, so that the return voyage again provided no meaningful data.[121]

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[117] Huygens complains about this (without assigning blame) in Letter No. 2798, "Christiaan Huygens à B. de Volder," dated 24 March 1693, OCCH, Vol. 10, pp. 433-434.

[118] See OCCH, Vol. 18., p. 643 and pp. 648-649. In Letter No. 2789, "J. de Graaff à Christiaan Huygens," dated 14 February 1693, OCCH, Vol. 10, pp. 396-398, de Graaff writes about the damaged springs of one of the clocks.

[119] On the inability to calibrate the clocks see, Johannes de Graaff's letters to Christian Huygens, Letter No. 2646, 21 December 1690, OCCH, Vol. 9, p. 578, Letter No. 2647, 23 December 1690, p. 579, Letter No. 2650, 26 December 1690, p. 592, Letter No. 2653, 29 December 1690, p. 584. See also Huygens's commentary on de Graaff's report, OCCH, Vol. 18, p. 643.

[120] See Letter No. 2718, 17 December 1691, "A de Graaff à Christiaan Huygens," OCCH, Vol. 10, p. 205, and Letter No. 2720, undated 1691, "J. de Graaff, G. Meybos et P. van Laer aux Directeurs de la Compagnie des Indes," OCCH, Vol. 10, pp 207-208.

[121] See Huygens's comments on de Graaff's journal in OCCH, Vol. 18, pp. 648-649.

Upon returning to Texel, de Graaff issued his highly negative report, claiming that even on the leg from St. Jago to the Cape, where nothing apparent went wrong with one of the clocks, the course inferred from it deviated wildly from the mariners' estimated course.[122] The fact that the clocks were useless on all but this leg reinforced this conclusion.

To Huygens's dismay, he realized when examining de Graaff's papers that de Graaff had misapplied (or simply misunderstood) the instructions on the correction for non-uniform gravity that Huygens had prepared on the basis of the sea-trial with the Alcmaer: de Graaff had added the correction when he should have subtracted it, and he had subtracted it when he should have added it![123] De Graaff had also made a calculation error of 24 seconds that propagated throughout, though, as Huygens notes, this error resulted in a deviation of only 1/10 of a degree.[124] So, Huygens proceeded to recalculate the ship's course.

To defend the accuracy of the recalculated course, Huygens proposed some revisions to the maps the Dutch East India Company was using. His clock indicated that the longitude between St. Jago and the Cape is 48 degrees and 14 min.[125] This accorded well with "the newest maps and globes"[126] by Nicolaas Visscher[127] and Blaeuw.[128] Since

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[122] De Graaff's report is missing (see the Editors's footnote OCCH, Vol. 18, p. 642), but from Huygens's commentary on it (OCCH, Vol. 18, pp. 643-651) we can infer much of its contents. See also Letters No. 2803, "Christiaan Huygens à B. de Volder," [19] April 1693, OCCH, Vol. 10, pp. 443-444, and Letter No. 2773, 16 November 1692, "S. van de Blocquery à Christiaan Huygens," OCCH, Vol. 10, pp. 340-341. In Letter No. 2796, "Christiaan Huygens aux Directeurs de la Compagnie des Indes Orientales," dated 6 March 1993, OCCH, Vol. 10, pp. 423-424, Huygens explains that after hearing from de Graaff he had been so discouraged that initially he did not want to examine de Graaff's papers.

[123] See OCCH, Vol. 18, p. 644; see also Letter No. 2786, "Christiaan Huygens à J. de Graaff," dated 10 February 1693, OCCH, Vol. 10, p. 389.

[124] See OCCH, Vol. 18, p. 644. Huygens notes other small calculation errors (see pp. 644-646), but these are not very significant.

[125] The modern value, using our reference location for the Cape, is 42 degrees 3 minutes, with St. Jago at 23 degrees 35 minutes W and 14 degrees 55 minutes N.

[126] Quoted from Letter No. 2796, OCCH, Vol. 10, p. 424. Here Huygens does not mention the specific maps and globes he used.

[127] This map is mentioned in Letter No. 2800, "B. de Volder a Christiaan Huygens," dated 6 April 1693, OCCH, Vol. 10, pp. 435-436, in Letter No. 2803, OCCH, Vol. 10, p. 443, and in Huygens's commentary on de Graaff's report, OCCH, Vol. 18, p. 646. In the Huygens archive in Leiden, "Hug. 45 Kaarten," one can

Visscher's map placed St. Jago seven degrees west of Tenerife, Huygens concluded that the Cape is 41 degrees 14 minutes east of Tenerife (in contrast to the 36 degrees 25 minutes he had adopted in the Report). The mariners's map put the Cape 38 degrees east of Tenerife, but in their estimated course they had put the Cape 46 degrees and 22 minutes east of Tenerife,[129] giving Huygens some license for arguing that the difference in longitude from Tenerife to the Cape was far from established. Huygens then took the close agreement between the longitude inferred from his clocks and Visscher's value after a journey of two months[130] as evidence for the reliability of his clocks on this leg of the voyage. He thought it very unlikely that a clock would speed up and slow down, yet sum to the same implied longitude that a clock with a regular motion would give.[131]

So, despite de Graaff's discouraging report, Huygens concluded his commentary on it by claiming: "I have thus shown that the clocks have either performed successfully,

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find "*Africae accurata Tabula* ex officina Nic. Visscher" with several courses between St. Jago and the Cape of Good Hope projected onto it. The difference in longitude between St. Jago and the Cape is about 48 degrees on this map. (Why this map was not reproduced in OCCH is unclear.) The map can be attributed to the Amsterdam map-maker Nicolaas Visscher II (1649-1702). (We have been unable to comfirm the claim by the Editors of OCCH that the map was published at Van Waesberge; see their footnote on Letter No. 2800, OCCH, Vol. 10, p. 435.) The Visscher map was probably produced between 1666-1690. The information on which the Visscher map is based goes back to at least the 1660s because an exact copy of this made by a different mapmaker, J. van Meurs, dates from 1670. For a discussion and reproduction of the map in the Huygens archives, see Eric Schliesser "Een Kaart van Visscher en de Grote Globe van Blaeuw in de Bewijsvoering van een Proef met Slingeruurwerken van Huygens," Caert-Thresoor, 19:2, (2000), pp. 51-55.

128 In OCCH the Globe is alluded to in Letter No. 2803, OCCH, Vol. 10, p. 443. In a footnote the OCCH's Editors incorrectly assumed that Huygens was referring to Blaeuw's map of Africa in his famous atlas. Joella Yoder has called our attention to an unpublished document in the Huygens Archives, "Hug. 50.III"; this document contains Huygens's tables of corrections to the mariners's estimates and the data as collected by de Graaff on his clocks on the voyage between St. Jago and the Cape. At the end of the table Huygens wrote, "My reckoning of 18 deg. 14 min. accords very well with the map the Large Globe of Blaeuw." The information in Blaeuw's hugely popular Large Globe was last updated between 1645-1648, but no changes to the representation of the South Atlantic were made after it first appeared in 1617; see pp. 29-30 of Tony Campbell's "A Descriptive Census of Willem Blaeuw's sixty-eight Centimetre Globes," Imago Mundi, 28, (1976), pp. 21-50; see also p. 515 of Peter van der Krogt's *Globi Neerlandici: The production of globes in the Low Countries*, Utrecht: Hes Publishers, 1993. Peter van der Krogt has enabled us to measure the longitude between St. Jago and the Cape on one of the Large Globes; it measured slightly less than 49 degrees.

129 See OCCH, Vol.18, pp. 646-647.

130 The Brandenburg had departed St. Jago on 2 April 1690, and had arrived at the Cape on June 3 (OCCH, Vol. 18, p. 643-644).

131 OCCH, Vol. 18, p. 647.

or were not enabled to do so."[132] In a letter to the Directors of the Dutch East India Company he maintained that "they [the clocks] have succeeded very accurately and precisely in the measurement of Longitude, namely, on the outbound journey from the island St. Jago to the Cape of Good Hope."[133]

Moreover, near the end of his commentary on de Graaff's report, Huygens also addressed the question whether the only correction needed is one based solely on the rotation of the Earth:

> One should also consider that by the clean result of the longitude measurement between St. Jago and the Cape the new correction to the motion of the clocks resulting from the rotation of the Earth was confirmed for the second time now; earlier I had shown through a demonstration based on the Laws of Mechanics that this had to be the case. In the journey of 1687 this correction has shown itself very probable, too. So, [given] that the longitude measurement with clocks that are driven by a weight, or that have pendulums attached, will be tested for some time to come, one should now hold that it is necessary for this correction to be born in mind and used. For, if one does not use it, one can err many degrees of longitude.
>
> In the same fashion is the calculation of the Equation of Time because of the Sun's orbit; through this and prior voyages it has been convincingly proven that it has been formulated properly -- about this less could be doubted.[134]

Recall that in the Addition to the *Discourse* Huygens had announced that a forthcoming second sea-trial would provide further evidence on "the soundness" of his "discovery" that the only correction for non-uniform surface gravity required is one based

132 OCCH, Vol. 18, p. 649.

133 See letter No. 2796, "Christiaan Huygens aux Directeurs de la Compagnie des Indes Orientales," dated 6 March 1693, OCCH, Vol. 10, pp. 423-424; see also, Letter No. 2795, "Christiaan Huygens à S. van de Blocquery," dated 6 March 1693, OCCH, Vol. 10, pp. 422-423.

134 OCCH, Vol. 18, pp. 649-650. The full voyage from Texel to the Cape would have provided a better test of Huygens's correction *versus the alternatives to it* than the leg from St. Jago to the Cape. So long as the test turns on the difference in longitude between the endpoints of a voyage, then the larger the total cumulative correction, the greater the contrast among the alternative rules of correction. In the leg from St. Jago, which is located at 14 degrees 55 minutes N latitude, the net daily corrections -- i.e., in Huygens's way of computing the corrections, the largest daily delay at the ship's latitude minus the largest daily delay at the point of origin -- changed sign once the Brandenburg crossed 14 degrees 55 minutes S, just as they had changed sign when the Alcmaer crossed 35 degrees N latitude. By contrast, the net daily corrections would have been all of the same sign in the full voyage from Texel to the Cape.

on the Earth's rotation alone. His commentary on de Graaff's report indicates that Huygens had no doubt that the results of the second sea-trial had confirmed his conclusions from the first sea-trial. Huygens's statement even echoes the language of the Addition by pointing to the close agreement between his theoretical argument and the empirical results from the sea-trials. We have nevertheless found little evidence that Huygens attempted to communicate this further result to the learned community.[135]

De Volder, who was again the referee, was not swayed by Huygens's somewhat *ad hoc* line of reasoning. In particular, he pointed out that in the Report of 1688 Huygens had relied on the agreement between his clocks and Tachard's measurement of the longitude of the Cape, but in assessing the accuracy of his clocks on the voyage of the Brandenburg Huygens ignored Tachard's results and switched to Visscher's map to locate the Cape.[136] Although Huygens's argument in the Report of 1688 had depended only on the longitudinal distance between the Cape and Texel and was independent of the convention of setting 0 degrees at Tenerife, he had nevertheless implied that the Cape is 36 degrees 25 minutes east of Tenerife; whereas in his commentary on de Graaff's report he had inferred a longitude for the Cape of 41 degrees 14 minutes east of Tenerife.[137] Huygens was thus not being consistent in locating the Cape relative to the 0 degree reference longitude of Tenerife.

In his reply to de Volder, Huygens agreed that the maps were inconsistent and, in effect, argued that none of the maps could be relied on to show accurately where St. Jago is located because no one had yet fixed its position by observing the satellites of Jupiter.[138] (Huygens could have gone on to say that, as Tachard was silent on the location of St. Jago,

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135 There is mention of the sea-trials in Letter No. 2886, "Christiaan Huygens à [E. Bartholinus]," [1694], OCCH, Vol. 10, p. 701.

136 See de Volder's Letter No. 2800, dated 6 April 1693, OCCH, Vol. 10, pp. 435-436.

137 In Letter No. 2800, OCCH, Vol. 10, p. 436, de Volder claims that Tachard put the Cape at 40 ½ degrees from the island of Ferro (also known as Hiero), which is about 2 degrees west of Tenerife, so that the difference between the Cape and Tenerife is "a bit more than 38 degrees which appears to accord with the maps of the [Dutch East India] Company."

138 See Huygens's Letter No. 2803, [19] April 1693, "Christiaan Huygens à B. de Volder," OCCH, Vol. 10, p. 443. Huygens anticipated this point in his commentary on de Graaff's report; see OCCH, Vol. 18, p. 647.

he himself had tried the next best thing: he chose to use the most reliable map he could find.) He also pointed out that possible landmarks during the voyage were out of sight.[139] Huygens also insisted "that the longitude between St. Jago and the Cape of Good Hope has been reckoned by the clock to be very close to 48 degrees, and this accords very well with the Maps."[140] In the next paragraphs of his final letter on the second sea-trial Huygens nonetheless conceded that this did not prove his clocks performed well: "....I do not want to pretend that one could conclude from this or based on the previous trial of Anno. 1687 that the perfection [of my method of] Longitude-measurement has been demonstrated conclusively...."[141] One suspects (as John Leopold first suggested to us) that Huygens was in part being strategic in this remark, not wanting to expend too much capital defending the clocks used on the two sea-trials, for he wanted to start promoting his new design, the Perfect Marine Balance. Some support for this conjecture can be found in various remarks Huygens made in his correspondence.[142] Whatever the reasons for the concession, however, Huygens was adament that de Volder help correct the bad impression de Graaff's report had made on the Directors of the Dutch East India Company.[143]

Using pendulum clocks to determine longitude at sea turned on two entirely separate issues: (1) the proper correction for the variation in surface gravity with latitude; and (2) consistently reliable performance of the clocks themselves. The earlier quotation from his commentary on de Graaff's report indicates that Huygens saw nothing in the data from the second sea-trial to cause him to question the accuracy of his correction for the

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139 We can infer from Huygens's letter to de Volder that de Volder had made some suggestions how to do this -- see Letter No. 2798, "Christiaan Huygens à B. de Volder," 24 March 1693, OCCH, Vol. 10, p 433. In a footnote the Editors of the OCCH admit they are not sure what took place. We, however, have found in the Huygens Archives at the University Library of Leiden in a folder, "Hug. 45 Kaarten" the map Huygens used to project the course of the second trial. He put the ship very close (about 2 degrees) to the South American coast even though no land had been sighted. The corrected course with the clock deviated 8 2/3 degrees from that estimated by the mariners.

140 See Letter No. 2803, [19] April 1693, "Christiaan Huygens à B. de Volder," OCCH, Vol. 10, p. 443.

141 *Ibid.*

142 See his own footnote to Letter No. 2795, "Christiaan Huygens à S. van de Blocquery," 6 March 1693, OCCH, Vol. 10, pp. 422-23, and the concluding lines of his Letter to de Volder No. 2803, op. cit. Note 122.

143 Letter No. 2803, Vol. 10, pp. 443-444.

variation in gravity. He nevertheless had to see reasons for questioning the likelihood of his marine pendulum clocks performing *consistently* at sea. Again in the second trial one of the two clocks had proved totally useless, and even the good clock had come to a stop during the leg of the voyage from Texel to St. Jago (though not from St. Jago to the Cape). Problems with springs had arisen on both trials, and de Graaff's failure to mount the clocks properly at the Cape attested once again to the sensitivity of their performance to their mounting. Huygens remarked in one of his manuscripts, "One of the great defects of the clock with a suspended pendulum on a boat is that the force of the pendulum causes a small movement in the whole clock and this [varies] more or less according to the freedom of the axles of the iron frame, from which arises the inequality of the hours."[144] We know from his workbooks that Huygens put a great deal of effort into developing his Perfect Marine Balance during 1693 and 1694.[145] That he did so was entirely appropriate, given the performance of the pendulum clocks in the two trials. Nonetheless, it does not indicate that he had lost confidence in his correction for non-uniform gravity.

At the end of the first paragraph to the Addition to the *Discourse*, Huygens proposes that marine pendulum clocks would provide a better way than seconds-pendulum measurements for establishing the variation of surface gravity. Whatever else may be said, the second sea-trial should have shown him that this was not true, for questions about whether any such clock had performed well would invariably be a complicating factor. A pendulum clock carefully calibrated at one location and then, properly crated, carried to another might well be used to measure differences in surface gravity. Richer had shown as much on his expedition to Cayenne even though he had ended up relying on the seconds-pendulum for

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144 OCCH, Vol. 18, p. 569.

145 See the manuscripts reprinted in OCCH, Vol. 18, pp. 546-596. The new marine clock is also mentioned in Huygens's last surviving letter, No. 2891, "Christiaan Huygens à Constantijn Huygens, frère," dated 4 March 1695, OCCH, Vol. 10, pp. 708-710. See Leopold's article, op. cit. Note 15, pp. 111ff and also his "Christiaan Huygens and His Instrument Makers" in *Studies on Christiaan Huygens*, pp. 231, for more information on the Perfect Marine Balance. Huygens died before this clock could be tested at sea.

his measure. This way of using a pendulum clock, however, amounts to virtually the same thing as using the seconds-pendulum. Marine pendulum clocks simply could not be expected to give, "by their acceleration and deceleration, ... an average more reliable than actually measuring the length of the seconds-pendulum in different countries." Measuring the lengths of pendulums remained the preferred device for determining local gravity in geodesic reseach until at least the 1950s.[146]

[146] See Chapter 4, "Gravity Measurements," in W. A. Heiskanen and F. A. Vening Meinesz, *The Earth and Its Gravity Field* (New York: McGraw-Hill, 1958.