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astronomers, geometricians and geodesists

ASTRONOMERS, GEOMETRICIANS AND GEODESISTS

Although the planet we inhabit has been the object of scientific attention since classical antiquity, it was not until the end of the 17th century that estimates of its size began to be expressed in terms approaching its true dimensions. This evolution paralleled the colonization of new territories and the ensuing increase in trade. It is not surprising that the first estimates of the Earth's size were made during the Hellenic expansion, nor that from the time of Ptolemy until the end of the 6th century it was chiefly Arab geographers who took an interest in the subject. But the rulers of every era were interested in knowing and measuring the extent of their dominions, and this factor undoubtedly provided a powerful stimulus to the advancement of a body of knowledge inevitably linked to power and projects of economic and military domination.

During the Scientific Revolution the renewal of old methods of observation and the advent of new measuring instruments made the birth of a science of geography whose greater precision was akin to the growing requisites created by overseas territorial discoveries and the new scale of trade in the global economic system. From the body of knowledge that constituted the geography of the humanists, a new specialty began to emerge: scientific geography. Sheltered by the prestige of astronomy and mathematics, it began to achieve ever-increasing levels of precision and autonomy. This new discipline, always with academic pretensions and never far removed from technical field laboratories, in line with any campaign of military expansion, soon redefined its scientific objectives around a program of delimiting borders and frontiers.

The consolidation of state and municipal bureaucracies required territorial intervention policies, which in turn contributed to the development of precise methods for drawing charts and maps. Of course the observations made were qualitatively and quantitatively different depending on the end being pursued; the degree of specialization varied according to whether the goal was to define a special zone subject to fiscal or administrative services, trace the direction of a communications link, or locate towns and villages, sources of raw materials or trade routes on a map. Consequently, only those states whose greater complexity required more sophisticated techniques of territorial intervention were in need of fostering a certain level of institutionalized geographical knowledge, within which geodesics and nautical astronomy preeminently obtained the highest laurels of experimental and geometric sciences. By the middle of the 18th century, the geographers who --equal in stature to mathematicians, physicists and botanists-- prestiged academies and published their research in established scientific reviews, were geodesists who scorned the Renaissance geographical tradition with disdain. One of the tasks of this chapter will be to analyze the remarkable progress geodesics underwent in just fifty years.

But in addition to those factors already mentioned, the development of geodesics required the presence of an impetus that stimulated theoretical and experimental research. Geography and astronomy would once again become involved in a polemic with scientific, ideological and social repercussions which would occupy the attention of European scientists to an enormous degree during the third and fourth decades of the 18th century. In fact, the question of the Earth's shape was the theme around which a sort of geodesic experiment was designed over which a new scientific discipline could be articulated. Defining a new piece of reality in theoretical terms and observations demands the concurrence of contributions from diverse fields, ranging from celestial mechanics and the physics of fluids to the perfection of practical technologies in the construction of instruments or in methods for evaluating experimental results and errors of measurement. Tangible modifications in the institutional structure of science, which made it possible to at least attempt if not actually carry out these projects, accompanied all these advances. Here we will discuss only the accelerated transition from a desktop activity that was barely professionalized and still followed nobiliary or conventual traditions to one that demanded the design and financing of costly scientific expeditions, transforming their predecessors' role of *savant* into the more modern status of *scientifique*. The history and antecedents of this process will be the subject of the pages which follow.

The Dimensions of the Planet

Measuring the longitude of a meridian terrestrial arc consists basically of determining the distance on a maximum circle that separates two points with a known difference in latitude. The cosine between the linear and angular distances gives the value of a degree which, assuming the Earth to be spherical, allows one to calculate the size of the planet. This operation, theoretically so easy to describe and understand, can in practice result highly complex, depending on the level of precision to which one aspires. As we shall see, it is a matter of knowing the trajectory of a meridian and fixing two points sufficiently distant (determining the latitude of two positions), before measuring the length that separates them. Until the 17th century this operation was never carried out with sufficient accuracy due to deficiencies in the methods and instruments of scientific observation as well as the absence of a consistent standard of measurement. However, a number of estimates were made before the 17th century with a rudimentary nature which not only reveals considerable ingenuity but also permits us to consider the main difficulties that the geodesists of the Enlightenment had be resolve.

Although Aristotle provided the first exact figure, proposing a length of 224,644 meters for the degree of the meridian, it was not until Eratosthenes that our story really begins[1]. Assuming the Earth is round, since otherwise the stars of the firmament would rise over the horizon at the same time at every point on the planet, contradicting everyday experience, it was possible to determine differences in latitude without having to express their value in absolute terms. Eratosthenes' method is still described in textbooks of the history of astronomy as one of the most brilliant milestones of ancient times. Let us reconstruct it once more in all its simplicity. Knowing that the bottom of a given well in Aman was illuminated by sunlight at twelve o'clock noon during the summer solstice, Eratosthenes verified that at that same moment the sun was observed in Alexandria at an angle of 7°12'. Knowing the distance between the two cities and assuming the road connecting them followed the direction of the meridian, he obtained a degree of about 700 furlongs, or approximately 140,840 meters. Note that then, as now, the unit of length known as a furlong was so imprecisely defined that its conversion into meters barely conveys an expression of magnitude, much less of comparative or absolute value.

According to Cleomedes, Posidonius observed a century later that the star Canopus, invisible in Greece, appears for a moment on the horizon in Rodas, while on the same day in Alexandria it rises to ¼ of a sign or 7°30'. Like his predecessor, he simplified the problem of distance by assuming that both points were on the same meridian[2]. The resulting estimate of 666.66 furlongs as the value of a degree was later corrected through new astronomical observations and reduced to 500 furlongs. This last figure is undoubtedly the result of rounding off, which made calculations easier and, of course, remained within the broad twenty-five percent margin of error existing between the values of the first determinations and those finally adopted as more accurate. However, in a "world" reduced to the eastern Mediterranean, more precise figures were not indispensable, and on such a small scale these variations did not exaggerate cartographic errors.

Given the markedly theoretical or speculative character of these estimates, Ptolemy adopted Posidonius' value, although he thought of a more universal procedure for obtaining degrees, less dependent on the astronomical or local peculiarities of a few exceptional viewing points. In fact, in his Geography Ptolemy proposes the angular difference of the meridian altitude of a star in two different places located on the same meridian as a method of determination. In the end, this would be the method used systematically throughout the modern world. Before its appearance, little was done except the attempt by Abul-Abbai-Abdallah-Al-Malmun in the 8th century. To measure the length of a degree, he counted the steps a man could walk during the time it took for the sun to "change its position" one degree[3]. Although more universal than the procedures of Eratosthenes and Posidonius, and more practical than that described by Ptolemy, the fundamental defect of this method was that of generalizing results obtained over a short distance to the whole maximum circle. Thus, although the number of steps was known with precision, a small error in the distance covered had large repercussions on the value arrived at for the arc of the meridian, a figure which, moreover, came out close to that already given by Posidonius. It is fair to say that the innovation came from the new emphasis placed on determining longitude and adopting --however approximately-- a standard of measurement. These are key points which would occupy a great part of the efforts made to measure a degree of latitude up to the end of the 17th century.

In fact, the gist of the method described by Fernel in his Cosmotheoria (1528) is surprisingly similar to what had been carried out by the Arab mathematicians. Knowing the latitudes of Paris and Amiens, cities which he supposed were on the same meridian, Fernel followed the route connecting them by wagon until the sun had changed its position by a degree. By carefully measuring the perimeter of one of the wagon wheels and counting its revolutions, he hazarded a figure of 57,020 toesas (approximately 111,230 metres), forever rememberable for its precision[4]. It was not only luck that accompanied this enterprise; the measuring standard was also much more precise. Fate and cleverness made good partners for the French physician.

Nevertheless, the technical resources of the epoch did not allow for more precise results. We can see this when we recall Snel's minor failure which occurred despite the use of a more theoretically perfect procedure. In his Eratostenes Batavus (1617), the first known manual on geodesics, Snel explains the operations undertaken to triangulate the distance between the cities of Alcmaer and Berg-op-Zoom, which yielded a degree measuring 55,020 toesas[5]. The method, designed by Gemma Frisius in 1573, had previously been used for the modest needs of urban planning and architecture, but never to measure large distances where the precise determination of the fundamental base, the exact knowledge of the unit of measurement, and the correct choice of intermediate points are such decisive factors in the final outcome. Thus, for example, Snel chose too small a base of only 168 toesas. This affected the choice of the first triangulation points, which could only with difficulty be seen at angles greater than 15°. The imprecision of the instruments of the time combined with bad luck ruined the results of the first geodesic project worthy of being called scientific. Thus, although the method was the most apt, its implementation required technical measures and theoretical resources that would only become available a century later. Meanwhile, solely the attempts of Norwood and Riccioli described in their Seamaris Practice (1637) and Geographie et hidrographie reformatae (1661), respectively, are worthy of mention here. The former determined a degree of 57,424 toesas between the cities of London and York, obtained after correcting his observations to account for errors due to the inevitable orographic irregularities in terrain. This was a notable contribution which perfected the methods known until then, as well as considerably complicating the practice of geodesic triangulation.

By the second half of the 1600s, a pressing demand for accurate cartography had arose. Geography, considered by Renaissance humanists as an empirical and aristocratic art, underwent a change in its epistemological status to the point of approximating experimental and geometrical sciences. Riccioli, the Italian Jesuit, took great heed of this matter. Convinced that subsequent to Snel's work the greatest difficulty in geodesic operations lay in the astronomical side of the observations, he strove to systematize existing methods of determining latitude. However, the imprecision of the instruments forced him to use a procedure which did not require celestial observations. The method, never revived again for practical use, yielded a measurement of 64,363 paces from Bologna or, to use the official French unit of measurement, 62,900 and 62,650 toesas according to J. Picard and G. D. Cassini respectively. This 250 toesas discrepancy between two prestigious astronomers of the Paris Academy of Sciences is an excellent example of the importance given at the end of the century to determining a standard of measurement and matching it to standards used by scientific communities in other nations. Greater needs for precision, soon followed by the appearance of new measuring devices, demanded a mobilization that would allow results to be exchanged and compared.

In 1669, at Colbert's behest, J. Picard began the triangulation work that would lead years later to the completion of the map of France. G. D. Cassini's original idea of developing a program to bring the new long-range astronomical instruments up to date was diverted towards a geodesic objective. Thus the astronomer's academic interests and the minister's political ones came together in a single specific project which enjoyed no shortage of institutional support and vital financial backing. Picard's operations, a synthesis published in La Mesure de la terre (1671) of everything that had been done until then, constituted a model that would be imitated for decades to come[6]. Even more than the results, the precision of which amazed all the astronomers of the 1700's, one must admire the accurate use made of the first quarter circles provided with telescope and micrometer, and the large astronomical sector with a ten-foot radius. These innovations, together with the thoroughness with which the base of 5,663 toesas was determined and with which a standard of length was wrought in iron, constituted a small revolution in astronomical techniques, the effects of which soon became patent. The thirteen triangles used to find the distance of 68,430.5 toesas along the arc of meridian of 1°11'57" contained between Malvoisine and Sourdom, towns near Paris and Amiens, permitted the finding of a degree with an established length of 57,060 toesas. Late 17th century astronomers now not only had an extraordinarily precise figure --Newton himself would use it in his calculations-- but also techniques and instruments well suited for proceeding to transform the practice of geography. After the publication of Picard's works, nothing stood in the way --at least in theoretical terms-- of drawing regional or local maps as closely to reality as desired. At a time when the noisy echoes of the cosmological polemics had scarcely died down, astronomy, which had contributed so much to the downfall of the classical paradigms of knowledge, invaded the realms of reality reserved to other more empirical disciplines, contributing observations with considerably more precision.

The instruments of the period, however, could never assure an error of less than 4" in determining a degree of latitude. In reality, as we shall see later, Picard's evaluation was overly optimistic, since the observations of latitude presented a dispersion of values greater than 10". Nevertheless, it was always possible to arrive at a happy compensation of errors by taking the difference between the two extremes of the arc into account and postulating a result, as the Paris academician did, who thus admitted an uncertainty of only sixty toesas in his final figure. This is how things took place, such as the error stemming from his astronomical observations that was compensated for during the geodesic phase of the mission when Picard adopted as a unit of measure a toesa one mil shorter than the toesa on deposit in the Academy. The artisanal techniques of instrument building still did not permit the construction of an exact replica of a fixed standard, much less its division into equal parts. It is hardly necessary to mention, therefore, that in addition to the errors introduced by the observer, there still were important flaws in the construction of astronomical equipment. Because of this, practical astronomy was a field in which the theoretical manuals were still on the horizon. It was still an age with a varied collection of empirical observation techniques and laundering of results, methods holding aspects closer to sleight-of-hand and personal hunches than the fixed rules required by the intersubjective communication of scientific results. All this was known and soon the best artisans working in glass and metal alloys would be needed in the Academies to replace scientists like Galileo, Huygens, Torricelli and others who built their own instruments. In La mesure de la terre, Picard himself recognized the utility of widening the triangulated arc of the meridian so that errors in the determination of latitude would have a much smaller effect on the value calculated for the degree. This recommendation made the project of extending geodesic operations southwards, as far as Collioure, possible. These new observations, however, posed problems and produced results the significance of which will be analyzed below. For the moment, it is enough to point out that until the publication of Newton's Principia the Earth was considered to be a solid sphere with a fairly accurately-known radius.

Leaving aside for the moment a description of the advances made in geodesics, let us proceed to examine another of the great observation programs that contributed scientific knowledge about the physical properties of our planet. Huygens' research on the cycloid, the movement of pendulums and centrifugal force, made possible experiments designed to demonstrate the variation of gravity and latitude. After a number of unsuccessful attempts by Richer in North America in 1670, an expedition to Cayenne was planned from which the Paris Academy of Sciences hoped to obtain precise data on the solar parallax, the obliquity of the ecliptic, astronomical refraction in the tropics and the length of a pendulum that marks seconds. Richer himself conducted this extensive observation program between 1672 and 1673, earning unconditional praise from his academic colleagues and from Newton himself[7]. For the Parisian institution the undertaking was as innovative as it was important. The results Richer set out to obtain clearedly answered key astronomical questions that had long been pursued by the European scientific community: some of the most important outcomes of the expedition included nothing less than the dimensions of the solar system, the elaboration of solar declination tables and the empirical verification of the theoretical model postulated by G. D. Cassini for refraction. However, the observations that would make Richer famous belonged to a less spectacular part of his mission, that is, the measurements of the length of the hourly pendulum.

The repercussions hoped for by the Academy explain the expectant atmosphere surrounding Richer's arrival. "We waited for Mr. Richer's return," relates Fontenelle, Permanent Secretary of the prestigious institution, "as if awaiting the verdict of a judge who had to decide on important difficulties that divided astronomers. One could say that the Academy was in suspense when Mr. Richer came back from Cayenne. Since he had very precise observations, made tirelessly for over a year, of all that had passed before his astronomer's eye, what arrived at the Academy was a ship laden with all the riches of America." [8]

In fact, Huygens' predictions on the variation of the length of the pendulum as regards latitude were fully confirmed. Academic expectations were fulfilled, although the interpretation of these results, as well as of those made by other astronomers in different parts of the world, corresponded to Newton. The experiment, the results of which are presented in Table [9], was very simple, consisting basically of determining the length of a pendulum that would beat seconds between two successive solar culminations. That is, assuming that the number of oscillations varies according to the intensity of centrifugal force due to the rotation of the earth, one would have to construct a pendulum that made 86,400 cycles per day.

AUTHOR	PLACE	YEAR	LAT.	RESULT	PREV. NEWTON	DIFF.
Richer	Paris	1673	40°50′	440.60	440.56	-0.04
Varin	Paris	1682	48°50′	440.56	440.56	0.00
Picard	Paris	1671	48°50′	440.50	440.56	0.06
Chazelles	Cairo	169..	30°2′	440.25	439.95	-0.30
Des Hayes	S.Domingo	1699-1700	19°48′	439.00	439.70	0.70
Des Hayes	S.Cristob.	1699-1700	17°19′	438.75	439.64	0.98
Des Hayes	Guadalupe	1682	15°00′	438.50	439.60	1.10
Des Hayes	Martinique	1682	14°44′	438.50	439.60	1.10
Feuillé	Martinique	1704	14°44′	437.83	439.60	1.77
Des Hayes	Gorée	1682	14°40′	438.56	439.60	1.04
Feuillé	Portobelo	1704	9°33′	437.58	439.53	1.95
Richer	Cayenne	1672	4°55′	439.35	439.48	0.13
Des Hayes	Cayenne	1699-1700	4°55′	438.50	439.48	0.98

Up to this point everything appears easy. The problem was, once again, putting it into practice. It will suffice to highlight the difficulties that, although simple, were nevertheless decisive. The first arose from the uncertainty that surrounded any determination of a longitude. Newton had predicted a maximum variation at 90° latitude of 2.2 lines for the length of the pendulum, while the absolute error of measurement approached 0.25 lines. The second problem arose from the fact that when comparing the movement of two pendulums, it is difficult to determine the exact oscillation at which one advances past the other, since during a certain period of time everything happens as if they were "ticking" together. To these difficulties we may add the inherent imprecision of the astronomical observations made during the experiment.

In sum, although the experiments carried out confirmed the hypothesis of the variation of gravity and latitude, the disagreement and dispersion of results among different observers justified cautious skepticism. In fact, Newton only dared to draw conclusion of importance in the variation of the isochronic pendulum. This, of course, was before the spectacular development at the end of the century of instruments and techniques of astronomical geographic observation permitted the accumulation of a fertile body of knowledge and the isolation of a well-defined set of problems that awaited solution. The dimensions of the planet were known and the observations with the pendulum showed a variation of gravity with latitude. Would this have any repercussions on the shape of the Earth? If its surface were subject to a variable "pressure," perhaps its equilibrium should be sought in a shape other than a sphere. However, as we have seen, even experimental science was far from offering conclusive data. The first answers to this question would appear with the theoretical development of Newtonian and Cartesian projects.

A Rotating Fluid Spheroid

The birth of a new scientific discipline requires more than merely accumulating observations. Together with a sustained level of social and academic demand, it must have a challenge of theoretical creation that can successfully be taken on by the scientific community. Since ancient times, the law of falling bodies compared to the stability and harmony of the heavens consitituted an enigma which various cosmological principles proposed to solve. At the end of the 17th century, the Aristotelian conceptional scheme of physics and cosmology had been replaced by the Copernican hypothesis of helio-centrism, Cartesian ether, the elliptical path of planetary orbits and ample empirical evidence. Several cosmological programs competed during the 1600's, including Descartes' excessively mechanistic one and the pan-vitalist program of Kepler, to cite only the two most extreme examples. All had to yield to Newton's proposals. Naturally, as is well known, this was a very complex process, which for the most part lies outside the scope of this study. For our purposes, it is enough to recognize the influence the principle of universal gravitation had on the problem of the shape of the Earth. With the publication of the Philosophiae Naturalis Principia Mathematica (1689), a theory about the shape of the planet came into being, which, while not exempt from important theoretical difficulties, required the subsequent attention of numerous scientists to finish the task of establishing a substantiated foundation for Newton's ideas. The generalization of this labor together with his complete works is known in specialized literature as the Newtonian program for physics. We shall proceed to expound on the ideas contained in Principia and the work accomplished by men like Maclauring, Stirling, Simpson, Taylor, Clairaut, Maupertuis and Euler.

Propositions 18, 19 and 20 of Book III deal with the the shape of the planet. Very briefly, Newton's line of argument is begun in Proposition 18, where the observations of Flamsteed and G. D. Cassini about the shape of Jupiter are extended to the Earth. If these astronomers had been able to determine polar flattening experimentally, Newton thought that an acceptable theory of fluids must be capable of explaining this phenomenon in mathematical terms. If, in addition, the laws of physics were universally valid, there was no theoretical reason to prevent treating all the planets in the solar system in the same way, Earth included.

Proposition 19 proves the necessary geometrical relation between the axes of the geoid. Thus, in the way Newton wished to present his ideas, he did not seem to find anything surprising about the fact that the earth is not a perfect sphere. Mere allusion to observations published by well-known astronomers is not a convincing enough argument, since nowhere is it demonstrated that the ellipse is a form of stable equilibrium. This would be one of the problems resolved later by Newtonian scientists of the 18th century. Here, however, we must highlight this analogical extrapolation from celestial mechanics to the physics of fluids, as from the epistemological point of view it is notable that again something that since Kepler's time had only been demonstrated for planetary movement was seen as "natural." But this is not the only daring hypothesis on which the Newtonian demonstration is built. The consideration of the planet as a mass of fluid in rotation was not very respectful of the most basic experiences of ordinary intellect, which were accustomed by common sense and popular speech to associate very different words to speak about liquids than the ones he used. In addition, the conceptual limitations of the mechanics of the period imposed burdens that prevented thinking of the term "fluid" without explicitly imagining masses of channeled water. Indeed, in order to calculate the relation between the axes of the geoid, Newton thought up a theoretical resort that has endured throughout the history of physics[10].

Assume a mass of fluid crossed by two imaginary canals that extend from the center to the polar and equatorial extremes, respectively. If this mass is at rest, the length of both must be the same, and consequently its figure is spherical. But if, on the contrary, a movement of rotation around the polar axis is introduced, the action of centrifugal force will provoke a diminution of weight in the equatorial column which will alter the original state of equilibrium in which the mass of fluid found itself. Newton declared that if we assumed it to be in stable equilibrium, as everyday experience indicates, we would have to admit a lengthening of the equatorial axis which would reestablish the equality of weight between the two columns. Once the theoretical appropriateness of the polar flattening had been established, the problem consisted in calculating its value, and for this it was necessary to first find a way of analytically expressing the attracting forces that act on the pole and the equator on the earth's surface. The mathematical scope of this question went far beyond the limits of late 16th century science, and forced Newton to exercise his brilliant perspicacity in order to find an approximate solution. His description will not be examined here since the frequently confused nature of his geometric reasoning (the analytic methods were not yet exact) would make the reading of these pages too difficult. The conclusion reached by Newton fixed the ratio between the axes of the terrestrial spheroid at 230/229.

Newton did not trust the values of meridian degrees obtained by Norwood, Snel, Riccioli or Fernel to obtain an experimental confirmation of his conclusions. Given the uncertainty surrounding the results, this lack of confidence was fully justified. Thus, using only pendulum observations, which were more numerous and precise as well as done at markedly different latitudes, it was possible to verify experimentally his ideas about the shape of the earth. Proposition 20 in Book III of <u>Principia</u> is devoted to comparing his theoretical predictions with the data supplied by Richer, Halley, Des Hayes, Picard and Couplet. Although, as we have seen, the dispersion of the findings was considerable, Newton thought he could infer a polar flattening of the planet from them, but determining its exact magnitude would require new experimental observations and more forceful theoretical elaborations that took the Earth's internal structure into account. "And this disagreement," Newton affirmed, "may come in part from the error in observations, from the inequality of the internal parts of the planet and from the height of the mountains; and in part from the different temperature of the air." [11]

The question could only be considered partially resolved, since the hypothesis of a homogeneous distribution of masses seemed too restrictive and in need of revision. Also, the mathematical resources of the epoch precluded solving equations that were the result of introducing further complex laws on the variation of density. But, even assuming its integration to be viable, the density would be impossible to detect experimentally because, as Huygens had proved, the movement of the pendulum was independent of the internal distribution of mass. These considerations, while not challenging the main issue (the non-spherical shape of the planet), left a large number of questions unanswered. The most important affected the meager development of observation techniques and the nearly total ignorance of how to treat error in a series of observations. The methods with the pendulum, although the principal empirical support for Newton's conclusions, would later be relegated to a secondary status with respect to geodesic observations aimed at measuring degrees. Even so, Newton pointed out the convenience of undertaking a research program in geophysics and of placing more emphasis on the local physical features where the experiments were carried out. In particular he pointed out as priorities the evaluation of variations in gravity with altitude, the distortion in the movement of the pendulum due to the presence of mountains, and the influence of changes in temperature on the expansion or refraction of matter.

But before embarking on the study of any of the experimental problems indicated by Newton, we will spend some time, as promised, on his theoretical contributions. Consider the theory postulated by Huygens[12]. His interest in the question of the shape of the Earth is a direct consequence of his rejection of the Newtonian conception of gravity. While research on the pendulum had put him in a position to confront this problem, he treated the topic most extensively in Discours sur la cause de la pesanteur (1690), a text hastily published in order to reply to the content of the recently published Principia. "I do not agree," Huygens explained, "with the Principle that all the parts one can imagine in two or more different bodies attract or tend to draw towards each other mutually. I cannot accept it, since I believe I can clearly see that the cause of such attraction is not explained at all by any principle of Mechanics, nor by the rules of motion. Nor am I persuaded of the necessity of mutual attraction between whole bodies, having proved that, even if there were no Earth, bodies would still tend towards a center, and this is called *pesanteur*." [13]

The text is clearly situated in the general Cartesian scheme of explaining the universe, where gravity, far from being an external force that acts on bodies, is the *connatus* or tendency they experience in the direction of a center. Huygens imagined a subtle fluid mass that revolved "enclosed" in a limited space from which the celestial bodies could not escape. Denser objects which did not participate in the movement of the cosmic ether or which simply were slower in their motion, would be pushed by the ethereal matter towards the center of the rotating system. "This is what the gravity of bodies truly consists of. One may say that gravity is the force that makes the fluid matter revolve circularly around the center of the Earth in all directions, and pushes those bodies which do not follow this movement away from the center and into their place." [14]

Thus, the simultaneous attraction of all material particles to one another, inversely proportional to the square of the distance that separates them, is replaced by a constant mechanical action that would exist even if the Earth did not[15].

This hypothesis about gravity provided a different answer for the flattening of the pole. Again considering the two Newtonian canals which cross the terrestrial fluid mass, it is evident that all the particles situated in the interior of the equatorial canal will experience the same connatus of fall towards the center, generated by the "pressure" of the surrounding ether. But, given that the Earth's rotation makes the *vi centrifuga* vary according to the distance of the surface from the axis of rotation, an alteration would be produced in the initial static equilibrium which Huygens assumed would be reestablished if the result of both "forces" at each point is perpendicular to the earth's surface. This is a condition of hydrostatic equilibrium without which the exterior particles would be drawn by a tangential component of the force that, contrary to everyday experience, would permit the detection of a slipping of matter from the equator towards the pole. After applying a simple algebraic calculation, this principle, known as the verticality of the plumb, made it possible to demonstrate that the relation between the axes was half of what existed between the forces acting on the equator. Thus, given that fc/fg = 1/289, the equatorial axis had to be 1/578 times longer than the polar axis.

Both theories agree, then, on the ellipsoidal character of the shape of the planet, although there is a discrepancy in the magnitude of the polar flattening. The important point, however, was the different perspective from which the same problem was approached. Even more important, the small numerical difference between the two methods seemed for the moment impossible to discern experimentally. Furthermore, although the theoretical discrepancies were basic, their effect on geographical practice was irrelevant because the flattening of the pole was so small. "...it seems as if the difference of the degrees," Newton concluded, "is so small that the figure of the Earth for geographical purposes can be considered spherical; especially if the Earth is a little less dense near the equatorial plane than at the poles."[16]

The 17th century, then, was nearing an end without any solid empirical support for the conclusions of Newton and Huygens, and with no possibility of designing a test to decide between the two contradictory theories. The topic of the shape of the Earth, which no one expected to have any repercussions of a practical nature, was nothing more than a purely collateral aspect in the development of Newtonian and Cartesian theory. In sum, everything pointed to the likelihood that the discrepancy would be forgotten and would never be more than a new, albeit marginal, aspect in the fundamental conflict which would soon engage the partisans of the two very different conceptions of the physical world.

Later events would however prove to the contrary. In the first place, there still remained various theoretical problems whose study would prove highly relevant to the foundations of fluid mechanics and the development of Newtonian physics. And secondly, the carrying out of geodesic measurements proved to be a magnificent testing bench for refining many experimental techniques and instruments of observation. Regarding the first issue, Newton's final conclusions left several questions up in the air, questions he was in fact unable to answer. Is the Earth homogeneous? Is the ellipse a figure in equilibrium for a mass of fluid that revolves around an axis? Why consider *a priori* that the equator was an axis of symmetry with respect to the parallel circles? If the Earth was strewn with irregular geographical features, to what extent was it correct to speak of an ellipsoidal figure and what reality underlay the mathematical calculations?

The first great contribution to discuss in this brief summary of theoretical results is that of P. Bouguer. The Parisian scholar wondered if the two principles of hydrostatic equilibrium used by Newton and Huygens were alternatives or mutually exclusive. Once solved, the problem could have important implications as it allowed one to differentiate, at least theoretically, between the Newtonian and Cartesian perspectives. His conclusion, however, supported both, since in hydrostatic equilibrium one had to satisfy simultaneously the Newtonian principle of the counterbalance of the columns and the principle of the verticality of the plumb proposed by Huygens. "...a planet considered as fluid can constantly maintain the same shape when all the columns --of which it can be supposed to be formed and which lead to a center-- have equal gravity; without this the columns would not counterbalance and the heavier ones would lift up the lighter ones from underneath. But it is also necessary to satisfy another condition: it is necessary that the directions of gravity be exactly perpendicular at all points on the surface, so that the molecules of fluid will not have any slope on which to roll towards one side or another. The satisfaction of these two laws is equally necessary; one assures repose in the interior, while the other establishes it on the exterior; and only the presence of both leave the planet in an ever permanent state." [17]

Bouguer's contribution, along with additional research by Maupertuis and Stirling which proved that an ellipsoid could be a shape with stable equilibrium[18], were produced in a context in which his sympathies for Cartesian physics were not concealed. This, undoubtedly, was not the best letter of introduction to the Royal Society nor to the "new geometricians," who were already decidedly advocating Newtonianism in Paris. On the other hand, the presentation of his conclusions in non-analytical terms, restricted to the problem of the shape of the Earth, left the way open for research of greater mathematical impact and more ambitious theorizing. Clairaut, after a series of studies published in the Philosophical Transactions and the Mémoires de l'Academie between 1737 and 1740, made public the general condition of hydrostatic equilibrium in his Theorie de la figure de la Terre (Paris, 1743). Earlier, in A treatise of fluxions (1732), Maclaurin had synthesized and developed a decade of research in which D. Bernoulli, Euler, Simpson and Hermann were the leading participants, in addition to the scientists already mentioned. Among other questions of great importance, they had managed to find an analytic procedure for calculating the force of attraction on the surface of an ellipsoid, and methods for an approximate solution through an advancement in the differential equation series of a secondary nature implicated in the problem. By 1738, Clairaut proposed a condition of equilibrium which, using Bouguer's ideas, was innovative in being expressed in analytical terms[19]. However, its theoretical scope was limited since he assumed that the Earth was formed by a mass of fluid constructed of similar concentric layers of variable density. This hypothesis, while it helped to simplify the problem of the shape of the Earth by considering it in macroscopic terms, constituted a major conceptual obstacle towards obtaining a local and univerally applicable condition of equilibrium.

"With my theory," Clairaut wrote to Euler in January 1742, "I have recognized that I was mistaken in Philosophical Transactions when I determined the shape of the Earth by assuming that it was composed of similar elliptical layers of different density. What led me to the error was the very memoir by Bouguer I was just telling you about, because I was satisfied to see that the columns were in equilibrium and that the tendency was perpendicular to the surface. Having better examined this problem, I have found that the layers could not be the same, but became larger and larger the further they were from the center, so that the densest parts are the ones closest to the center." [20]

A few days later Euler hastened to indicate his agreement with the new orientation postulated by Clairaut: "The principle you are using is, without doubt, that a fluid mass cannot be in equilibrium except when each molecule is equally compressed everywhere." [21]

And, in fact, Euler had understood completely. Clairaut finished the edition of the general principle of hydrostatic equilibrium in its definitive formula: "A mass of fluid cannot be in equilibrium except when the forces of all the parts contained in a channel of any shape which crosses it cancel each other out." [22]

Analytically, F being the attractive force and r the distance to the center of attraction, it was equivalent to requiring that $F.dr$ be a "complete differential."

The Theorie de la Figure de la Terre concluded half a century of theoretical and experimental study on the shape of our planet. It is impossible to summarize here the scope of these achievements. We will only point out that the polemic was settled in favor of the Newtonian thesis and established --whatever the structure of the Earth-- two limits, maximum and minimum, for the polar flattening. These limits were 1/230 and 1/577, respectively corresponding to the hypothetical assumptions of a uniform distribution of mass or of its concentration around the center of the system [23]. We can also see that these extreme values were identical to those arrived at by Newton and Huygens half a century earlier.

The Birth of a Polemic

By the end of the 17th century, the first critiques of Newton and Huygens' theories on the figure of the Earth surfaced. J. G. Eisenschmid, an astronomer from Strasbourg, comparing the results obtained by Snel, Picard, Riccioli and Eratosthenes, concluded that our planet was flattened at the equator. In his Diatribe de figure telluris elliptico-sphaeroide (Strasbourg, 1691), he concludes --contrary to Newton's view-- that the observations of the hourly pendulum were less precise than those derived from measuring meridian arcs. The figures he used were as follows:

Author	Place	Length
Snel	Holland	55020
Picard	France	57060
Riccioli	Italy	62900
Eratosthenes	Egypt	63000

Although in his opinion the simple consideration of this table was enough to confirm the increase in degrees from the pole to the equator, he also attempted a theoretical justification. This explication found its way into the work of Thomas Burnet, a member of a group of English naturalists and geographers known for their persistent effort to reconcile the first contributions to natural sciences produced during the Scientific Revolution with religious dogma, especially with the text of the Old Testament[24]. For Burnet the Earth had to be oblong. His reasoning in Telluris theoria sacra (London, 1681) was simple. The action of centrifugal force, which was greater at the equator than at the pole, exerted a "force" on the Earth's surface matter, tending to pull it away from the center. The surrounding air, on the other hand, exerted a "pressure" on the Earth which, in addition to insuring equilibrium, prevented any free movement that did not occur tangentially in the equator-pole direction. Initially, during the first great phase of Earth's history, both its perfectly flat surface and its shape and interior were in a state of "natural" equilibrium and perfection. The constant action of the Sun and the other planets, Burnet continued, provoked a catastrophe of the magnitude of the Great Flood, which gave rise to the formation of mountains and other geographic irregularities. This marked the beginning of a second phase in which, among other effects, centrifugal forces moved large quantities of matter from the equator towards the pole until the planet assumed the shape of a spheroid flattened at the equator.

Independently of the theological connotations of this scheme --famous for the harsh rebuttals it provoked from Beaumont, Warren and Keill in the Newtonian camp-- for Eisenschmid the argument was compatible not only with experimental data but also with Cartesian philosophy. His arguments, which according to Duhamel de Monceau were heard at the 1696 Academic assemblies, did not possess, however, either the theoretical impact of Huygens' proposals or the theoretical support of empirical data, since the figures given by Snel and Eratosthenes could not be considered sufficiently precise. Thus, around the turn of the century, neither Burnet nor Eisenschmid enjoyed enough prestige for their opinions to be influential beyond the circle of their own adherents.

In 1683 Cassini obtained the backing he needed to extend the Paris-Amiens meridian southwards to the city of Collioure. The analysis of Picard's operations, as we have seen, suggested enlarging the triangulated distance in order to avoid potential, perhaps decisive, errors committed in the determination of latitude at the ends of the arc. The following year, when the town of Bourges had already been reached, Colbert died and his successor, less interested in sponsoring scientific endeavors, withdrew financial support from the project. In spite of this, Cassini was determined to verify the degrees measured up to that point. Accompanied by his son, he went to Italy in 1694, where he had an opportunity to participate in a number of important geodesic operations, especially those relating to reconstructing the meridian that P. Riccioli had described in Bologna. This trip and those he made to the Netherlands and England in 1697 and 1698 allowed him to carry out experiments with long-radius instruments (16 feet 3 inches 8 lines) equipped with micrometer and astronomical lens. Of the many topics touched upon in his memoirs, at least one deserves commentary. In attempting to reconstruct the meridians already described, he found it impossible to compare the results due to the diversity and imprecision of knowledge about the equivalence between the different systems of measurement. Ahead of his age by several decades, he proposed the definition of a standard unit to be of general use among scientists. The geometric fathom is thus defined as "...the ten millionth part of the semi-diameter of the Earth."[25]

Reference to this suggestion has not been found, however, even within the small social group for which it was written. This item of information is presented here as proof of the existence of early interest in the subject according to **geometric** criteria.

In 1700, seventeen years after the geodesic work was suspended, a new petition to the King of France met with a positive response and the project was renewed. The long series of operations that had been carried out in different phases and with different immediate objectives would lead to the drawing of a map of France. In the same year G. D. Cassini, his son Jacques, Maraldi, Couplet and Chazelles headed south to extend the meridian towards the city of Collioure. They obtained an average degree of 57,097 toesas on an arc with an amplitude of 8°30'. The length of the eight triangulated degrees diminished from south to north, and this led Cassini to the conclusion that his work confirmed the "modern hypotheses" of Huygens and Newton. Fontenelle (Permanent Secretary of the Paris Academy of Sciences, succeeded by Grandjean de Fouchy in 1740) made the same mistake in interpretation when he summarized the memoir's conclusions in these words: "Assuming that this decrease in the value of a terrestrial degree is systematic between the equator and the pole (...) the Earth is a globe flattened at its poles." [26]

This inference was of course based on a serious error which would soon be corrected. Nevertheless, it is important to call attention here to the fact that neither the Permanent Secretary of the Academy nor its most prestigious astronomer showed any reservations about accepting the Newtonian thesis. This illustrates the narrow attitude of the French scientific community, excluding figures like Huygens, when it came to important consequences deriving from an acceptance of Principia. The phenomenon was common all over Europe and confirms a reality that did not escape Voltaire's wit: "Newton's philosophy has seemed to many as unintelligible as the wisdom of the Ancients; but the obscurity of the Greeks is due to the fact that they had no insight, whereas Newton's mists come from the fact that his insight is too distant from our eyes. He found truths, but he sought for and found them in an abyss. It is necessary to go down into it and bring them up to the light of day."[27]

In fact, the structure of the work was so complex that the first reactions came several years after its publication. In 1713, a new memoir by Cassini completely rectified his earlier conclusions. Comparing the degrees measured to the south of Paris with the Paris-Amiens degree triangulated by Picard, he confirmed that the decrease of the degrees from the equator to the pole "...is characteristic of an ellipse, whose greatest diameter represents the axis of the Earth and whose smallest diameter represents the axis of the equator. Revolving around the larger axis, this ellipse forms, by its revolution, a spheroid whose poles are the ends of the larger axis and whose equator and parallels are represented by circles. This is the shape we attribute to the Earth." [28]

According to his calculations, the Earth would have the shape of a revolving ellipsoid with an eccentricity of 1/11 and a difference between the polar and equatorial axes of 1/262, which, as we can see, is similar to that proposed by Newton and double that of Huygens. The crucial point to emphasize here is that Cassini based his conclusions on data drawn from experience rather than theoretical assumptions. This is particularly noteworthy since the thesis of an oblong Earth rested exclusively on geodesic measurements with results which contradicted those obtained in experiments carried out with the hourly pendulum.

If we consider Fontenelle's position as representative of the majority opinion in the Academy, the following words have striking significance. "The arguments derived from the different length of the pendulum," said Fontenelle in 1713, "in different climates or the unequal centrifugal force that results from the daily movement of the Earth, are perhaps too subtle to produce a certain conviction; one can not even be very sure of the principles, and the consequences might then be different." [29]

Although his commentary on the influence of temperature on experiments with the pendulum was reasonable, we cannot fail to be surprised by such a partial and shallow judgement of the theoretical principles that Newton and Huygens used to defend a thesis completely opposite the one held by the French astronomers. Given that the 56,960 toesas of the degree to the north of Paris had been found by measuring an arc of only 1°22', it seems obvious that it was necessary to extend the meridian to Dunkirk in order to confirm the previous conclusions. In reality, despite the prestige of the astronomers who had participated in the operations, the difference of 37 toesas that could have been produced by a small maladjustment of the instrument or simply an individual error by an observer was not a sufficient argument to change the shape of the Earth from a "watermelon" to that of a "melon." Thus in 1718, the Duke of Orleans authorized the continuation of the project. La Hire, Maraldi and Cassini himself covered the distance between Paris and Dunkirk within the year and, with 28 triangles and an arc of 2°12', found an average degree of 56,960 toesas. The degree previously measured by Picard between Paris and Amiens of 57,060 toesas was reduced with the new observations to 57,030 toesas. The memoir that presents the results ends with the calculation of an eccentricity of 1/7 and a ratio between the axes with a value of 96/95 [30]. Although slightly more deformed, the shape of the Earth was still a spheroid flattened at the equator.

In 1722 J. Cassini published <u>De la Grandeur et Figure de la Terre</u>, an important synthesis of everything that had been done on the subject in France between 1683 and 1718. Note that it was published in the Academy's new collection <u>Suite des Memoires de l'Academie des Sciences pour 1718</u>, and that it was not distributed until 1722 although dated 1720. The fact that this book inaugurates a new series of official publications, the minor disarray with the dates and the emphatic manner in which Fontenelle presented a preview of its contents in 1721, are all indications of the degree of anticipation its conclusions had awakened in the European scientific community[31]. If the memoir of 1713 aroused doubt on the part of some French academicians, the fresh experimental confirmation which came out of the geodesic operations of 1718 was widely accepted. The reliability of the scientists who had participated throughout the entire series of observations was beyond all suspicion. In addition to the Cassinis, the real driving force behind the entire project, Picard, La Hire and Maraldi also took part. Any European Academy would have been proud to include any of them as active members.

The conclusions of their works were based on studies developed over two decades, and the prestige of the institution itself was implicated. Furthermore, the publication of the Grandeur et Figure de la Terre altered the terms of the initial debate established between Huygens and Newton. It was no longer a matter of comparing supposedly different theories, but rather of choosing between the explanatory power of a mathematical theory and the results derived from a carefully conducted systematic program of observation. From the Paris Academy of Sciences, Cassini's conclusions were presented as a paradigmatic model of doing science. Who could argue against the strength of the data? Or, to put it another way, how could English science, so firmly tied to the empirical method of knowledge and so critical of Cartesian philosophy, react to the experimental successes of French astronomers and geographers? For the science of the first half of the 1700's, this debate, while posed in the above terms, was enormously fruitful since it questioned problems until then poorly understood.

With respect to the theme that occupies us, a single example will suffice to clarify what we want to say. In order to measure the length of an arc of the meridian, it was enough to *imagine* the possibility of situating a long line of wooden stakes set into the ground that would mark the maximum circle, and to determine the distance separating them. However, the irregularities of the terrain --rivers, mountains, buildings-- made it impossible to carry out in practice. This rudimentary and "logical" procedure had to be replaced by the more sophisticated method of triangulation. For Cassini and all those who helped him to conduct the geodesic operations, the two methods were equivalent. However, this assumption is only true if the Earth has a spherical shape. Clairaut wrote in 1733, "To extend the perpendicular, with one of the sides already given, one takes points outside this line with which one constructs, using trigonometric operations, the prolongation of the perpendicular; these operations which were believed to be equivalent to those of the pivots and which conserve the line in the vertical plane, however, constantly remove it from this plane; this is what I want to show in the

Memoir, demonstrating that only with the hypothesis of the spherical Earth is the line thus drawn always on the same plane, and that on any other it is a curve of double curvature, whose nature and properties I will give, through a very simple procedure, deducing if the Earth is elongated or shortened and in what ratio, assuming that the Earth is of elliptical nature". [32]

Independent of the modifications that Clairaut's memoir introduced in Cassini's data, what caused the greatest impact on the Academy's astronomers was the justified need of adopting a hypothesis about the shape of the Earth before beginning the observations. It is hardly necessary to stress the profound epistemological significance contained in this conclusion.

Let us again take up the thread of our chronological description. In subsequent years, new contributions by other scientists created sufficient motivation to force an institutional commitment by the Academy to the work of some of its members. There were memoirs of minor importance such as that of Jacques de Roubaix[33], but the work of Delisle and Mairan merit further discussion.

J. N. Delisle was an influential member of the European scientific community, known for his tireless work in promoting astronomical research projects. A teacher of G. de Fouchy, Louville and L. Godin, he was one of the first to openly support Cassini's theses. In 1720 he presented a memoir to Abbot Bignon, who succeeded Colbert and preceded Maurepas as a zealous protector of the sciences. In it he proposed measuring a parallel arc that would definitively confirm the thesis of the oblong earth. "All the consequences and determinations that M. Cassini has deduced for the shape and magnitude of the Earth can pass uncontested up to the precision with which it has been possible to assure the exact amount of the inequality, given that, in addition, the other suppositions Mr. Cassini has been obliged to realize are equally true; thus it is clear that it is not possible to conclude from what has been observed something that has not been observed at all, unless one assumes that one knows the relation between the one and the other, and that, therefore, only through hypothesis can one conclude the magnitude and the shape of the whole Earth having measured a part of it." [34]

The topic was considered important and indicative of the great knowledge the new science could awaken. The climate was such that even the smallest contribution could guarantee its author a place in posterity. We believe that one must interpret Delisle's project from this double perspective. According to him, measuring one meridian was not enough to draw conclusions about all the maximum circles of the planet. His specific proposal was to triangulate the parallel that passes through Paris and joins the cities of St. Malo and Strasbourg. Contrary to popular belief, he thought this operation could be realized with only 1" of error in the time, in other words 15" of the arc. His predictions were so optimistic that no one even argued with them, since it was nearly impossible to determine longitude with an error of less than 30", which would have required the triangulation of a distance four times greater than the distance already known along the north-south direction.

Delisle's text may also contain a veiled, discreet criticism of the haste with which Cassini's thesis had been accepted. The truth is that the unanimous applause of the academy members still seems excessive. Looking more closely at the results of the observations, the south of Paris, on an arc of 6°19', obtained an average degree of 57,097 toesas; to the north, on an arc of 2°12', this value was 56,960 toesas. Thus a difference of 137 toesas --3.7 times greater than the difference reached in 1713-- permitted the drawing of conclusions whose hasty nature could not have escaped such learned company. In 1733, commenting on the precision with which one could perform astronomical observations, Fontenelle wrote that "...the most skilled observers are in agreement that one can barely be accurate within ten seconds." [35]

Recalling that the degree between Paris and Dunkirk was determined with an arc of 2°12' and that two observations were necessary, one at each end, it becomes apparent that, accepting Fontenelle's statement as valid, the possible error could be more than 150 toesas. If to this we add the 35 or 40 toesas predicted in the degree between Paris and Collioure, we must conclude that the inaccuracy of Cassini's data must have aroused some skepticism among the members of the Academy. Here is the scholarly opinion of J. B. J. Delambre, hired at the end of the century by the National Assembly to direct the geodesic commission that triangulated the same extension of the Paris meridian: "It seems to me," said Delambre, "that if a similar work had been presented to the Academy by a scholar who was an outsider to this body, the commissioners, in their benevolent impartiality, would have been able to applaud the magnitude and importance of the work, and to praise the author for whatever specific corrections he might have made in the geography of France, and for establishing the fundamentals of a map highly superior to those that existed previously. They would have been able to add that the average degree among the eight and a half that had been measured must give a probable value of the degree of spherical France, that the work had been justly entitled Grandeur de la Terre, and they had found it even better than expected, since this magnitude of the average degree, reduced to our actual toesa, is very approximately that which results from our operation --he refers to the geodesic work at the turn of the century--; but, with thanks to the author, they would have advised him to omit the word *Figure* from the title, because it is clear that the small differences shown between the degrees of the north and those of the south do not exceed the possible and probable errors in observation. Recognizing that the measurement would have been made with all the care that Geography could at that time require, they would have added that this measurement was too imperfect to establish the shape of the Earth." [36]

Although Delambre, in his contributions to the history of physics and mathematics, perpetually argued with the authors who preceded him, acting as a self-appointed authority over them and introducing constant value judgements and notable distortions of historical reality, the commentary we have quoted seems, at least on this occasion, justified. A more accurate understanding of the actual structure of the Academy, institutionally conceived as a scientific tribunal directly dependent on the Maison du Roi, would have allowed him to understand the reasons for such a closed-minded commitment to the opinions defended by one of its most influential members. R. Taton recognized this when, studying this overlapping of diverse interests, he stated with thoughtful prudence, "His immediate successors," referring to G. D. Cassini, "however, defended this hypothesis with a certain obstinacy." [37]

In this context, the memoir published by Mairan in 1722 was highly significant, since its objective was to integrate Cassini's results with the explanatory ideal of Cartesian philosophy. Taking as his premise the question of what ought rightly to be the object of analysis, he wrote, "...the problem is to find the true shape of the Earth through direct observations. From its present shape one can deduce its primitive shape and from that the primitive directions of gravity, which we could study without their having been changed and, to put in one way, concealed by centrifugal force." [38]

In essence, this hypothesis was equivalent to assuming that the results of the geodesic operations were precise, so as to later try to offer a theoretical justification of the "facts." Mairan was so committed to Cartesianism that he had no qualms about introducing new theoretical elements that could never be experimentally verified. In brief, his reasoning is as follows: the Earth originally was much more oblong than it is today, with centrifugal action being responsible for the gradual decrease of its eccentricity: In his opinion, since gravity, in addition to being directed towards the center, must also be perpendicular to the surface, our planet must have been formed by layers of variable density which, through the effect of successive "refractions," permitted the simultaneous verification of both conditions on the *directrice de la pesanteur*. So that this *pesanteur* would not contradict the results obtained with the hourly pendulum, he declared that the law that regulates its behavior must be inversely proportional to the product of the two principal radii of curvature. Mairan's memoir, although it is an *ad hoc* hypothesis, managed to explain, in addition to the variation in degrees of the meridian, the thornier topic of the experiments on the length of the pendulum. In integrating it into the general scheme of neo-Cartesian interpretation, he placed the question of the shape of the Earth at the center of the Newton v. Descartes debate.

Once the results of Cassini's and Richer's observations were interpreted theoretically, the Paris Academy of Sciences wanted to present the issue of the shape of the Earth as a decisive test between two rival theories. With the prestige of the Cartesian vortices reestablished, ardent defenders of the French scientific tradition soon made their appearance: Fontenelle, Bragelogne, Louville, Molières, D'Anville, Childrey, Longhansen, Manfredi, Castel.... Speaking for all of them, Fontenelle wrote in 1729: "The annual movement of the Planets, without exception always moving from West to East, is one of the most solid proofs of Descartes' vortices. Nothing is more natural, nor better conforms to precise reasoning, than to conceive that this direction is common to all the Planets because it is that of a great fluid that revolves around a center and carries them all along." [39]

With respect to the impossibility of experimentally verifying Mairan's thesis on the complex internal structure our planet must have, Fontenelle proposes a clearly inadequate explanation: "M. Mairan's theory assumes that the inequality of gravity in the two hemispheres derives from their distance to the Sun." [40]

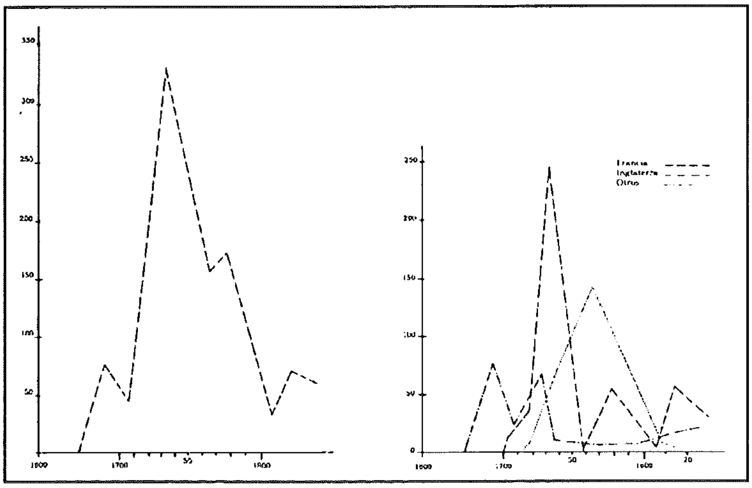
It is evident that during the third decade of the 1700's there was a retrenchment of French scientists around the Cartesian tradition. The polemic was well served.

The Earth under Examination (1687-1755)

Up to this point we have centered on the development of the program outlined by Newton, and seen that the theoretical initiative rested with the scientific nucleus located in London. Through the Philosophical Transactions, this group even managed to lend institutional support to the first markedly Newtonian contributions to French science. Later, we saw how the French academic approach to the problem of the shape of the Earth was channeled primarily through experimental programs linked to astronomy and geodesics. At the beginning of the third decade of the 1700's the main elements were already in place to shape one of the most visible and tense scientific polemics of the Enlightenment. Prior to proceeding to analyze the polemic, we will spend some time on a quantitative study of the dimensions of the debate and also on a consideration of some of the causes that affected its scope and rapid growth. We will soon see that, as the years passed, the initiative on this issue began to move from London to Paris. Among the general reasons for this we must not forget the fact that, as the century progressed, the French Royal Academy of Sciences and French science became the center of attention for European scientific activity. But this, in our opinion, does not sufficiently explain the importance given in France to geodesic observations, and along with that to the question of the shape of the Earth.

Newton's conclusions, as we have already seen, relegated the problem to a marginal plane of science, since the instruments and methods of observation could not yield data that would permit a comparison of his predictions with those put forth by Huygens. In addition, the respective shapes of the terrestrial geoid and the sphere were so close that they had no practical repercussions on geography. Thus it was hardly likely that the question would stimulate European states to give priority to the development of this line of research. As a result, both technical-scientific and politico-economic reasons coincided in the convenience of postponing investigation of the question.

A bibliometric analysis of the scientific literature of the period shows us that in fact this must have been its normal evolution. Graph I shows that



GRAPH I: Publications on the theory of the figure of the Earth.
GRAPH II: Publications on the figure of the Earth, and gravity.
Source: D.H. Hall (1976)

following the contributions of Newton, Huygens and Eisenschmid, a decline began which was abruptly interrupted by the massive contribution of geodesic observation that began in France in the second decade of the 1700's. Graph II illustrates the change of initiative alluded to above[41]. Excluding the contributions cited by Newton, Desaguliers, Stirling and Maclaurin, it becomes apparent that English science did not concern itself with the question of the shape of the Earth.

In fact it seems fair to say that this topic, except with respect to the theoretical foundations of some aspects of Principia, never acquired the character of a scientific problem and certainly never produced the kind of anxious concern that occurred in France. Only a few distinguished members of the Newtonian camp in the Royal Society, such as Desaguliers, published impassioned texts that harshly criticized the observations made by J. Cassini. In sum, as the peak in Graph II demonstrates, the debate over the shape of the planet was an essentially continental and predominantly French phenomenon. Furthermore, it was in the Paris Academy of Sciences, the sanctuary of Cartesianism, that the dispute was magnified.

After the third decade of the century, the intensity of the polemic and the importance of the issues in question caused these considerations to be exported to other countries. This process coincided with the creation, revitalization or support of institutions that, following the French model, were being established in Uppsala, Berlin, Saint Petersburg, Turin and Vienna. Other countries, such as Spain and the Italian states, were also clearly affected, since the great geodesic expeditions of the third and fourth decades of the 18th century not only brought practical astronomy back into style, but also required all countries to bring their relevant institutions and personnel up to date[42]. For example, many Jesuit schools throughout Europed housed astronomical observation posts which had been poorly maintained and were hardly prepared for the launching of a large scale program, like the one discussed here, to renew a kind of scientific activity which, because it was merely accumulative, had entered into a state of crisis at the end of the 17th century. Their incorporation into international projects was an easy task only as long as the previous observations of a dispersed, amateur and isolated character could be integrated and even programmed from a great center of data collection and analysis, as the Academy of Sciences would be for several years. Delisle's project to draw a precise map of the content, sponsored from Saint Petersburg, also fell into this category. What was exported, then, was an institutional model, including its objects of study and its research norms. The shape of the Earth, a problem that was initially solely a French affair due to the Academy's commitment to neo-Cartesianism, became an international affair.

But there is still a great deal to add about the process described. Let us put aside, for the moment, the circumstances that determined the impassioned convictions in the dispute and their identification with nationalist sentiments. The collapse of Cartesianism, the greatest achievement of French culture during the preceding century, was observed with complacency in London. Within the European context, in which France was an economic and cultural power, the real reason for their concern must be sought in the determined policy of intervention and ordering of geographic space begun by Colbert. In fact, the protection of manufacturing, the struggle for the suppression of internal duties, the rise of a network of land and river communications, the exploitation and inventory of natural resources, all stimulated the development of geographical research. An adequate response to these economic objectives required the cartographic recognition of French soil. The drawing of the map of France was thus a priority on its scientific policy during decades.

"One is well aware," wrote Cassini in 1733, "of the advantages that can be extracted from the exact knowledge of the extent of the realm, of its boundaries and the precise position of distant places.

Without this knowledge it would be difficult to adopt correct measures for many projects useful to the State and commerce, such as the construction of new roads, bridges and highways, and navigable rivers, which can greatly ease the transport of provisions and merchandise from one province to another, and contribute to the wealth of the Kingdom." [43]

Together with the medium and long-range projects mentioned above, other more peremptory projects accelerated the development of this line of research due to its extraordinary repercussions on military logistics and strategy. The importance of cartographic information did not escape Cassini either, when in 1718 he explained in utter clarity: "The location of many significant places and of most of the cities of Artois and France is of great utility in the drawing and correcting of the specific maps of this country which ordinarily is the theater of war and which it is so important to know with precision." [44]

Perhaps this text needs no comment to justify the reasons for which very costly and lengthy geographic investigations were financed with such steadfastness. Most of the Enlightened literature is full of considerations on the utility and benefits that would accrue to the State from the advancement of the most varied projects of research and institutionalization. Although these claims for utility formed as much a part of the strategy carried out by the men of science in order to obtain financial support for their research as of the spirit and general ideology of the period, there is no question that in the specific case we are analyzing the practical consequences of their utility was very soon appreciated. In 1748 Grandjean de Fouchy, Permanent Secretary of the Paris Academy of Sciences, commenting on the recent work of Cassini de Thury, confessed that, "Since 1746, the King has ordered Mr. Thury to follow the progress of his armies closely, and to link the conquests he was planning to the triangles of the meridian; at the same time that war compelled his enemies to respect the arms of the King, Geography had to take advantage of the success to assure and broaden knowledge useful to all nations." [45]

In 1760, all the geodesic work related to the question of the shape of the Earth completed, the same Cassini III himself, in a public lecture at the Academy, confirmed the French scientific community's commitment to the interests and needs of the State. He said, "...there is not at present any astronomer in this Academy who has not journeyed for the progress of the Academy and of geography, because they have taken part in the measuring of degrees from all over the world." [46]

Earlier we stated that the reconciliation of Cassini's results with those of Newton and Huygens would have been considered only a marginal question in the science of the first half of the 18th century. The effort the Secretary of State and War was ready to make in the field of practical geography, led by Maurepas, was thus amplified by the fact that two contradictory conceptions of the world were involved, the "patrimony" of two scientific communities so closely identified with national ideals.

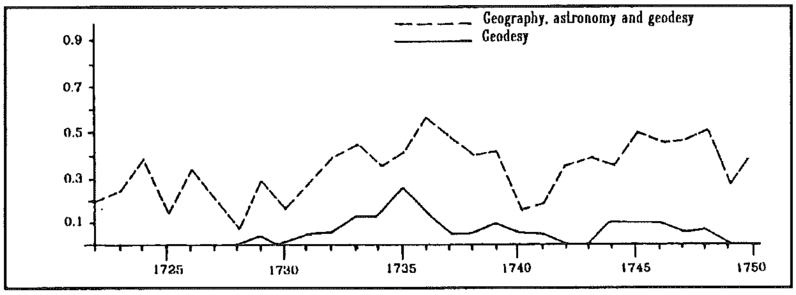
It is not that England was impervious to the practical applications of modern science. This is obviously not true. But England held different strategies in matters of scientific policy, if such terminology can be applied to the period in question. In fact, England's programs, in consonance with a larger maritime vocation and a more advanced colonial project, tended primarily towards the consolidation of a great data bank for navigation. England thus stimulated research related to the problems of measuring longitude, developing precise nautical instruments, and compiling stellar charts. In the field of astronomy, the programs of systematic observation carried out by Flamsteed, Molyneaux, Bradley and Halley, led to notable discoveries like *aberration* and *nutation*. Their merit has never been disputed in the annals of astronomy, but it is noteworthy that these investigations were closely linked to the particular talents of individual astronomers. By contrast, the French program required massive accumulations of data and the mobilization of all the technical and human resources available in order to finish such an ambitions collective enterprise as the drawing of the national map. This huge labor generated a "critical mass" of research with results sufficient to present geodesics as a new scientific discipline, autonomous of geography and astronomy.

The financing and the people were lead by the hand of this great project. But this alone was not enough to account for the rise of geodesics. Fontenelle, in his "Eloge de M. Delisle" of 1726, commenting on the eminently empirical character of geography, said, "...ordinary needs do not demand great accuracy from Maps. It is true that, in those which are to serve Navigation, it is necessary to have maps that can never be too perfect, but only Navigators feel this need, since their lives depend on it." [47]

This text illustrates our earlier considerations on the direction in which English astronomy evolved. It also provides us with a magnificent evaluation of the approximate and cumulative character of the geography of the times. These ordinary needs could be fully satisfied with data that permitted the location *grosso modo* of a geographic point or the outline of a coast or border. The question of the shape of the Earth required, on the other hand, measures of extraordinary precision, and this provided enough stimulus to lead astronomers, geometricians and geographers to probe deeply into the methods and instruments of observation. Geography passed from a descriptive to a mathematical discipline and astronomy reached for heights of precision never seen before. Geometry, furthermore, invaded the territory of both and contributed the "models" necessary for the interpretation of the experimental data. An outstanding testimony of this can be found in the brief historical outline written by La Caille on the astronomy of his age: "Although the Astronomers have worked assiduously in the formation of Collections of Observations, it is necessary to realize, nevertheless, that from 1692 until approximately 1725, practical Astronomy made no appreciable effort. It made no favorable attempt to introduce more precision into the observations."[48]

The effort La Caille refers to was undoubtedly a direct consequence of the research carried out to determine the amount of polar flattening on the planet. If aberration and nutation were detected in England, France produced the first mathematical theory that explained them and the systematic correction of the astronomical observations of these apparent movements in the position of stars.

Graph III presents, in percentages, the effort carried out at the Paris Academy of Science on the topics under discussion. The broken line shows the quantity (in percent) of memoirs published on astronomy, geography and theories of the shape of the Earth. The solid line represents those that address strictly geodesic problems, that is, measurements with the pendulum, determination of degrees and descriptions of specific research methods. The graph shows that the interest of the Academy was growing around the third decade of the 1700s, with the number of contributions stabilizing following the sharp oscillations of previous decades.



GRAPH III: Published Memoirs by the Academy of Sciences (Paris)

As the number of memoirs published increased, the solution to the problem of the shape of the Earth seemed to gradually recede from the reach of contemporary science. The conflict between data gathered through experimental research and the theoretical predictions remained irreconcilable. Greater and more generalized levels of precision had been achieved, reaching a conclusion that the Earth was a spheroid flattened at the poles, but when comparing the distances measured in degrees it was impossible --unless the data were distorted-- to resolve the magnitude of the flattening. This proved discouraging to scientists, explaining the sudden drop in publications visible in the graph. The exhaustion of the subject under the assumptions with which it was being approached was evident. Even when postulating masses of non-uniform fluid with a radial distribution of densities --remember what has already been said about Clairaut-- it was thought that the Earth must have a regular shape that was an exact replica of some mathematical entity. This conception, at the end of the forties, was entering a state of crisis. By then nobody dared to defend what was beginning to be seen as a simplistic view of nature. Some risked proposing modifications of the law of universal gravity by introducing some corrective term (Clairaut). Others built a theoretical model and explained to what extent each observer had erred in his results (Euler). The rest proposed new methods based on the parallax of the Moon and showed their skepticism with respect to the possibility of determining the shape of the Earth with a basis in geodesic measurements (Manfredi, Maupertuis and

Boscovich). All, however, recommended starting systematic programs of geophysical research. This is the unequivocal conclusion Boscovich reached after effecting the geodesic measurements to determine the length of a degree in the Vatican States between the cities of Rome and Rimini. The Italian Jesuit wrote, "Here is what I think in general about all this. In the first place I am convinced that the enterprise designed to determine the size and shape of the Earth by measuring degrees, far from being finished, is scarcely begun.... Until now the more degrees measured, the less certain the shape of the Earth." [49]

If from the purely experimental point of view the subject was exhausted, given the dispersion of the existing data, Boscovich's reflections also touched on theoretical assumptions: "The presumption of regularity and simplicity is a source of errors, which has often infected philosophy." [50]

Thus, geodesic observations were abandoned for the moment, in order to directly address the problem of the internal structure of the planet from the assumption of a regular and more or less arbitrary distribution of masses.

In 1746, G. Fouchy, Permanent Secretary of the Paris Academy, commenting on a memoir by Guettard, wrote, "Until now Geography has not had greater objectives than describing the surface of the Earth, and demarcating on it the different divisions of which it is susceptible, whether with relation to the sky or with respect to the different borders of the Empires which have successively divided it. This year, we must pay attention to a geographical work of another kind. It is not a question of dividing the different regions of the Earth along the extents of Empires and their provinces, but with relation to the different materials the earth holds within itself. The memoir presented by M. Guettard on this subject is, properly speaking, an attempt at a new mineralogical science." [51]

Thus once again scientific and economic interests were to coincide. From France, a leading world economic power together with England, this hidden disdain for the science of the outlines and borders that encompassed both the political and economic expansion of the "Empires" was easily comprehensible. Nevertheless, the program developed on the sidelines of astronomy and contributed nothing to the solution of the problem of the shape of the Earth. Something else would happen with one of the more exciting proposals, from the political and scientific point of view, derived from all those years of geodesic research. During the whole period under study, the theme of standards of measurement had become an obsession for scientists. Any conclusion about the length of a degree of a meridian or parallel rested, as we know, on the most exact determination of a unit used in the measurements of the fundamental base and on corroboration. The new precision required for geodesic observations and the growing exchange of information between different scientific communities made the rationalization of a problem as basic as the unit of measurement indispensable. La Condamine understood this, and three years after his participation in an expedition to America he proposed a "New project for an invariable measure adequate to serve as a common measure in all nations." [52]

From the point of view of the administrative central bureaucracies, the unification of a system of weights and measures was necessary to establish a uniform and direct fiscal system, and for the ordering of economic relations. But the structure of property and the production system of the *ancien regime* made such reform impossible. It would be necessary to wait for the National Assembly held after the French Revolution to give the matter priority and recommend Delambre, Méchain, Borda and others to urgently undertake the research necessary to institute the decimal metric system. Meanwhile, Fouchy again alludes to the obstacles erected by those who took advantage of the existence of internal customs tariffs and of the diversity of systems of measures in order to speculate with prices. "...the objection deserves a response: some tradesmen, it is said, find in these differences in standards of measurement a profit of which they would be deprived if there were only a single one used throughout the world; and it is this profit that stimulates their trade and motivates them to provision the fairs and markets."[53]

Thus, the geodesic research projects contributed to the clarification of new problems that permitted improvements in our knowledge of the Earth as well as in the precision and analysis of the data collected. The polemic, once Newton's thesis was confirmed, disappeared, and once more the question of the shape of the Earth and the magnitude of its polar flattening was relegated to secondary status.

NOTES

1. With cautious skepticism, we have adopted the following equivalent for the furlong: 1 furlong = 177.7 meters. More or less complete comments on these first measurements which will be described later can be found in the introductory chapter of all works dedicated to the shape of the Earth. The bibliography, if we wished to be exhaustive, would be very long. We will cite only a few works which in our judgement used primary sources. J. Cassini, De la grandeur et de la figure de la Terre, which appeared as Suite des Mèmoires de l'Académie royale des Sciences, Paris, 1720; J. S. Baily, Histoire de l'Astronomie moderne, Vol. III, Paris, 1782; J. B. Delambre, Abregé d'Astronomie ou leçons elementaires d'astronomie théorique et pratique, Paris, 1813; J.F. Lalande, Astronomie, 3 vols., Paris, 1792, vol. III.

2. The error committed by Posidonius in assuming both cities to be on the same meridian was no greater than 1°30'. See C. M. Taisbak, "Posidonius vindicated at all costs? Modern scholarship versus the stoic earth measurer," in Centaurus, 18: 253-269, 1974.

3. For these and other measures of the length of a degree in Arab science, see W. Hartner, "An unusual value for the length of the meridian degree: 66½ miles, in Ibn Yunus' Hakimitic Zij," in Centaurus, 24: 148-52, 1980. In Babylon, as told by Al-Fargani, it was observed that the pole rose one degree in a distance of 111,800 meters. See José María Millás Vallicrosa, La otra Forma de la Tierra de R. Abraham Bar Hiyya Ha-Bargeloni, Madrid-Barcelona, 1956.

4. For the toesa we use the following equation: 1 toesa = 1.949 meters. La Caille, with more sophisticated astronomical and geodesic methods, obtained 57,074 toesas in the middle of the 18th century.

5. The full title of the book is Eratostenes batavus de terrae ambitus vera quantitate a Willibrodo Snelio. Lugduni Batavorum ap. Iodocum a Colster, 1617. On this measurement see H. Bosmans, "Le degré du méridien terrestre mesuré par la distance de Berg-op-Zoom et Malines," in Anales de la Societé scientifique de Bruxelles, 24, Part 2, 113-134. See also J. B. J. Delambre, Histoire de l'Astronomie moderne, II, Paris, 1821, pp. 92-110. A century later, Musschembroeck would review the memoirs of his countryman (cf. "De magnitudine terrae" in Phisicae experimentales, Amsterdam, 1729), correcting the results, which he raised to 57,033 toesas.

6. In addition to the work cited (La Mesure de la terre, Paris, 1671), see Volumes VI and VII of the series Mémoires de l'Académie royale des Sciences depuis 1666 jusqu'en 1699, where a description of these works will be found. To place Picard's work in historical context, a very useful book is C. Wolf, Histoire de L'Observatoire de Paris depuis sa fondation à 1793 (Paris, 1902); Gallois, "L'Académie des Sciences et les origines de la carte de Cassini," in Annales de géographie, 18, 193-204, 1909; G. Picolet, "La correspondance de Jean Picard avec Joham Hevelius (1671-1679)" in Revue d'histoire des sciences, 31, 3-42, 1978. An analysis of Picard's work can be found in J. Loridan, Voyages des astronomes françaises à la recherche de la figure de la Terre et de ses dimensions, Lille, 1890, p. 19ff., and also in J. F. Lalande, Astronomie, op. cit., articles 2583 and 2654. See L. Lagarde, "Historique du problème du Meridien origine en France," in Revue d'histoire des sciences, 32, 289-304, 1979. This work shows the intimate relation between a long series of projects --measurement of the meridian, expeditions to the Mediterranean (Chazelles and Feuillé), to the Antilles and South America (Feuillé)-- all relating to the interest in calculating the exact position of the island of Hierro with relation to the Paris meridian. See also J. P. Duvois, Voyageurs français en Amerique. Colonies espagnoles et portugaises, Paris, 1978.

7. J. Richer, "Observations astronomiques et physiques faites en l'isle de Caienne," in Memoires de l'Académie royale des Sciences depuis 1666 jusqu'a 1699, VII, 231-326 (Paris, 1679). See also J. Olmsted, "The scientific expedition of John Richer to Cayenne (1672-1673)." in Isis, 34, 117-128, 1942-43.

8. Hist. 1700, p. 111.

9. The table is constructed with data which comes primarily from original sources, and when it does not we

have compared several works before adopting the shape to be included. There were other determinations of the length of the hourly pendulum which we excluded because of their considerable inaccuracy, such as those carried out under Couplet's direction at the request of the Paris Academy of Sciences, which earned this judgement from Newton: "...the observations made by this gentleman are so crude that we cannot place any confidence in them." (Principia, Book III, Prop. 20). We express the length of the pendulum in lines, a unit equivalent to about 2.23 millimeters (1 toesa = 6 feet; 1 foot = 12 inches; 1 inch = 12 lines and 1 line = 12 points). The table of predicted lengths is given by Newton (loc. cit.) for variations of one degree of latitude; we have tried to give the possible approximate value for each specific latitude, so the column "Difference" is only meant to indicate amounts of error rather than exact quantities.

10. An actual detailed analysis of the Newtonian propositions can be found in J. Plana, "Sur la théorie mathématique de la Figure de la Terre, publié par Newton en 1687. Et sur l'etat d'équilibre de l'ellipsoï de fluide à trois axes inégaux," in Astronomische Nachrichten, No. 850, 36, 150-70, 1853. See also on this whole section of this chapter, our A. Lafuente, "La mecánica de fluidos y la teoria de la figura de la Tierra entre Newton y Clairaut (1687-1743), Dynamis, 3, 55-90, 1983. Of course, I. Todhunter, A history of the mathematical theories of attraction and the figure of the earth, 2 vols., London, 1873, continue to be indispensable.

11. Newton, Principia, Book III, Proposition 20.

12. An current mathematical analysis can be found in J. Plana, "Note sur la figure de la terre et la loi de la pesanteur à sa surface, d'apres l'hypothèse d'Huygens, publié en 1690," in Astronomische Nachrichten, No. 839, 35, 371-78, 1853.

13 C. Huygens, Discours sur la cause de la pesanteur, a text published as an appendix to his book Traité de la Lumière, Leyden, 1690. The citation is from p. 159.

14 C. Huygens, Discours, p. 137.

15. An analysis of the treatment given to gravity according to the Cartesian tradition can be found in R. Dugas, La mécanique au XVIIIème siècle, Neuchâtel, 1954, pp. 312ff. and 446ff. See also R. S. Westfall, Force in Newton Physics, New York, 1977, pp. 177ff.

16. Newton, Principia, Book III, Prop. 20.

17. P. Bouger, "Comparaison des deux loix que la Terre et les autres Planetes, doivent observer dans la figure que la pesanteur leur fait prendre," in Mem. 1734, pp. 21-40 and Hist. 1734, pp. 83-87. The quote is from p. 27.

18. P. L. M. Maupertuis, "De Figuris quas Fluida rotata induere possunt, Problemata duo; cum conjectura de Stellis quae aliquendo prodeunt vel deficiunt; et de Annulo Saturni. Authore Petro Ludovico De Maupertuis, Regiae Societatis Londinensis, et Academiae Scientiarum Parisiensis Socio," in Philosophical Transactions, No. 422, vol. 37, pp. 240-56, 1732. This volume covers the years 1731-32 and was published in 1733. The conclusions of this work were later reprinted in his famous Discours sur les differents figures des Astres (Paris, 1732) and in "Sur les loix de l'Attraction," in Mem. 1732, pp. 343-362. See A. Lafuente and José L. Peset, Maupertuis, el orden verosímil del cosmos, Madrid, Alianza Ed., 1985. Also see Fontenelle's comments on this memoir in Hist. 1732, pp. 112-117. Also very important is J. Stirling's memoir, "Of the Figure of the Earth and the Variation of Gravity on the Surface," in Philosophical Transactions, No. 438, Vol. 39, pp. 98-105, 1735. (Actually published in 1738.) See I. Todhunter, op. cit., pp. 77ff.

19. In reality, before the publication of the Théorie de la figure de la Terre, Clairaut's dedication to the topic was intense. His most important memoirs were the following: "Investigationes aliquot, ex quibus probetur Terrae figuram secundum leges attractionis in rationis inversa quadrati distantiarum maxime ad Ellipsiu accedere debere, per...." in Philosophical Transactions, No. 445, Vol. 40, pp. 19-25, 1737. Also, "An Inquiry concerning the Figure of such Planets as revolve about an Axis, supposing the Density continually to vary, from

the centre towards the surface," in Philosophical Transactions, No. 449, Vol. 40, pp. 277-306, 1738. On the methods for solving the differential equation that resulted from requiring this condition of equilibrium, Clairaut wrote two memoirs, "Recherches générales sur le calcul intégral", in Mem. 1739, pp. 425-436, and "Sur l'integration ou construction des équations differentielles du premier ordre," in Mem. 1740, pp. 293-323. See the article in the Dictionary of Scientific Biography (DSB ed. Ch. C. Gillispie) written by R. Taton on Clairaut. Also M. Kline, Mathematical Thought from Ancient to Modern Times, New York, 1977, pp. 425-6, C. A. Truesdell, "Rational Fluid Mechanics, 1687-1765," in Leonhardi Euleri Opera Omnia, Series II: Mechanica, Vol. 12, pp. VII-CXXV, Lausanne, 1954, and R. Dugas, Histoire de la mecanique, Neuchätel, 1950.

20. A. C. Clairaut to L. Euler, Paris, January 4, 1742, in Leonardi Euleri Opera Omnia, Series Quarta A: Commercium Epistolicum, Vol. V.: Correspondance de Leonhard Euler avec A. C. Clairaut, J. D'Alembert et J. L. Lagrange, redigé par A. Juskevic et R. Taton (eds). Basel-Boston-Stuttgart, Birkhäuser Verlag, 1980, p. 102ff.

21. L. Euler to A. C. Clairaut; Berlin, January-February 1742. Cit. ant. p. 110ff.

22. A. C. Clairaut, Théorie, op. cit. p. 1. In the Introduction he recognizes that Bouguer for the first time demonstrated the necessity of verifying the two principles that he called "Principe des Canaux" and "Principe de Surface de Niveau," "...because I have found that there were an infinity of hypotheses about gravity, in which these two principles would give the same curve, without making the forces of all the parts of the Fluid mutually counterbalance." P. 31. On p. 5 he formulates the general principle of equilibrium in the following terms: "For a mass of fluid to be able to stay in equilibrium, it is necessary that the forces of all the parts of the fluid enclosed in any channel that crosses it, mutually destroy one another".

23. See I. Todhunter, op. cit., pp. 191ff. A complete reconstruction of the topic of the shape adopted by non-homogeneous fluid masses in rotation, with numerous references to Clairaut's work, can be found in M. O. Callandrau, "Mémoire sur la Théorie de la Figure des Planètes, in Annales de l'Observatoire de Paris, 19, pp. E1-E52, 1889; P. Apell, Traité de mecanique rationelle, Vol. IV, 2nd edition, Paris, 1937, and V. Liapounoff, "Sur l'équation de Clairaut et les équations les plus génerales de la théorie de la figure des planètes," in Mémoires de l'Académie des Sciences de Saint-Pétersbourg, 15, No. 10, 1904.

24. Cf. Bowen, Empiricism and Geographical Thought, from Francis Bacon to Alexander von Humboldt, Cambridge, 1981, pp. 106ff. See also H. Capel, La física sagrada: Creencias religiosas y teorías científicas en los orígines de la geomorfología española. Barcelona, Ediciones del Serbal, 1985.

25. G. D. Cassini, "Observations astronomiques faites en France et en Italie, en 1694, 1695 et 1696 par MM. Cassini de l'Académie Royal des Sciences," in Memoires de l'Académie Royal des Sciences, contenant les ouvrages adoptez par cette, Vol. 6 (Amsterdam, 1735). The definition of "brasse géometrique" is on p. 23.

26. Hist. 1701, p. 96. In the new edition of the Memoires... of the Paris Academy of Science published in Amsterdam in 1743, Fontenelle's error is corrected. In lieu of these words, the following appears: "It can be seen that a meridian is an ellipse, the equator always remains perfectly circular, and that the shape of the Earth is a spheroid." The first to point it out was Maupertuis in his Lettre d'un Horloger Anglois à un astronome de Pékin (1740). All the information we provide can be found in J. F. Lalande, Astronomie, III, No. 2676.

27. Voltaire, Eloge de Newton. Also reproduced in R. Dugas, Histoire de la mécanique, op. cit., p. 208.

28. J. Cassini, "De la figure de la Terre", in Mem. 1713, p. 325.

29. Hist. 1713, p. 84. The best summary we have found of the set of operations realized in France by the Cassini can be found in J. Loridan, Voyages des Astronomes..., op. cit.

30. J. Cassini, "De la grandeur de la terre et de sa figure," in Mem. 1718, pp. 310-324.

31. Hist. 1721, pp. 84-98.

32. A. C. Clairaut, "Determination géometrique de la perpendiculaire à la meridienne tracée par M. Cassini; avec plusieurs Méthodes d'en tirer la grandeur et la figure de la Terre," in Mem. 1733, pp. 406-7.

33. J. de Roubaix, Dissertation physique sur la variation du barométre, la forme du globe terrestre, la diminution des graves, et le flux et reflux, Leyde, 1719. A work in which he supports Cassini's thesis. See J. F. Lalande, Astronomie, No. 2676.

34. J. N. Delisle, "Nouvelles réflexions sur la figure de la Terre," Archives de l'observatoire de Paris, AOP. ms. A-7-7-. Numa Broc (La géographie des philosophes..., op. cit., p. 38) states that this manuscript, dated 1716, is kept in the Bibliothèque National (Paris) BNP, Mss. Françaises, No. 9671. We are not surprised at such an early reference by Delisle to his countryman's thesis. A few years later, in 1737, informed of the conclusions drawn from the operations carried out by Maupertuis in Lapland, he slightly modified his initial commitment. Wanting to take an active part in the geodesic work to solve the problem of the shape of the earth, he proposed to the Tsar of Russia the triangulation of part of his State. Through numerous contacts he succeeded in publishing the project in various magazines, such as the Philosophical Transactions (No. 449, vol.40, pp. 27-49) under the title, "A proposal for the Measurement of the Earth in Russia, read at a Meeting of the Academy of Sciences of St. Petersburg." It was also printed in French as Projet de la mesure de la Terre en Russie, lû dans l'Asamblée de l'Académie des Sciences de S. Petersburg, le 21. Janvier 1737 par M. de Lisle, premier Professeur d'Astronomie à S. Petersburg (S. Petersburg, 1737) which, when it was mentioned in Memoires des Trevoux (38, 201-206, 1738), earned the following reproach: "Perhaps it seems to someone that initially there is an inclination towards the system of M. Newton on the shape of the earth."

35. Hist. 1733, p. 56.

36. J.B.J. Delambre, Grandeur et figure de la Terre, Paris, 1912, pp. 24-25.

37. R. Taton, "Gian Domenico Cassini," in DSB III, p. 103.

38. D. de Mairan, "Recherches géometriques sur la diminution des degrés terrestres en allant de l'équateur vers les pôles, où l'on exprime les conséquences qui en résultent tant à l'égard de la figure de la terre que de la pesanteur des corps et de s'accourcissement du pendule," in Mem. 1720, p. 60.

39. Hist. 1729, p. 51.

40. Hist. 1729, p. 61.

41. Graphs I and II are from the excellent book by D. H. Hall, History of the Earth Sciences during the Scientific and Industrial Revolutions with Special Emphasis on the Physical Geosciences, Amsterdam, 1976, pp. 183 and 189.

42. C. André y G. Rayet, L'Astronomie pratique et les observatoires en Europe et en Amerique depuis la milieu du XVIIIe siècle jusqu'à nos jours, 5 vols. Paris, 1874-78. See especially Volume V concerning the observatories of Italy.

43. J. Cassini, "De la Carte de la France et de la perpendiculaire à la meridienne de Paris," in Mem. 1733, p. 389.

44. J. Cassini, "De la grandeur de la Terre et de sa figure," in Mem. 1718, p. 252.

45. G. de Fouchy, "Sur la comparaison des mesures de Snelius à celles qui ont été faites en France," in Hist. 1748, p. 110. The commentary of the Secretary of the Academy was relative to the memoir by C. F. Cassini de Thury, "Sur la jonction de la Meridienne de Paris à celle que Snelius a tracée dans la Hollande; avec des Réflexions sur la Carte de la France" in Hist. 1748, pp. 123-132. This same aspect was emphasized by Condorcet in his "Eloge de César-François Cassini de Thury" in Hist. 1748, pp. 54-63.

46. The speech was given at the academic session of November 12, 1760, and published in Mem. 1757 (printed in 1762). The citation is from page 331.

47. Fontenelle, "Eloge a M. Delisle," in Hist 1726, p. 75-84. The Secretary of the of the Academy expressed himself in similar terms in his "Eloge de La Hire," when in referring to geodesic and geographic observations he stated, "This class of operations does not require refined theory, but rather great skill and sureness in its execution, a great deal of careful attention and clever precautions, and, in sum, its great utility compensates for its meager geometrical brilliance." Hist. 1718, p. 79.

48. La Caille, "Discours sur les progrés que l'Astronomie a faits depuis une trentaine d'anées," in Ephemerides des mouvements cèlestes, pour dix années, depuis 1765 jusqu'en 1775, 3 vols., Paris 1763. Vol. III, pp. 31-49. The quote is from page 32.

49. The original memoir of results was published as a book under the title, De litteraria expeditione per Pontifician ditionem ad dimetiendos duos meridiani gradus et corrigendam mappam geographicam jussu et auspiciis Benedicti XVI, Pont. Max., suscepta a partribus Soiet. Jesu Christophoro Maire et Rogerio Josepho Boscovich (Rome, 1775). We have used the French translation that was published in Paris in 1770, Voyage astronomique et geographique dans l'Etat de l'Eglise.... The quote is from pages 491-92. Earlier Boscovich had already addressed the theme, coming to similar conclusions. In the Disertatio de Telluris figura (Rome, 1739) he displayed his skepticism about the possibility of determining the shape of the Earth with measurements of degrees of the meridian, since in his opinion the inaccuracy of the astronomical observations could be decisive. In De inaequalitate gravitatis in diversis terrae locis (Rome, 1741) he insists more carefully on his earlier points of view. He adds that the law of variation of gravity with latitude cannot be considered demonstrated by experiments made with the pendulum, since the dispersion of the data made plausible the existence of internal cavities in the Earth or other geographical irregularities. On this work by the Yugoslavian Jesuit, see the article by Zeljko Markovic, "R. J. Boscovic et le théorie de la figure de la Terre," in Conférence donnée au Palais de la Découverte (5. 9. 1960) Paris, 1960.

50. Voyage astronomique..., op. cit., p. 29.

51. Hist. 1746, p. 105. Guettard's memoir is "Memoire et Carte mineralogique sur la nature et la situation des terreins qui traversent la France et l'Angleterre," in Mem. 1746, pp. 365-392.

52. The memoir is inserted in Procés-Verbaux de l'Academie des Sciences, manuscript in the Academy of Science Archives, AAS (hereinafter *Reg.*), Reg. 1748, pp. 191-211.

53. Hist. 1747, p. 83.