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the rise and fall of a polemic

THE RISE AND FALL OF A POLEMIC

The academicians left the port of La Rochelle for South American shores when the polemic was at its height. At the end of a long year of preparations, the moment for action drew near. At last Prometheus would have his verdict. Soon the academicians would begin their observations. The hour of irrefutable data had arrived. Astronomy would dominate the stage in the cosmographic debate and deliver to the Academy the certainties of a ruling supported by rigorous experiments and carefully verified figures.[1] Everything was ready for the restoration of peace on Parnassus: "It was undoubtedly the most famous hour that had ever occurred in science," wrote Maupertuis.[2]

In fact, in Paris they all felt they had been catapulted into history as protagonists of an adventure that they had tried to prepare for with utmost care. Events, however, would soon prove that the difficulties awaiting them were far greater than they had foreseen. We shall consider these presently, but first we must mention a factor whose identification as a problem --in addition to its being a notable innovation-- constituted a decisive step forward in the development of science. Could a theory be rejected on the basis of observations whose results, far from being consistent, spanned a wide range of error? In other words, could astronomy and geodesics, not being exact sciences, aspire to the design of conclusive experiments? Were the expectations prevailing among Parisian academic circles justified?

Of course, the implantation of Newtonism on the European continent did not have to wait for the return of the South American expeditionaries. Since the results contributed by Maupertuis had been made known --and even earlier than that-- there had been powerful reasons for preferring Newtonian physics over the Cartesian model, no matter how strongly the Paris Academy of Sciences considered a decisive test indispensable. But what our argonauts from Peru never suspected was that all their diligence as observers would not be sufficient to measure the amount of the planet's polar flattening. What was worse, the greater the number of meridian degrees known, the more the possibility of measuring the numerical relation between the axes of the Earth receded. This was the unforeseen and, at the time, surprising conclusion reached by Boscovich, when the polemic was already waning, after having triangulated a meridian between Rome and Rimini. In 1755 the Italian Jesuit wrote, "This, then, is what I think about the matter in general. In the first place, I am convinced that the undertaking designed for the purpose of determining the size and shape of the Earth through the measurement of degrees, far from being finished, has scarcely begun.... Up to now, the more degrees that are measured, the less certain is the shape of the Earth."[3]

This conclusion, more than a recognition of resounding failure, signified a breakthrough of a deeper knowledge of the limitations of experimental science and empirical practice. Certainly the relationship between theory and observation was more complex that what had naively been thought in 1736. The years spent in the Viceroyalty of Peru by our scientists, as we will see below, were proof of this. Their project to obtain viable data and compare rival theories, was an undertaking as complex as the spirit with which they went to America was naive.[4]

Adverse Orography

We have already had occasion to describe the operations that were necessary in order to measure a degree. This was an area lacking major theoretical complications, and in which ample experience existed. Our academicians, therefore, had reason to feel sure that they would accomplish their mission. In addition, during the months prior to the journey a large part of the scientific activity going on in the Academy had been devoted to studying and bringing up to date the best methods for carrying out the procedures. The individuals chosen for the task, particularly Godin and Bouguer, seemed to possess the theoretical knowledge and practical experience required for managing observation instruments. Nor were the instruments themselves neglected. The best qualified artisans available (Graham, Langlois and Lemaire) built the quadrants, an astronomical sector twelve feet in radius, the hourly pendulum and the various lenses that were taken to America by the group. Even the measurement standard, on which depended all the precision of the observations with the pendulum and those leading to resolving the fundamental and corroborating bases, took the attention of Fouchy, Mairan, Godin and Langlois. They hoped to leave nothing to chance, and thus they were able to take with them an iron toesa constructed with every imaginable precaution in accordance with the standard established in 1668 at the foot of the stairs to the great Châtelet de Paris.

Instrument construction, however, was still being done using artisan methods and procedures. Although important advances had already been made in England, it was still not a question of precision manufacturing, much less one of specialized organization of tasks in the workshop. Even excluding laboratory or exhibition instruments, the construction of a quadrant, for example, presented significant technical difficulties. To insure the stability of an entire metallic armature, or guarantee parallelism between the planes defined by the quadrant and the axis of the lens were only two of the problems for which there were neither standard ways of solving nor sufficiently developed metallurgical techniques. These points aside, the main question still remained: how to accurately draw the lines on a rule or the limb of an astronomical instrument? [5] On a quadrant circle like the twenty-four-inch radius used by Jorge Juan,[6] the divisions were marked to correspond to differences in amplitude of 10', so that on a circular length of 102.05 centimeters, forty equidistant marks had to be drawn; that is, the distance between two consecutive marks was approximately 89 mm. The problem was not so much that the distance had to be small, but that it had to be exactly 89 mm, because an error of 1 mm was equivalent to somewhat more than 7" in the angular measurements. The corrections made by Bouguer to the observations executed with his quadrant alert us to the great technical limitations in the building of precision instruments. The following table shows the deficiencies found between the division of 40° and 50°, revealing errors of up to 3 mm in the markings:[7]

Division of the limb	Correction
40°	15"
41°	-25"
42°30'	-15"
44°30'	-36"
45°	-18"
47°	-15"
48°30'	-5"
50°	0

Source: P. Bouguer, "Table des Equations que j'ay employées pour la correction des angles de position observées avec mon Q. de cercle". AOP. ms. B-5-7.

Naturally, as the number of marks on the limb increased, so did the chances of error. Because of this, in order to achieve greater precision, they stopped marking at amplitudes of about 10' and the instrument was fitted with a micrometric screw. The one Bouguer had installed on his sector was capable of dividing an angle of 4'35" into one thousand parts, the value of each one of them being 16'''30^{IV}; that is, in theory one could effect angular observations accurate to a quarter second of an arc. Obviously no one believed such extraordinary precision to be possible even though these were the figures that the most optimistic gave in their records. In fact, even if we accept such precision as theoretically possible, in practice the error of the rule was around 10".

The initial project, as stated in the edict granted by Philip V to the expeditionaries, consisted of "...making astronomical observations below the equator and there measuring the degrees, not only of longitude but also of latitude, from which can be figured the exact form of the earth and correct measurement of the degrees of parallel could be inferred [...] in the vicinity of the city of San Francisco de Quito, choosing a portion of the equator and a meridian that can be measured easily."[8]

It was therefore imperative that the academicians reach Quito, the final destination of their trip and the base of operations from which they would plan the steps that were necessary to measure a degree of meridian and another one of parallel. This last task, according to Bouguer, was considered in Paris to be "the prime objective which we ought to set for ourselves."[9] But before they reached Quito, an attempt was made by Bouguer and La Condamine to change the original plans. The entire design of the expedition had been drawn up without any regard for topography or for the deficient sanitary conditions prevailing in colonial South America. Just a few days after disembarking in Cartagena, several members of the company contracted malignant fevers which no one knew how to treat. This was only a first warning, and it would be followed by many other such episodes. Even more difficult to foresee from Paris were the difficulties that triangulation would raise for them in the zone located between the two Andean ranges, where they would have to install markers at altitudes higher than four thousand meters and camp out in adverse climatic conditions for days at a time until the persistent mists dispersed and permitted visibility. They must have become aware of all this on their journey from Cartagena to Guayaquil. Until then, no doubt, they trusted that their mission, although less comfortable, would not be much different from missions that had already been undertaken in France--an optimistic view which must have been dashed by the difficult crossing from Portobelo to Panama via the Chagres River. The worst, however, was yet to begin. In order to reach their final destination they still had to climb the Andean cordillera by way of Guaranda and Chimborazo.

There were too many difficulties and Bouguer and La Condamine wanted to avoid them. On March 12, 1736, they sent Godin a memorandum explaining their reasons for the change of plans: "...our work will be simpler and more accurate and transportation easier for ourselves and our instruments, which will reduce costs considerably. It will also be much easier to guarantee the company's sustenance."[10]

Bouguer added other considerations about the greater benefits that would accrue to the Spanish crown from carrying out work near the Pacific coast, since the meridian "would pass through the Isthmus of Panama and the westernmost capes of South America." There was, then, a good set of reasons which, however, seriously contradicted the orders of the Royal Edict. Godin's opposition provoked an argument among the company which, due to various circumstances and new motives, was never resolved throughout the nine years of their stay in the Viceroyalty. In fact, during their stay in Manta, the dissension among the expeditionaries was so violent that it caused the first rift into two groups. Seniergues wrote to the brothers Bernard and Antoine Jussieu in February 1736: "...tomorrow we must go to a beach to see if the terrain is suitable for measuring a base there. Sir Godin does not like this idea. He wants to go to Guayaquil and from there to Quito in a straight line. Sir La Condamine has said in front of everyone that if no one wants to stay, he will stay alone. If he decides to do this Sir Bouguer will certainly stay with him...Sir Godin and those two have not spoken to each other for quite some time...It does not seem possible that they will be able to finish the journey together."[11]

But besides the administrative tasks imposed by the Royal Edict and the internal conflicts, Bouguer and La Condamine's proposal meant that the group would renounce the plan to measure a degree of parallel. It was an issue of great scientific interest that had been discussed at length in the Academy. Obviously the measurement of the two degrees initially projected would have yielded a larger quantity of information. But, what about its quality? That is, was the accuracy with which both programs of observation could be carried out comparable? No doubt, the answer was negative, as Maupertuis, Clairaut and Bouguer himself finally proved: with the methods available at the time it was not possible to insure an error of less than 30" in the measurement of longitude. In short, triangulating a parallel that had to traverse at least one of the Andean cordilleras was a difficult task whose results would not even be very reliable. Godin, on the other hand, insisted on keeping their commitment. The disagreement became so serious that they both decided to appeal to the Minister of the Admiralty, the Count of Maurepas, to resolve the dispute. Bouguer's letter to him, written on February 15, 1737, indicates how serious the problem was. "M. Godin until now is determined to start for the equator. ...I cannot avoid all these considerations whose evidence compels me to employ all reasonable means, even to the point of protesting, in order to change the decision on which M. Godin is firm, and, I am sure, Sir, that you will do me the honor of approving them."[12]

In fact, on March 9 Maurepas ordered them to abandon the idea of triangulating an arc of parallel. The operations, instead, would be carried out along the inter-Andean corridor in the vicinity of the city of Quito.

While these disputes were being resolved, which delayed the reconnaissance of the zone through which the meridian passed and where the triangulation markers had to be installed, they proceeded to measure the main base. The procedure was as rudimentary as its results were accurate. It consisted of measuring, toesa by toesa, the thirteen kilometers that separated two haciendas in Oyambaro and Caraburu. To do this they formed two groups --one with Bouguer, La Condamine and Ulloa, and the other with Godin and Juan-- which took twenty-five days to cover the distance, using every precaution imaginable. The two results differed by only two inches and ten lines, which is an error of around 10^{-5}. The averaged figure of $6273^{\text{toesas}}\ 4^{\text{feet}}\ 3^{\text{inches}}\ 7^{\text{lines}}$, the arithmetic mean of the actual measurements, was merely a quantity that had to be reduced to its value on the earth's surface. Here the real technical and scientific problems of the expedition began.

The Perversity of Numbers

The ground on which the base was measured had a drop of 126 toesas; thus just as in the case of the different imaginary sides which made up the triangulation, it was necessary to know the value of the drop in the plane of the horizon before proceeding to reduce the distances to what they would be at sea level. Once this calculation was made it was necessary to determine the height of each observation point barometrically, as well as its inclination with respect to the meridian. Besides instrumentation errors and those made by the observer, which could be avoided partially by increasing the number of observations, a new factor of inaccuracy was also introduced: all of the angular measurements were affected by atmospheric refraction, a phenomenon which for great distances, high levels of environmental humidity, and large differences in altitude could have a significant effect which the academicians did not want to ignore. Similarly, thermometric fluctuations affected the toesa standard and the base measure had to be corrected for the effects of expansion. Of such phenomena, little was known save their existence; there was no tested theory, nor adequate empirical practice. This was logical, inasmuch as the instruments available did not make allowance for them, nor had the limited demand for accuracy that characterized the science of the day exerted sufficient influence. Our expeditionaries could have opted for less demanding results more in accord with their real capabilities, but the events in Europe left them no alternative. If their ambition for fame and international recognition had inspired them to undertake the voyage, the objective of their mission changed once the expedition to the North had returned to Paris and solemnly presented its conclusions. History would not remember them for having decided the polemic in favor of Newton because Maupertuis had just proven, in unimpeachable terms, the polar flattening of the planet. Thus it was necessary to carry out a program of observations whose breadth and rigor would diminish Maupertuis' achievement. Indeed, as Voltaire realized with great perspicacity in 1745, that is how it would be: "...the Peruvian mission, owing to the vast program of observations which it had the double merit of beginning and carrying out, has stood as a model for all scientific expeditions coming after it. At first glance, our scientists have not added anything more than a few numbers to celestial science, but the scope of their research was really much wider, and the impulse which they gave to observational studies more lasting, than has commonly been supposed."

It is impossible to describe here the whole of their research. We will not only have to omit the botanical, medical, cartographic and anthropological works; we will also have to limit our selection of issues of purely geodesic or astronomical nature.

In fact, our expeditionaries intended to consider in detail all physical phenomena which might have any effect on their observations. Soon enough, however, they were inundated with a huge mass of empirical data whose codification and interpretation was virtually impossible owing to the absence of adequate theoretical tools. They planned to approach a proliferation of systematic observations of particular phenomena (expansion of materials, atmospheric and astronomical refraction, variation of the verticality of the plumb-line in the presence of mountainous masses, speed of sound, variation of the maximum obliquity of the ecliptic, expansion of a column of mercury, and so forth) without having at hand a theory of errors which would equip them with a global perspective on experimentation while minimizing the significance of relatively minor effects. For example, after arduous observations, difficult calculations, and multiple polemics related to the problem of atmospheric refraction, they wound up employing an approximate solution which introduced a relative error, whose impact on the value of a degree was irrelevant compared to the significant divergence of results in observations of latitude. But our academicians, far from bearing such considerations in mind, chose the easier route, given the impossibility of formulating a convincing theory. It will suffice to observe that Godin even produced negative refractions before renouncing his theoretical model. What matters here is not so much documenting a small failure but rather stressing that, even in the best of cases, solving this question would not have increased the accuracy of their results. La Condamine recognized this when he published his research in Quito in the Memoires de l'Academie: "Inasmuch as one cannot be sure of a real measurement of several inches over a distance of more than six thousand toesas, it would be useless to take the precision in calculating beyond the limits of our capability, especially since even one foot of error in the longitude of the base would not produce a difference of more than one and a third toesas in the degree."[13]

The confidence with which such a wise conclusion was expressed hardly reflects his concerns during 1741. At that time he was debating the value of correction due to refraction with Godin and Bouguer, as if it were a crucial matter. And this was not solely due to the permanent state of personal wrangling in which they found themselves.

The manuscript documents that are preserved confirms the presence of a certain amount of satisfaction with merely accumulating data on everything within the range of their instruments. The observations on the expansion of the toesa are a good example. The measurements taken showed such diversity that Juan and Godin finally proposed two different laws, one for contraction and one for expansion. This was an unfortunate outcome, bordering on the absurd, arising from the idea that it was necessary to establish a table of corrections for the effects produced by decreases or increases in temperature. This did not keep them from finishing the job. With their results expressed in terms of a coefficient of expansion, the value they would have found for iron is as follows:

Juan	$K = 38.\ 10^{-5}\ {}^{o}C^{-1}$
La Condamine	$11.\ 10^{-5}\ {}^{o}C^{-1}$
Bouguer	$22.\ 10^{-5}\ {}^{o}C^{-1}$

The variation among the three coefficients clearly illustrates the technical impossibility of measuring differences of longitude that were less than the margin of error contained within the divisions marked on the rule itself. The observers were not ignorant of this; nevertheless, they believed they were making valuable contributions to science. This is not the only instance of the *naïveté* that can be found in their writings. The frequency of such examples, however, in no way diminishes the importance of certain other, more sensible objectives, even if the results were not any more brilliant. The subject of measuring altitudes with the barometer merits consideration here. Of course, this was an instrument still in its trial phase, whose precision was dubious. Eliminating air bubbles or achieving tubes with a constant cross section were problems that had been only imperfectly resolved. The drawing of the divisions on the scale, even the location of zero, represented difficulties of a kind difficult to imagine today.[14]

We do not mean to overemphasize these obstacles of a technical nature. Mariotte's law established an algebraic relationship between variations in a column of mercury and increases in altitude. The barometer was a theoretical instrument whose practical testing was inadequate. That is, the existence of appreciable discrepancies between the values predicted by the theory and those actually observed could be demonstrated; and these were especially notable when the differences in altitude measured with the barometer were not very great. Contrary to Mariotte's assumptions, not all the strata of different density in which he supposed the atmosphere to be divided by analytical effects had the same *elastic virtue*. As a result, a geometric progression was not the best way to mathematically correlate variations in the mercury with local altitude: "Geometric progression," Bouguer wrote, "would be a certainty if all [the layers of air] were always comparable to springs of similar flexibility, if each mass of air transported upwards or downwards produced the same effect as the one that took its place."[15]

Rejecting the existing theoretical model, they were left to search for a stable algebraic expression which would permit them to find the altitude of a marker without having to carry out a specific program of observations at each triangulation station. The discrepancies between theory and practice were made even more serious inasmuch as they were unsystematic. Still, one must ask a simple question: rather than rejecting Mariotte's atmospheric model, why did they not question the reliability of their instruments of measurement? We do not know their reasons, although the answer must be due to the general attitude of lack of confidence in theoretical models and the concomitant disposition favorable to the dictates of empirical observation. Be that as it may, it is certain that their data show that, as Jorge Juan concluded, "...at distances near the surface of the earth the expansion [of mercury under pressure] is explained by another reason completely; and we suppose that layers or strata of equal weight into which the atmosphere is thought to be divided expand in arithmetic progression, and corresponding to each one of them there is an equal increase or decrease in the height of the mercury in the barometer."[16]

In practical terms, this is the equivalent of recognizing a great variation of results when attempting to measure small increments of altitude. Or, to put it another way, the inaccuracy with which the divisions on the instrument were drawn could be decisive if the height of the terrain to be measured was too low. But if they had taken this critical attitude toward the barometer to its ultimate conclusions, what sense would their observations have made and what viable alternative would they have had in order to resolve the problem of barometric surveying?

Geodesic surveying required connecting some point of the triangulation with the Pacific coast. This was not an impossible plan, although in practice it was complicated and not very precise. To design an accessory triangulation in order to reach sea level would have delayed the research too much and would have required considerable additional effort, inasmuch as the only route possible --along the road that Pedro V. Maldonado built between Quito and Esmeraldas-- traversed rain forests and unexplored regions. There remained one last resort as an alternative: attempting an observation from the Esmeralda River basin of one of the Andean peaks linked to the main triangulation. This was the procedure used by Bouguer after the barometer experiments proved to be less fruitful than they had expected. Still, even though this was a more rigorous method of surveying, the practical results were far from satisfactory; in fact, the persistent mists of Quito not only made the observations with the quadrant difficult but also insured that its accuracy would be doubtful. Even if it had been viable, the undertaking seemed to be totally useless, as well as risky and fraught with hardships. Bouguer and Godin openly disagreed over this point. Once the geodesic phase of the mission was concluded, Bouguer proposed getting on with the geometric survey. Godin, who, along with most of the expeditionaries, only wanted to complete the research and return to Europe, did not want to hear of it. But besides personal motives and the predictable discomfort of the tasks to be carried out, there were also good scientific reasons to reject the proposal: "Yes, sir, I hold it necessary for the accuracy of our work to find out the height of the stations above sea level; but I do not believe that the means which you propose are the most accurate; the reason being that I have not examined the other means which might be used. Do you believe, for example, that it will be accurate over a distance of one hundred toesas, approximately? That would yield two toesas of error in the magnitude of one degree."[17]

In fact the influence of an error of two hundred meters in the measuring of altitude was not very significant. Godin declared himself clearly in favor of a barometric survey which, even though it might have produced errors of similar magnitude, at least would make the work more comfortable. Let us return therefore to a consideration of the only method for measuring altitude that was practicable in 1740.

The rejection of Mariotte's theory meant that they had to empirically find a stable algebraic relation between local altitude and the height of the mercury in the barometer. The new program of observation consisted basically of establishing zero on the scale and determining the difference in altitude required for the mercury to descend one line on the scale. Since it was assumed that arithmetical progression was the proper mathematical method for describing the phenomenon, the problem was reduced to establishing the first term of the progression, as well as its ratio. The first experiment, carried out years before by Cassini, had ended by assigning to these parameters the values of sixty feet and one, respectively. When this ratio is expressed algebraically, our academicians tried to find an expression of the form $a = c_1.h + c_2.h^2$ (Bouguer would also introduce the third-order term $c_3.h^3$), where a was the altitude of the place, h the observed descent in the column of mercury expressed in lines, and c_1, c_2 and c_3 were constants to be determined empirically. The following table presents the solutions obtained by different authors:

Author	r points	Altitude	Altitude (toesas) *n* lines
Cassini	1728	103.680	$9.92n + 0.08n^2$
Feuillé	3456	103.680	$9.83n + 0.17n^2$
Godin	1502	128.798	$12.25n + 0.07n^2$
Bouguer (Paris)	1152	136.080	$13.05n + 0.75n^2$
Bouguer (Ecuador)	1202	169.920	$16.33n + 0.06n^2$
Juan	806	171.176	$16.47n + 0.04n^2$
Juan (average)	371	149.033	$14.35n + 0.02n^2$

The discrepancies among them, as we will demonstrate in the following table by comparing the altitude predicted for some of the places triangulated, are so significant that it is necessary to question the very foundations of the procedure.

Place	Altitude mercury	Altitude Godin	Altitude Juan	Altitude Bouguer
Caraburu	21.3.3	1,434	1,280	1,697
Oyambaro	20.7.9	1,614	1,413	1,819
Tanlagua	18.9.9	2,188	1,816	2,515
Pambamarca	17.3.4	2,732	2,168	3,080

Juan recognized this, once he had concluded his studies, because "...one can see the impossibility of assigning a progression that holds true for all altitudes...so we will always obtain some figures that will not agree exactly with the rule."[18]

If this were true after considering the algebraic expressions obtained by the expeditionaries one at a time, the analysis of all of them taken together was certainly discouraging. A variation of more than three hundred toesas (that is to say, about ten percent of its mean value) in the altitude of a point indicated poor results for a research project that initially seemed so promising. This no doubt justified not only Bouguer's interest in undertaking the geodesic survey, but also Godin's resistance to prolonging this phase of the mission any further. Scientific and personal reasons once again intertwined to definitively impede joint research. When Bouguer, ignoring the decision made by the head of the expedition, moved to the Isle of Inca in the Esmeraldas river in March 1741 to attempt an observation of the summit of Pichincha, Godin sent him the astronomical sector brought from Paris, informing him of his decision not to maintain contact with anyone who ignored his authority. Once again the lack of satisfaction produced by the results of their observations affected personal relations within the French expedition. After a long series of observations, none of the methods of analysis proposed was convincing. Again our academicians had to choose, from all the figures available to them, simple arithmetic values for altitude. Their determination to isolate a physical phenomenon and to refine a precise technique for the use of the barometer had failed. Fortunately the accuracy required for the final goal of measuring one degree of meridian had not been compromised; their initial program continued to be completely attainable, except that now they had a better understanding of the problem of how to reduce the margin of error so that they could wind up their operations on South American territory.

Endless Calculations

While the observations of the particular phenomena we have just described were being carried out, the bulk of their research between 1737 and 1739 were directed at triangulating the meridian. This involved measuring the distance between two very distant points on a meridian, using the technique devised by Frisius and later perfected in France by Picard, Cassini and other astronomers. In theory it was a simple matter. Once the fundamental base *AB* had been determined, points *1, 2, 3...* were selected until the proposed distance on the meridian was covered, and they began to observe the angular amplitudes with the extreme points of the opposite side that were seen from the three vertices of each imaginary triangle. Once the three angles and one side (*AB*) were known, one could obtain by trigonometric calculations the side *A1* which became a "base" for the next triangle *1A2* (see the figure on page xxx). Once they had covered the four hundred kilometers that approximately embraced the more than the three degrees of latitude triangulated, the accuracy of the observations just carried out was checked by determining a new *base of verification* according to two different methods: first, by induction, connecting it to the above triangulation, and secondly, by the same procedure with which the *fundamental base* had been found. If both results were compatible and the difference lay within an acceptable margin of error, the geodesic phase of the mission was concluded. Obviously, one had to reduce the distances to sea level and to find the inclination of the sides with respect to the meridian, operations that required knowing the absolute and relative altitudes of each marker, as well as its latitude.

Graphic of
Triangulation

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While the program was simple on paper, in practice it turned out to be not only difficult but extraordinarily arduous and risky. The various markers had to be located in highly visible places and, therefore, it was decided to place them on the summits of the two Andean ranges. This obliged the researchers to make onerous treks and difficult climbs which delayed the observations; to make matters worse, at times the presence of clouds impeded observation and it was necessary to stay on the mountain for days, suffering intense cold and violent storms. There is no doubt that these difficulties were considerable, and the natives of the country began to wonder about their objectives. Antonio de Ulloa's testimony could not be more eloquent: "Now it is only fair to consider the mixed opinion the people of this country formed about us. On the one hand they admired our resolution; on the other they were surprised by our persistence; and finally, even the most educated people were very confused. They asked the Indians how we lived in those places and they were amazed at the reports they received. They saw that everyone refused to help us, even though they were able-bodied and used to hard work. They saw how we calmly spent indefinite periods of time in these places, and the forbearance with which, after finishing one quarantine of tasks and loneliness, we went on to do another. And they did not know what to attribute all this enthusiasm to. Some thought we were crazy. Others thought we were inclined to greed and were convinced that we were looking for precious minerals by using special methods we had devised; others thought we were magicians, and all were steeped in endless confusion because none of the scenarios they imagined would make up for going through the hardship and difficulties of such a life. Even now this is still a source of doubt among many of these people, who cannot be persuaded of the true goal of our voyage, being unaware of its importance."[19]

In their respective accounts of the trip, all the expeditionaries included extensive references to the great difficulties they underwent and to the almost heroic character which the fulfillment of the mission entailed. That much was true and no one can deny it. There are so many anecdotes relating to incidents throughout the two years of the triangulation that we could go on at great length. At times the hardships were so unbearable that one of the groups into which the company was divided refused to use a given vertex in the triangulation. This is what happened on Pichincha with the group that included Godin and Juan. The consequence was that there were two different triangulations in the early phases of the operation: "...each of the two companies," Juan tells us, "had a different series of triangles, and we did not meet again until the terrain permitted it, which was in the ninth triangle."[20]

But topography was not the principal cause that motivated different triangulations. Serious personal conflicts acted even more decisively, to the point where each group decided to measure the base of verification in different places, far apart from one another. But since, in addition, Bouguer and La Condamine distrusted each other so thoroughly, to the extent of breaking off their scientific cooperation, they ended by measuring three different degrees of meridian: "We have three different geometrical measurements of the length of the meridian. M. Godin has measured a series of triangles which differs at both ends from the one M. Bouguer and I measured, each separately.... Jorge Juan assisted with Godin's measurements, and Antonio de Ulloa with those of Bouguer. I have always acted alone, except on a single occasion."[21]

This situation was very serious, in that it entailed the public recognition of the internal conflicts which had altered the original plans of the Academy of Paris. And the existence of three different values did not imply any greater guarantee of the quality of results with respect to the final goals of the expedition. On the contrary, it reinforced suspicions regarding the possibly irresponsible behavior of the expeditionaries. Inasmuch as their scientific activities, frequently called geometrical, were of an experimental nature and --as we have seen-- permitted different interpretations, the results were disquieting both to the observers themselves and to their critics in Europe, since they had not been suitably brought into agreement.

Thus it came about that instead of submitting an accurate result --as Paris had hoped-- they presented three more or less plausible approximations of the value of one degree in the proximity of the equator. All of the calculations, extraordinarily confused, were done in triplicate and that gave rise to the most severe criticism. This lack of agreement among the expeditionaries, however, permits us as historians to analyze their diverse observations and to form an opinion about the precision with which such findings could be attained during the first half of the eighteenth century. Let us begin with the angular measurements, comparing the three triangulations. Their analysis shows that the errors of closure for the different triangles (more or less than 180°) are considerably greater in Juan and Ulloa than in La Condamine; it is necessary, nonetheless, to be prudent in assessing the quality of the observers. Indeed, La Condamine asserts that he measured up to seven triangles with exact precision, which bearing in mind the instruments they had and the real conditions of observation is virtually impossible and, above all, suspicious. Should we suspect manipulation of results? Possibly the answer is an affirmative one. We know that the quadrants they carried could not insure angular measurements with errors of less than 10", while the mean error of closure in their data is 5.2"[22] But this would not be sufficient proof, inasmuch as there might have been a felicitous compensation of errors. The following table, which establishes a comparative summary of the three triangulations, permits us to make progress.

	Concluded	Exact	Error +	Error -	$\frac{1}{n}\sum[O-C]$	$\left(\frac{\sum[O-C]^2}{n(n-1)}\right)^{1/2}$
Condamine	0	7	12	14	5".2	1".16
Ulloa	1	2	17	13	14".9	3".69
Juan	1	1	17	14	12".5	3".28

Analysis of the Results and Error of Triangulation

In view of the results we believe it is highly improbable that La Condamine, having made observations on numerous occasions in the company of Bouguer and Ulloa with the same type of instrument, and without having any special reputation as an astronomer, could have achieved results that were three times more accurate than those achieved by his companions. While the observations discussed up till this point were difficult due to the physical duress under which they were made, what would come later, when the data were processed and reduced to the ideal situation of measurements at sea level, was certainly an overwhelming task. The consideration of all the possible causes of error was impossible, as the trigonometric and logarithmic calculations necessary to achieve such a task would have been interminable. La Condamine, with more ingenuousness than the rest of the group, complained to Bouguer, accusing him of not having warned and advised him on how to complete such an enormous task. In 1746 he wrote to his compatriot, "I have concluded that the whole calculation cannot be done rigorously, but only approximately.... Look at the angles between the tangents and the chords. Your explanation on these points is enigmatic and undoubtedly deliberately so; surely it is no use to make a person who becomes feverish just at the idea of numbers have to calculate and recalculate endlessly, without ever being able to retrace his steps and discover the cause of the error in the calculation he is making without committing a new one--and to begin making verifications, which are sometimes a new source of error, and this makes him lose tenfold more time than if, being less punctilious, he had given up making incidental and accessory verifications. I see a method that is shorter and no doubt is the one you use by directly obtaining the angles formed by the chords of the small arcs which form the sides of the triangles, instead of reducing the angles observed between two markers to the corresponding horizontal arc."[23]

La Condamine was right; all the expeditionaries used simplifications that produced approximate results, without any great effect on the measurement of the degree. In fact, the basis of verification, as can be seen in the following table, confirms this.

	Base measured (toesas)	**Base induced (toesas)**	**Difference (toesas)**
Bouguer	5,259.8571	5,259.6497	0.2084
Godin	6,197.6111	6,196.5992	1.1039

After two long years their efforts seemed to be repaid by excellent results. As is evident from these pages, we have some doubts about the methods used, but this, far from being a more or less scandalous suspicion, is only one of the inherent characteristics of the empirical procedures of the times. For the academicians, on the other hand, it meant a discovery they were ashamed to admit, and they feared it might ruin their conclusions. This, in part, is the cause of the polemic kept alive by Bouguer and La Condamine through their writings in France. For the former everything had been done with a mathematical rigor that his opponent discredited in declarations about the difficulties under which the observations had been made, the errors detected in the instruments and the hypotheses that simplified the calculations.

In fact, there were many causes for conflict and frequent scientific controversies that led to personal quarrels. The multiplicity of diverse observations and the great number of confused and repetitive calculations served to obtain corrections that were not, however, applied by the expeditionaries. Each academician chose to surmount the difficulties by thinking up *ad hoc* simplifications which were nearly always baseless. Thus the quality of the final result must be judged on its own terms.

The long list of hardships they had to face, jointly with the uncertainty of the partial results obtained, led them to seek, above everything else, the end of the mission. This objective could only be satisfied by reaching a conclusion, in other words, by ranking the value and significance of each specific and partial problem before them. Besides the accuracy of the results, beyond the irritation or the scientific usefulness of the debates, we must point out the new countenance that began to emerge, especially for La Condamine, with regard to the need to adopt a hypothesis that would permit conclusions consistent with the reasonable expectations for an achievable level of accuracy.

Dance of the Stars

With the geodesic phase of the mission concluded, a new set of problems and observation techniques came into play. Now it was necessary to determine the position of the extremes of the triangulation, and with that the amplitude of the arc of the meridian. Practical astronomy became the primary activity for the members of the company. We cannot occupy ourselves in detail with the minutiae of the work carried out during this new stage. We begin, however, by stating that it took four years to measure the latitude, an excessive amount of time that, in spite of all the problems they had to confront, reflects poorly on the practical experience of the astronomers.

They reached the Viceroyalty of Peru carrying a twelve feet in radius sector in seriously damaged state. Bouguer wrote in a memoir that was unfortunately never published, "Now that I think about it, I have decided that the lens of the sector we used for the observations of the obliquity of the ecliptic deviated from the plane of the instrument by more than ten or twelve minutes.... We were therefore mistaken by nearly one minute in the distance from Orion to the zenith [...] Also, one could not close one's eyes and ignore the deviation of the limb with respect to the plane of the meridian which was approximately known from prior observations of the Sun."[24]

These shortcomings were enough in themselves to render any observation with the sector so inaccurate as to be useless. But these were not the only deficiencies. Juan informs us that in addition the framework was not sufficiently rigid, nor the micrometer stable enough.[25] In sum, the situation was desperate. The quadrants each expeditionary carried also appeared to be unreliable. In spite of the fact that systematic series of observations were made to measure the latitude, the variation in results was alarming. Let us look at the following table.

Observers	Place	Num. of Observ.	Latitud average	Desv. typique
Juan-Ulloa	Cartagena	18	10°26'0.6"	44,8"
Godin-Juan-Ulloa	Quito	16	13'36"	21"
Godin-Juan-Ulloa	Caracol	10	1°37'48"	38"
Godin-Juan-Ulloa	Guayaquil	27	2°11'15.3"	30.5"
Godin-Juan-Ulloa	Panamá	15	8°57'53.3"	32.5"
Juan-Ulloa	Lima	11	12°3'35.5"	10.5"
Godin-Bouguer-La Condamine-Juan-Ulloa	Portobelo	15	9°33'56"	40.3"
La Condamine-Juan-Ulloa	Cartagena	8	10°26'2"	43.34"

The average error found of 30" must be considered, both in absolute and relative terms, as excessively large, since if the measurement of the amplitude of the arc of meridian triangulated had been affected by this inaccuracy, the mission that took them to Quito would have been a thundering failure. But it was not only the instruments that were deficient. The observers were also inexperienced. In fact, we can say that not one of the French academicians, much less the two Spanish sailors, was accredited in Paris as a good astronomer. Godin, the only one who could begin to deserve such consideration, had spent years performing bureaucratic jobs such as editing the Connaissance des temps. We are convinced that the first years of their stay in America served as practical training for the observations they carried out in 1740 and 1741.

It was necessary to build a new instrument. If they were not very deft as observers, imagine the difficulties they must have faced in confronting a task that even the artisans themselves had not been able to execute satisfactorily. The technical problems that had to be resolved were extremely complex, and the new undertaking implied acceptance of a substantial delay in operations. But, was there any alternative? The idea of a hasty return to Paris was unthinkable.

Since they had decided to observe the stars at the zenith, so as to lessen the influence of astronomical parallax, it was possible to considerably lengthen the radius of the new sector and achieve greater precision with a shorter limb. The instrument of passages had undoubted advantages, but its construction, as it was larger and heavier, was more complex because of the difficulty of ensuring the rigidity and stability of the entire apparatus. This was a key problem, as is proved by the abundance of mechanical details included by all the expeditionaries in their descriptions of the instrument. Its solidity assured, the next major problem was correctly installing it: "...the errors that can be committed come from three different sources. In the first place, if the instrument is not well situated on the plane of the meridian; in the second, if it is not well fixed and situated vertically; and finally, if in its construction the lens has not been perfectly aligned with the plane of the limb."[26]

These were three important points, on whose solution the precision of all the measurements depended. In addition to these considerations, instruments were constructed with different materials whose coefficients of expansion were different, thus altering their reliability when observations were not made at the same temperature. All these were issues situated on the very frontier of the physics of the day and whose novelty impeded correct solutions. Here we will discuss only two of the many problems this caused.

Since Newton had proposed the principle of universal attraction, no observations had ever been made in the proximity of great mountainous masses. The subject was important for two reasons: first, because if one could detect the attraction of a mountain like Chimborazo on the pendulum, then one would have a supplementary test of the law of gravity. Second, and more significantly, if one could confirm the phenomenon, the verticality of the plumb line of all astronomical sectors would be affected. Thus it was necessary to quantify it in order to apply the corresponding correction to all the observations. The experiment designed to study this phenomenon did not yield a conclusive result, nor could it have. None of the available instruments had the requisite sensitivity. The conclusion reached by La Condamine was a speculation that was more or less true: "If Chimborazo was a volcano, which cannot be doubted, the greater part of this apparently enormous mass could be an immense cavity, a great nave, either hollow or filled with snow and, in this case, although having the same volume, the mass would be so diminished that not only is it not surprising that we only found seven or eight seconds of attraction in our observations, but even if it had been imperceptible, this still means we could not draw any conclusions against the reality of Newtonian attraction."[27]

That is, the experiment proved nothing, but the principle of attraction was a "reality." Thus did La Condamine take sides in the struggle between Newtonians and Cartesians which he had left in Paris in 1735, before obtaining any conclusive results.

The other question on which we wish to comment is the problem of how to divide the limb they used. The solution found was as original as it was exact. Let us look at the method devised by Bouguer and La Condamine. Inasmuch as they wanted to measure the altitude of the zenith of stars whose value was known approximately, it was not necessary to draw on the limb all the divisions that were less than the angular amplitude sought. The idea was to simplify as much as possible the operation of marking the divisions so as not to introduce the errors which we saw when we considered the quadrant. It was sufficient to mark on the copper bar which constituted the limb only the points on which it was foreseen that the plumb would fall. Those points would thereby form, with the vertex of the instrument, an angle twice the one they were originally seeking. Inasmuch as the vertical did not coincide exactly with those marks, a micrometer was used to complete the observation. To avoid errors derived from a deficient mounting of the center of the instrument, the measurement operation was carried out twice, turning the entire artifact 180°, and adopting the mean value of the two observations. How could one trace the two aforementioned marks on the limb precisely? Bouguer, with the radius of the instrument, drew an arc on the copper plate; using geometrical procedures he divided the radius into K equal parts and transcribed one of them on the said arc of the limb, obtaining an angle whose value was:

$$=\frac{360}{2}\times\frac{1}{K}$$

La Condamine, on the other hand, once the radius had been divided, transcribed one of the parts onto a segment of copper perpendicular to the axis of the instrument which, then, was the secant of the same arc drawn by his countryman. The end points marked on the limb defined an angle whose value was:

$$=\arcsin 1/K$$

Without doubt the accuracy of the observations carried out with a limb constructed by either of the two procedures was quite superior to those that had been obtained with the usual methods of engraving on astronomical instruments. In sum, with all the precautions and rectifications which they deemed pertinent, at the end of 1739 they had finished the transit survey and begun the first series of proof observations. The three carried out between November 12, 1739 and January 13, 1740, measuring the altitude at the zenith of *epsilon*-Orion, did not yield satisfactory results. After some improvements the new series still produced deficient results: "...the distance from the star to the zenith," wrote La Condamine, "is, after the whole deduction is done, still too great by 27 or 28 seconds using the first result, and by 18 seconds using the second result, so that the mean error is at least 22"½."[28]

Therefore, with so great a margin of error it was necessary to go over the entire process of the construction of the instrument again. At first, as was logical, they directed their inquiries towards the problem of the rigidity of the outfit; after reinforcing the mechanical structures they believed themselves finally to be in possession of an extraordinarily precise sector: "...it came out so finely tuned, precise, solid and so easy to use, that we noticed a strange movement in the stars.... We mentioned this discovery to Bouguer and La Condamine who both doubted it, wanting to attribute it to some defect of our instrument; they were satisfied by several observations which they repeated with telescopes fixed to the wall, whereby the motion of *epsilon*-Orion was clearly perceptible.[29]

Of course the stars were not fixed, as they have small movements which can be detected with very sensitive instruments. This discovery which, if confirmed, would have had extraordinary scientific and philosophical significance, merited a specific program of observations, especially because its definitive verification would imply the revision of almost all the current stellar tables and ephemerides.

The plan they developed consisted of making simultaneous observations from three points located along the meridian of Quito: "...it was the method that seemed most appropriate to use," La Condamine wrote to Maupertuis in 1741, "to enable us to determine with some certainty the magnitude of the degrees based on the hypothesis of the variation of the star confirmed up to now by observations; M. Godin estimates these variations at 40" or perhaps 50". It is clear that this daily variation prevents at each observation the determination of the distance to the zenith by the ordinary method, which is that of Picard's."[30]

Obviously, a variation such as this, estimated at between 40" and 50", not only required the modification of all known practical methods of astronomical observation, but also invalidated the conclusions relating to the shape of the earth that had been obtained using geodesic methods. It was therefore necessary to quantify it and find a stable law to describe stellar variation. If they found it, they would go down in history. At last success would be the reward for all their trials and tribulations.

But was not a variation of almost a minute exaggerated? How could they imagine that neither Picard, nor Bradley, Flamsteed, the Cassinis nor any other famous astronomers had detected it? The question is difficult to answer, but we can put forward a few suggestions. In the first place, it is evident that they believed themselves to be in possession of a perfect instrument, which had taken two years in its preparation and construction. But also, as the recent discoveries by Bradley on aberration and nutation and by Bouguer on refraction seemed to indicate, they were convinced that they were participating in a glorious moment for science and a time of profound transformations in astronomy. Their ambition for fame, together with their isolation from European scientific centers, took care of the rest.

All the observations carried out during 1741 revealed serious divergences among the various observers. New adjustments in the micrometer brought some of the results into somewhat closer agreement, but their divergence seemed irreducible. And finally, in spite of his great efforts and interest, La Condamine had to humbly recognize the true cause of so many errors: "I am tempted to attribute most of the errors to my own mistakes."[31]

The stars were fixed. The dream was over.

If they were to bring their work to a conclusion, they would have to begin making observations again. The ones already accomplished presented a dispersion of results that justified La Condamine's desperation. The following table of measurements of distance from *epsilon*-Orion to the zenith of Quito on different days is sufficiently eloquent testimony to this:

Date	Observer	Zenith Altitude
January,1737	Godin,Bouguer,La Condamine	1°10'0"
July,1737	Godin,Bouguer, La Condamine	1°10'5"
September,1740	Bouguer	1°10'15.7"
October,1740	Bouguer	1°10'16.4"
December,1740	La Condamine	1°10'20.3"
January,1741	Bouguer	1°10'17.1"

But if all the errors were individual ones, what guarantee did the expeditionaries have now that they would be able to measure the value of a degree? Furthermore, would the results obtained by each one of them be very different, bearing in mind the profound personal differences that divided them and brought them into conflict? Once again, there arose the suspicion that they would look ridiculous in France upon presenting very divergent measurements of a degree. In order to avoid this, they decided to exchange the value of one minute with one another, that is, one-sixtieth of the result heretofore measured. They did not communicate the exact value, since their respective jealousies prevented it, but the approximate value was enough to ensure some peace. The exchange took place on March 22, 1742: La Condamine and Bouguer had a minute of meridian of 945 toesas, and Godin had 946 toesas.[32] This was really cause for satisfaction, and it also meant there was no need to require any closer approximation in order to avert the failure of the mission.

They still made new series of observations which finally led to definitive results. A year later, in the early months of 1743, Bouguer declared that the expedition was over; the rest of his companions soon followed him. Now they faced a new adventure, the return voyage. The summary of their results was as follows:

	Godin	Juan	Ulloa	Bouguer	La Condamine
Length meridian	195,776.5	195,725.4	185,743.7	176,873.3	176,887.0
Arc value	3°26'46".4	3°26'53"	3°26'52".5	3°7'1"	3°7'1"
Degree Value	56,809.1	56,764	56,771.6	56,745.6	56.750
Adopted Value	56,810	56,767.8	56,767.8	56,753	56,750
Average Value			*56,769.78*		
Standard Dev.			*22.63*		
Average Error			*0.04%*		

Without going into an analysis of the partial difficulties that had arisen throughout these years, we can say that the numbers as presented here seemed carved in stone. The mean error was small and the agreement among the expeditionaries notable. But only a part of the problem was resolved in this manner. A comparison between the degrees measured at different latitudes ought to have to yielded a value for the magnitude of the polar flattening. But that was not going to be possible.

In 1743, coincident with the return to Europe of the first South American expeditionaries, Clairaut published his authoritative Théorie de la figure de la terre, in which he demonstrated the existence of a narrow margin of oscillation between the upper (1/230) and lower (1/573) limits of the polar flattening of the planet. Such a small band of error demanded standards of accuracy that were far beyond the capacity of the astronomy of the day. His conclusion, moreover, acknowledged the existence of sufficient proof of universal gravitation and, with regard to the shape of the earth, made it clear that it was necessary to wait for the results of the Quito expedition before adopting a definitive value for the flattening: "The preceding theory is therefore in accord with all the pendulum measurements and with the observation of the diameters of Jupiter; if, however, it happens that the measurements which we await from Peru, compared with those made in Lapland, show a difference in the axes of less than 1/230, this theory would have every possible confirmation, and universal gravitation, which fits so well with the movement of the planets, would also be in accord with their shapes.[33]

Thus the results from the South American expedition, the conflicts in which were already known in Paris, were not indispensable for proving Newton's thesis, although by confirming it they constituted supplementary proof.

The Agony of Debate

Up to now we have limited our discussion to the description of partial operations, along with the predictable difficulties and errors that arose at certain phases of the mission. We will now attempt to draw a more quantitative and global conclusion concerning the results. To this end we will compare the value of the degree obtained at different latitudes, just as the members of various expeditions did, looking for the relation between the axes of the planet. The degrees in question are as follows:[34]

Place	Latitud	Observer	Degree	Degree Boscovich	Diff.*	Diff.* calculada	Error
Ecuador	0°0'	Bouguer-La Condamine-	56,751.5	56,751	---	---	---
Ecuador	0°0'	Juan-Ulloa	56,767.8		---	---	---
Lapland	68°19'	Maupertuis	57,438	57,422	671	---	---
France	49°23'	Cassini-Maupertuis	57,183	57,074	323	461	138

As we can see, according to Boscovich, the polar and equatorial degrees were considered to have been correct, hence the calculated difference of 461 toesas between the degrees of France and the equator. There is also an error of 138 toesas with respect to the deviation obtained using the measurements corrected by Boscovich,[35] definitely an excessive error for the value obtained in France. With these values, the ratio between the axes, comparing these degrees in pairs, is:

Ecuador-Lapland	212/213
Ecuador-France	313/314
France-Lapland	127/128

The variation, as we can see, was significant, and thus the question of the shape of the earth could not be resolved definitively. An examination of the results obtained with the pendulum, an alternative method for measuring the magnitude of polar flattening, produced a similar situation of uncertainty. We will follow Boscovich again in this brief analysis:

Place	Year	Observer	Latitud	Length Pendule (lines)	Length Boscovich	Latitud	Diff.* calcul.
Quito	1736	Bouguer	0°25'	438.82	439.21	0°0'	---
Quito	1736	La Condamine		438.84			
Quito	1736	Juan		438.76			
Riojama	1737	Bouguer	0°9'	438.82			
Riojama	1737	La Condamine		438.93			
Portobello	1736	Godin	9°33'	439.08	439.30	9°34'	0.09
Portobello	1736	Bouguer		439.08			
Paris	1672	Richer	48°50'	440.60	440.67	48°50'	1.46
Paris	1682	Varin		440.56			
Paris	1735	Godin		440.53			
Paris	1735	Mairan		440.57			
Pello	1736	Maupertuis	66°48'	441.13	441.27	66°48'	2.06

Supposing the results obtained on the equator to be exact, a comparison between these longitudes of a pendulum that beats seconds at different latitudes yields the following results for the ratio between the axes of the terrestrial globe:

Ecuador-Pello	1/180
Ecuador-Portobelo	1/132
Ecuador-Paris	1/170

Thus neither the circumstances in which the works were carried out in the Viceroyalty of Peru, nor the comparison of their conclusions with those obtained on other expeditions, did anything to allay the feelings of failure which we have mentioned before.

The dispersion of values for the polar flattening of the earth, which was greater than the limits tolerated by fluid mechanics, gave rise to replies of different colours, based on distinct conceptions of the relationship between theory and experiment, or between physics and mathematics. La Condamine had good reason to assert that, in spite of the error which he surmised in his own observations and in those carried out by Maupertuis (which was in no case, he assured us, greater than forty toesas per degree) the solution to the problem of the shape of the earth eluded the capacity of the science of the day: "But what is the measurement of flattening and in what relation do the degrees of latitude increase as we approach the poles? This is what we still do not know and what perhaps it is not possible to know; at least not without having available a much greater number of degrees measured."[37]

However, it did not look as though a greater accumulation of findings would have permitted more accuracy or assured an unequivocal solution, given the theoretical resources of the times. The comparison that he himself carried out between the value of a degree with what was obtained by Cassini and La Caille between Paris and Amiens, Juan and Ulloa in Quito, J. Cassini in France, Maupertuis in Lapland, and the corrected value for Paris-Amiens, yielded an excessive variation in values for the polar flattening, ranging from 1/132 to 1/303.[38]

No matter what analysis was done, the results did not yield any definitive conclusions. Astronomy seemed overwhelmed by the high standard of accuracy required.

For Euler the problem was due to the inadequacy of the methods of observation themselves and the poor quality of the instruments. Based on the principles of fluid mechanics --the discipline which in his opinion ought to decide the debate-- it had been demonstrated that the difference between the axes of the globe was 1/230; from this the error committed on each of the different expeditions could be quantified. Only acceptance of the principle of the regularity of the shape of the earth justified such an approach; thus, considering its internal composition to be regular and homogeneous, the experimental deviations calculated by Euler would have been the following: the degree of Lapland erred by 27 toesas, the one measured by La Caille in Africa by 43, the degree of Peru by only 15, and the degree of France by 125.[39] A new polemic was provoked by the publication of this memoir, inasmuch as La Caille, who had been the one responsible for the last triangulation of the meridian of Paris, refused to admit so great an error in his observations. As in the polemic between Buffon and Clairaut over the modification of Newton's universal law of gravitation, interesting questions regarding the simplicity and homogeneity of the physical world were raised again. Once again the intriguing relationship between theory and experiment became a topic for discussion.

The attitude of P. Bouguer, who was perhaps the last Cartesian in the Academy, supported the inductivist alternative. The comparison of degrees, independently of whether they confirmed Newton's theories or not, revealed, in his mind, the greater complexity that ought to be attributed to the shape of the earth. Its meridians, rather than being ellipses, formed a curve which he called *barocentric* or *gravicentric*. Degrees on it would not vary as the square of the sine of the latitude, but rather "...they are approximately like the sine raised to the power of $3^{10/11}$: but, without doubt, in order to facilitate the calculations and to make the *gravicentric* curve geometrical, just like the curved line which the Meridian forms, this fractional power can be confused with the whole one, in which 4 is the exponent."[40]

We think the richness of nuance produced by these different interpretations of the results is very suggestive. There was a great deal of criticism of a thesis so unwilling to accept the predictive value of the theories and so committed to rigid empiricism. Bailly himself, several years later, questioned whether a hypothesis like Bouguer's could be called physics.[41] And finally, with greater modesty, Juan wanted to shift the question towards the evaluation of experimental errors: "This is why some people do not want the assumption to be exact, and they want the curve, through whose resolution the globe of the earth is produced, to be an ellipse; and they go looking for another curve which fits all the degrees measured. M. Bouguer is the one who has solved this problem as can be seen in the Memoirs of the Academy of Sciences for 1746, p. 443. But I am very far from believing that the disparities which are found in the excesses of the degrees proceed from the supposition that the curve be an ellipse. I conjecture that they originate only in the slight error which one unavoidably commits when measuring degrees."[42]

In fact, this was the point of view that would finally prevail, once the new geodesic operations were carried out at Cap by La Caille and between Rome and Rimini by Boscovich. The goal for which these expeditions had been designed since 1734 was unattainable; many technical advances would be required before astronomy could resolve the questions posed by celestial and fluid mechanics. Our scientists had to accept this.

But, in our opinion, herein lies the greatest achievement of this intense and prolonged effort to resolve a scientific dispute. Besides incorporating the practice of science into several countries, such as Spain and Sweden, and revitalizing activity in some of the Italian states, it had to articulate a whole series of specific problems relating to a less ambitious but more precise objective: the geometrization of the planet. Observe how Voltaire, who followed the debate attentively, summed up its achievements: "...the Peruvian mission, owing to the vast program of observations which it had the double merit of beginning and carrying out, has stood as a model for all scientific expeditions coming after it. At first glance, our scientists have not added anything more than a few numbers to celestial science, but the scope of their research was really much wider and the impulse which they gave to observational studies more lasting than has commonly been supposed."[43]

It is well known that on this South American expedition, in addition to the observations already discussed, many other studies relating to botany, geography, medicine, military and civil engineering, cartography, anthropology and colonial administration were also carried out. Outstanding among these were Jussieu's herborizations, the detailed descriptions of the cinchona tree by Jussieu and La Condamine, the European discovery of rubber, the map of the province of Quito, the description of the Amazon, the report on the state of the colonies carried out by Juan and Ulloa in the Noticias secretas de América, and so forth. While this is all true and we are in agreement with Voltaire regarding the model character this expedition had for later ones, we must confine ourselves here to the strictly geodesic aspects of the mission.

Aware of the degree of precision required by their work, our academicians arrived in Quito having decided not to exclude the influence that any discernible physical effect might have on the quality of their measurements. Convinced that such a project was attainable, imbued with enlightened optimism, they confronted a natural world which they thought would be transparent before their technical equipment. Nature, however, does not reveal herself so spontaneously. They soon found that their figures, as Voltaire said, could only achieve precise meaning when integrated into the framework of a specific goal. This meant that they were able to bring their work to a conclusion without, however, reaching a decisive answer on the issue of the shape of the earth. When the difficulties were prioritized and each one of the operations to be carried out articulated, a new way of looking at the earth, midway between geography and astronomy, resulted. A larger number of observations, considerably better carried out, yielded four different values for the degree of the meridian near the equator, and this cast considerable uncertainty on the definitive conclusions about the objective at hand. This uncertainty, which presented itself during the last months of their stay in America, obliged the expeditionaries to redirect the goal of their work and think of it as a kind of *geodesic experiment* which, although not good enough to corroborate the theoretical predictions derived from celestial mechanics, was sufficient to undertake the geometrization of the planet. At the end of their work they began to understand the naive fragility of their ideas about the presumed neutrality of observation: they proved the impossibility of presenting results that did not previously assume hypotheses about the shape of the Earth and its internal configuration. The very interpretation of the data obtained, extraordinarily copious in all geodesic research, required the consideration of error analysis in the context of the overall evaluation of the attempted objective.

New degrees were measured in search of greater empirical accuracy to reduce the uncertainty. However, the more they found out about the dimensions of the terrestrial meridians --a purely geodesic goal-- the farther they found themselves from the possibility of finding an exact value for the relation between the axes. Boscovich realized this after having compared the Italian degree with the measurements in France, Lapland, Quito and Cap. "In general nothing is certain about the shape of the Earth, if one only considers the measurements of degrees; but if one adds the longitudes of the isochronic pendulum, which we already have from fairly exact observations, we can suppose that it is *very likely* that the irregularities in the network of the parts are greater on the surface and near the surface than in the interior of the Earth."[44]

We will not dwell on the repercussions this conclusion must have had on the development of a new research program in the field of geophysics. The need to rethink future geodesic research from assumptions about the interior structure of the planet led La Condamine to write, "All this opens up a vast field for the most profound speculation, and provides a theme for a vast number of problems that even the great geometricians have not addressed. Too intimately linked to the physical world, they take flight in the sphere of possibilities: what is real and what is theoretical are equally subject to mathematical proof."[45]

Before this program was ready, before the speculations about which La Condamine was talking materialized, before the effort to comprehend reality from the theoretically comprehensible was extended to a new piece of reality, once again the inadequacies of pure induction, of the mere accumulation of empirical results, had been proven. Something had been gained in the comprehension of some of the most subtle mechanisms that govern science. La Condamine's discursive tone did not, however, conceal the reality that very soon, as we saw at the beginning of this chapter, he would denounce Boscovich and that he would not escape the sometimes cutting irony of Voltaire: "The journeys to the ends of the earth to confirm a truth that Newton had demonstrated in his laboratory have left doubts about the accuracy of measurements."[46]

From the experimental point of view the issue of the shape of the Earth had been exhausted. The rest of the polemic rested on stubbornness on the one hand and arguments about specific details on the other. The most brilliant result of the polemic's slow agony was the formation of geodesics as a new scientific discipline.

NOTES

1. The most experimental aspects of the polemic are discussed by J. B. J. Delambre, Grandeur et figure de la Terre (Paris, 1912), and in Histoire de l'Astronomie au dix-huitième siècle (Paris, 1827). See also: A. D. Butterfleil, History of the Determination of the Figure of the Earth from Arc Measurements (Worcester, Mass., 1906), and J. Loridan, Voyage des astronomes françaises à la recherche de la figure de la Terre et de ses dimensions (Lille, 1890).

2. P.L. M. Maupertuis, "Lettre sur la figure de la Terre. Lettre XIII," in Oeuvres, II, 257-266 (quotation on pp. 262-263).

3. R. J. Boscovich, Voyage astronomique et geographique dans l'Etat de l'Eglise... (Paris, 1770), pp. 491-492. The work here cited is a French translation of De litteraria expeditione per Pontifician ditionen ad dimentiendos duos meridiani gradus... (Rome, 1755). See Z. Marković, "R. J. Boscovic et la théorie de la terre," Conférence donné au Palais de la Découverte (September 5, 1960) (Paris, 1960).

4. We have discussed in detail all the technical aspects as well as the observations carried out by the American expeditionaries in A. Lafuente and A. J. Delgado, La geometrización de la Tierra (1735-1744) (Madrid, C.S.I.C., 1984).

5. For more information see M. Daumas, Les instruments scientifiques aux XVIIe et XVIIIe siècles (Paris, 1953).

6. The radii of the quadrants carried by the expeditionaries were as follows: Godin, 22 inches; Bouguer, 30; and La Condamine, who carried two, 12 and 36 inches, respectively.

7. See the complete table in Lafuente and Delgado, La geometrización, p. 60.

8. The Royal Decree is dated August 14, 1734; A.G.I., Indiferente General, 333.

9. P. Bouguer, "Relation du vouyage au Pérou par ordre du Roy pour determiner la figure de la terre," A.O.P., ms. C-2-7, passage 1, fol. 7.

10. P. Bouguer, "Memoire sur les avantages qu'il y a faire passer sur la coste comprise entre les caps de St. Francisco et de Ste. Helene la Meridienne que nous devons tracer," signed in Monte-Christi, March 22, 1736; A.O.P., ms. C-2-7, fol. 1^v.

11. J. Seniergues to B. and A. Jussieu, Panama, February 18, 1736; A.M.P., ms. 179.

12. "Extraits de quelques lettres et de quelques autres écrits deposés au Sécretariat de l'Académie royales des Sciences," A.O.P., ms. B-5-7. See also P. Bouguer, Justification des Memoires de l'Académie des Sciences de 1744 et du livre de la "Figure de la Terre" (Paris, 1752), pp. 11-12.

13. Ch. M^a La Condamine, "Extrait des operations Trigonometriques, et des observations Astronomiques, faites pour la mesure des degrés du Meridien aux environs de l'Equateur," Mémoires de l'Académie royale des Sciences (1746), p. 623.

14. See W. E. K. Middleton, The History of the Barometer (Baltimore, 1964); A. Wolf, A History of Science, Technology, and Philosophy in the Eighteenth Century, 2nd ed., (London, 1952), pp. 289ff.

15. P. Bouguer, "Sur les dilatations de l'air dans l'atmosphère," AOP, ms. C-2-7, p. 23.

16. Juan, Observaciones, p. 126.

17. Godin to Bouguer, Au pied du Signal de Sinazanan, May 2, 1739, AIF, Papiers J. M. de la

Gournnerie, ms. 2118.

18. Juan, Observaciones, p. 130.

19. Ulloa, Relación histórica, I, 137.

20. Juan, Observaciones, p. 217.

21. La Condamine, "Extrait des operations," p. 642.

22. See Lafuente and Delgado, La geometrización..., pp. 128ff.

23. La Condamaine to Bouguer, Deniecourt, October 17, 1746, AOP, ms. C-2-7, pp. 10-11.

24. P. Bouguer, "Remarques historiques et critiques sur les observations faites au Pérou de la distance de l'étoile d'Orion au zenith," AOP, ms. C-2-7, pp. 10-11.

25. Juan, Observaciones, pp. 4-5, 16.

26. P. Bouguer, "Sur la maniere d'observer la distance meridienne au zenith des astres fort élevés avec des instruments de grands rayons," AOP, ms. C-2-7, fol. 4^v.

27. La Condamaine, "Letter à M. Dufay sur les observations faites à Chimborazo, Montagne de la Province de Quito, pour reconnaître par Expérience l'éffet de l'attraction Newtonienne," at Riobamba, 36 leagues to the south of Quito, October 23, 1738; AOP, ms. C-2-7. See also P. Bouguer, La figure de la terre, determiné par les observations des Messieurs De La Condamine et Bouguer (Paris, 1749), pp. 364ff.

28. La Condamine, Mesure des trois premiers degrés du méridienne dans l'hémisphère austral (Paris, 1751), p. 163.

29. Juan, Observaciones, pp. 271-272.

30. La Condamine to Maupertuis, Quito, January 20, 1741. The letter was read at the January 13, 1742, session of the Academy of Sciences of Paris, Reg. 1742, p. 2.

31. La Condamine to Bouguer, Quito, August 3, 1741; BNP, Nouvelles acquisitions françaises, ms. 6197, fol. 17^r.

32. See Delambre, Grandeur et figure de la Terre, pp. 107ff.

33. A. C. Clairaut, Thèorie..., p. 305.

34. To shorten our analysis without abusing details, we have followed closely the treatment of the facts given by Boscovich, Voyage astronomique et geographique..., pp. 481ff.

35. The difference between the degree of France and that of Ecuador is in reality 330 toesas, very close to the 323 toesas given by the measurements. The most deficient measurements were those carried out in Lapland.

36. This summary of results can be found in Delambre, Historie de l'Astronomie, p. 362. As we did in our study of the degrees of the meridian, we have here simplified our analysis following that of Boscovich, Voyage..., pp. 493-494.

37. La Condamine, "Extrait des operations..." p. 637.

38. La Condamine, loc. cit.

39. On this point, see Delambre, Historie de l'Astronomie..., p. 362; and Boscovich, Voyage..., p. 484.

40. Bouguer, La figure de la Terre..., pp. 290-291.

41. P. Bailly wrote in his Histoire de l'Astronomie Moderne, 3 vols. (Paris, 1775-1782), III, 40: "One might ask M. Bouguer whether this hypothesis was physical, that is, whether it agreed with the known laws of Nature."

42. Juan, Observaciones..., p. 312.

43. The text is from a letter of Voltaire (Versailles, January 7, 1745) cited by Loridan, Voyage....

44. Boscovich, Voyage..., p. 493.

45. La Condamaine, Mesure des trois premiers degrés..., p. 263.

46. Voltaire, Siècle de Louis XV, chapt. 43.