



LXXXIV. On repeating circles

Baron de Zach

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Now, $\frac{800}{13,05} = 22,99$ miles, the apparent velocity of the waves, to which the rate of the ship, $6\frac{1}{2}$ knots per hour, being added, the sum 29,49 is the true velocity of the waves in sea miles per hour.

The velocity of the waves no doubt depends greatly on the force and continuance of the wind that puts them in motion. This may account for the great difference in the results of the few experiments that have been made for ascertaining the velocity of the waves of the sea. Capt. Horsburgh mentions that Dr. Wollaston, by some experiments, found the velocity of the waves near the coast of Yorkshire to be nearly 60 miles per hour; and that Captain Tate found the waves in the China Sea to have a velocity of only 16 miles. It would therefore be desirable that more experiments should be made on this subject.

Trusting that this will be allowed a place in your valuable Journal, I am, gentlemen,

Your most obedient servant,

56, Great Hermitage-street,
May 8, 1823.

DAVID THOMSON.

LXXXIV. On Repeating Circles. By the Baron de ZACH.

[Concluded from p. 363.]

AFTER all these experiments and reflections upon repeating circles, we thought we might make this further experiment, whether such a circle could show the effect which would be produced on the plumb-line and on the level by the attraction of mountains. This idea was more especially suggested by the favourable nature of the place for such an investigation. The city of Marseilles, which stands at the distance of 8000 toises from the sea, is surrounded on the north by a chain of calcareous mountains, which rise to the height of two or three thousand feet above the level of the sea. To the south-west, at the same distance from the town, is a small rocky island, level with the water, called *l'Ile de Planier*, upon which a lighthouse has been erected. This favourable situation suggested to us the idea of going to make the observation of latitude at the foot of these mountains, and in the midst of the sea; and afterwards to combine these two points of observation by a geodesical operation, in order to see whether we should find, in the differences of the astronomical and geodesical latitude, some anomaly similar to that found, at infinitely smaller distances and elevations, between Barcelona and Montjouy. This anomaly the astronomers and geometricians of France did

did not hesitate to attribute to the attraction of a little hill, the summit of which was 630 feet above the level of the sea.

This is the real object of the experiment which we undertook and executed in the summer of the year 1810, and which we gave to the public, in all its details, in a work published at Avignon in 1814, under the following title: "*L'Attraction des Montagnes et ses Effets sur les Fils-à-plomb, ou sur les niveaux des instrumens d'astronomie, constatés et déterminés par des observations astronomiques et géodésiques faites en 1810 à l'ermitage de Notre-Dame des Anges sur le mont de Mimet, et au fanal de l'île de Planier, près de Marseille, suivis de la description géométrique de la ville de Marseille, et de son territoire,*" 2 vols. 8vo.

What was our surprise on finding that, in a work which appeared in London in 1822, a celebrated English artist says in a paper *upon*, or rather *against*, repeating circles: "A celebrated astronomer, a few years ago, in the south of Europe, made observations for finding the attraction of a mountain with a small instrument of the construction R; and obtained a deflection of the level equal to two seconds; and, although his telescope could not have been more than 15 inches long, from this experiment brought out a density of the earth nearly coinciding with the Schehallien experiment, and with the more recent one which Cavendish obtained by direct attraction."

We had, at first, some difficulty in recognising ourselves in this account; it was not till after we had read it again and again, that we at length discovered two things; 1st, that this celebrated artist meant to point us out in his excellent paper; 2nd, that he had never read (at least with attention) the work of which he speaks, and in which he makes us do and say what we never did or said; in which we had, on the contrary, done and said the very reverse of what he attributes to us.

As an apology for this estimable artist, it has been represented to us that he does not understand French; and consequently that, as our work was written in that language, he could not have comprehended it. This is a slight misfortune which we will set right in a very few words. This great artist makes us determine the density of the earth. Now the truth is, that we never thought of doing it, that we never attempted to do it, nor did we ever express the least wish to make any such observation. We knew very well that, to effect that, we must have made the plan, the profile, the elevation, and all the dimensions of Mont Mimet, in order to calculate its mass, its capacity, and its distance from our point of observation, as Dr. Maskelyne did at Schehallien. Now this is what we never wished to undertake, and what we never did undertake.

take. The artist in question was therefore deceived, and laboured under a complete mistake, when he said *that we had brought out a density of the earth nearly coinciding with the Schehallien experiment and with the more recent one which Cavendish obtained by direct attraction.* Not a word of all this was to be found in our work. This celebrated artist informs us again in his very instructive and interesting paper, that “it is possible that.....a result might have been obtained of an equal quantity contrary to attraction.” But if he had properly read or understood our work, he would have found that not only we had said the same thing, but that we actually expected this contrary result.

We extract what we said on this subject (p. 358): “We will candidly confess that in undertaking this work we were not without apprehension, that instead of finding the effect of an attraction we should find that of a repulsion; that is to say, an absurdity which would have only served to prove the insufficiency of our mechanical and physical means for determining a quantity so minute. We were not ignorant that this had happened to M. Méchain at Barcelona and Montjouy in his search for a quantity almost double that which we were going to seek.” And further (p. 359): “We have formally declared that, so far from seeking the cause of these anomalies in local attractions, or in the irregularities of the density of strata of the earth, we were much more inclined to lay the blame on the instruments, and even on the observations. We have since seen that Don Rodriguez, in his ingenious examination of the three degrees of the meridian measured in England, held this opinion in common with us.” If we had believed in the reality of our result, and if that belief had been erroneous, we at least had, as companions in error, Bouguer, De la Condamine, Maskelyne, Méchain, and Delambre, who all held the same belief, and even for quantities much smaller than ours; for M. Delambre, in fact, speaks of $0''.65$ of the attraction of mountains found with 13 and 15 inch repeating circles of Lenoir. In the second volume of the *Base Métrique*, he says (p. 631): “At Dunkirk it appears that the inequality of attraction must be very small, since the distance from the tower to the sea is more than 1000 toises.” And here we must remark that his observations of latitude differ from those of Méchain by $0''.65$; upon which he asks, “Can this small difference be caused by the unequal densities of the earth?”

At least we did not mistake the direction of this result; we did not take repulsion for attraction, as befell a great astronomer and geometrician; a circumstance which we have re-

corded in our three letters published in 1812 in the *Bibliothèque Britannique de Genève*.

Bouguer, De la Condamine and Maskelyne wished to determine the density of our earth; and they undertook their experiments *ad hoc*. Méchain and Delambre wished to determine the magnitude and figure of the earth, and to fix the length of an invariable and universal standard of measure; they had made all their experiments to that end: our project had nothing in common with theirs; it was neither our intention to determine the density, nor the magnitude, nor the figure of the earth, nor the length of an universal measure. Our aim was merely to try whether a repeating circle of Reichenbach would exhibit the same anomalies given by a repeating circle of Lenoir of nearly the same dimensions.

The English artist highly disapproves, and even condemns us without mercy for having dared to make use of a repeating circle of such small dimension when we wanted to determine results of so delicate a nature. But is this esteemed artist ignorant, or has he forgotten, that the French astronomers had used instruments of the same kind, of the same dimensions, and certainly otherwise very inferior to ours, for operations far more delicate, far more important than ours, which was only an object of the private curiosity of an amateur?

The artist, in his paper, calls ours a *dwarfish experiment*, and says that it might be permitted to stand "on its own little base." Without doubt it is of small stature; but all the colossal experiments made for the last 30 years in France and Spain stand upon these same dwarfish bases; for all the world knows (if we except the author of the English paper, who appears not to know,) that all these colossal experiments were made with dwarfish repeating circles. He will answer that they are not the better for that. Be it so; but we will remind this great artist of the insult which a David dared to offer to a Goliath; when at Milan, with our pigmy of Reichenbach of half a foot radius, we stood the assault of an 8-foot colossus of Ramsden, we found a great discordance between these two instruments; we then made the same reflection, and almost in the same terms, as the English artist. We asked in our first letter published in 1812 in the *Bibliothèque Britannique*, which we have since more loudly repeated, "Where lies the error? In the colossal or in the dwarfish instrument?" Our readers know on which side victory has been obtained.

The English artist, while he blames us for using a repeating circle of such small dimension to determine the density of the earth,

earth, which we never had the slightest intention of doing, would have seen, if he had read or understood our work, which it appears he has not done, that such an instrument was precisely and absolutely the one which it was necessary to employ, since our principal object was to try whether such an instrument would produce again the same phænomena which a similar instrument had exhibited at Barcelona. We repeat, our project, and our intention in the operation which we undertook and executed in the environs of Marseilles in 1810, had nothing to do with the determination of the density, the magnitude, or the figure of the earth, nor of an invariable and universal measure deduced from that magnitude and figure. These were the objects accomplished in France and Spain with the instruments in question, which were also thought quite sufficient for determining the deviation of plumb-lines or of levels caused by the attraction of mountains. M. Méchain asserts this formally in the second volume of the *Base Métrique* (p. 491): "We have taken advantage (says he) of our abode at Perpignan, to make observations of latitude, in the hope that they may serve to ascertain whether the attraction of the Pyrenees alters the meridional altitude of the stars at Perpignan by causing a deviation of the plumb-line, or of the level of these instruments towards the south, as has been conjectured." We have seen above, that M. Delambre suspected that an effect of $0''\cdot65$ might be exhibited by these instruments. He attributed the difference of three seconds found between Barcelona and Montjouy to this same effect of attraction; and we, on the contrary, in our work *L'Attraction des Montagnes* (p. 359), said: "We have formerly declared, that, so far from seeking the cause of these anomalies in local attractions, or in the irregularities of the density of the strata of the earth, we were much more inclined to lay the blame on the instruments, and even on the observations."

What then become of the censures and criticisms of this celebrated artist? Is it possible that he has dissertated *de lanâ caprinâ*? But indeed, let us see whether it was so absurd as this famous artist (whose opinion in these matters is certainly of great weight) would make us believe, to have employed a small repeating circle in finding the effect of the attraction of mountains.

Setting aside that the greatest astronomers and geometers of France had done the same thing as ourselves, and had even gone beyond us, since they executed a work of the highest and most extensive importance, and one which cost the state millions; instead of which our modest dwarfish operation was no expense to any Government whatever, and was

only undertaken for the gratification of the curiosity of an amateur who pursued it for his own amusement; that it did not pretend to decide on any of those elements, proposed with so much pomp and éclat to all the nations of the earth, which none of them would receive, and which France herself has rejected;—setting aside, I say, all these considerations, we will confine ourselves to the task of showing, that our attempt was not so utterly irrational as it is said to be, from the circumstance of our having dared to undertake our experiment with a repeating instrument so small as to be regarded with an eye of contempt. Let us see whether it merits this disdain, and whether the modest David cannot take his sling again and hurl a stone at this haughty Goliath.

We will ask then, in the first place, how it happened that this despised pygmy of half a foot in height should have given a better latitude than the eight-foot colossus? How it happened that the irregularities in the little repeating circle of Reichenbach were much less than in the great mural of Ramsden? Three years afterwards we presented to the Observatory at Milan a large three-foot repeating circle of Reichenbach with a fixed level, which had been constructed on purpose for us*; and one of the most able observers of that Observatory found, after thousands of observations, the same latitude which we found with our pygmy. (*Effem. Astron. di Milano per 1815. Append. p. 3. Cor. Ast. vol. v. p. 300.*)

2°. We wish to know how it has happened, that with our pygmy we should have been able to discover that the latitude of the Observatory of Padua was so ill-determined with another colossus, a fine eight-foot mural of Ramsden, and in which the error was not less than $22''$! Several years after, exactly the same latitude which we had established with our pygmy was found with other English and German instruments, both repeating and non-repeating. (*Cor. Ast. vol. i. p. 457; vol. ii. p. 8; vol. v. p. 297.*)

3°. We will ask how it has happened, that with this same dwarf, we were enabled to discover that the latitude of the Observatory of Bologna was in an error of $18''$? We determined with this little circle a new latitude, which was afterwards confirmed by observations made with other repeating circles. (*Cor. Ast. vol. ii. p. 8—471.*)

4°. We will ask how it happened, that at the Observatory of Turin, with this same little circle we were able to determine a latitude which had never been determined before; and that the able astronomer of that Observatory afterwards found the same latitude with a 15-inch repeating circle of Fortin,

* *Effemerid. Astronom. di Milano per 1812. Append. p. 3.*

and with a three-foot meridian circle of Reichenbach? (*Cor. Ast.* vol. ii. p. 52; and vol. v. p. 499.)

We might accumulate these questions and show that this modest little 12-inch circle, so convenient and so transportable, has rendered similar services at Verona, at Venice, at Genoa, at Rimini, at Florence, at Pisa, at Lucca, at Naples, &c. &c.

Now we will ask, in the last place, with what other instrument has so much ever been effected, and in so short a time? Could it have been accomplished with sectors of twelve and fifteen feet? With quadrants of eight feet? With meridian circles of eight feet? We have, we trust, said enough to demonstrate, that our pygmy may enter the field with these magnificent colossuses, and to justify ourselves for having admitted it to the honour of the experiment in which we used it at Marseilles.

We have shown in the course of the present letter, that the greatest differences between the observations made with our 12-inch repeating circle never exceeded three or four seconds. Let us see whether the great instruments have succeeded better, and let us first cast an eye over the observations of Gen. Mudge, made with one of Ramsden's most perfect 12-foot sectors. We shall find that in spite of the beauty and the excellence of this magnificent sector, constructed by one of the greatest artists of England, in spite of all the precautions and all the skill of so adroit and experienced an observer as General Mudge, he could not avoid anomalies to the extent of four seconds. Don Rodriguez, in his paper printed in the *Philosophical Transactions*, goes still further, and suspects an absolute error of five seconds in the latitude of Arbury, "in spite of the goodness of the instrument and of the skill and care of the observer." In another passage he says: "It must, however, be acknowledged that no reproach attaches to the greater number of observers; they have done all in their power; but in general too much confidence has been placed in the goodness of their instruments."

Let us see whether the ten-foot sector of Sisson, with which Dr. Maskelyne determined the attraction of the Schehallien, would have done us better service, if we had made use of it, instead of our own little circle, to determine the attraction of Mont Mimet. The Doctor made with this instrument 337 observations, but of these he calculated only 40; we calculated them all, and we found that the greatest differences they contained extended to eight seconds.

In our work *L'Attraction des Montagnes*, we gave a table of all those observations which had been left for nearly forty years

years without being submitted to calculation; and we there said (p. 691), "that if the observations had been confined to one star, as was the case at the measurement of the three degrees at Peru, it would have been quite as possible to have an amplitude of an arc of the meridian of $50^{\circ},04$ as one of $58^{\circ},70$; and that consequently the effect of the attraction of the Schehallien mountain might have been either $3'',5$, or $7'',8$, a difference which would have completely absorbed all the effect of the attraction which we found at Mont Mimet."

It is clear from hence, that if instead of Reichenbach's small circle we had used at Mont Mimet Sisson's great sector, which Dr. Maskelyne used at the Schehallien mountain, the censure of the English artist would have been quite as applicable and as just. At the same time he will perceive that all the reflections he has made on this subject, we had made before him; and that we were not ignorant, as he seems to believe, that it was very probable we should obtain a result in a direction contrary to that of the attraction.

Again: let us see whether a three-foot repeating circle of Reichenbach would have done its duty better. Open the *Ephemeride Astronomiche di Milano* for the year 1815, *Appendice*, p. 16 & seq., and you will see, in the observations made with that instrument, anomalies which extend to five seconds. The observations of the pole-star combined from month to month also exhibit differences of two and three seconds. If, then, we had carried this instrument to Mont Mimet, and had observed there as we did at *Notre Dame des Anges*, and at *l'Ile de Planier*, three stars, during twelve days of the months of July and August, we might have obtained for the first twelve days of these two months the following comparative table:

	First 12 days.	Greatest Diff.	Eph. of Milan for 1815.
Polar-star	of July	$2'',34$	p. 19
upper culm.	of August	$2,75$	p. 20
Pole-star	of July	$3,82$	p. 24
lower culm.	of August	$2,61$	p. 25
δ Cassiopeiae	of July	$3,28$	p. 33
upper culm.	of August	$3,91$	p. 34

By comparing this table with that which we have given above (see Phil. Mag. page 363) of our little circle, it will be seen that the anomalies of these two circles are precisely the same.

It was not therefore quite so absurd as has been represented to use a 12-inch repeating circle for a mere experiment of curiosity of no importance, whilst the greatest astronomers and geometricians of France had, before us, used instruments of the

the same kind and of the same dimensions, and certainly of a very inferior quality, in operations which involved the most serious and important consequences.

Thus we are neither the first nor the only persons who have thought it possible to effect with a small circle what ought only to have been undertaken with large instruments. Two of the greatest astronomers and geometricians of Germany, not only thought but did the same thing, and even more. The celebrated Professor Gauss of Göttingen was of opinion not only that whatever could be done with a large repeating circle might be done as well with a small one, but was even inclined to prefer the results obtained by means of the latter. See the 1st vol. of my *Correspondance Astronomique*, in which will be found the following passage (p. 457):

“The Baron de Lindenau wrote to me some time ago, that M. Gauss inclined rather towards the results obtained by small circles, than towards those obtained by large ones. It would be not only curious but very instructive to know the reasons which have determined this great geometrician, who is at the same time an astronomer not of the closet, but of the starry heavens, in forming so important a judgement.”

Professor Gauss did not confine himself merely to thinking and saying; he also acted. In the xxviiith vol. of our *Correspondance Astronomique Allemande* (p. 481) will be found the observations of the latitude made by this able astronomer at the Observatory of Göttingen in the year 1813 with a Reichenbach's 12-inch repeating circle similar to ours. It will there be seen that the greatest differences, as was the case in our experiment, and in those made with all the large circles, did not exceed 2",55. The mean of four latitudes obtained by two stars observed above and below the pole, gave a difference of only 1",80, and this latitude differed only by 1",6 from that which Tobias Mayer had determined with his large and beautiful Bird's 6-foot mural*. But what will this great English artist say when he sees that M. Bessel† at Königsberg, with one of Cary's 18-inch pygmies, also ventured to attack the 8-foot meridian colossus at Greenwich, and affirms that he detected it in an error of 5 and 6 and even 7 seconds?

We shall not here enter into a discussion as to the point on which side lies the fault of these great differences. It is sufficient for the object we have in view, to show that such anomalies have been found, and announced as real, by one of our first astronomical observers, whatever be the instrument with which they were obtained.

* *Attraction des Montagnes*, p. 449. *Corres. Ast.* vol. ii. p. 64.

† *Cor. Ast.* vol. vii. cahier iii. p. 274.

M. Bessel finds that he differs not only from the great meridian circle at Greenwich, but also from those by Ramsden at Dublin, and at Palermo, and from a Reichenbach's 3-foot repeating circle at Milan. M. Bessel says distinctly (p. 274), "that these differences did not surprise him, since his former observations made with Cary's circle had already suggested them to him."

What are we to conclude from all this? That we as yet possess no instrument *either great or small* with which we can arrive at certainty within 2 or 3 seconds. Whatever can be said most agreeable to reason and truth on this subject has already been said by M. Gauss in a letter published in the first volume of the Memoirs of the Astronomical Society of London, p. 132.

"A point," says he, "which has occupied the attention of astronomers for some years, though it involves only a few seconds, is yet of the highest importance, both in reference to the art of astronomical observation, and on account of the numerous astronomical elements, whose exact determination depends on it; I mean the minute differences in the declinations of stars, the obliquity of the ecliptic, and the altitude of the pole, which appear in their determination by different though very excellent instruments. There is no doubt these differences arise from the action of gravity on the different parts of each instrument, though hitherto the mode of this action has not been clearly pointed out, nor is it possible to pronounce decidedly which instrument has afforded the right and which the wrong result. We know, in fact, very little of the extent to which the yielding of the metals may go; and it seems too hazardous to deny the possibility of this cause exercising a notable influence on the divisions, and in consequence on the observations in any instrument, whatever be its construction, without grounding such denial on sufficient proof. In our meridian circle, the great artist has done every thing to obviate the flexure of the telescope by a well-adapted system of counterpoises: still a doubt may remain, whether all the flexure be done away with by that means, or rendered quite insensible; and the only direct means of ascertaining the point seems to be, the combination of immediate observations of a heavenly body, with those of its image reflected in an artificial horizon."

Thus, then, although we cannot decide which instrument gives the true and which the false result in a question of two or three seconds, whether the 18-inch circle, the 3-foot meridian circle, or the 8-foot mural circle, like that at the Royal Observatory at Greenwich; and since this great 8-foot circle
does

does not act like a meridian telescope, as the 3-foot meridian circles of Reichenbach do, with the utmost perfection; shall we therefore assert, that we must sing a *requiem* over the construction of this circle at Greenwich, and *leave it to stand on its own GREAT base*? We are very far from thinking so; we know better how to do justice to the great artist who constructed this *chef-d'œuvre*, although he deceived himself in thinking that it would act as a transit instrument. That this is not one is amply proved by the numerous observations of the astronomer royal. This detracts nothing either from the merit of the instrument, which for taking polar distances is perfect; nor from the merit of the author of so extraordinary a work, who is and who will always be one of the greatest artists of his age. We have often heard the late Mr. Ramsden say, that he would construct a sector with which he could determine the length of his workshop; that he would make barometers with which he would measure the height of his desk, &c.: all these were only *façons de parler*, to express, that he would do his utmost to give to his instruments every possible degree of perfection; and the hopes indulged by the artist who constructed the mural circle at Greenwich, that this instrument might also serve as a transit, only prove that this great man pursued his work with so much care and exactness, that he thought his instrument would add, to all its other perfections, that of serving as a transit instrument. Experience has, indeed, since proved the contrary.

It is now the fashion to decry repeating circles. These instruments have certainly their defects, like all others, and we were the first to discover and to point them out. Nevertheless we ought to be just, and to give to every thing its deserts. We have already asked above, with what other portable instrument could we have accomplished what we did in our travels, with a 12-inch repeating circle and an 8-inch repeating theodolite? Mr. G. Dollond is much more just in this matter, and considers this sort of instrument with more candour and a more accurate perception of its real utility. This great optician, in his description of a new 15-inch repeating circle of his invention, inserted in the first volume of the Memoirs of the Astronomical Society of London, says (p. 57):

“The repeating principle upon which the instrument is founded, is too well understood to require any explanation; I shall therefore only remark, that I consider it to be of very great advantage to *portable* instruments; particularly as they cannot be prepared (on account of their price and dimensions)

with such accurate divisions*, or such powerful telescopes, as those larger instruments possess, that are furnished for fixed observatories."

This is precisely what we said in the first number of the sixth volume, page 63, of our *Correspondance Astronomique*: "We believe, nevertheless, that repetitions may be of infinite use in the small circles used in travelling, the divisions of which cannot go higher than ten seconds."

We believe, above all, that the repeating theodolites of M. Reichenbach's construction are superior to every other instrument, even to repeating circles, for observations of the azimuths and of terrestrial angles, because such observations are always made on a horizontal plane; and with these small and very portable instruments, though their divisions be only to every ten seconds, we can succeed in obtaining the exact second, as our readers may convince themselves by casting an eye over our observations of the azimuths and over the terrestrial angles given in our work, *L'Attraction des Montagnes*. It is also well known that the repetition of horizontal angles does not present those anomalies which are observable in the repetition of vertical angles. Of this we have suggested the probable reason in the Number just quoted (p. 64).

A celebrated London artist has said that he had heard that some of his contemporaries were endeavouring to bring to perfection repeating instruments, but that he advised them to do nothing of the sort, but rather to employ their time and talents in the construction of instruments which promise more success, and to endeavour to bring to perfection the art of dividing. But it appears to us, that this artist is mistaken if he imagines that the only use of repetition is to correct the defects of division; it is also of use in correcting the smallness of the instrument, defects of the level, defects of the telescope, defects of observation, &c. It appears that his celebrated contemporary Dollond has a more accurate perception and a sounder judgement concerning the principle and the nature of repetition; and we hope that the excellent artists with which London abounds will continue to improve this kind of instrument as well as every other. It is no less desirable to serve the humble retreats of modest amateurs, than the sumptuous observatories established by imperial and royal munificence. Every body cannot procure 8-foot circles and 15-foot

* The divisions of this 15-inch repeating circle are for every 10". Lenoir's of the same distance for every 20". Reichenbach's 12-inch for every 4".

sectors,

sectors, and drag them over mountains and valleys. It is only in England that a school-master can buy an instrument which is too dear for a sovereign*.

As we before said, *every thing in its place*. When M. Rüppell prepared to leave us for his travels in Africa, we did not advise him to carry a repeating instrument, either great or small, for the same reason that we advised him not to carry a chronometer of the best quality, or of great value, for the purpose of observing longitudes. We explained our reasons for this advice in the first volume (p. 514) of *Correspondance* above mentioned. If M. Rüppell had wished to make his observations with a repeating circle, however small, he would rarely have found in the whole course of his travels a convenient, solid and secure spot, sheltered from curiosity, from suspicion, or even from danger. He would have wanted an assistant, indeed he would have been wholly dependent upon one, to make his observations and to adjust the level of the circle. With a sextant of reflexion there are none of these difficulties. With such an instrument, with an artificial horizon, which places and keeps itself level, and with a chronometer in his pocket, he may make an observation alone in concealment, in any spot, and in the smallest opening through which the sun shines. These are instruments which he may put in his pocket, conceal, and carry in his portmanteau. In the countries traversed by M. Rüppell a question of seconds is of no importance. There, where the latitudes and longitudes are quite unknown or incorrect by many degrees, a minute is as much or even more than a second with us. A sextant of reflexion does not so easily get out of order as a repeating circle; it is more easy to handle; one may rectify it every moment, find the error of collimation at every observation, use it upon sea and upon land, for the heavens and for the earth, determine the seasons, latitudes, longitudes, azimuths, and terrestrial angles, with a precision beyond what is actually necessary or what can be reasonably desired. The sextant of reflexion was therefore in this instance preferable to repeating circles, and even to all non-repeating instruments, *because it was here in its place*.

People had gone the length of saying that repeating instruments had arrested the progress of astronomy, and had prevented a great number of useful observations which would otherwise have been made, since those made with instruments of this description demanded a great expense of time†, which might

* This anecdote is well known in London.

† It would be possible to dispute this expense of time, and to show that it

might have been better employed with non-repeating instruments. We do not dispute that this charge may be well founded with regard to the great observatories which are furnished with those large non-repeating instruments; but we think the general accusation as unjust as it is ill-founded. We may reckon in Europe twelve mural quadrants constructed by the first artists of England, by Bird, Sisson, Ramsden; without counting the two in the Royal Observatory at Greenwich, that in the Observatory of Göttingen, and that in the Observatory at the *Ecole Militaire* at Paris, which Bradley, Maskelyne, Mayer, and La Lande, have used with so much assiduity;—what has been done with the others? Where are the observations and the catalogues of stars which they have produced? Repeating circles were not invented at the time they were constructed; at any rate they were not in use, and consequently could not prevent the use of other instruments, nor retard the observations which might have been made with them.

We are acquainted with six of Reichenbach's three-foot repeating circles distributed among five of the great observatories of Europe, without reckoning that at Milan which M. Oriani has employed in his labours;—what has been done with the others? Nothing at all. It follows, then, that these instruments have not occasioned loss of time, and have not prevented, stopped or retarded the observations which might have been made with the twelve mural quadrants, and with the seven sectors; all constructed by the greatest artists of London. If we are asked the true and precise reason of this dearth of observations which is so much complained of, we shall give it in few words. It is, that in every observatory where there are good repeating or non-repeating instruments, there is not at the same time a Bradley, a Maskelyne, a Pond, a Brinkley, a Mayer, a La Lande, a Piazzi, an Oriani, a Bessel, a Struve, or a Littrow.

It is not, in fact, so great as it has been represented. M. Oriani employs only three or four minutes of time in making the observation of a star by four repetitions. For the zodiacal stars he makes only two repetitions, and expends only a minute, and often only 30 or 40 seconds. He has generally been contented with four repetitions (and that is sufficient); he has never exceeded eight. (See the *Appendices aux Ephém. Ast. de Milan* for the years 1812 and 1813.) We should like to know whether Piazzi, in making *one* observation, does not consume as much or even more time in turning his circle and in reading the revolutions and the parts of his four microscopical micrometers,