

Fermat to de la Chambre

Sunday, January 1, 1662

SIR,

1. It is right to obey you and, by your intervention, to finally put an end to the old quarrel which has so long existed between M. Descartes and me on the subject of refraction, and perhaps I will be fortunate enough to propose to you a peace which you would find to be advantageous to both parties.

I had formerly told you, in my first letter, that M. Descartes has never demonstrated his principle; for, aside from the fact that comparisons can scarcely serve as the basis for demonstrations, he employs his in the wrong way and even assumes that light's travel is easier through denser bodies than through rarer ones, which is obviously false. I will not repeat anything to you about the flaw of the demonstration itself, even if the comparison which he uses were good and admissible in this matter, because I have treated all this at good length in my letters to M. Descartes during his life, or in those which I wrote to M. Clerselier after his death.

2. I only add that having seen the same principle of M. Descartes in the works of several authors who have written after him, their demonstrations do not seem at all admissible either, and do not in any way deserve the name "demonstrations." Hérigone, in his demonstration,¹ makes use of equipoises and the ratio of weights on inclined plains. Father Maignan desires to suc-

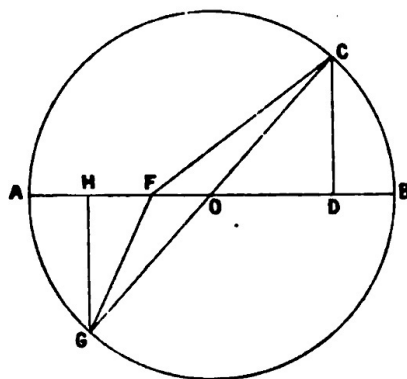
¹Cursus mathematicus tomus quintus, Paris, chez Simeon Piget, MDCXLIV, pp. 129-130. Axiom V: "The powers which rays of light have of penetrating various transparent media, increase or decrease proportionally to the change of media; and there is the same proportion between the powers of the rays of incidence and refraction as between the pressures they would receive from equal weights." (NB: Sometimes the "power" of an angle was taken to mean its sine. – JAR)

ceed in another manner.² But it is easy to see that neither one of them makes a demonstration, and that after having read and examined their demonstrations with care, we are just as uncertain of the truth of their principles as we would be after having read M. Descartes.

To escape this predicament and to try to find the true reason of refraction, I indicated to you in my letter that, if we wished to employ in this study this very common and quite established principle, that *nature always acts along the shortest paths*, we could easily get our explanation from it. But, first of all, because you have doubted that nature in directing light along the two legs of a triangle, could ever act by a path so short as that of the base, I set out with an easy example to cause you to see the opposite of your thought, or, rather, your doubt.

3. Let there be, in the separate figure (*fig.* 100), the circle ACBG, with diameter AOB, center O, and another diameter GOC. From the points G and C let there be drawn perpendiculars GH, CD onto the first diameter. Let us suppose that the first diameter AOB separates the two different media, of which the lower of the two, AGB, is denser, and the upper, ACB, rarer. Let it be rarer to such a degree that, for example, the passage through the rarer medium be easier than passage through the denser by a ratio of two to one.

Fig. 100.



From this assumption, it follows that the time expended by the body in motion, or by light, to go from C to O is less than that taken to go from O to G, and that the time of movement from C to O, made in the rarer medium, is

²Perspectiva horaria seu de horographia gnomonica tum theoretica tum practica libri quatuor. Rome, 1648; in-fol., pages 631-647.

only half the time of the movement from O to G. Consequently, the measure of the entire movement of the two lines CO and OG can be represented by the sum of half of CO and the entirety of OG. Similarly, if you take another point, such as F, the time of motion along the two lines CF and FG can be represented by the sum of half of CF with the entirety of FG.

Now, assume that the radius CO be 10, and consequently that the total diameter COG will be 20, that line HO be 8, and OD also be 8, and finally that line OF be only 1. I say that in this case the movement along line COG will be made in a longer time than the motion along the two sides of the triangle CF, FG.

For if we prove that half CO plus OG is greater than half CF plus FG, the conclusion will be clear, since these two sums are the precise measures of the times of these two movements. Now, the sum of half CO with OG is exactly 15, and it is obvious by the construction that the line CF is equal to the square root of 117 and that FG is equal to the square root of 85. But half of the first root added to the second, is less than $\frac{59}{4}$, and $\frac{59}{4}$ is less than 15. Thus, the sum of half CF with FG is less than the sum of half CO with OG, and therefore the movement along the lines CF, FG is made more quickly and in less time than the movement along the base COG.

4. I arrived at this point without much difficulty, but it was necessary to take the study further, and because, to satisfy my principle, it is not sufficient to have found a point such as F, through which the natural movement is made more quickly, easily, and in less time than along COG: but rather it is still necessary to find the point which allows the journey to be made in less time than any other point. For this, it was necessary to have recourse to my method *of maxima and minima*, which tackles this sort of question with fair success.

As soon as I attempted to take up this analysis, I had two obstacles to surmount: the first was, that, although I was sure of the truth of my principle, which is that there is nothing as probable or apparent as the assumption that nature always acts by the easiest means, which is to say either along the shortest lines when time is not a consideration, or in any case by the shortest time, in order to complete its work and to soon arrive to the goal of its operation (which the preceding calculation confirms, even more so since it appears that light has more difficulty in traversing dense media than rare ones, since you see that refraction aims towards the perpendicular in my example, just as experience confirms, which is yet contrary to the assumption

of M. Descartes). Nevertheless I was advised on all sides, and principally by M. Petit for whom I have infinite esteem, that the experiments accord precisely with the proportion of refraction that M. Descartes has given, and that, although his demonstration be faulty, it is feared that it would be useless for me to attempt to introduce a proportion different from his, and that the experiments which would be made after I had published my invention would be able to destroy it on the spot.

The second obstacle which stood in opposition to my study was the length and the difficulty of the calculations, which, in the solution of the problem that I spoke to you about in my letter and of which I gave you the indication that it was not the easiest, first involves four lines by their square roots and consequently involves also a number of asymmetries that produce very tedious results.

I freed myself from the first obstacle by my knowledge that there are an infinite number of proportions, different from the true one, which approach it so closely that they could fool the most able and exact observers.³ Therefore having only the second obstacle to vanquish, I had often resolved to employ the well-loved Geometry⁴ (as Plutarch calls it) to satisfy you and to satisfy myself as well. But the apprehension of finding after a laborious and painful operation, some irregular or fantastic proportion, and my natural inclination to laziness, left things in this state up until the most recent reprimand that M. le Président de Miremont delivered me on your behalf, which I took as a law stronger than either my apprehension or my laziness: and so I resolved myself to obey you without further delay.

5. I therefore proceeded without delay by virtue of obedience, as the monks say in the performance of their orders, and I put the entire analysis into form, in which my impassioned desire to satisfy you inspired me to a route which obviated half my work and which reduced the four asymmetries which I originally considered the first time into only two, which particularly comforted me.

But the reward of my work was more extraordinary, more unforeseen, and happier than ever. For, after having run through all the equations, multiplications, antitheses and other operations of my method, and having

³As Kepler says, in chapter 21 of the *Astronomia Nova*: “There are, however, occasions upon which a false hypothesis can simulate truth, within the limits of observational precision.”

⁴Plutarch, Marcellus, XIV, 5

at last solved the problem as you will see on a separate sheet,⁵ I found that my principle gave exactly and precisely the same proportion of refractions that M. Descartes had established.

I was so surprised by such an unexpected outcome, that I had difficulty coming out of my state of astonishment. I repeated my algebraic operations many times and always the success was the same, even though my demonstration assumes that the passage of light through dense bodies is more retarded than through rare ones, an assumption which I find quite true and indisputable, and yet M. Descartes assumed the contrary.

What must we conclude from all this? Would it not suffice, Sir, for the friends of M. Descartes, that I leave him in free possession of his theorem? Will he not have enough glory in having known the actions of nature at first glance, and without the aid of any demonstration? I thus cede him the victory and the battlefield, and I content myself that M. Clerselier at least allows me to enter into the society of the proof of this truth which is so important, and which must produce such admirable consequences.

6. I indeed add, in favor of his friend, that it seems that this great truth of nature did not dare to stand up against this great genius, and that it surrendered to discovery by him without being forced to do so by demonstration, in the example of those fortified places, which, although otherwise strong and not easily taken, would surrender, based solely on the reputation of the attackers, without even hearing the cannon.

I therefore break the news to you, Sir, I break it to M. Clerselier and to all of the friends of M. Descartes, that it will no longer rest with the skepticism of geometers that the world should wait any longer for the marvels that M. Descartes has led us to expect, with reason, from his elliptical and hyperbolic lenses, provided that we are able to find sufficiently skilled labor to produce them and to adjust them.

There still remains a slight difficulty which the comparison with M. Descartes seems to engender. It is that it has yet to be made clear why the bullet which is shot into water does not approach the perpendicular, as light does; but, beyond the suspicion one could hold that reflection is mingled into this example of refraction, and that the shape or the gravity can contribute to the difference of this movement, I was careful not to enter into

⁵See the *Analysis ad refractions*, printed as the penultimate section of the *Maxima and Minima* paper in the *Œuvres*.

a purely physical matter. It would be imposing on you, Sir, and I would be barging into a domain where you are the master.

I end this letter, therefore, after having declared to you that I consent, if you find it apropos, that the reconciliation between the Cartesians and myself be published in the Academies, and after having beseeched you to receive at least the effect of my prompt obedience as a certain and more-than-demonstrative proof of the passion with which I am, Sir, your very humble and very obedient servant,

FERMAT.

Toulouse, on the First of the Year 1662

P.S. If you continue to persist in not allowing light to have successive motion, and to maintain that it moves in an instant, you have only to compare the greater or lesser ease or resistance, in regards to changing media. For this ease or this resistance being more or less great in different media, and this in varying proportion with different media, they can be considered to be in a determinate ratio and consequently fall in the calculation just as easily as the time of movement, and my demonstration will still work in the same manner.

I have not elaborated my entire operation: and this was not necessary since my method is printed in its entirety in the sixth volume of the *Cours mathématique* (Mathematical Course) of Hérigone and I have already said enough to be understood. If you order me to travel through all the detours of a formal analysis, I will do so, and I would not even have too much difficulty in making the demonstration by construction, i.e., speaking in the language of Euclid.⁶

⁶See both the letter “Demonstration to M. de . . .” and the “Synthesis for Refraction” in Fermat’s writings on *Minima and Maxima*.