

Fermat to M. de . . .

1664

SIR,

Since M. de . . . speaks and since you order it, to you, Sir, whose reputation is so great and so well established, I will allow my Geometery to awaken, which was sleeping for a long time in a profound slumber, and, to start to get into the subject, I would very much like to tell you the saga of our Optics and our diffractions, in the form of a story, in order to leave you free to judge and that you may be able to pronounce yourself without prejudice.

After having read the Book of the now-deceased M. Descartes and having examined with some attention the proposition which serves as the foundation of his *Dioptrics* and which establishes the proportion of refractions, I doubted his proof; his demonstration seemed to be a veritable paralogism:

First of all, because he founds it upon a comparison, and since you know that Geometry does not pretend to deal with such figures, comparisons there being even more unacceptable than in the commerce of human intercourse.

Second, because he presupposes that the motion of light, which propagates through the air and through rare bodies, is more difficult, or if you prefer, slower than through water and other dense bodies; which seems to offend common sense;

And finally, because he supposes that one of the directions or determinations of motion of a ball is maintained in its entirety after the encounter with the second medium.

I even added a few more reasons, which would be superfluous or boring to go through with you. He read my writings, he responded to them, and, after some back-and-forth between us, we parted like the indicted and the witness, the one in the affirmative, the other in the negative, although I finally received letters from him full of civilities.

Since his [Descartes] death, M. de la Chambre, having published his own Treatise on Light, did me the honor of sending it to me. I took the opportunity of writing him the letter that you saw, in which I gave him evidence that, in order to be sure of being free from paralogisms in a matter so obscure, I saw no more certain means than to search for refractions by this unique principle, that nature always acts by the shortest paths, on the basis of which I indicated to him that one could find the point of refraction with geometry, by reducing it to the problem or theorem that you know. But, because I considered this invention very difficult and cumbersome, since these questions of *maxima and minima* ordinarily lead to long-winded operations which are easily scrambled by an infinite number of asymmetries which are found on its path, there I left my thought for several years, waiting for some geometer less lazy than I to make either the discovery or the demonstration.

Nobody wanted to take up this task. However, I received some letters from M. de la Chambre from time to time, in which he urged me to add geometry to my principle and to demonstrate it in a form revealing the true foundation of refraction. That which restrained me in advance was the assurance that M. Petit and others had given me, that their oft-repeated experiments to measure refractions in water, crystal, glass, and many other different liquids, were in precise agreement with the proportion of M. Descartes; such that it seemed useless to me to go looking for some other one with my principle, since nature had elucidated herself so clearly in his favor.

The objection that you make in your writing was not difficult for me and I had responded to it in my letter to M. de la Chambre for this reason, that anything that presses against or holds firm on any point of a curved line is considered to hold firm or press against the straight line which touches the curve at the said point; and thus, although the sum of the two lines of reflection may be the greatest in concave, spherical, or other mirrors, it is always the shortest of all those which could fall upon the line or the plane which touches the mirrors at the point of reflection, and this does not require a greater proof, since M. Descartes assumes it just as I do.

The whole difficulty was reduced, therefore, to the fact that it appeared that I had to combat not only men, but also nature. Nevertheless, the last requests of M. de la Chambre were so pressing that I resolved, about two or three years ago, to attempt to rescue my analysis, imagining to myself that there are an infinite number of different proportions between which the senses could not distinguish, and that I could then perhaps find one which would approach the one of M. Descartes and which nonetheless would not be

the same.¹

I did a formal analysis with a method of mine, which Hérigone had earlier printed in his *Mathematics Course*. I resolved all the asymmetries with difficulty, and then, all of a sudden, at the end of my calculations, everything unraveled and there appeared a very simple equation which gave me exactly the same proportion as M. Descartes.

At that moment I thought I had erred, for I could not believe that it was possible to arrive at the same conclusion by completely opposite routes, M. Descartes imagining, as one of his means of demonstration, that the motion of light meets more resistance in air than in water, and myself supposing the exact opposite, as you will see in the copy of my demonstration, which I have tried to redo from memory to completely meet your requirements, my original having been sent to M. de la Chambre, following my usual laziness.

Therefore, I have reworked this question many times, by changing the positions, and I have always arrived at the same conclusion, which confirmed two things to me: one, that the opinion of M. Descartes on the proportion of refractions is quite true; two, that his demonstration is quite false, and full of paralogsms!

The Cartesian gentlemen later saw my demonstration, which was communicated to them by M. de la Chambre; first, they stubbornly persisted in rejecting it, although I presented it ever so slowly to them. They should have been happy that the battlefield was left to M. Descartes, since his opinion was found to be truthful and confirmed, though for reasons different from his own. The most famous conquerors hardly considered themselves less happy when victory was won by auxilliary troops, rather than their own. The Cartesians did not want to be ridiculed in the first encounters: they wanted my demonstration to be faulty, since it could not hold, without destroying that of M. Descartes, which they always intended to put above dispute. But, since the most skilled geometers who saw mine seemed to give it their approval, the Cartesians finally paid me a compliment by way of a letter from M. Clerselier, who is the one who published the letters of M. Descartes. They proclaimed it a miracle that the same truth could be found at the end of two opposite

¹That is, there are so many possible proportions, that it is possible for the true proportion to be so close to that of Descartes, that the senses could not distinguish between them. As Kepler writes in chapter 21 of his *Astronomia Nova*: “The lack of any perceptible difference in effects between the as yet unknown true hypothesis and the false one. . . does not make the effect identical. For there can be a small discrepancy which the senses do not perceive.”

and entirely different pathways and made a pronouncement that they would be quite willing to leave the matter undecided and admitted that they did not know with whom to agree on this matter, M. Descartes or me, and that posterity would have to be the judge.²

It is to you, Sir, who are doubtless destined by your extraordinary merit to have great dealings with Posterity, to inform them, if you think it apropos, about this famous quarrel, or if you would rather “file” this writing among your useless papers, I consent, as it is all the same to me.

But it is not the same for the very humble prayer that I make to you, to consider me your, etc.

FERMAT.

²A parody of Clerselier’s letter, of May 6, 1662, in which he says that nature could not decide which path to take, that of least distance or that of least time, just as the Cartesians could not decide who is right, Descartes or Fermat. However, since nature unequivocally announces herself on the side of least time, Fermat has a clear idea of how posterity will judge this quarrel.