

# Fermat to Mersenne

September 1637

MY REVEREND FATHER,

1. You ask my opinion of the Treatise on Dioptrics of M. Descartes; it is true that the short time that M. de Beaugrand gave me to peruse it seems to excuse me of the obligation of satisfying you exactly and in complete detail. Besides, considering that the subject itself is very subtle and prickly, I do not dare to hope that unformed and undigested thoughts could give you any great satisfaction. But moreover, when I consider that the study of truth is always laudable, and that we often find ourselves groping about among the shadows for what we seek, I believed that you would not consider it bad were I to seek to disentangle my own imagination upon this subject, which, still obscured and encumbered, I will perhaps further clarify later, if my fundamentals are endorsed, or if I do not change my mind.

2. The knowledge of refractions has long been studied, but without success. Alhazen and Vitellion worked on it without having much success; and those who have come afterwards have often remarked that everything comes down to establishing a certain proportion, by means of which, from knowing one refraction, the others could be determined; such that all the foundations of Optics must consist in this point, that is, in the conformity and the relationship that one known refraction has to all others.

Assuming this, it was necessary that those who sought to establish the principles of Optics sought out this fitness and relationship.

Maurolic, the Abbot of Messine, in his posthumous *Treatise on Light and Shadow*<sup>1</sup>, maintained that what are called the angles of incidence are proportional to the so-called angles of refraction. Were this proposition true,

---

<sup>1</sup>*De lumine et umbra*

it would have sufficed to allow us to distinguish true figures which have transparent bodies – figures which produce so many marvels; but, since it was not well demonstrated by Maurolic, and since experience itself seems to convince us that it is false, it remained for M. Descartes to exert his mind, and to discover for us some new lights in these bodies which, being the only ones capable of doing so, have nonetheless produced until now some great darkness.

His Treatise on Dioptrics is divided into several sections, of which I think the most important to be the first two, which deal with light and refraction, since they contain the foundations of the Science, from which we later see him draw out beautiful conclusions and consequences.

3. This is nearly his argument: Light is nothing other than the inclination of luminous bodies to move; yet this inclination towards movement must probably follow the same laws as movement itself; and thus, we can establish the effects of light based on the knowledge we can derive from motion.

He then considers the movement of a ball in reflection and in refraction, and since it would be useless and boring to copy his entire discourse here, I will content myself to simply remark to you the observations that I made on it.

4. First of all, I doubt, with reason it seems, whether the inclination towards motion must follow the laws of motion proper, since there is as great a difference between one and the other, as between the ability to act and the action itself. Otherwise on this subject, it seems to me that there is a particular incongruity, as the motion of a ball is more or less violent to the same degree that it is pushed by different forces, whereas light penetrates transparent bodies instantly and appears not to be affected by succession.<sup>2</sup> But geometry is not at all suited to further explain matters of physics.

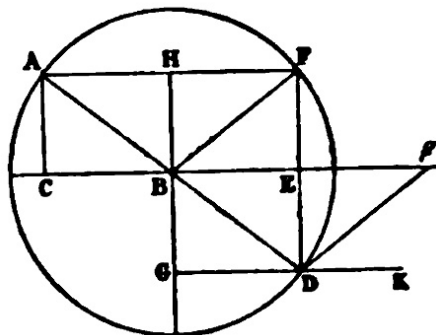
5. In the figure (*fig. 53*) by means of which he explains the ratio of reflection, on page 15 of his Dioptrics, he says that the determination for the motion to go in one direction can, just as is the case for motion or any other quantity for that matter, be divided into as many parts as it is composed of, and that one can easily imagine that the motion of the ball which is moving from A towards B is composed of two other motions, one of which descends

---

<sup>2</sup>Descartes held the motion of light to be instantaneous.

from line AF towards line CE and the other at the same time moves it from the left AC towards the right FE, such that these two motions combined, lead the ball towards B, along the straight line AB.

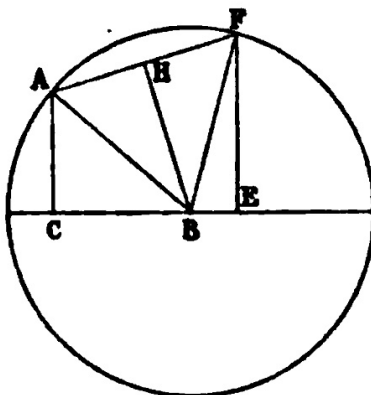
**Fig. 53.**



Taking this as given, he draws from it the consequence of the equality of the angles of incidence and reflection, which is the foundation of Catoptrics.

For me, I cannot accept his reasoning as a legitimate proof and demonstration. For example, in the following figure (*fig. 54*), in which AF is no longer parallel to CB, and where the angle CAF is obtuse, why may we not imagine that the determination of the moving ball from A towards B is composed of two others, one of which moves from line AF towards line CE, and the other advances towards AF?

**Fig. 54.**



For it is true to say that to the extent the ball falls on line AB, it moves towards AF, and that this advance must be measured by perpendiculars drawn from various points which can be taken between A and B along the line AF. And this must be understood when AF makes an acute angle with AB; otherwise, if it were right or obtuse, the ball would not advance towards AF, as it is easy to understand.

Assuming this, by the same reasoning as the author, we would conclude that the shiny body CE prevents only the first movement, not being opposed except in direction; such that, giving no hindrance to the second, the perpendicular BH being drawn, and HF made equal to HA, it follows that the ball must reflect towards point F, and thus angle FBE will be greater than ABC.

Thus it is clear that of all the infinite divisions of the determination of motion, the author has only chosen the one which serves to reach his conclusion; therefore he has accommodated his means to his end, and we know as little as we did before. Certainly, it seems that an imaginary division, which could be varied in an infinite number of ways, could never be the cause of a real effect.

We can, by a similar reasoning, refute the proof of these foundations of Dioptrics, since they are established by a similar train of reasoning.

6. There you have my thoughts on these new propositions, from which he draws out such beautiful consequences when he treats the shape that lenses must have, that I would wish that the foundations upon which they are established were better proved than they are now; but I fear that they are as lacking of truth as they are lacking of proof.

I had the plan of setting forth to you my thoughts on this subject; but, although I cannot again satisfy myself exactly, I will wait to hear about all the experiments that you have made or that I pray you make in the future, on the many proportions of angles of inclination and refraction. I would be much obliged if you would let me know of your findings as soon as possible, and in return I promise to tell you new things on this subject.

Everything that I have just told you does not prevent me from having a great deal of respect for the mind and ingenuity of the author. All the same, it is necessary to work together to discover the truth, which I believe is still hidden on this subject.

7. You have sent me two Discourses, one against M. de Beaugrand, and

the other by M. Desargues. I have seen the second, which is pleasant and very thoughtful. As for the first, it is not bad, if we removed the sour language; for the cause of M. de Beaugrand is completely deplored. Right after receiving the book he sent me, I had written to him the same reasons that you communicated to him.

Since you have given me reason to hope for such a favor from you, I await from you the other books of M. Descartes and the book of Galileo *De motu*.

I am, my Reverend Father, your very humble servant,

FERMAT.