Correspondence on optics between René Descartes, Marin Mersenne, Pierre Fermat, Thomas Hobbes, Claude Mydorge and Pierre Bourdin [originally: *Oeuvres de Descartes* (ed. by Ch. Adam and P. Tannery) Paris: Vrin vol. 1–3, 1964-74]

Translated from Latin and French by Peter McLaughlin

Published in P. Damerow, G. Freudenthal, P. McLaughlin and J. Renn: *Exploring the Limits of Pre-classical Mechanics: A Study of Conceptual Development in Early Modern Science: Free Fall and Compound Motion in the Work of Descartes, Galileo, and Beeckman,* New York: Springer-Verlag, 1992 [2nd ed. 2004].

5.2.5 Fermat: From a Letter to Mersenne (April or May, 1637)

First of all I doubt, and rightly so it seems to me, whether the inclination to motion must follow the laws of motion itself, for there is as much difference between the two as between potency and act. Furthermore, there seems to be a particular disparity in the fact that the motion of a ball is more or less violent to the extent that it is impelled by different forces, whereas light penetrates transparent bodies in an instant, and there seems to be nothing successive about it. But it is not geometry's business to probe further matters of physics.

In the figure [see Fig. 5.16] by which he explains the reason for reflection, page 15 of the *Dioptrics*, he says that the determination to move in a particular direction, just like motion and in general any other quantity, can be divided into all the parts of which one can imagine it to be composed; and that one can easily imagine that that [determination] of the ball [Fig. 5.21] which moves from A to B is composed of two others, of which the one makes it descend from line AF to line CE, and the other in the same time makes it move from AC on the left towards FE on the right, so that the two joined together conduct it to B along the right line AB.

This assumed, he infers the equality of the angles of incidence and of reflection, which is the foundation of catoptrics.

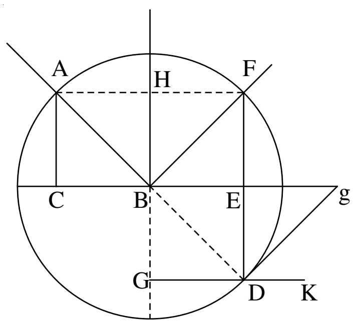

Fig. 5.21 (AT I, 358)

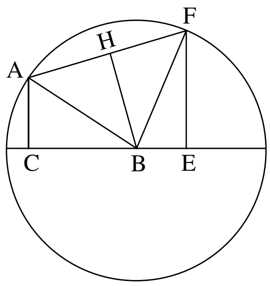

Fig. 5.22 (AT I, 359)

As for me, I cannot admit his reasoning as a proof and a legitimate demonstration. Since, for instance, in the attached figure [Fig. 5.22], in which AF is no longer parallel to CB and where the angle CAF is obtuse: Why can we not imagine that the determination of the ball that moves from A to B is composed of two others, one of which makes it descend from the line AF to the line CE, and the other of which makes it advance along AF? For it is true to say that to the extent that the ball descends in the line AB, it advances along AF; and that this advance is to be measured by the perpendiculars drawn on the line AF from the various points that can be taken between A and B. Whereby it should be obvious that AF makes an acute angle with AB; otherwise, if it were obtuse, the ball would not advance along AF, as is easy to understand. This assumed, we can by the same reasoning of the author [Descartes] infer that the smooth body CE only hinders the first movement, since it is opposed to it only in that sense; and thus the second not being hindered at all, the perpendicular BH being drawn and HF made equal to HA, it follows that the ball should reflect to point F; and thus the angle FBE will be greater than ABC. It is therefore evident that, of all the divisions of the determination to motion, which are infinite, the author has taken only that which can serve to give him his conclusion; and thus he has accommodated his *medium* [i.e. middle term] to his conclusion, and we know as little about it as before. And it certainly seems that an imaginary division that can be varied in an infinite number of ways can never be the cause of a real effect.

(AT I, 357-359) Oeuvres de Descartes (ed. by Ch. Adam and P. Tannery) Paris: Vrin, vol.1

5.2.6 Descartes: From a Letter to Mersenne for Fermat, (Oct. 5, 1637)

You tell me that one of your friends who has seen the *Dioptrics*, finds something there to object to, that he first of all doubts *whether the inclination to motion must follow the laws of motion itself, for there is as much difference between the two as between potency and act*. But I am convinced that he has formed this doubt based on what he imagined that I myself doubted; and because I said on page 8 line 24 : *For it is easy to believe that the inclination to move ought to follow the same laws as motion*, he thought that, when saying that something is easy to believe, I meant it is merely probable. In this he is very far removed from my sentiments. For I hold all that is merely probable almost for false; and when I say that something is easy to believe, I do not mean that it is only probable, but that it is so clear and so evident that there is no need to stop and demonstrate it. As indeed one cannot rightly doubt that the laws followed by motion, which is the act, as he himself says, are observed by the inclination, which is the potency to this act; for although it is not always true that what was in the potency is in the act, it is nonetheless completely impossible that there is something in the act that was not in the potency.

As for what he says afterwards: *that there seems to be a particular disparity in the fact that the motion of a ball is more or less violent to the extent that it is impelled by different forces, whereas light penetrates transparent bodies in an instant, and there seems to be nothing successive about it*, I do not follow his reasoning at all. For he cannot set this disparity in the fact that the motion of a ball can be more or less violent since the action that I take to be light can also be more or less strong; nor in that the one is successive and the other is not, for I think I have made it clear enough by the comparison to the blind man's cane and by that of the wine that descends in a vat, that although the inclination to move is communicated from one place to another in an instant, it does not fail to follow the same path that a successive motion must take, which is all that is in question here.

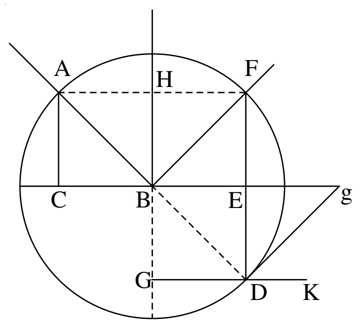

Fig. 5.23 (AT I, 452)

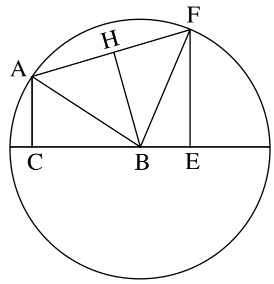

Fig. 5.24 (AT I, 452)

He adds after this a discussion that seems to me to be no demonstration at all. I do not want to repeat his words, since I have no doubt that you have kept the original. But I will say only that from what I wrote, namely that *the determination to move can be divided* (I mean really divided and not at all in the imagination) *into all the parts of which one can imagine it to be composed*, there is no reason at all to conclude that the division of this determination, which is done by the surface CBE [Fig. 5.23], which is a real surface, namely that of the smooth body CBE, is merely imaginary. And he commits a quite manifest paralogism in that, supposing that the line AF [Fig. 5.34] is not parallel to the surface CBE, he wanted us in spite of this to imagine that this line designates the direction to which this surface is not at all opposed; without considering that, just as it is only the perpendiculars, not on that [line] AF as wrongly drawn in his imagination, but on [line] CBE which mark the sense in which the surface CBE is opposed to the motion of the ball, so too, it is only the parallels to that same CBE that mark the sense in which it is not at all opposed to [the motion].

(AT I, 451-452) Oeuvres de Descartes (ed. by Ch. Adam and P. Tannery) Paris: Vrin, vol.1

5.2.7 Fermat: From a Letter to Mersenne for Descartes (Nov. 1637)

… 2. I shall cut short our dispute about reflection, about which I could however continue further, and prove that the author [Descartes] has accommodated his medium to his conclusion, of whose truth he was already certain; for if I should deny that his division of the determinations to motion is the one that must be taken, since we have an infinite number of them, I would force him to prove a proposition that would be very unpleasant to him. But since we do not doubt that reflections make equal angles, it is superfluous to dispute about the proof, since we know the truth; and I suppose it would be better without haggling to turn to refraction, which was the purpose of the *Dioptrics*.

3. I recognize with Mr. Descartes that the motive force or power is different from the determination and consequently that the determination can change without the force's changing and *vice versa*. The example of the first case is found in the figure on the 15th page of the *Dioptrics*,[13] where the ball impelled from point A to point B is deflected to point F; thus the determination to move in the line AB changes without change or diminishment of the force, which sustains its motion. We can take the figure on page 17[14] for the second case. If we imagine that the ball is impelled [Fig. 5.25] from point H to point B, since it falls perpendicularly on to the canvas CBE, it is evident that it will proceed in the line BG and thus its motive force will be weakened and its motion retarded without the determination's changing, since it continues its motion in the same line HBG.

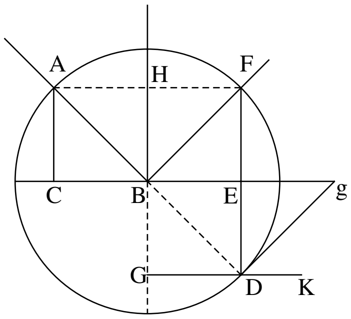

Fig. 5.25 (AT I, 465)

4. I return now to the demonstration of refraction in the same figure of page 17. *Let us consider* (says the author) *that of the two parts of which one can imagine this determination to be composed, it is only the one that makes it tend downward that can be changed in some fashion by the encounter with the canvas; and as for the one that makes it tend towards the right, it ought always to remain the same as it is, because the cloth is not at all opposed to it in this sense*.

5. I remark first that the author has not remembered the difference that he had established between the determination and the motive force or the speed of motion. For it is certainly true that the cloth CBE weakens the motion of the ball but does not hinder it in continuing its determination downward; and although it is slower than before, one cannot say that because the motion of the ball is weakened, the determination that makes it go downward is changed. On the contrary, its determination to move [Fig. 5.26] in the line BI is just as compounded, in the sense of the author, of that which makes it go downward

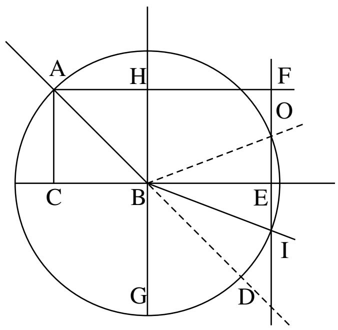

Fig. 5.26 (AT I, 465)

[13] AT VI, 95; see Fig. 5.16. Figure 5.25 given here was originally published in Clerselier's edition of Descartes' correspondence in 1664.
[14] AT VI, 97; see Fig. 5.17.

and that which makes it go from left to right, as is the first determination to move in the line AB.

6. But let us stipulate that the determination along BG or downwards is changed, to speak with the author; we may conclude that the determination along BE, or from left to right, is also changed. For if the determination along BG is changed, it is because in comparison to the first motion the ball which is now deflected and takes the path of BI advances less proportionally along BG than along BE than otherwise would have been the case. However, because we suppose that it advances proportionally less along BG than along BE than it would have otherwise, we may also say that it advances proportionally more along BE than along BG than it would have otherwise. If the first gives us to understand that the determination along BG is changed, the second can just as well let us conceive that the determination along BE is also changed, since change is just as well caused by augmentation as by diminution.

7. But let us again stipulate that the determination downward is changed but not that from left to right, and let us examine the conclusions of the author whose words are: *Since the ball loses nothing at all of the determination that it had to advance toward the right, in twice the time that it took to go from the line AC to HB, it ought to cover twice the distance in the same direction.*

8. See how he falls again into his first error, not distinguishing the determination from the force of motion. And to show you better, let us apply his reasoning to another case. Let us suppose in the same figure [Fig. 5.26] that the ball is impelled from point H to point B, it is certain that it will continue its motion in the line BG and that its determination does not change at all, but also [that] its motion is slower in the line BG than it was before. And nevertheless, if the reasoning of the author were true, we could say: since the ball does not lose any of the determination that it had to advance along HBG (for it is exactly the same), then in the same time as before it would cover the same path. You see that this conclusion is absurd and that to make the argument valid, it would be necessary that the ball lose nothing of its determination and nothing of its force; and thus we have quite manifestly a paralogism.

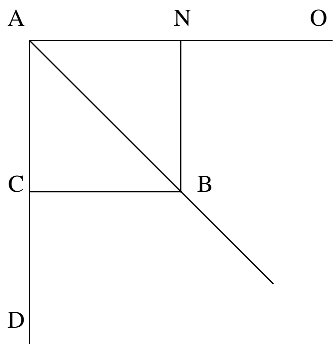

Fig. 5.27 (AT I, 468)

9. But in order clearly to destroy the proposition we must examine two sorts of compound motions made on two straight lines. Let us consider [Fig. 5.27] the two [lines] DA and AO which make the angle DAO, of whatever size you will; and let us imagine a heavy body [*un grave*] at point A, which descends in the line ACD at the same time that the line advances along AN such that it always makes the same angle with AO, and that the point A of the same line ACD is

always in the line AN. If the two motions, that of the line ACD along AO and that of the same heavy body in the line ACD are uniform, as we may suppose them to be, it is certain that the compound motion will always conduct the heavy body in a straight line such as AB; and if you take a point such as B from which you draw the lines BN and BC parallel to lines DA and AO; then the heavy body will be at point B, in the same time that it would have been at point C if there had only been the motion on ACD and that it would have been at point N if there had been only the other motion alone; and the proportion of the force that conducts it on AD to the force that conducts it along AO is as AC to AN that is to say, as BN to BC. It is this sort of compound motions that was used by Archimedes and the other ancients in the composition of their helices; their principal property is that the two motive forces do not impede each other at all, remaining always the same. But since this motion cannot be applied very well, it is necessary to consider another version and to engage in a particular speculation.

10. Let us suppose in the same figure a heavy body at point A, which is impelled at the same time by two forces, one of which pushes it along AO and the other along AD, with the result that the line of direction of the first motion is AO and that of the second is AD. If there were only the first force alone, the body would always remain on AO, and on AD if there were only the second [force]. But since both forces mutually impede and resist one another, let us suppose (and it should be remembered that we also suppose these motions to be uniform, for otherwise the compound motions would not be carried out in straight lines) that in one minute of an hour, for example, the second force makes the body depart from its direction AO according to the length NB which it must describe parallel to AD; for the body that is transported on AD by the second force, finding itself hindered by the first, will continue on and advance from A toward D by parallels to AD. Let us suppose as well that in the same minute of an hour, the first force makes the body depart from its direction AD according to the length CB, parallel (for the reasons given above) to line AO. It is completely certain that in one minute of an hour the body will be found at point B, which is the intersection of the two lines BN and BC. The compound motion will occur on the line AB and we can say that the body traverses the line AB in one minute.

11. Let us suppose now the angle DAO to be changed [Fig. 5.28] and, for example, to be greater. In the next figure, the same things being posited, I say that in one minute of an hour, as before, the body departs from the direction AO according to the line BN – equal to that to which we have given the same name in the preceding figure. For, since the forces are the same, the second will equally diminish the determination of the first, and will in equal times remove the body from its direction as much as before, because there is always the same resistance.

We may conclude the same thing for line BC.

The compound motion will thus occur here on the line AB, and the line AB will be traversed as before in one minute of an hour. But because in the two triangles ANB of the first and second figures, the sides AN and NB of the first figure are equal to those of the second and because the angles ANB which they comprise are unequal, it follows that the bases AB will be unequal (and consequently the compound motion will be swifter in the second than in the first), and that there will be such a proportion of the speed of the compound motion of the first figure to the speed of the compound motion of the second figure as the length of the line AB in the first figure has to the length of the line AB in the second.

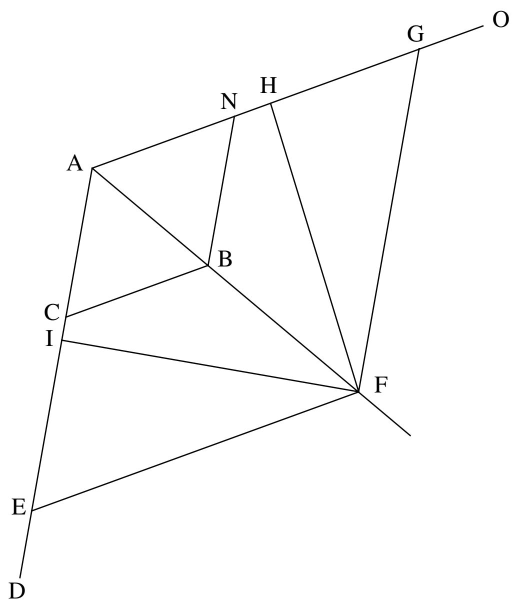

Fig. 5.28 (AT I, 472)

12. I now take an arbitrary point in the line AB [Fig. 5.28], such as F, from which I draw the lines FE, FG parallel to AO and to AD, i.e., FG and BN, as the construction shows us; then FE is to FG as CB to BN. But in the preceding figure the lines BN and BC are equal, respectively, to the lines BN and BC of this second figure (and by the same reasoning we can take an arbitrary point in the line AB of the first figure, in order to draw a conclusion similar to the preceding one). Thus, whatever point that you take in the line AB whether of the first or of the second, the parallels will be to one another as CB and CN, i.e., always in the same proportion. From point F let us draw the perpendiculars FH, FI on the lines AO and AD. In the parallelogram GAEF, the angles AGF and AEF, being opposite [one another], will be equal; thus the triangles GFH and EFI are equiangular; and consequently FI is to FH as EF is to FG. But FI is to

FH as the sine of angle DAF is to the sine of angle OAF; and consequently, making, if you will, the same construction in the first figure, you conclude, to avoid prolixity, that the sine of angle DAB is to the sine of angle OAB in the first figure as the sine of angle DAF to the sine of angle OAF in the second figure.

13. This thus assumed and demonstrated, let us consider the figure on page 20 of the *Dioptrics*,[15] in which the author supposes that the ball, having first been impelled from A to B, being at point B, is impelled in such a manner by the racket CBE, which (doubtless in the sense of the author) pushes along BG. Therefore, from the two motions of which the one pushes along BD and the other along BG a third is made which conducts the ball in the line BI.

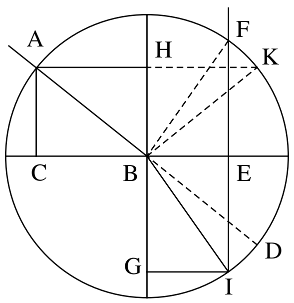

Fig. 5.29 (AT I, 473)

14. Let us now imagine a second figure similar to this one, in which the force of the ball and that of the racket are the same, and that only the angle DBG is greater in this second figure. It is certain by the demonstrations we have just given that there will be the same proportion of sine of the angle GBI to sine of angle IBD in this second figure [Fig. 5.29], which we imagine to be drawn and which we omit to avoid lengthiness. Whereas if the propositions of the author were true, there would be the same proportion of the sine of angle GBD to the sine of angle GBI in the figure of the author that the sine of the angle GBD to the sine of the angle GBI in this second figure which we have imagined. But since this proportion is different from the other, it follows that it cannot hold.

15. Moreover, the principal ground of the author's demonstration is based on the fact that he believes that the motion compounded on BI is always equally swift, even if the angle GBD made by the lines of direction of the two motive forces happens to change; this is false as we have already plainly demonstrated.

16. I do not want to maintain, that in the application of the figure to refraction, which he makes on page 20,[16] one should take my proportion and not his; for I am not sure whether this compound motion should serve as the rule for refraction; on this matter I shall tell you my sentiments another time at more length.

(AT I, 464-474) Oeuvres de Descartes (ed. by Ch. Adam and P. Tannery) Paris: Vrin, vol.1

[15] AT VI, 100; see Fig. 5.19.
[16] AT VI, 100; see Fig. 5.19.

5.2.8 Descartes: From a Letter to Mydorge (March 1, 1638)

First of all, where he [Fermat] says *that I have accommodated my medium to my conclusion and that I would be hard pressed to prove that the division of the determinations that I use is that which must be taken* (after which he quickly moves to other matters), he shows that he has not at all responded to my first letter, in which I clearly proved what he demanded, showing that one must consider in the division of these determinations, not the line drawn the wrong way in his imagination, but the parallel and the perpendicular to the surface where reflection is made.

In the article that begins *I remark first*, he would have it that I supposed such a difference between the determination to move here or there and the speed, that they are not found together and cannot be diminished by the same cause, namely by the cloth CBE: which is contrary to my meaning and contrary to the truth; although this determination cannot be without some speed, nonetheless, the same speed can have various determinations and one and the same determination can be joined to various speeds.

In the following article there is a sophism, or what is the same in matters of demonstration, a paralogism, in these words: *it advances proportionally less along BG than along BE than it would have otherwise, we may also say that it advances proportionally more along BE than along BG than it would have otherwise*. He slips in the word *proportion*, which is not in my book at all, to deceive himself. Since it advances [Fig. 5.30] proportionally less along BG than along BE (i.e., comparing only BG and BE with one another) it also advances proportionally more along BE than along BG, but he concludes that it is true absolutely speaking that it advances more *along BE than it would have otherwise*.

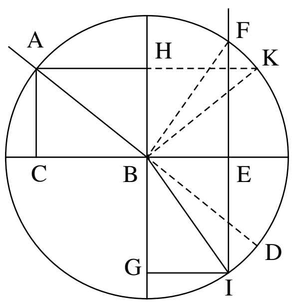

Fig. 5.30 (AT I, 473)

A little later where he says these words: *See how he falls again into his first error*, it is he himself who falls again into his own [first error], believing that the distinction between the determination and the speed or force of motion prevents the one and the other from being able to be changed by the same cause. And he commits a paralogism with these words: *since the ball loses nothing of its determination to speed*, which he has by no means taken from me, for I say nothing of the kind anywhere; and his error is all the greater as he accuses me of committing a paralogism in making it.

Everything that follows after that is only to prepare the reader to accept another paralogism, which consists in the fact that he speaks of the composition of motions in two diverse senses and infers about the one what he has proved about the other. Namely, in the first sense, it is properly only the determination of this motion that is compounded, and its speed is not, except insofar as it accompanies this determination as one sees in the second figure [see Fig. 5.28], so that making AB equal to NA and also to BN, this compound motion, which goes from A to B, is neither more nor less swift than each of the two simple [motions], which go the one from A to N and the other from A to C in the same time; and thus one cannot say that it is its speed that is compounded but only that it is its determination to go from A to B that is compounded of two which are the one to go from A to N and the other from A to C. And, nonetheless, the speed of the motion from A to B can be equal or greater or lesser according as the angle CAN is 120 degrees or more acute or more obtuse; not because it is composed of [the speeds] of the two other motions, but because it must accompany the compound determination and accommodate itself to it. Whereas in his second sense, which is my sense, in the figure on page 20,[17] it is only the speed of the motion that is compounded: namely it is compounded of that which the ball had coming from A to B (for it continues from B toward D) and that which the racket which impels it at point B adds to it. So that, here, it is the speed that follows the laws of composition and not the determination, which is obliged to change in various fashions according to what is required for it to accommodate itself to the speed. And the force of my demonstration consists in the fact that I infer what the determination will be, from the fact that it cannot be other than that which I explain, in order to accord with the speed or better with the force which begins it in B. But his paralogism consists in what he concludes about the composition of the speed after having proved nothing except about the composition of the determination, calling both of them composition of motion.

And he continues his paralogism up to the end, where he concludes that the compound motion on BI (that is to say the motion whose speed is compounded) is not always equally swift when the angle GBD made by the lines of direction of the two forces (that is to say by the lines that mark how the determinations of the two forces are compounded) is changed; drawing this conclusion from that which he had already proved concerning the motion whose determination – and not whose speed – is compounded that the speed changes when the angle changes. But you can see the faults better than I and if some difficulty in all this should remain which I have not explained enough please oblige me by pointing it out to me.

(AT II, 17-21) Oeuvres de Descartes (ed. by Ch. Adam and P. Tannery) Paris: Vrin, vol.2

[17] AT VI, 100; see Fig. 5.19.

5.2.9 Hobbes: From an Optical Manuscript of 1640 (*Tractatus opticus II*)[18]

Chapter 1, Section 13

There are two kinds of bodies in the universe, the one, the parts of which cohere in such a way that they are not easily separated; such are those that we call solid [*dura*], some more, some less. The other [kind] are those the parts of which flow away and are taken apart from each other by any impression of motion, and these they call fluid. Here is not the place to discuss whether some body is indeed fluid in such a way that it can always be separated into separables in reality [*re ipsa*] as it can always be divided into divisibles in thought. Let us imagine a body, as solid as you wish, which another body intrudes or impinges upon or presses by as light a motion as you wish. We say that one body intrudes or impinges upon another or that one body presses another, when, by motion approaching up to contact with the opposed body, it would proceed beyond, were it not impeded by the opposition of that body. I say that the intruding body with that part of it by which it touches parts of the opposed body brings it about that this part yields; but if indeed the striving [*conatus*] or force [*vis*] by which the intruding body tends beyond the contact does not propel the opposing part at all, then neither will the double force propel it; for, since a double force produces a double effect or a multiple effect or some increase or other, it would produce a double or a multiple or an increase of nothing. But twice or thrice or whatever times nothing is nothing. Thus the part of body which is opposed to an intruding body will either yield to the lightest motion or cannot be moved by any motion at all; but in as much as solid bodies of whatever kind yield to some multiple or other of a force, therefore they also yield simply to any force whatever.

(Ch. 1: "On Light, Illumination, and Transparency," §13; Schuhmann transcription, private communication; see also Alessio, p. 154)

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[18] The passages translated here from Hobbes's (Latin) optical manuscript of 1640 (*Tractatus opticus II*) were transcribed by the late Karl Schuhmann and were to be published in a new Hobbes edition. We are grateful to him for allowing us to make use of these transcriptions and for providing us with photocopies of the figures.

Chapter 2, Section 4

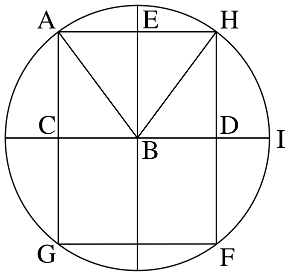

Fig. 5.31 (Hobbes, *Tractatus opticus II*)

… Moreover if perhaps by *determination* he wanted to signify some determined velocity by which the ball more and more approaches the line DH, then the determination will be that velocity; and thus the perpendicular determination towards CD will also be a velocity. And since this velocity of the ball is removed by the encounter with the plane in B, part of the ball's velocity is removed. Therefore he contradicts his own supposition, which stipulated that the ball moves with equal velocity before and after the encounter with the plane. Hence *pace* Descartes it remains valid that reflection occurs through the resistance of the body which is impinged upon in as much as the impulse restitutes itself from the striking ball.

(Ch. 2: "On the Nature of Reflection and Refraction," from §4; Schuhmann (ed.) forthcoming; see also Alessio, p. 162)

Chapter 2, Section 8

In reflection solid [*dura*] and fluid [media] behave in the same manner, in refraction however in contrary ways. A solid body, namely, projected obliquely from a more fluid medium into a more solid medium is refracted in a direction away from the perpendicular. Descartes does not correctly explain the physical cause of this. In order to make it known to you, let us draw a figure [like that] which is found in his Dioptrics page 17 and let us take up again as much of his demonstration as is necessary.

Supposing therefore that a ball moved through the air from A to B penetrates in B a more solid medium, such that on penetrating it loses half of its velocity, whereby this loss of velocity is to be reckoned in the determination from AF to CE only, so that its lateral determination from AC to HB will remain the same, it is then argued as follows:[19] *Then, having described the circle AGD with its center at B, and having drawn at right angles to CBE the three straight lines AC, HB, FE so that the distance between FE and HB is twice that between HB and*

[19]Hobbes translates the passages cited from the *Dioptrics* into Latin. We adapt our translation of Descartes' French to translate Hobbes' Latin. There are no significant differences between the two versions, except that Hobbes (rightly) inserted a correction into the scribe's copy naming the circle AGD, not AFD as in the *Dioptrics*.

AC, we shall see that the ball must proceed towards the point I, etc. In the first place experience contests this. Let us in the first place posit that point A is elevated sixty degrees above the horizon CE; line AH will therefore be the sine of thirty degrees and thus equal to half a semidiameter. Hence FE which by hypothesis must be as far away from HB as HB is from AC will be tangent to but not cut the circle.[20] Whence it follows that point I is at the end of the diameter through CBE. And thus a ball elevated in the rare medium to sixty degrees and moved to B where it meets the more solid medium, which takes away half of its velocity, will not at all penetrate it, but rather be reflected—which is contrary to experience; for at whatever elevation and in whatever diversity of media, a projected ball will either penetrate [the medium] or be reflected. Hence let us posit A in the air and at an elevation of thirty degrees and let us also suppose a more solid medium, which deprives [*mulctet*] the ball of the third part of its velocity, which seems roughly to be the case [*fere accidit*] in water; it would follow that FE falls outside the circle, say in KL. The ball will hence not penetrate the water but be reflected, and this is contrary to experience. Now, I myself have experienced that a lead ball shot from a gun [*sclopeto*] at an elevation of five degrees penetrates and plunges deep into [the water], but at an elevation of two degrees it is reflected. Let us now turn to Descartes' demonstration. *For, since the ball loses half its speed in passing through the sheet CBE, it must take twice as much time to go below CBE from B to some point on the circumference of the circle AGD as it took to go from A to B above the sheet. And since the lateral determination, namely from HB to FE, is supposed not to be removed but to remain the same, the time in which the ball passes from the line AC to HB will be equal to the time in which the same ball must pass from line HB to line FE. From which it follows that the ball must pass to the circumference at point I where FE and the circumference intersect*. He is misled by the ambiguity of that word *determination*. If someone understands by *determination* merely the aspect (*aspectus*) or the situation (*situs*), such that it is not a quantity, then neither the *determination* that is downwards nor any part of it can be removed by the encounter with the plane CBE, and so each of its *determinations* will remain intact, both in a solid media and a liquid; and thus the ball will continue from A through B directly to D. But if he understands by *determination* a determined motion, as a motion from point A towards a determined point B or from right line AC to another right line HB determined by

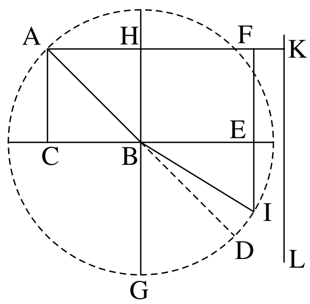

Fig. 5.32 (Hobbes, *Tractatus opticus II*)

[20]If angle ABC = 60° and ABH = 30°, then AB = 2AH. And if HF = 2AH, then HF = AB.

position, then the ball impinging on B it is not understood to lose half of the velocity or motion *simpliciter* from point A toward point B, but that much of the perpendicular motion from right line AH towards right line CBE. But even if the ball loses half of its velocity downward by the encounter with plane CBE, it does not follow that this means that double the time that it took for the ball to pass from A to B must be expended in order for the ball to pass from B to the circumference of the circle AGD.

(Ch. 2: "On the Nature of Reflection and Refraction," §8; Schuhmann (ed.) forthcoming; see also Alessio, pp. 163-4. For discussion see 2.5.2.2)

5.2.10 From Mersenne's *Ballistica*

And it must be noted that a heavy body A, which is carried or impelled to the right towards B with one degree of speed [*celeritas*] and downwards to E with one degree of speed, by means of which it arrives at F, has not acquired two degrees of speed; nor [has it acquired] three degrees in point D, in as much as it moved with two degrees of speed from A to C and with one [degree of speed] from A to B, it arrives at D by the line AD; for in such a case the line AF would be to the line AD as 2 to 3 (since line is to line as speed to speed), but this is not true, since AF is to AD as two to the [square] root of 10, or as the root of 2 to the root of 5. That is, the speed from A to F to the speed from A to D is not as the compound of AB and BF to the compound of AB and AC; rather the velocities [*velocitates*] are as the subtenses AF, AD or as the roots of the sum of the squares of the sides.

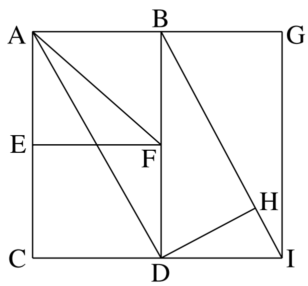

Fig. 5.33 (Mersenne 1644, prop. 32, p. 110)

(Mersenne, *Cogitata physico-mathematica* (*Ballistica*), 1644, prop. 32, p. 110. The substance of this example is attributed to Hobbes by the editors of Mersenne's *Correspondance*; see MC 10, 577)

5.2.11 Descartes: From a Letter to Mersenne for Hobbes (Jan. 21, 1641)[21]

First, he says, that I would have expressed myself more clearly if I had said *determined motion* instead of *determination*. On this matter I do not agree: Although it might be said that the velocity [*velocitas*] of a ball from A to B is compounded of two others, from A to H and from A to C, I thought one should abstain from such a way of speaking, lest it by chance be so understood that in a motion compounded in this way the quantity of these velocities as well as the proportion of one to the other remains the same; which is not at all true. Because if, for example, we suppose the ball to be carried from A to the right with one degree of speed [*celeritas*] and downwards also with one degree, it would arrive at B with two degrees of speed in the same time in which another [ball] also carried from A to the right with one degree of speed and downwards with two, would arrive at G with three degrees of speed: Whence it would follow that the line AB is to AG as 2 to 3, whereas it is really as 2 to $\sqrt{10}$ etc.

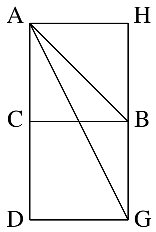

Fig. 5.34
(AT III, 288)

What he then says about the ground removing the downward speed is contrary to the hypothesis: I supposed nothing at all to be taken away from the speed; and it is contrary to all experience: otherwise a ball hitting the ground perpendicularly would never rebound. Therefore my demonstration has no difficulties in any part; but he himself was greatly mistaken in as much as he did not distinguish motion from determination; the motion itself should by no means be diminished, so that reflection occurs at equal angles.

Furthermore, his assumption that *what does not yield to the smallest force cannot be moved by any force at all*, has no appearance of truth. Who would believe, for example, that a weight of one hundred pounds in a balance would yield a little to a weight of one pound placed in the other pan of the scale, just because it does yield to a weight of 200 pounds? However I willingly concede that the part of the earth which a ball hits yields a little to it just as the part of the ball striking the earth is also somewhat bent inwardly, and that since the earth and the ball restitute themselves after the collision, the rebounding of the ball is assisted by this; but I assert that this rebounding is always more impeded by the indenting of the ball and the earth than it is assisted by their restitution; and from this it can be demonstrated that the reflection of the ball, and of other bodies in the same way not perfectly solid never occurs at exactly equal angles. But without demonstration it is easy to find out by experience that softer balls

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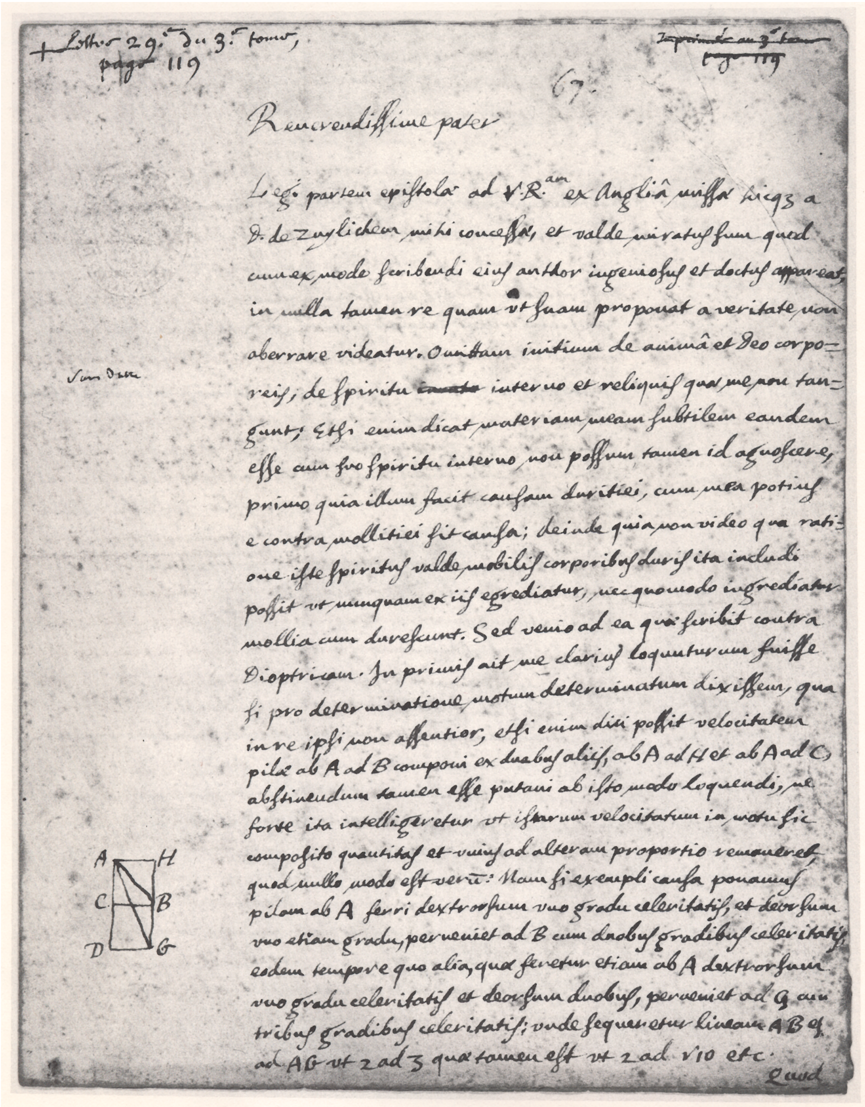
[21] A number of improvements to the translation given in the first edition of this book have been made after consulting new transcriptions from Hobbes's 1640 optical manuscript by K. Schuhmann and the new translations by N. Malcolm in his edition of Hobbes's *Correspondence*.



Lettre 29e du 3e tome, page 119

~~Imprimée au 3e tome page 119~~

67.

Reverendissime pater

Legi partem epistolae ad V.R.am ex Angliâ missae hicque a D. de Zuylichem mihi concessae, et valde miratus sum quod cum ex modo scribendi eius author ingeniosus et doctus appareat, in nulla tamen re quam ut suam proponat a veritate non aberrare videatur. Omittam initium de animâ et Deo corporeis, de spiritu ~~innato~~ interno et reliquis quae me non tangunt; Etsi enim dicat materiam meam subtilem eandem esse cum suo spiritu interno, non possum tamen id agnoscere, primo quia illum facit causam duritiei, cum mea potius e contra mollitiei sit causa; deinde quia non video qua ratione iste spiritus valde mobilis corporibus duris ita includi possit ut nunquam ex iis egrediatur, nec quomodo ingrediatur mollia cum durescunt. Sed venio ad ea quae scribit contra Dioptricam. In primis ait me clarius locuturum fuisse si pro determinatione motum determinatum dixissem, qua in re ipsi non assentior, etsi enim dici possit velocitatem pilae ab A ad B componi ex duabus aliis, ab A ad H et ab A ad C, abstinendum tamen esse putavi ab isto modo loquendi, ne forte ita intelligeretur ut istarum velocitatum in motu sic composito quantitas et unius ad alteram proportio remaneret, quod nullo modo est verum: Nam si exempli causa ponamus pilam ab A ferri dextrorsum uno gradu celeritatis, et deorsum uno etiam gradu, perveniet ad B cum duobus gradibus celeritatis, eodem tempore quo alia, quae feretur etiam ab A dextrorsum uno gradu celeritatis et deorsum duobus, perveniet ad G cum tribus gradibus celeritatis; unde sequeretur lineam AB esse ad AG ut 2 ad 3 quae tamen est ut 2 ad √10 etc.

Quod

Plate V. Autograph of a letter from Descartes to Mersenne for Hobbes (Jan. 21, 1641), anonymous private collection. From the auction documentation, July 1998.

do not rebound as high nor at such great angles as more solid balls [*duriores*]. It is thus seen how wrong he was to adduce the softness of the earth to demonstrate the equality of the angles; especially since it follows from this that, if the earth and the ball were solid and did not give way at all, there would be no reflection at all; which is unbelievable. It is also clear how right I was to assume that both the earth and the ball were perfectly solid, so that the matter can be submitted to mathematical examination. [...]

But I am surprised that he asserts that my demonstration is not correct since he clearly offers nothing in support of this reproach except that he says some things contradict some experience, which in fact conform to experience and are most true. But he does not seem to attend to the difference that exists between the refraction of a ball or some other body entering water and the refraction of light, a difference that is twofold and very great. First, the one refraction is made towards the perpendicular and the other in the contrary manner [*modo contrario*]. And while rays of light pass more easily by a third of their impetus, more or less, through water than through air, the ball on the other hand need not to be deprived [*mulctari*] of a third part of its velocity by the same water; for there is no connection between these two. Secondly, a weak light is not refracted at different angles by the same water than a strong light; but it is clearly different with the ball, which, impelled by a great force into water cannot be deprived by it of so great a part of its velocity as it would be if it proceeded more slowly. And thus it is not surprising that he has experienced that a lead ball shot from a gun [*sclopeto*] with very great force enters the water at an elevation of five degrees since it will likely not be deprived of more than a thousandth part of its velocity.

Furthermore he insinuates that I suppose that all loss of velocity is to be reckoned [*computandam*] in the motion downward: but I unwaveringly said it was to be reckoned in the motion as a whole taken *simpliciter*. ...

(AT III, 288-291)Oeuvres de Descartes (ed. by Ch. Adam and P. Tannery) Paris: Vrin, vol.3

5.2.12 Hobbes: From a Letter to Mersenne for Descartes (Feb. 7, 1641)

...You say, *thirdly*, that where I say *he would have expressed himself more clearly if he had put determined motion in place of determination*, he does not agree with me but responds with these words: Although it might be said that the velocity of a ball from A to B is compounded of two others, from A to H and from A to C, he thought he should abstain from such a way of speaking, lest it by chance be so understood that in a motion compounded in this way the quantity of these velocities as well as the proportion of one to the other remains the same; which is not at all true. Because if, for example, we suppose the ball to be carried from A to the right with one degree of speed and downwards also with one degree, it would arrive at B with two degrees of speed in the same time in which another [ball] also carried to the right with one degree of speed and downwards with two, would arrive at G with three degrees of speed: Whence it would follow that the line AB is to AG as 2 to 3, whereas it is really as 2 to $\sqrt{10}$.

I reply: Since Mr. Descartes admits that *it might be said* [*dici posse*] *that the velocity of the ball from A to B is compounded of two others, from A to H and from A to C*, he should also have admitted that it is indeed true; because he maintains that nothing that is not true may be said [*dici posse*] in philosophy by a philosopher. But *he abstained from this way of speaking because something false could seem to follow from it, namely that the ratio of the line AB to the line AG is not as 2 to $\sqrt{10}$, but as 2 to 3*; but this is not a proper reason for abstaining. Since, if this falsity were not rightfully inferred from that manner of speaking, then he should not have been afraid of paralogisms which others might thereupon commit on their own accounts; but he himself believed this conclusion to be true, which he himself also drew, but by means of fallacious reasoning. Since, *if we suppose that the ball is carried from A to the right with one degree of speed and downwards also with one degree*, it would nevertheless not arrive at B with two degrees of speed. Similarly, *if A were carried to the right with one degree and downwards with two*, it would nonetheless not arrive at G with three degrees as he supposes. Let us suppose two straight lines AB, AC constructed at a right angle [Fig. 5.35], and let the velocity from A towards B have the same ratio to the velocity from A towards C as the line AB has to the line AC; these two velocities compound the velocity which is from B towards C. I say that the velocity from B towards C is to the velocity from A towards C or from A towards B [i.e., from B towards A] as the straight line BC is to the straight line AC or AB. Let straight line AD be drawn from A perpendicular to BC and let the straight line FAE be drawn through A parallel to BC; and likewise also BF and CE, perpendicular to FE. Therefore in as much as the motion from A to B [i.e., from B to A] is composed of the motions from F to A and from F to B [i.e., B to F], the compounded motion AB does not contribute more speed to the motion from B towards C than the components FA, FB can contribute; but the motion FB contributes nothing to the motion from B towards C: this motion is determined downwards and does not at all tend from B towards C. Therefore only the motion FA gives [*dat*] motion from B to C. Similarly it is proved that AC gives motion from D towards C by virtue of AE alone; but the speed which AB draws from FA and by which it acts from B towards C is to the whole speed AB in the proportion FA or BD to AB. Likewise, the speed that AC has by virtue of AE is to the whole speed AC as AE or

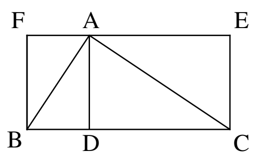


Fig. 5.35[22]

[22]Hobbes's original is lost; the figure is reconstructed from the text and from the figure in Descartes' reply (see Fig. 5.38).

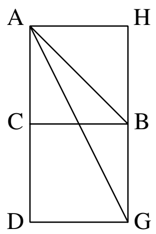


Fig. 5.36
(AT III, 305)

DC is to AC. Hence the two speeds by which the motion from B towards C occurs joined together are to the speed along AC or along AB [i.e. BA] taken simply as the whole BC is to AC or AB. And therefore referring to the previous figure [Fig. 5.36], the speeds through AB, AG will be as [the lines] AB, AG themselves, i.e., as $\sqrt{2}$ to $\sqrt{5}$, i.e., as $\sqrt{4}$ to $\sqrt{10}$, i.e., as 2 to $\sqrt{10}$, and not as 2 to 3. Hence, no absurdity follows from this way of speaking, as Descartes believed. You see, Father, how even most learned persons are inclined to commit paralogisms because of exaggerated self-confidence.

Fourthly you write that he says, *I should not say that speed is destroyed by the ground because he assumed the contrary, and because it is contrary to experience; otherwise a ball hitting the ground perpendicularly would never rebound*.

I reply, that it certainly was not I in my letter that destroyed his hypothesis, but I said that he himself destroyed it and therefore that he ought not to make use of it (for as far as my opinion in this matter is concerned, I do in fact think that a motion once given cannot ever be removed and thus also not diminished). But that you may judge whether or not he himself destroys the hypothesis, let us take up again the figure [Fig. 5.37]. He supposes A to move towards B by a motion that certainly should never cease but which will not always be in that direction [*in ea determinatione*], as he says. That is, what moves will always move uniformly but not always along the same path [*viam*] or straight line. This I concede. Furthermore, the determination (or path) from A towards B is composed of two other paths (or determinations) of which one is downwards from A towards C, the other lateral from A towards H. This too I concede. From this he thinks he can show that the motion from A to B continues from B to F by the angle FBE equal to the angle ABC, without the destruction of his hypothesis. This I deny. When a ball which moves from A to B arrives at B, it loses the determination (or path) that it had downwards from AH towards CB; thus the determination to the right from AC towards HB remains; but the degree of velocity that it [the ball] had at the beginning is retained; therefore it will go to the circumference of the circle in G. Therefore, he is obliged to demonstrate that, if the entire velocity that it had from A towards B is retained, it is impossible for the ball to advance farther than E in the same direction [*in eadem determinatione*]; which he could not do, unless the determination from A towards H is taken to be a motion. But he himself seems to understand this determination as a motion since in the demonstration of it he attributes to it a quantity; thus the determination or path of the ball does not have a quantity except in so far as while following it the ball describes a line

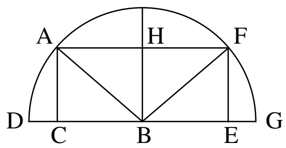


Fig. 5.37 (AT III, 306)

of such and such a length. However, if these two determinations, the perpendicular and the lateral, are motions, it is manifest that the ball, when it comes to B loses that part of its motion which it had from A towards C. Therefore after impact in B it moves less swiftly than before – which is the destruction of his own hypothesis. He adds that such a diminishment of motion is contrary to experience, since we see that those things that hit the ground perpendicularly rise up again in the perpendicular. But I wonder how it can be known from experience whether the reflection occurs in the perpendicular because there is no loss of motion or because motion is restituted; for the same effect can occur by various means. It is true that experience teaches us that reflection occurs according to equal angles but not by what cause.

[…]

You write in the *tenth place* that Mr. Descartes complains *that I insinuate that he reckoned the whole loss of velocity in the motion downwards, but that he unwaveringly said it was to be reckoned in the motion as a whole taken* simpliciter.

I reply: I confess that he said straightforwardly that this loss is to be reckoned in the motion as a whole; but when he said the determination – the perpendicular not the lateral – is diminished in the first penetration of the solid [*duri*] body, he said as a consequence that the perpendicular motion as a whole is diminished; for the determination cannot be diminished unless by determination he means motion. Thus, he does not unwaveringly say that the loss of motion is to be reckoned in the motion as a whole *simpliciter*. If therefore he has said each of two contradictory propositions, and I ascribe to him the second of them, this is not insinuating anything. Furthermore, if he reckons the total loss of velocity in the motion as a whole but reckons none in the lateral motion, it is necessary that he reckons it all in the perpendicular alone.

(AT III, 303-312)Oeuvres de Descartes (ed. by Ch. Adam and P. Tannery) Paris: Vrin, vol.3

5.2.13 Descartes: From a Letter to Mersenne for Hobbes (March 4, 1641)

… Ad 3. I believed that, what I professed *can be said,* can be understood in a sense in which it is true, but it can also be understood in a sense – and in a much more obvious sense – in which it is false; and I therefore have abstained from a way of speaking which is less apt and which would offer the readers an occasion for error; and this reason was completely proper. But it is most improper that he does not admit it to be proper and it is clearly insolent and absurd that he wants to charge me with not rightly understanding the case, whereas it is he himself that does not understand it even now, as will soon become clear. He proposes a completely empty cocoon of a demonstration, so as to deceive the insufficiently attentive. For, in the first place, I would like to know what he supposes when he says: *let the velocity from A towards B have the same ratio to the velocity from A towards C as AB itself has to AC itself; these two velocities compound the velocity which is from B to C*. He cannot suppose that the ball moves from A towards B [Fig. 5.38] and at the same time towards C; this cannot be done. But without doubt he wanted to say *from B towards A* where he said *from A towards B*; thus namely that we conceive the ball to move from B towards A on line BA while at the same time this line BA moves towards NC, thus that at the same time the ball advances from B to A and line BA to line NC; thus the motion of the ball would describe the line BC. But, whether by chance or purpose, he has confused things so that he seems to say something in what follows, where in truth he says nothing which is not plainly trivial. In order to prove that the velocity from B to C is compounded of the velocities from B to A and from A to C, he divides each of them, saying *in as much as the motion from A to B* (i.e., from B to A)[23] *is compounded out of the motions from F to A and F to B* [i.e., B to F], *the compounded motion AB does not contribute more speed to the motion from B towards C than FA contributes, nor does AC contribute more than AE etc*. From this he should have inferred that BC is compounded of FA and AE and not of BA and AC, and thus its triviality would also have been apparent: because FA and AE are the same as BC. But he says that the speed BC is compounded of BA and AC because FA and AE are contained in BA and AC, and this is just the same as if he said that an ax is composed of a forest and a mountain because the forest contributes wood for the handle and the mountain [contributes] iron dug up from it. And after these ineptitudes this most urbane person accuses me of having committed a paralogism; but I ask, on what head? I beg of you: precisely where I said that I did not want to make use of an improper way of speaking.

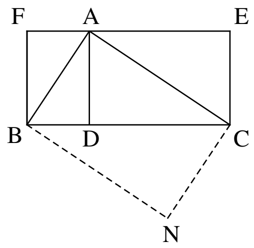


Fig. 5.38 (AT III, 323)

Ad 4. Here he shows that he errs in the very issue on which he said a little before that *I should not have been afraid of the paralogisms which others commit on their own accounts*; but he himself commits a paralogism in that he considers a determined motion instead of the determination which is in the motion. To understand this it should be noted that a determined motion is to the determination itself of the motion as a flat body to the flatness or surface of that body. For in the same way as it does not follow that, when one surface changes, the others change, too, or that more or less body is joined [*adjungi*] to them even though they all are in the same body and could not be without it; so too, it does not follow that, when one determination changes the other [also] changes or that

[23]Descartes' insertion.

more or less motion is joined to it, even though neither of them could be without a motion. If our friend had understood this issue he would not have said that *I was obliged to demonstrate that if the entire velocity that it had from A towards B is retained* [Fig. 5.39], *it is impossible for the ball to advance farther than E in the same direction* [*in eadem determinatione*]. He would have seen that this is demonstrated precisely by demonstrating that the determination to the right is not changed because the motion cannot be increased or decreased in that direction without at the same time adding to or taking away from the determination [in that direction], just as a body cannot be changed in its surface without the surface's changing, too. Nor would he furthermore have said: *But if these determinations are motions etc*. They are no more motions than surfaces are bodies; and he was mistaken and himself committed a paralogism in that he considered a determined motion instead of the determination of a motion; as I promised that I would prove.

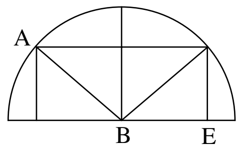


Fig. 5.39 (AT III, 325)

(AT III, 322-326) Oeuvres de Descartes (ed. by Ch. Adam and P. Tannery) Paris: Vrin, vol.1

5.2.14 Hobbes: From a Letter to Mersenne for Descartes (March 30, 1641)

As concerns the dispute over the difference between *determined motion* and *the determination of motion*, I see that it is necessary that I present my opinion more overtly and explicitly than was hitherto the case.

First, it should be known that just as every man is either Peter or Socrates or some other individual, and yet this word *man* is a generic term [*vox communis*] (namely one of the five predicables [*vocum*] which Porphyry listed in his *Isagoge*[24]) thus, too, every motion is either *this* or *that motion*, namely, [a motion] *determined* by its *termini a quo* and *ad quem*. As namely Socrates and man are not two men, nor two entities [*res*], but one man under two designations [*appellationes*] (as the same entity is designated by the name [*nomen*] *Socrates* and by the name *man*), so also *motion* and *determined motion* are one and the same motion and one entity under two names.

Secondly, it should be known that what is the efficient cause of some proposed motion is also the efficient cause by which this motion is so determined that the determination of motion taken in an active sense is the action of a movent by

[24]Porphyrius: *Isagoge et in Aristotelis categorias commentarium*. Hobbes writes "Isagoge ad Artem"; Malcolm (HC 1, 103) conjectures: "Isagoge ad Ar[istote]lem," but Hobbes may simply have conflated Porphyry's title with the similar title of Vieta's *In artem analyticam isagoge*.

which the patient is carried in one direction rather than in another. But if on other occasions this word *determination of motion* signifies in a passive sense, that is, [signifies] something in the patient, then it means the same as *to be moved thus,* that is, *moved determinately*, and in this sense *determined motion* and *determination of motion* are one and the same.

And Mr. Descartes, at the place where he says that I am mistaken in saying *determined motion instead of the determination which is in the motion*, understands the determination to be in the body moved and to be passive, and thus *determined motion* and *the determination of motion* are one and the same. But in what manner does he understand this *determination* to be in the motion? Perhaps as in a subject? This is absurd since motion is an accident, just as it would be absurd to say that whiteness is in color, since whiteness is as much a determination of color as moving to the right or to the left is a determination of motion. But although it is absurd for the *determination* to be *in a motion* as an *accident* in a *subject*, nevertheless Mr. Descartes has not abstained from [asserting] it. No wonder, since he said that *determined motion* is to the *determination itself of the motion* as a *flat body* is to the *flatness or surface of the same body*; but *flatness* is in a body as in a subject. Thus the comparison should be made in the following way: *determined motion* is to the *determination itself of motion* as a *determined surface* (i.e., a *flat* or *rounded* surface, etc.) to the *determination of the surface* (i.e., to the *flatness, roundness*, etc.). Since just as much as a flat surface differs from the *flatness of the surface* so does a *determined motion* differ from the *determination of the motion*. Nor is what you say later at all valid: *In the same way that it does not follow that, when one surface changes, the others change, too; thus it does not follow that when one determination changes another also changes*. For as concerns *accidents* in a *subject* (as are two different *surfaces*) one can be lost and another remain. But inasmuch as there is only *one accident* under *two names*, as one motion under the names *determined motion* and *determination of motion*, if what is signified by one name is lost, that which is signified by the other is also lost.

Thirdly, it is to be objected that *one motion* cannot have *two determinations*; for in the figure attached [Fig. 5.40], let A be a body which begins to move towards C, having the straight path AC. If someone tells me that A moves in a straight path to C, he *has determined* that motion for me; I can myself trace the same path as unique and fixed [*unam et certam*]. But if he says A moves along a straight path toward the straight line DC, he has not shown me the *determination* of the motion, since there are infinitely many such paths. Thus the motions from AB to DC and from AD to BC are not determinations of *one single motion* of the body A towards C, but rather the determinations of two motion of two bodies, one of which goes from AB to DC, the other of which from AD to BC.

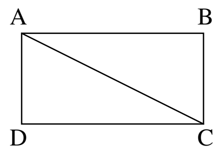


Fig. 5.40 (AT III, 344)

Fourthly, it should be shown in what way *two determined motions* – of which one is that of the body having the length AB moved perpendicularly to DC, the other being that of the body having the length AD moved laterally to BC – produce the *motion* of the body posited in A *determined* from A to C. Hence assuming AB to be carried perpendicularly to DC in one minute of time, and in the same way AD carried to BC [laterally] in the same minute of time, it follows that at the end of this given minute of time the body A is somewhere on CD and also somewhere on BC; it will hence be in C where BC and DC meet. And since AB, AD and A cover in the same minute of time the spaces AD, AB, AC, the velocities by which AB, AD, A are carried will be in the proportion of the lines AD, AB, AC.

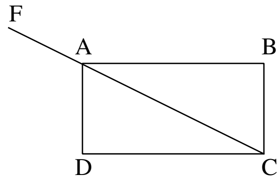


Fig. 5.41 (AT III, 345)

Fifthly, it should be noted [Fig. 5.41] that whether A is moved towards C by these two motors AB, AD, as by two winds, or by one motor only, say by a wind that blows from F, it will always be the very same motion carried out from A towards C and will always have the same properties.

Finally, since the two motions [Fig. 5.42] of the bodies AB and AD contribute [*conferunt*] by their velocities [*velocitatibus*] the velocity by which the one body A is moved, it should be considered what part of the speed [*celeritatis*] each of the motions contributes separately. It is clear, however, that the motion of body AB towards DC does not contribute all its velocity to the body A, nor does the motion of the body AD towards BC contribute all of its velocity; because one prevents the other from being able to proceed in the most direct path in which they started, one moving to DC, the other to BC. It should thus be asked, in what proportion the force of each, AB and AD, is diminished. Let the perpendicular DE be drawn from D to AC. I say that the *perpendicular motion* from AB *downwards* gives to the motion of the body A towards C as much speed as suffices to move this body towards C over a space as great as AE; and that the *lateral motion* of the body AD gives as much speed to the motion of the same body A as suffices to carry it over a space as great as EC. And since the speed by which AD is carried laterally is to the speed by which AB is carried perpendicularly, as the straight line AB to the straight line AD as was shown above, the straight lines AD and AB are to one another as AE and EC,[25] and the *lateral*

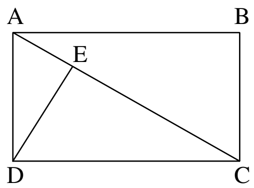


Fig. 5.42 (AT III, 346)

[25]Hobbes is mistaken here: the correct relation is not AD : AB = AE : EC but rather AD : AB = AE : DE = DE : EC; thus AE, DE, and EC are in continued proportion, and DE is the *mean proportional* between AE and EC. AE and EC are in the *double proportion* of AD and AB (or AE and DE); from this it follows that $AD^2 : AB^2 = AE : EC$.

speed of the body AD will be to the *perpendicular* speed of the body AB as EC to AE.[26] And, compounding, as the speed, both the lateral and the perpendicular together, is to the perpendicular speed alone, so, too, are the two straight lines AE and EC at the same time to the single straight line AE.[27] Since the speed which is at the same time both *lateral* and *perpendicular* moves body A through the space AC in one minute, the *perpendicular* speed alone suffices to move the body A in the same minute through precisely as much space as AE; by the same reason the *lateral* speed alone would suffice to move body A over precisely as much space as EC in the same minute [*eodem minuto secundo*]. And this is what I meant when I said that the speed of the body A towards C is compounded out of the two speeds AE and EC, undoubtedly diminished in their composition, and not of the entire speeds AD and AB.

And therefore, with this stated, I wanted to demonstrate, why Mr. Descartes inferred from my assertion a false consequence, namely, that in a motion compounded this way [Fig. 5.43] the absurd consequence follows that he had inferred. *Let us posit that the ball is carried to the right with one degree of speed and downwards with one degree of speed; it will reach B with two degrees of speed in the same time in which another, which is also carried to the right with one degree of speed and downwards with two, would reach G with three degrees of speed. From this it would follow that the line AB is to the line AG as 2 to 3, whereas it is as 2 to √10*. But from the demonstration just given above it follows clearly enough, I believe, that according to my principles the speed from A to B will not be to the speed from A to G as 2 to 3, but as √2 to √5, which is the proportion of the lines AB to AG themselves, and also the same as the ratio of 2 to √10. Or the speed from A to B is to the speed from A to G not as the composite of AH and HB to the composite of AH and HG, but as the subtenses AB and AG themselves, that is, as the roots of the sum of the squares of the sides. But I admit that the argument as I wanted to put it in my earlier letter to you dated Paris, February 7, is not correct. I do not at all defend my errors, least of all stubbornly. Unless Mr. Descartes does the same, I will surely be superior in morals. But as to the truth of the matter in dispute between us, what have I not shown sufficiently? What if, knowing the truth of some proposition of Euclid's *Elements* and attempting its demonstration, one does not succeed? Would it therefore be less true given that it has been proved either by others or by myself at some other time?

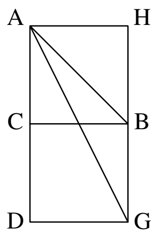


Fig. 5.43
(AT III, 347)

(AT III, 342-348) Oeuvres de Descartes (ed. by Ch. Adam and P. Tannery) Paris: Vrin, vol.3

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[26] AT and the original have mistakenly "AE to EC."

[27] If AB : AD were in fact equal to EC : AE, then according to a standard composition rule (cf. *De corpore*, II,13, §§19 and 28 for a formulation of the rule) it would be true that AB + AD : AD = EC + AE : AE.

5.2.15 Descartes: From a Letter to Mersenne for Hobbes (April 21, 1641)[28]

His last reflections about which you have written me are as poor as all the others that I have seen of his. For, in the first place, although man and Socrates are not different *supposita*, we nevertheless signify something different by the name of Socrates than by the name Man, namely the individual or particular differences. In the same way a determined motion is not at all different from a motion, but nevertheless the determination is something else than the motion.

Secondly, it is not true that the efficient cause of motion is also the efficient cause of the determination. For example, I throw a ball against a wall; the wall determines the ball to return towards me, but it is not the cause of its motion.

Thirdly, he makes use of a very shallow subtlety asking whether the determination is in the motion as in its subject; as if it were at issue here to know whether motion is a substance or an accident. For it is not at all unseeming or absurd to say that one accident is the subject of another accident, as when we say that quantity is the subject of other accidents. And when I said that the motion was to the determination of motion as the flat body is to its flatness or to its surface, I did not at all mean by this to make a comparison between motion and body as between two substances, but only as between two concrete things in order to show that they were different from those things that can be taken as abstractions.

Finally, his conclusion is very bad that when one determination is changed then the others have to change, too; because, says he, all these determinations are but one accident under different names. If this is so, then according to him it also follows that man and Socrates are but one thing under two different names; and consequently no individual difference of Socrates could be lost, for instance, the knowledge he had of philosophy, without at the same time his also ceasing to be a man.

What he says then, namely, that a motion has but one determination, is the same as if I said that an extended thing has but one figure only; and this does not prevent this figure from being able to be divided into a number of parts, as the determination also can be.

What he reproves about the *Dioptrics* (p. 18) shows only that he is merely looking for occasions for reproof, since he wants to attribute even the printer's errors to me. For at that place I talked about the double proportion [*proportion double*],[29] as the simplest case in order to explain it most expeditiously, since

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[28]This letter was published only in French by Clerselier but is said by him to be a translation ("voici la version"). The Latin edition of Descartes correspondence contained what is apparently a retranslation of the French into Latin; it was therefore not included in the AT edition. The original is lost.

[29]Hobbes had spoken of line HF (see Fig. 5.17) as being "double AH" in length. Although Descartes here actually says "double proportion," he obviously means

the true one cannot be determined because it changes in accord with the diversity of the subjects. But if in the figure the line HF was not made exactly double the line AH, this is the fault of the printer and not mine. And when he says it is contrary to experience, he is completely wrong, since in this matter experience varies according to the kind [*varieté*] of thing that is thrown into the water and according to the speed by which it is moved. And I did not take the trouble to correct the printer's error at this place because I easily believed that there would by no means be a reader so stupid as to be unable to understand that one line is double the other simply because the figure represented one that didn't have this proportion, nor [a reader] so unfair as to say that I deserved to be reproved for this.

(AT III, 354-56) Oeuvres de Descartes (ed. by Ch. Adam and P. Tannery) Paris: Vrin, vol.3

5.2.16 Descartes: From a Letter to Mersenne for Bourdin (July 29, 1640)

Whom will he persuade that, when I treated of reflection, I did not know that a mobile which tended in part downward [Fig. 5.44] when moving from A to B, afterwards tends upwards when reflected from B to F. And what verisimilitude would my reasoning have had, if I had denied this. However, I did not explain this change of determination from downwards to upwards, because it is clear enough of itself [*per se est satis nota*]; it follows from the fact that when a mobile hits the surface of a solid body perpendicularly it must also rebound perpendicularly; which no one, as far as I know, has ever called into doubt; nor is it my custom in such matters to linger with what is well known and easily grasped. And it would have been least appropriate at that place where I dealt with reflection in passing and in connection with refraction, in which no such change in determination to the contrary is ascertained.

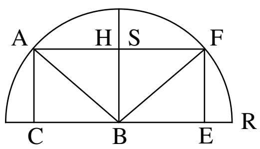

Fig. 5.44 (AT III, 107)

Here he quibbles again and attributes to me incorrect and clearly inept expressions; for it is not the *determination to the right* that *carries the mobile 4 handbreadths* (or *covers 4 handbreadths* as he says equally ineptly further down) but the force as determined to the right, nor can this be inferred from my words, as is seen on page 15 line 2[30] and in all other places where I have dealt with this

"twice the length" not "in the proportion of the squares." This may be the fault of the French translation.

[30]Page 15 of the *Dioptrics*; AT VI, 94; see above 5.2.4.

matter. I said that the determination brings it about not that the mobile moves 4 handbreadths nor that it simply moves, as if it were the cause of motion, but that it moves to the right since it is indeed the reason [*causa*] why the motion occurs towards the right. But the kernel of this quibbling was already introduced above. Saying that the mobile is carried *all the way to B*, he adds *or 5 handbreadths* (which I never wrote) and afterwards he said, *both partial determinations carry the mobile to B*, so that it seems to follow: *therefore 5 handbreadths*. But although, loosely [*improprie*] speaking, it can be said that *the determination carries the mobile to B*, in the sense that it is the cause why it goes in the direction of B, it cannot however be said that it carries the mobile *to B, that is, 5 handbreadths*, since it is not the reason [*causa*] why it goes such a distance. And I wonder that there is anyone in the world who does not blush at attributing to me such things during my lifetime and has no fear lest it be perceived to what extent he deliberately does not to seek the truth.

He indeed reveals to us a mighty secret: as if from the fact that I said that figure is to be distinguished from quantity, it is truly necessary for me to be shown that nonetheless the one is not separated from the other and that no extended body can be given which does not have both a quantity and a figure.

He complains that I did not make a mistake and that I did not get into the rut that he himself soon gets stuck in. It should be noted that the encounter with the surface CBE divides the determination into two parts but does not divide the force, nor is this surprising, since though a force cannot be without a determination, nonetheless, the same [component] determination can be joined to a greater or lesser force and the same force can remain though the determination changes in whatever manner. Thus, though a figure does not exist without a quantity, it can still be changed without the quantity's changing. And although the surface of a cube is divided into 6 square faces, the cube itself is not thereby divided into 6 parts, but rather the whole body cleaves to each of these faces and corresponds to it.

(AT III, 111-113) Oeuvres de Descartes (ed. by Ch. Adam and P. Tannery) Paris: Vrin, vol.3

5.2.17 Descartes: From a Letter to Mersenne (Aug. 30, 1640)

The principle which I assumed in my *Dioptrics* and which it seems that the quibblings of Father Bourdin have prevented you from noticing, is that the force of motion is not at all changed or diminished by reflection. From this it follows that the determination downwards must necessarily be succeeded by another determination upwards; and thus the ball cannot slide along the surface it arrives at, unless this surface is so soft that it greatly diminishes the motion; but it is not such surfaces that are being dealt with here, for reflection in such cases does not make an equal angle.

(AT III, 163)

5.2.18 Descartes: From a Letter to Mersenne (Dec. 3, 1640)

But this does not prevent the velitation of Father Bourdin from containing quibblings that were not invented solely by ignorance, but rather by some subtlety that I don't understand. And as for his enclosure, which you say consists in the [objection] that he cannot conceive how the water should not at all slow down the ball from left to right just as well as from up to down, it seems to me I had sufficiently foreseen it in so far as on page 18[31] I considered the refraction by a sheet to show that it is not at all done in the depths of the water but only at its surface. And I explicitly pointed out at the end of page 18, that one should consider only the direction to which the ball is determined when it enters the water because afterwards whatever the resistance of the water it cannot change its determination. Thus, for example if the ball which is impelled from A towards B, when it is at B, is determined by the surface CBE to go towards I whether there is air below the surface or whether there is water, this does not change its determination at all, but only its speed, which is diminished much more in water than in air. But I believe that what perplexes him is the word *determination*, which he wants to consider without any motion, which is chimerical and impossible; in speaking of the determination to the right, I mean all that part of the motion that is determined towards the right. Nonetheless I did not think I ought to mention motion there to avoid embarrassing the reader of this surprising calculation of the velitation, where he says that 3 and 4 are 5 and gives no word of explanation. For it can clearly enough be seen in what I have written that I have tried to avoid superfluous words.

(AT III, 250-251) Oeuvres de Descartes (ed. by Ch. Adam and P. Tannery) Paris: Vrin, vol.3

5.2.19 Descartes: From a Letter to Mersenne (April 26, 1643)

The first [principle] is that I assume no real qualities in nature, which would be attached to substance, like little souls to their bodies, and which could be separated by divine power; and thus I attribute no more reality to motion or to all other properties [*varietez*] of substance, which are called qualities, than philosophers commonly attribute to figure, which they do not call *qualitatem realem* but only *modum*. The principle reason for my rejection of the real qualities is that I do not see that the human mind in itself has any notion or particular idea to conceive them by. When mentioning them and maintaining that they exist, one mentions a thing that one does not conceive and does not understand oneself. The second reason is that philosophers only assumed these

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[31] Page 18 of the *Dioptrics*; AT VI, 98-99.

real qualities because they believed they could not otherwise explain all the phenomena of nature; I for my part have found that one can much better explain the phenomena without them.

The other principle is that everything that is, or exists, always remains in the state it is in unless some external cause changes it; thus I do not believe that there can be any quality or mode that perishes on its own accord. And just as a body that has some shape never loses it unless it is removed from it by the encounter with some other body, so, too, having a certain motion, it must retain it always unless some cause that comes from elsewhere hinders it. This I prove by metaphysics: for God, who is the author of all things, being completely perfect and immutable, it seems to me contradictory that any simple thing, which exists and consequently has God as its author, has within it the principle of its own destruction. And heat, sound, and other such qualities cause me no difficulty; for they are only motions in the air, where they meet various obstacles which make them stop.

Motion not being a *real quality* but only a *mode*, one cannot conceive that is something other than the change by which one body removes itself from some others, and there are but two properties [*varietez*] of it to be considered; the one is that it can be more or less swift; and the other that it can be determined towards different directions. For although the change can proceed from various causes, it is completely impossible if these causes determine it towards the same direction and make it equally swift, that they give it any diversity of nature […].

I also believe that it is impossible that a ball perfectly solid [*dure*], however large it may be, which encounters in a right line with a smaller one also perfectly solid can make it move along the same straight line faster than it moves itself. But I add that these two balls must encounter each other in a

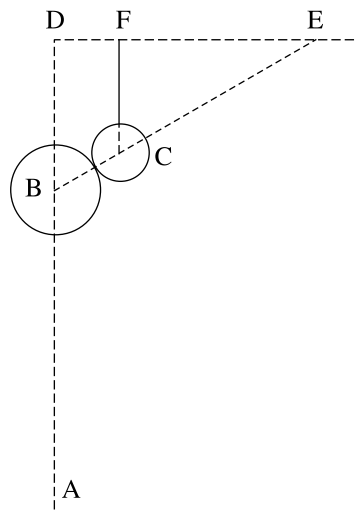

Fig. 5.45 (AT III, 652)

straight line, that is to say the centers of the one and the other must be in the same straight line along which the motion is made. Thus, for instance, if the large ball B coming from A to D in a straight line [Fig. 5.45] encounters the small ball C from the side, which it makes move towards E, there is no doubt whatsoever that even if these balls are perfectly solid, the little one ought to leave more quickly than the large one moves after having encountered it; and constructing the right angles ADE and CFE, the proportion which holds between the lines CF and CE is the same as holds between the speed of the balls B and C Note that I assume the centers of these balls to be in the same plane and thus that I do not imagine them to roll on the ground, but to encounter each other in free air. …

(AT III, 648-652) Oeuvres de Descartes (ed. by Ch. Adam and P. Tannery) Paris: Vrin, vol.3