Selected Correspondence of Descartes

René Descartes

[Brackets] enclose editorial explanations. Small ·dots· enclose material that has been added, but can be read as though it were part of the original text. Occasional •bullets, and also indenting of passages that are not quotations, are meant as aids to grasping the structure of a sentence or a thought. Every four-point ellipsis . . . . indicates the omission of a brief passage that has no philosophical interest, or that seems to present more difficulty than it is worth. (Where a letter opens with civilities and/or remarks about the postal system, the omission of this material is not marked by ellipses.) Longer omissions are reported between brackets in normal-sized type. —The letters between Descartes and Princess Elisabeth of Bohemia, omitted here, are presented elsewhere on this website (but see note on page 181).—This version is greatly indebted to CSMK [see Glossary] both for a good English translation to work from and for many explanatory notes, though most come from AT [see Glossary].—Descartes usually refers to others by title ('M.' for 'Monsieur' or 'Abbé' or 'Reverend Father' etc.); the present version omits most of these.—Although the material is selected mainly for its bearing on Descartes as a *philosopher*, glimpses are given of the colour and flavour of other sides of his life.

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Glossary

accident: Often used to mean 'non-essential property': your being more than 5' tall is an accident of you, whereas some philosophers would say that your having the power of thought is not. But quite often 'accident' is used just to mean 'property or quality', with no special emphasis on non-essentialness.

a priori, **a posteriori**: In Descartes's day these phrases were used to mark the difference between •seeing something happen and working out what will follow from it and •seeing something happen and working out what must have caused it, i.e. between •causally arguing forward and •causally arguing backwards; quite unlike Kant's use of the terms to mean •'independently of experience' and •'on the basis of experience'.

animal spirits: This stuff was supposed to be even more finely divided than air, able to move extremely fast and seep into tiny crevices. Descartes describes their formation on page 163.—Apparently some people thought of spirits as so rarefied as to be almost mind-like(!), and thus suitable to mediate between mind and body; but Descartes is innocent of this absurdity. Its most famous occurrence is in Donne's superb lines: 'As our blood labours to beget / Spirits as like souls as it can, / Because such fingers need to knit / The subtle knot that makes us man. . . .'.

art: Any human activity that involves techniques or rules of procedure.

AT: This refers either to *Œuvres de Descartes*, edited by Charles **A**dam and Paul **T**annery, or to Adam and Tannery themselves.

beg the question: Until fairly recently, to 'beg the question' was to offer a 'proof' of P from premises that include P. It now means 'raise the question'. It seems that complacently illiterate journalists (of whom there are many) encountered the phrase, liked it, guessed at its meaning, and saw no reason to check on the guess.

burning mirror: A concave mirror which can reflect the suns ray to a point, creating enough heat there to start a fire.

catoptrics: The part of optics that deals with reflections.

chimera: A chimera can be a fabulous beast or monster, or a thought or idea of image of something fantastic, fabulous, etc. In Descartes's usage it is always the second meaning that is at work.

circular: Descartes holds that all motion is in a closed loop (despite his always calling it 'circular', he has no views about its shape). His reason for the loop thesis is this: Absolutely all space is full of extended substance(s), there are no gaps; and no material substance can shrink, or expand, or spatially overlap another material substance. Therefore, if body b_1 is to move from location L_1, it must shove aside body b_2, which must shove aside b_3. . . and so on; so if an infinite chain of movements is to be avoided, somewhere along the way there must be body b_n which is pushed into location L_1, thus closing the loop. (It has to be instantaneous: L_1 mustn't be empty for a split second between the departure of b_1 and the arrival of b_n.)

common notion: In Descartes's usage, a 'common notion' is a really basic elementary logical truth.

common sense: The phrase '*the* common sense' was the name of a supposed faculty or organ or brain-region where inputs from the various senses are processed together and united.

concurrence: God's concurrence in an event is his going along with it, in some (supposed) sense that is weaker than •his outright causing it but stronger than •his merely not preventing it.

CSMK: This is volume 3 of *The Philosophical Works of Descartes*, translated by John **C**ottingham, Robert **S**toothoff, Dugald **M**urdoch, and Anthony **K**enny.

doctor: Learned man.

efficient cause: This is an Aristotelian technical term. The •formal cause of a coin is its design, the plan according to which it was made; its •material cause is the stuff it is made of; its •final cause is its purpose, namely to be used in commerce; and its •**efficient cause** is the action of the die in stamping the coin out of a metal sheet. So the efficient cause is what you and I would call, simply, 'the cause'.

eminently, formally: These are scholastic technical terms that Descartes adopts for his own purposes. To say that something has (say) intelligence 'formally' is just to say that it is intelligent; to say that it has intelligence 'eminently' is to say that it has intelligence in some higher form that doesn't involve its being straightforwardly intelligent. The distinction comes into play through the doctrine that whatever is present in an effect is also present in its cause. Obviously something can be caused to be rigid by a cause that isn't itself rigid; and God presumably doesn't straightforwardly have many of the qualities he causes other things to have—he isn't square or muddy or (for that matter) given to telling bad jokes. So the doctrine takes the form 'Whatever is present in an effect is

also present *formally or eminently* in its cause. Descartes's only explanation of this terminology is to say that 'x has Fness eminently' means 'x has the power to cause things to have Fness', which you'll notice turns the doctrine into a triviality.

de volonté: Descartes repeatedly associates rationally loving x with joining oneself *de volonté* with x. This doesn't mean joining oneself voluntarily, by volition [*volonté*]; it is a technical term, which he explains on page 191 where he equates 'x joins itself to y *de volonté*' with 'x considers itself and y as forming two parts of a single whole'. A bit less abruptly, you join yourself *de volonté* with the person you love if you *will yourself into a state in which you feel as though* you and that person are the two parts of a single whole.

ens per accidens, per se: A pyramid is a collection of stone blocks that constitute an *ens per accidens* = an entity by happenstance. It just happens to be the case that they are inter-related in a way that makes them a pyramid, a thing, an *ens*. They don't have any features that intrinsically draw them together, somehow making them *belong* together as a single entity; that would be an *ens per se.*

heaven: Sometimes Descartes uses 'the heavens', as we still sometimes do, to mean 'the whole visible universe outside the earth'. But in the *Principles of Philosophy* and some of his letters 'heaven' occurs as a technical term referring to any large spherical mass of rotating fluid material with a star or planet at its centre. The earth, he says, 'is completely immersed in a very fluid heaven'.

indifferent: A situation where your will is 'indifferent' with respect to your doing A is a situation where you are under no external pressure to do A and none to refrain from doing A. For finer tuning, see page 175.

ineffable: Too great to be fully described in words. (The antonym 'effable' occurs these days only in jokes.)

inform: When Descartes says that your body is 'informed' by your soul, he means only that your body *has* that soul, is united with it in the standard body-soul manner. It's odd that he uses this verb in this way: it echoes the Aristotelian doctrine that your soul is *the form of your body*; and that doctrine, whatever it means, is denied by Descartes's thesis that your body is one substance and your soul is another.

interpenetration of dimensions: Descartes holds that it impossible for two distinct •portions of matter to overlap spatially: for any two such items, the volume of them both is the sum of the volumes of each separately. For him this is equivalent to saying that two distinct •regions of space can't overlap; and he expresses by saying that he rejects the 'interpenetration of dimensions'.

metempsychosis: The movement of a soul from one body to another.

mœurs: A person's *mœurs* includes his morality, his basic habits, his attitudes and expectations about how people will behave, his ideas about what is decent. . . and so on. This word—rhyming approximately with 'worse'—is left untranslated because there's no good English equivalent to it.

moral certainty: A degree of certainty that is high enough for practical purposes, high enough to make practical doubt unreasonable; similarly with **morally impossible**. (In this phrase 'moral' is used in its old sense of 'having to do with human behaviour'.)

natural light: If you know something to be true just by thinking hard about it in the right way, Descartes will say that you know it 'by the natural light'.

numerical identity: To say that x is numerically identical with y means simply that x is y, which is equivalent to saying that x and y are *one*—that's how 'numerical(ly)' comes into it. Why have any adjective or adverb in these contexts? Because the writer thinks that the reader might take the unvarnished 'identity' to refer to some kind of mere similarity.

objective: When Descartes speaks of the 'objective being' of an idea he is referring to its representative content, the being that is its object, the item that it is about.

parhelia: Two bright patches flanking the sun, sometimes called 'false suns'.

passion: When Descartes speaks of 'passions' that people and other animals have, he using the word in about the same sense as we do. Outside the animal context the word is the antonym of 'action': action/passion = doing/undergoing.

Pelagian: Follower of Pelagius, a 4th-century theologian whose stress on the role of human effort as a means to salvation was thought by many to push divine grace out of the picture.

pineal gland: This is the current name for the gland that Descartes always refers to as 'the gland called "the conarium"'.

prejudice: This translates the French *préjugé* and the Latin *præjudicium*. These basically mean 'something judged or believed in advance' (of the present investigation, of the evidence, or of etc.). These days 'prejudice' usually has the narrower meaning of 'something pre-judged concerning race, sex, etc.'. To avoid that taint, CSMK uses 'preconceived opinion' (7 syllables); the present text will use 'prejudice' (3 syllables) accompanied by this warning.

princess: When Descartes speaks of *Queen* Christina as a princess he is following a usage that used to be fairly common for 'prince' (and its cognates in French and Latin), namely as standing for any ruler of a state, whether a king or queen or duke or count etc.

principle: In Descartes's writings a *principe* (French) or *principium* (Latin) is often a certain kind of universal proposition—e.g. in the title standardly translated as *Principles of Philosophy*. But he sometimes uses one of these words in a sense, once common but now obsolete, in which it means 'source', 'cause', 'driver', 'energiser', or the like (see pages 23 and 215). The English 'principle' also had that sense; Hume's *Enquiry Concerning the Principles of Morals* is, he tells us, an enquiry into the *sources in human nature* of our moral thinking and feeling.

privation: A privation in x is x's not having something that it ought to have. If a person can't speak, that is a privation in him; a rock's lack of the ability to speak is not a privation in it but a mere **negation**.

rarefied: In early modern times, 'rare' and the French *rare* meant the opposite of 'dense', and was usually understood to mean 'very finely divided'.

real quality, **real accident:** These phrases use 'real' in its old sense of 'thing-like' (from Latin *res* = 'thing'). The core thought is this: if heat, for example, is a 'real quality' or 'real accident', then any instance of heat can be thought of independently of anything's *having* it. When a thing x comes to be hot, what happens is that it comes to *have* a real quality, a particular instance of heat. Descartes rejects this, and holds that predicative propositions should be thought of as having the form 'x is-hot' rather than 'x relates-by-possession-to hotness'. When on page 158

Descartes says that he doesn't credit *motion* with any more reality than is generally attributed to *shape*, he means that philosophers generally wouldn't speak of a ball's being round as a result of a thing-like instance of roundness that the ball possesses; and he says that the same goes for the ball's being in motion.

reflection, **refraction:** How light bounces off a mirror, how light *tilts* as it enters a translucent medium. The problem with refraction was to get a sound general account of how the angle at which the light meets the surface of the translucent body [incidence] relates to the angle at which it carries on from there [refraction]. This could involve light going from air into glass or from glass into air; this problem was central to the making of optical lenses,

reminiscence: Plato's doctrine that things you know without having learned them from experience or from other people are things you *remember* from a previous life when the soul you now have was joined to a different body.

School: The 'Schools' were philosophy departments that were almost entirely under Aristotle's influence, as mediated by Roman Catholic philosophers and theologians.

science: In early modern times the English word 'science', the French *science* and the Latin *scientia* applied to any body of knowledge or theory that is (perhaps) axiomatised and (certainly) well founded and conceptually highly organised.

sensible: Translating French *sensible* and Latin *sensibilis*, this usually means 'capable of being sensed', i.e. '. . . of being perceived through the senses'. But on page 217 and perhaps elsewhere, Descartes uses 'sensible quality' to refer to what are commonly called the 'secondary qualities' such as colour, smell, sound, etc. and *not* including shape and size, though these are perceptible by the senses.

soul: This translates *âme*. It doesn't obviously mean any-thing different from *esprit* = 'mind', and has no theological implications.

species: When on page 103 Descartes speaks of 'the species that enter the eyes' etc. he is using the language of a theory of Aristotle's that he doesn't actually believe. According to this theory, when you see a kitten a tiny *representation of a kitten* enters your eyes, and this **representative something-or-other** is called a 'sensible species'. All Descartes needs from this on page 103—and presumably all he intends—is to speak of eyesight as involving a **something-or-other** entering your eyes.

speculative: This means 'having to do with non-moral propositions'. Ethics is a 'practical' discipline, chemistry is a 'speculative' one.

substantial form: When Descartes first **uses** this term here, on page 25, it is not clear what he means by it. In many other places—e.g. on pages 75 and 136—he merely **mentions** it as an item in false Aristotelian metaphysics. In his letter to Regius on January 1642—starting on page 148—he says that he isn't denying that there are substantial forms but merely saying that he can do (meta)physics without them.

subtle: When Descartes speaks of some matter as 'subtle', he means that it is *extremely* finely divided, more fluid than water; and he usually thinks of the ultra-tiny particles composing it as moving very fast.

transubstantiation: The doctrine that in the Eucharist the bread comes to be part of the *substance* of Christ's body although it still has the *qualities* of mere bread.

violent: Aristotle divided motions into 'natural' and 'violent': the movement to the ground of a dropped pebble is natural, its upward movement when you throw it up is 'violent'.

Thus when on page 57 Descartes rejects the natural/violent distinction, he is rejecting Mersenne's apparent assumption that some states of water are natural and others are not (though he would hardly say that the others are 'violent').

vivid: This belongs to the pair
        'vivid' and 'clear',
which translates the Latin
        clarus and *distinctus*
and the French
        clair and *distinct*.
Every other English translator has put
        'clear' and 'distinct'
but this is certainly wrong. The crucial point concerns *clarus* (and the French *clair*). The word can mean 'clear' in our sense, and when Descartes uses it **outside** the *clarus et distinctus* phrase, it seems usually to be in that sense. But **in** that phrase he uses *clarus* in its other meaning—its more common meaning in Latin—of 'bright' or 'vivid', as in *clara lux* = 'broad daylight'. If in the phrase *clarus et distinctus* Descartes meant *clarus* in its meaning of 'clear', then what's left for 'distinctus' to mean? Descartes's only explanation of these terms is in *Principles of Philosophy* 1:45–6, a passage that completely condemns the usual translation. He writes: 'I call a perception *claram* when it is present and accessible to the attentive mind—just as we say that we see something *clare* when it is present to the eye's gaze and stimulates it with enough strength and accessibility. I call a perception *distinctam* if, as well as being *clara*, it is so sharply separated from all other perceptions that every part of it is *clarum*. . . . A perception can be *clara* without being *distincta* but not vice versa. When someone feels an intense pain, his perception of it is *clarissima*, but it isn't always *distincta* because people often get this perception muddled with an obscure judgment

they make about something they think exists in the painful spot. . . .' and so on. He can't be saying anything as stupid as that intense pain is always *extremely clear*! His point is that pain is vivid, up-front, not shady or obscure. And for an idea to be *distincta* is for every nook and cranny of it to be vivid, i.e. for it as a whole to be in our sense 'clear'.—Sometimes when *clair* and *distinct* occur together, the traditional translation is forced on us because *distinct* is used as a relational term rather than a one-place predicate;

there's an example of this on page 137, where notions are spoken of as *claires* and *distinctes les unes des autres*—clear and distinct *from one another*.

we: Sometimes when this version has Descartes speaking of what 'we' may do, he has written of what 'one' may do. It is normal idiomatic French to use *on* = 'one' much oftener than we can use 'one' in English without sounding stilted. He often slides from *on* to *nous*, clearly not intending any distinction; for example, paragraph (i) on page 66.

Letters written in 1619–1637

to Beeckman, 26.iii.1619:

. . . .In the past six days. . . .I have been working more dili-gently than ever before. In that short time, with the aid of my pair of compasses, I have discovered four remarkable and completely new demonstrations.

The first concerns the famous problem of dividing an angle into any number of equal parts. The other three have to do with three sorts of cubic equations:

(1) equations between a whole number, a root number and a cube root—[equations of the sort $\pm a \pm bx = x^3$];

(2) equations between a whole number, a square root, and a cube root—[equations of the sort $\pm a \pm bx^2 = x^3$];

(3) equations between a whole number, a root number, a square root and a cube root—[equations of the sort: $\pm a \pm bx \pm cx^2 = x^3$].

I have found three sorts of demonstrations for these three sorts of equations, each of which has to be applied to different terms owing to the difference between the signs + and -. My account of this is not yet complete, but what I have found to apply in one case can easily be extended to others. It will thus be possible to solve four times as many problems, and much more difficult ones, than was possible by means of ordinary algebra. . . . Another thing I'm investigating at present is the extraction of roots consisting of many different terms. If I find out how to do this, as I hope I shall, I'll really put this science in order, if only •I overcome my natural idleness and •fate gives me the freedom to live as I choose.

Let me be quite open with you about my project. What I want to produce is not something like Lull's *Ars Brevis*, [a fourteenth-century work purporting to provide a universal method of solving problems], but rather a completely new science that would provide a general solution of all possible equations in-volving any sort of quantity, whether continuous or discrete. The solutions would be different depending on the nature of the equation: in arithmetic, for example, some problems can be solved by means of rational numbers, while others require irrational numbers, and others again we can only imagine how to solve but can't actually solve. So I hope I shall be able to demonstrate that certain problems involving continuous quantities can be solved only by means of straight lines or circles, while others can be solved only by means of curves produced by a single motion, such as the curves that can be drawn with the new compasses (which I think are just as exact and geometrical as those drawn with ordinary compasses), and others still can be solved only by means of curves generated by distinct independent motions which are surely only imaginary, such as the notorious quadratic curve [a curved line discovered by Hippias in the first century BCE; called 'quadratic' because it was used in attempts to square the circle.] With lines such as these available, I think, every imaginable problem can be solved. I'm hoping to demonstrate what sorts of problems can be solved exclusively in this or that way, so that almost nothing in geometry will remain to be discovered. This vast task is hardly suitable for one person; indeed it's an incredibly ambitious project. But I have glimpsed a ray of light through the confusing darkness of this science, and I think I'll be able with its help to dispel even the thickest obscurities. . . .

After I left Middelburg I reflected also on your art of navigation, and discovered a method for working out, simply

by observing the stars, how many degrees east or west I had travelled from some place I knew, no matter where on earth it might be, or whether I had been asleep during the journey and had no idea how long I had slept. It is hardly a subtle discovery, and I can hardly believe that no-one has made it before now. I suspect that it has been neglected because of the difficulty of applying it; for in order to make the measurement we would need an instrument thirty times as exact as the instruments currently used to work out the height of the pole star; so the measurement couldn't be very exact, although astronomers do measure ·angular· minutes and seconds, and even much smaller intervals, with existing instruments. But if that is the only drawback with it, I would be very surprised if sailors thought it such a useless discovery. So I would like to know for sure whether or not a similar discovery has been made before. [It had.] If you know of any such, write and tell me about it. It is still a confused speculation in my head, but I would work it out more exactly if I suspected it was as novel as it was certain. . . .

to Beeckman, 23.iv.1619:

Your letter reached me almost on the same day you sent it. I didn't want to leave here—[Breda, Holland]—without writing to you once more, to keep up what will surely be a lasting friendship between us. But don't expect anything from me at the moment, for while I am preparing for tomorrow's departure my mind has already started traveling. I am still uncertain 'where fate may take me, where my foot may rest' [from Virgil's *Aeneid*]. The preparations for war haven't yet led to my being summoned to Germany, but I suspect that many men will be called to arms, though there will be no outright fighting. If that state of affairs continues, I'll travel around in Denmark, Poland and Hungary until I can find a safer route—one not occupied by marauding soldiers—or until I have definitely heard that war is likely to be waged. If I stop somewhere, as I expect to, I promise to see to it that my *Geometry* is put in order, and I will salute you as the promoter and prime author of my studies.

For it was you alone who roused me from my state of idleness, and reawakened the learning which by then had almost disappeared from my memory; and when my mind strayed from serious pursuits it was you who led it back to worthier things. Thus, if I happen to produce something that has some merit you can rightly claim it all as your own; and I'll send it to you without fail, so that you can use it—and check it for errors. That's what I was doing the other day when I sent you a piece on navigation. You must have read my thoughts! for you were sending me the exact same thing—your discovery about the moon is the same as mine. I did think that the method ·of using the moon to fix one's location· could be made easier with the aid of certain instruments; but I was wrong about that.

As for the other things I boasted of having discovered, I really did discover these with the help of the new compasses—I'm not wrong about *that*. But I shan't send these to you piecemeal, because I'm thinking of writing a complete work on the subject some day—a work that I think will be new and pretty good. For the last month I have set my studies aside because my mind was so worn out by these discoveries that I hadn't the strength to discover the other things in this area that I had planned to investigate. But I have the strength to keep my memory of you ever fresh.

[Those closing words are a sample of something Descartes does often, namely to word his (polite or friendly) signing-off in a way that links it with what has just gone before. Most of these signings-off are omitted from the present version.]

to Beeckman, **29.iv.1619:**

I don't want to miss any opportunity of writing to you and demonstrating my affection for you and my remembrance of you—not dimmed amid all the fuss of travel.

Three days ago I had a conversation about Lull's *Ars Brevis* with a learned man whom I met in an inn at Dordrecht. He was a loquacious old fellow, who kept his rather bookish learning not so much in his brain as on the tip of his tongue. He boasted that he could apply Lull's method, doing it so skillfully that he could talk for a whole hour on any subject I cared to mention; and if he was required to talk for another hour on the same topic he would find fresh things to say, and could go on with this for twenty-four hours at a stretch. Should you believe him? See for yourself!

I questioned him carefully, to see whether his method consists in arranging dialectical headings in a certain way and deriving arguments from them. He said that it does, but he added that Lull in his writings hadn't supplied certain 'keys' that 'are essential' for revealing the secrets of the method. In saying this, I suspect, he was trying to impress an ignorant listener rather than to speak the truth.

I'd be happy to go on about this if I had the book; but since you do have it, please look into it when you have time and tell me whether there's any intellectual substance in that 'method'. I'm so sure of your intelligence that I'm certain that you'll easily see what is missing in the way of so-called 'keys' that are essential for understanding the rest. I'm writing to you about this so as not to miss an opportunity to discuss a learned question with you, which is what you asked for. When I ask for the same thing from you, please don't go to too much trouble.

Today I set off for Denmark. I'll spend some time in Copenhagen, where I hope to have a letter from you. Ships leave here for that city every day. You don't know where I'll be staying; but I'll shall make a point of inquiring among the sailors whether they have a letter for me; so it is not likely to go astray. . . .

⊕ [Between 1622 and 1626, two letters from Descartes to his father, and three to his older brother.]

⊕ [18.vi.29: A very friendly letter to Ferrier, enthusiastic about his researches on telescopic lenses, and inviting him—firmly and with convincing detail—to leave Paris and come and live with Descartes.]

to Gibieuf, **18.vii.1629:**

Ferrier will give me some news, and I don't expect you to take the trouble to do that; but I do expect to put you to some trouble when I complete a little treatise that I am starting. I wouldn't have told you it was under way if I hadn't been afraid that it will take so long to finish (more than three years, I expect) that you'll forget your promise to correct it and give it the finishing touches. I may eventually decide to burn it, or at least to keep it within the circle of my friends until I have carefully reconsidered it. If I am not clever enough to produce something worthwhile, I'll try at least to have the good sense not to publish my shortcomings.

to Mersenne, **8.x.1629:**

I don't think I was so uncivil as to ask you not to ask me any more questions. I am honoured by your taking the trouble to send them to me, and I learn more from them than I do from any other sort of study. But of course I should have said: 'Forgive me if I don't make the effort to reply as precisely as I would try to do if I weren't wholly taken up with other thoughts.' My mind is not so strong that I can tackle many tasks at once; I never make any discoveries

except through a long sequence of thoughts, so I have to devote myself •wholly to a topic when I want to investigate some •part of it. I had experience of this recently when I was investigating the cause of the phenomenon that you write about in your letter [parhelia, see Glossary]. About two months ago a friend showed me a full description of the phenomenon and asked me what I thought of it. I couldn't answer this in a way that satisfied me until I had interrupted my current work and made a systematic study of the whole of meteorology. But I think I can now give some explanation of the phenomenon. I have decided to write a little treatise on the topic [namely the future *Meteorology*, published along with the *Discourse on the Method* in 1639.]; this will explain •the colours of the rainbow (a topic that has given me more trouble than any other), and •all terrestrial phenomena in general. That's why I asked you for a description of the phenomenon at Rome in particular; I wanted to know whether it agreed with the description I had seen. There was this difference: you say that the phenomenon had been seen at Tivoli, whereas the other account says that it was seen at Frascati. Please tell me whether you know for sure that it did appear at Tivoli. . . . There's no need to hurry; I haven't yet begun to write.

Please don't speak to anyone about this, because I have decided that when I publish this specimen of my philosophy I'll do so anonymously, so as to hear what people say about it. It's one of the most beautiful subjects I could choose, and I'll try to expound it in such a way that it will be a pleasure to read for those who understand only Latin. [He later decided to write it in French.] I would prefer it to be printed in Paris rather than here ·in Amsterdam·; if it wouldn't be asking too much, I'll send it to you when it is finished, so that you can correct it and place it with a publisher. . . .

[The letter now touches on the following topics: The news that Mersenne put some questions to Beeckman and is offended by Beeckman's responses; a request for suggestions about where Ferrier might get financial support to continue with his important research on the making of lenses for telescopes; questions in geometry and musical theory. Then:] As for your other question ·about the movements of a pendulum·, I needed a long time to think about this, for there are many different forces to take into account. Firstly, take the case where

> the pendulum's weight is in a vacuum where there's no air to slow it down,

and take it as given that

> by doubling the force on it we can halve the time it takes to travel the same path;

then the calculation I made earlier is as follows. If the cord is 1 foot long and it takes the weight 1 second to from a certain point A in its swing back to its mid-point B, it will take $\frac{4}{3}$ seconds when the cord is 2 feet long; if the cord is 4 feet long, it will take $\frac{16}{9}$ seconds; if 8 feet long, $\frac{64}{27}$ seconds; if 16 feet long, $\frac{256}{81}$ seconds (which is not much more than 3 seconds), and so on in due order. [Descartes adds that he isn't offering an account of a related aspect of pendulum-movement because it involves some extremely difficult calculations.]

⊕ [8.x.29: Descartes writes to Ferrier expressing some optimism about his chances of getting research support, and giving much advice about how to improve them. Then five pages on technical aspects of lens-making.]

⊕ [26.x.29: Ferrier replies to Descartes, with fervent gratitude for his support and interest; and then, having been invited to do this, he offers a dozen pages on his research, and the engineering and mathematical problems it involves.]

⊕ [13.xi.29: Descartes to Ferrier. Pleasure at the trouble Ferrier has taken; then about fifteen pages of technical stuff; then 'If there's anything

in this that you don't understand, tell me and I won't grudge the paper I'll need for a reply'; and finally 'If you had a year or two to settle all this, I'd make so bold as to suggest that your work will enable us to answer the question "Are there animals on the moon"?']

to Mersenne, 13.xi.1629:

I'm sorry you have been put to a lot of trouble in sending me your description of parhelia, for what you saw is just like the one I had seen. Still, I'm indebted to you for this, and even more for your offer to see to the printing of the little treatise I'm planning to write—but I should tell you that it won't be ready for over a year. Since I wrote to you a month ago, all I have written is an outline of the contents. Rather than explaining just one phenomenon, I now plan to explain all the phenomena of nature, i.e. the whole of physics. [This projected larger work was to become *The World or Treatise on Light*, which Descartes refers to as his 'Physics' (later on he refers to his *Principles of Philosophy* in that way.)] I like my present plan better than any other I have ever had, for I think I see how to expound my thoughts in a way that some will find satisfying and others won't have any reason to reject.

[A paragraph commenting on the supposed discovery that Gaudey had made in geometry. While contending that it was neither as new or as useful as Mersenne had thought it was, Descartes has good words about it. Then:]

You ask for the basis of my calculation of how long it takes the pendulum-weight to fall [i.e. to get from a given point to the mid-point of its swing] when the cord supporting it has this or that length. I'll have to deal with this in my Physics, but you shouldn't have to wait for that; so I shall try to explain it here. I start with the assumption ·of the law of inertia, which says· that a body that is caused to move will keep moving in the same way for ever unless some other cause

goes to work on it. In other words, in a vacuum a moving thing keeps on moving at the same speed. Thus, suppose that an unsupported weight is falling to the earth at a certain velocity at time t. If it lost its heaviness at t, it would stay at that velocity throughout the rest of its fall. But of course that's not what happens; it keeps its heaviness, which gives it at each moment a new force pushing it down. So at each moment in its downward journey it moves faster than it did just before, because it still has the impetus it brought with it into that moment and an additional impetus—an additional downward push—because of its heaviness. [Having explained •why falling bodies accelerate, Descartes goes on to explain •the rate of acceleration; but we can afford to by-pass that because AT [see Glossary] point out that the explanation fails because it involves Descartes in misunderstanding the diagram that he uses.]

. . . .You asked me for a precise account of how much the air resists the movements of bodies through it; but there is no answer to that. How much a given environment of air resists the movement of a given body varies greatly, depending on whether

the air is hot or cold, dry or wet, clear or cloudy,

and on whether

the body is made of lead or iron or wood, is round or square or some other shape,

and hosts of other factors. And this applies to all the questions you raise about air resistance.

[The next topic concerns the vibrations in a taut string that is plucked. (It's not clear what Mersenne's question about this was.) Descartes's reply is accompanied by a diagram, but we can do without it. What matters is this: The taut unplucked string is on a straight line; then it is on a curve on one side of that line followed by a curve on the other side; at the middle of the string each curve will be at

the maximum distance from the straight line; add these two maximum distances for a given back-and-forth motion of the string, and call that distance D. The value of D will of course decrease as the vibration goes on. Now hear Descartes:]

In a vacuum the distance D decreases in geometrical proportion. That is to say, if D is 4 units at the first motion and 2 at the second, it will be only 1 at the third motion; and if it is 9 at the first motion and 6 at the second, it will be 4 at the third, and so on. If the vibrations all take the same length of time, their speed will decrease in proportion as D decreases. I say 'in a vacuum', for in air I believe that the motions will be a little slower towards the end than they were at the beginning, because the motion will have less force then and thus won't so easily overcome the air resistance. But I'm not sure about this; perhaps on the contrary the air even aids the motion at the end, since the motion is circular [see Glossary]. But you can test this by ear. Check whether sound of a plucked cord is sharper or flatter at the end than at the beginning: for if it is flatter that's because the air is slowing it down; if it is sharper that's because the air is making it move more quickly.

[This letter is incomplete. It now tails off in the middle of a sentence mentioning further questions about vibrating strings.]

to Mersenne, 20.xi.1629:

This proposal for a new language seems more remarkable at first than I find it to be when I look more closely. There are only two things to learn in any language: the meanings of the words and the grammar. As for the meanings of the words, •the proposer doesn't offer anything specific; in his fourth proposition he says 'the language is to be translated with the aid of a dictionary', and anyone who knows a bit about languages can do *that* in any common language without •his help! I'm sure that if you gave Claude Hardy—·who is said to know thirty-six oriental languages·—a good dictionary of Chinese or any other language, and a book in the same language, he would undertake to work out its meaning.

Not everyone could do the same, because of the difficulty of the grammar. That, I imagine, is your proposer's whole secret; but there's nothing difficult in it. If you make a language

•with only one pattern of conjugation, declension and word-construction, and

•with no defective or irregular verbs introduced by corrupt usage, and

•with nouns and verbs that are inflected, and sentences that are constructed by prefixes or suffixes attached to the primitive words, and

•with all the prefixes and suffixes listed in the dictionary,

it's not surprising if ordinary people learn to write the language, with the help of a dictionary, in less than six hours. That's all his first proposition says.

The second says 'once this language has been learned, the others can be learned as dialects of it'. This is just sales talk. He doesn't say how long it would take to learn them, but only that they could be regarded as dialects of his language, which he takes as basic because it doesn't have the grammatical irregularities of the others. Notice that in his dictionary he could handle each primitive word by bringing in synonyms of it from all the other languages. To signify *love*, for instance, he could use *aimer*, *amare*, $\phi\iota\lambda\epsilon\iota\nu$, and so on; a Frenchman, adding to *aimer* the affix for a noun, will form the noun *amour*, a Greek will do the same with $\phi\iota\lambda\epsilon\iota\nu$, and so on. So it's easy to see what is going on in his sixth proposition, about 'inventing a script'. For if

he put into his dictionary a single symbol corresponding to *aimer*, *amare*, and each of the synonyms, a book written in such symbols could be translated by everyone who had the dictionary.

The fifth proposition also strikes me as mere advertising. As soon as I see the word 'mystery' in any proposition I begin to suspect it. But all he means, I think, is that because he has thought hard about the grammars of other languages in order to simplify his own he can teach them more easily than the average instructor.

There remains the third proposition, which is a total *mystery* to me. He says he will expound the thoughts of the ancient writers going by the words they used, while taking each word as expressing the true definition of the thing the word refers to. To put it plainly: he will expound the thoughts of these writers while giving their words a sense they never gave them themselves; which is absurd. But perhaps he means it differently.

Now this plan of reforming our grammar, or rather inventing a new one to be learned in five or six hours and applicable to all languages, would be useful if everyone agreed to adopt it—except for two difficulties I can see standing in the way.

(1) The discordant combinations of letters would often make the sounds unpleasant and intolerable to the ear. Why has common usage led to words' being inflected differently in different languages? Solely in order to remedy this defect. Your author, with his single grammar for all the languages, has no such remedy; for what is easy and pleasant in our language is coarse and intolerable to Germans, and so on. The most he could do is to avoid discordant combinations of syllables in one or two languages; and so his 'universal language' would do only for one country. But we ·Frenchmen· don't need to learn a new language to talk only to Frenchmen!

(2) There will be trouble over learning the words of this ·supposed 'new'· language. If each person uses as primitives the words of his own language he won't have much difficulty except that he'll be understood only by the people of his own country; if he is to be understood by foreigners he'll have to *write* what he wants to say, so that the foreigner can look up all the words in the dictionary; and this is too burdensome to become a regular practice. If your man wants people to learn primitive words that are common to every language, he won't find anyone willing to take the trouble. It would be easier to get everyone to agree to learn Latin or some other language in current use than to get them to learn this new one that doesn't yet have •books for practice in reading or •speakers for practice in conversation. If this invented language is to do any good, it seems to me, it would have to be in connection with writing. Suppose I had a big dictionary for all the languages in which I wanted to be understood, and put for each primitive word a symbol—e.g. a single symbol for *aimer*, *amare* and $\phi\iota\lambda\epsilon\iota\nu$: then those who had my dictionary and knew my grammar could translate what I wrote into their own language by looking symbols one by one. But no-one who had anything better to do would take this much trouble. . . . So I don't see that all this has much use. Perhaps I am wrong; I just wanted to write to you all I could conjecture on the basis of the six propositions that you sent me. When you have seen the system, you'll be able to say if I worked it out correctly.

I believe, though, that a system could be devised for constructing a universal language—a system of primitive words and associated symbols—that could be taught very quickly. The crucial thing is *order*—the order in which thoughts enter the human mind. Think about the natural order of the numbers: in a single day one can learn to name every one of the infinite series of numbers, and I can use this

to name a given number to someone who doesn't understand any language that I know; I can write (say) '271' and direct the minds of a Serb, an Eskimo, and a Mongolian to that number although I haven't the faintest idea what *words* their languages use to name it. Well, the same could ·in theory· be done for all the other words needed to express all the other things that the human mind is confronted by. If this ordering were discovered, I'm sure that the language would soon spread throughout the world. Many people would willingly devote five or six days to learning how to make themselves understood by the whole human race.

[Two remarks on the above paragraph: •The sentence about numbers is a rather free rendering of what Descartes wrote, but it's true to his intent. •Descartes's proposal assumes that any thought that is complex enough to be the content of a whole sentence

   (1) consists of a number of simpler thoughts,
   (2) which go through the thinker's mind in an ordered series,
   (3) the order being *natural*, and thus the same for everyone, no matter what language(s) he understands, and
   (4) the order being systematic in a way that would let it be learned as the system of numerals can be learned.

Most language-theorists these days would regard **(1)** as dubious; and all would reject **(2)** outright, thus making **(3)** an answer to a question that doesn't arise. And it's hard to believe that Descartes really accepted **(4)**.]

I don't think that your author has thought of this. It isn't suggested by anything in his propositions, and anyway the discovery of this language depends upon the true philosophy [here = 'psychology']; for we need that if we are •to number and order all the thoughts of men or even merely •to separate them out into clear and simple thoughts, which in my opinion is the great secret for acquiring solid science [see Glossary]. If someone explained correctly what the simple ideas are out of which all human thoughts are compounded, and if his explanation were generally accepted, I would venture to expect there to be a universal language that was easy to learn, to speak and to write, and—the main thing—that

would help men's judgement by presenting matters to them so clearly that it would be almost impossible for them to go wrong. Contrast that with what we have now: almost all our words have confused meanings, and men's minds have been accustomed to them for so long that there's hardly anything they can perfectly understand.

I maintain that •this language is possible and that the science it depends on can be discovered, thus enabling peasants to be better judges of the truth of things than philosophers are now. But I don't expect ever to see •it in use. That would require changes in the whole scheme of things—*big* ones, turning the world into a terrestrial paradise. . . .

to Mersenne, 18.xii.1629:

I was astonished to hear that you have often seen a corona around a candle, apparently just as you describe it, and that you have a device that lets you see it at will. I rubbed and rolled my eyes in all sorts of ways to try to see something similar, but with no success [but see page 33]. I'm willing to believe that the cause of this ·difference between us· must have to do with the liquid of the eye; this could easily be confirmed if not everyone saw the coronas at the same time. I would like to know when *you* see the coronas: does it happen

   •at night, when your eyes are full of the vapours of sleep?
   •after you have been reading for a good while?
   •when you have gone without food for some time?
   •when the weather is dry? or rainy?
   •whether you were indoors? or out in the open air?

and so on. When that is settled, I think I could explain the matter. The corona that can be seen around the sun is quite

different; this is proved by the very thing you tell me, namely that the order in which the colours appear is different ·in the two phenomena·. I don't want to dispute the point that Gassendi is so convinced of. I'm willing to believe that he has on several occasions seen a corona with a diameter of 45 degrees; but my guess is that there are coronas of many sizes below that one, and that the ones that appear only as a white or reddish circle are smaller. If the empirical evidence doesn't support that, I admit that I don't yet know what explains the coronas.

Please tell me who the author is who relates that 'Dutch sailors saw three suns separated from each other by a pattern of six rainbows'. The thing is beautiful and regular, and its basis is like that of the phenomenon at Rome.

Thank you for the other comments you sent me. I shall be obliged if you will continue sending me comments on anything to do with nature that you think is worth explaining, and especially anything that is universal and can be checked by anyone—those being the only topics that I have undertaken to deal with. As for particular observations that depend on the reliability of individual witnesses, I have never discussed these and have decided to say nothing about them.

Thank you also for offering to take care of the little treatise that I have in hand. I'm a little ashamed of putting you to so much trouble, but since you have kindly offered to help me I'll send it to you if by God's grace I complete it. It will be a long time before I have it published: although I've decided not to put my name to it, I don't want this work to be released until it has been thoroughly checked by you and other intelligent people (we can find some) who are willing to take the trouble. Your judgement would be enough if I weren't afraid that your affection for me would bias you in my favour. I want this mainly because of ·its implications for· theology, which has been so dominated by Aristotle that it's almost impossible to expound any philosophy without making it seem to be directly contrary to the Faith. Incidentally, please tell me •whether there's anything definite in religion about the extent of the created world, i.e. whether it is finite or infinite; and •whether in all these regions called 'imaginary spaces' there are genuine created bodies. I wasn't keen to touch on this topic, but I believe I'll have to go into it.

[Descartes now responds to questions that Mersenne has put to him, about the psychology of musical sounds and the physics of a vibrating string. The final topic is the physics of falling bodies, and specifically:] something you say Beeckman told you about this. I'll approach this through your last question: Why did I say that the speed is impressed by heaviness as 1 at the first moment, as 2 at the second moments, etc.? Forgive me but that's *not* what I think. Rather, the speed is impressed by heaviness as 1 at the first moment, and by the same heaviness as 1 at the second moments, etc. Now, 1 at the first moment and 1 at the second moment make 2, and with 1 at the third moment this makes 3; in this way, the speed increases in arithmetical progression. This is sufficiently proved, I thought, by the fact that heaviness *stays with* the body that has it, which it can't do without pushing the body downwards at every moment. Consider a mass of lead (say), falling under the force of its own heaviness: God suddenly takes away its heaviness, making it light as a feather; it will go on falling, at least in a vacuum, because it is moving and there's no reason why it should stop; but its speed won't increase. (I'm **assuming** that anything that moves will, in a vacuum, continue to move. I'll try to demonstrate this in my treatise.) But suppose that after some time God restores the heaviness to the lead momentarily and then takes it away again. At the second moment wouldn't the lead be pushed by the force of its heaviness just as it was at the first moment? So wouldn't

its speed be twice as great? And this applies to all the other moments of its fall. It follows that if you let a ball fall 50 feet in an absolute vacuum, no matter what stuff the ball is made of it will take exactly three times as long to fall the first 25 feet as it will take to fall the last 25 feet. But in air it is an entirely different matter. Now back to Beeckman: although what he told you is false, namely that once a falling body reaches a certain point it goes on falling at the same speed, it *is* true that after a certain distance the increase in speed is so small as to be imperceptible. I'll explain to you what he meant to say; ·I can do this· because he and I have discussed this together in the past.

[Descartes starts by saying that Beeckman accepts Descartes's **assumption** and his figures for speed-increase in a vacuum] which I tried to establish twelve years ago at Beeckman's suggestion and still have among my notes from that time. But what follows is something that he has added of his own accord, namely that the faster a body falls the more air-resistance it meets. I was doubtful about this at first, but now that I have examined it carefully I can see that it is true. From this he draws the following conclusion. The force that creates speed always increases uniformly (i.e. by one unit at each moment), whereas the air-resistance always impedes it in a non-uniform way (less than a unit at the first moment, a little more at the second moment, and so on). So, he says, there must be a point at which air-resistance exactly equals the thrust from heaviness, reducing the thing's speed at the same rate that its heaviness is increasing it. At the moment this happens, it is certain that the body doesn't fall more quickly than it did at the immediately preceding moment; and at the subsequent moments the speed will neither increase nor diminish, because from then on the air resistance remains uniform (its previous variation came from variation in the body's speed, and that has been taken away),

and the force of the body's heaviness always pushes it in a uniform way.

This argument is plausible, and anyone ignorant of arithmetic might be convinced by it; but as long as you can *count* you can see that it is unsound. If the air resistance increases in proportion to the increase in the speed, the resistance can't increase at a proportionally greater rate than the speed does. Suppose that at the beginning of the motion the speed is 1 if there is no air resistance, and only $\frac{1}{2}$ if there is air resistance (i.e. that the air resistance is also $\frac{1}{2}$). Then at the second moment, when the heaviness adds another unit to the speed, the speed would be $\frac{3}{2}$ if again there were no immediate air resistance. But how much air resistance *will* there be? One might say that the air resistance won't be proportionally as great as it was the first time, because now the body is already moving; and if that's right the proposition that Beeckman infers will be even less true. But one can't say that the resistance will be proportionally greater than it was the first time, i.e. that it will reduce the speed by a half, from $\frac{3}{2}$ to $\frac{3}{4}$, and at the third moment the weight will add yet another unit to the speed, which will be $\frac{7}{4}$ unless the air resistance reduces it by $\frac{1}{2}$, leaving $\frac{7}{8}$. Thus in the succeeding moments the air resistance will be $\frac{15}{16}$, $\frac{31}{32}$, $\frac{63}{64}$, $\frac{127}{128}$, $\frac{255}{256}$, and so on ad infinitum. As you can see from this, the numbers always increase and are always less than a unit. Thus the reduction in speed due to air resistance is never as great as the increase in speed due to heaviness, which is one unit at every moment. The same is true if you say that air resistance reduces the speed by $\frac{2}{3}$ or $\frac{3}{4}$. Yet you can't say this at the first moment it reduces the speed by one unit, for in that case the body wouldn't fall. So it is demonstrated mathematically that what Beeckman wrote is false. If you write to him, I shan't mind if you tell him this—it may teach him not to deck himself out in someone else's feathers.

But to come back to the falling body, one can see from the calculation that the non-uniformity in the speed is very large at the beginning of the motion but almost imperceptible later on, and that it's even less perceptible in a body made of light matter than in one made of heavy matter. Your two excellent experiments can show this empirically. [In the next sentence, *pouce*, which ordinarily means 'thumb', here refers to the length of a thumb, say about three inches.] If you follow the calculation above and represent a moment by a very small space, you'll find that a ball that falls 50 feet will move almost three times as fast over the second *pouce* as it did over the first *pouce*, though it won't move perceptibly faster over the third *pouce* than over the second, and that it will take no longer to fall the first 25 feet than to fall the last 25 feet, save what it needs to fall 2 or 3 *pouces*, and this amount will be quite imperceptible. That's what will mainly happen if the ball is made of light matter, but if it is made of iron or lead, the non-uniformity in the motion won't become imperceptible so early in the journey; yet if the fall is from a great height you will hardly be able to perceive it any better, since the motion will last for a shorter time than it would if the ball were made of light matter.

[Then ten more pages on topics Mersenne had raised: physics, ancient music, and natural expressions of states of mind (laughing, crying).]

to Mersenne, i.1630:

I am sorry about your erysipelas.... Please take care of yourself, at least until I know whether a system of medicine can be discovered that is based on infallible demonstrations—which is what I'm investigating right now.

The familiar 'corona' around a candle has nothing in common with the corona that appears around stars, for there's no gap between it and the candle: it's simply secondary light coming from the rays which pass straight through the iris; like a ray of sunlight that enters a room through a small hole and lights up the inside. But you'll see the colours more clearly if you look at a candle from seven or eight feet away, across the edge of a quill or even across a hair held upright straight in front of your eye: place the hair right up against the eye and you'll see a great variety of fine colours. I'll now respond to the rest of your letter point by point [Descartes numbers these items 1–12.].

[**(1)** Remarks about the physics and psychology of listening to music. **(2)** One sentence about the physics of a bouncing ball. **(3)** Sounds again. **(4)** Dismissing as 'ridiculous' something that Beeckman had said about the transmission of sound. Then:]

(5) Most small bodies seen through eyeglasses appear transparent because they *are*; but many of them jumbled together are not transparent, because they aren't joined together in a uniform way, and this jumbled arrangement is enough to make opaque what was originally transparent. You can see this from a piece of glass or a sugar-crystal: when it is crushed it won't be transparent any more, though each part of it is transparent.

[**(6)** One sentence about *qualities*. **(7)** Remarks about how big a concave mirror would have to be to produce heat at a considerable distance. **(8)** Music. **(9)** Christian virtue in relation to natural virtue. **(10)** Criticising someone (unnamed) whose writings on music theory plagiarise the work of others. **(11)** The physics of bells. **(12)** The physics of breaking a cord by putting it under tension. Then an unnumbered paragraph on how someone who knows several languages can fairly quickly get the gist of a new one; also dismissing as 'puerile' some claims about where the Romans and the Germans got their names for God from. Then:]

Thank you for offering to send me Gassendi's observations. I didn't want to put you to so much trouble, since they aren't yet published. All I want are answers to these questions:

- •Has he seen a number of sunspots? If so,
- •how many has he seen at the same time?
- •Did they all move at the same speed?
- •Did they always appear to be round?

And also the answers to these:

- •Has he observed *for certain* that refraction [see Glossary] due to the air makes stars near the horizon appear higher in the sky than they really are? and if he has,
- •did this refraction effect also occur with the moon?; and
- •was this refraction effect greater or smaller with stars close to the northern horizon than with stars close to the southern horizon?

But these questions call for such accurate instruments and exact calculations that I doubt if anyone has yet been able to answer them definitively. If anyone could do it, I would expect it to be Gassendi.

I think I heard you say once that you had made an accurate investigation of the weights of all the metals and made a list of them. If that is right, I'd be grateful if you would send it to me if that's not too much trouble.

I would also like to know whether you have any empirical data on whether ·a projectile·—a stone thrown from a sling, or a ball shot from a musket, or a bolt from a crossbow—travels faster and has greater force in the middle of its flight than at the start, and whether its power increases. The common opinion is that it does, but I have reasons for thinking that this is wrong. I find that any projectile must have more force when it is first launched then it has just after that.

to Mersenne, 25.ii.1630:

[This letter mainly consists of six numbered items, preceded by a comment on this correspondence: Mersenne asks questions but says that he only wants answers that Descartes can come up with easily. Descartes comments, in effect: 'Do you think I am omniscient? I can't answer your questions off the top of my head; I'd be willing to take trouble over them if I saw any prospect of finding the answers, but in the case of the questions in your last letter that seems to be impossible.' **(1)** A question about how far sounds travel. **(2)** Listening to sounds—sharps and flats. Then:]

(3) About the bouncing of a ball: what I said was not •that the cause of this is entirely ·what happens to· the air inside the ball, but •that it is mainly due to the continuation of the motion that all rebounding bodies have, i.e. due to the fact that

> any moving thing continues to move for as long as it can; and if it can't continue to move in a straight line it doesn't come to a halt but rebounds in the opposite direction.

The air inside a ball acts as a spring that helps it to rebound; and so does the matter of nearly all other bodies—those that bounce and those that other bodies bounce off, such as the strings of a tennis racket, the wall of a handball court, etc. As for the air that follows or precedes a bouncing ball, that's an imaginary idea of the scholastics, and in my view it is quite pointless.

[**(4)** The physics and psychology of hearing sounds. **(5)** Remarks about devices that were thought to turn water into air; leading on to this:]

Why does the air inside the barrel of a gun resist the force of many men? Not because air is denser than water but because it's composed of parts that can't pass through the

sides of the barrel, and consequently cannot be condensed. Whenever something is condensed, it loses some of its parts and retains the bulkier parts—think about squeezing a wet sponge. If a vessel filled with the most highly rarefied air that could be imagined had no pores that any of the air could escape through, then all the forces in the world wouldn't be powerful enough to condense it *at all.* But in fact all bodies that can be condensed (air included) have some particles that are small enough to pass through the pores of any bodies—even gold and diamonds. . . .

[**(6)** A technical discussion of mirrors. Then an unnumbered paragraph about clocks and stretched cords. Then:]

Thank you for your observations on metals. I couldn't draw any conclusions from these, except that it's hard to perform accurate experiments in this area. If your bells were all the same size, the *difference* between performance in air and performance in water would have been the same for all of them; yet that's not what I find in your results. Also, you treat gold as lighter than lead, which it is clearly the wrong way around. And you treat pure silver as being as heavy in water as in air, and bronze heavier, which is impossible; but perhaps that was a slip of the pen.

[A bit more about coronas and candles, and a request for news about Ferrier's work on telescope lenses.]

⊕ [4.iv.30: To Mersenne, with remarks about sun-spots, mathematical problems posed by Claude Mydorge, children as linguistic inventors, optics, music, and snow.]

to Mersenne, 18.iii.1630:

[Four pages of complaint against Ferrier, who told Mersenne that he was going to live with Descartes but hasn't answered Descartes's long letters on this subject; complaints also about his conduct as a researcher. Then:]

You ask whether there's a discoverable essence of beauty. That's the same as your earlier question as to why one sound is more pleasing than another, except that the word 'beauty' seems most at home with the sense of sight. But in general 'beautiful' and 'pleasing' each signify merely a relation between our judgement and an object; and because men's judgements are so various, there can't be any definite standard of beauty or pleasingness. I can't explain it any better than I did in my treatise on music [*Compendium Musicae*]. I have it right here, and will quote the passage word for word:

> Among the objects of the senses, those most pleasing to the mind are neither the easiest to perceive nor the hardest, but the ones that are not •so easy to perceive that they don't fully satisfy the natural inclination of the senses towards their objects and not •so hard to perceive that they tire the senses.

I explained what I meant by 'easy or difficult to perceive by the senses' in terms of the divisions of a formal garden. If there are only one or two shapes arranged in a single repeated pattern, they will be easier to take in than if there are ten or twelve arranged in different ways. But that's not to say that one design can be called absolutely more beautiful than another; to some people's fancy one with three shapes will be the most beautiful, to others it will be one with four or five, and so on. The one that pleases most people can be called *the most beautiful*, period; but there's no way of fixing what this is.

Secondly, what inclines some people to dance may incline others to weep. This is purely because it stirs up ideas in our memory: those who have enjoyed dancing to a certain tune feel a new wish to dance the moment they hear a similar one; and someone has never heard a galliard without some affliction befalling him will certainly be downcast when he hears it again. This is so certain that I think that if you

whipped a dog five or six times to the sound of a violin, it would begin to howl and run away as soon as it heard that music again.

[Then a paragraph about how different sounds are made by a flute. And finally a request:]

If you happen to meet someone who mentions me and remembers that I am still alive, I would be glad to know what he says about me, and what he thinks I am doing and where he thinks I live.

to Mersenne, 15.iv.1630:

I'm aware of being enormously in your debt for all your kind services; there have been so many of them that I can't thank you for each individually. I assure you that I'll repay you in any way you ask, if I can; and I will always let you know where I am living, provided—please!—that you don't tell anyone else. If anybody thinks that I am planning to write, please try to remove this impression, not to confirm it. I swear that I *wouldn't* be planning to write if I hadn't already told people I do plan to do so. I did this so ·as to motivate myself with the thought· that if I didn't produce anything they could say I hadn't been able to carry out my plan. If people are going to think about me, I am civilised enough to like them to think well of me; but I would much prefer them to have no thought of me at all. I don't *want* fame as much as I *fear* it, because those who acquire it seem to me always to lose some degree of freedom and leisure, which are two things I possess so completely and value so highly that no monarch in the world is rich enough to buy them from me.

This won't prevent me from completing the little treatise that I have begun, but I don't want this to be known, so that I'll always be free to disavow it. My work on it is going very slowly, because I enjoy •learning much more than •writing down the little that I know. I'm now studying chemistry and anatomy simultaneously; every day I learn something that I can't find in any book. I wish I had already started to research into diseases and remedies, so as to find some cure for your erysipelas, which I'm sorry you've been troubled by for so long. Moreover, I'm so contented when acquiring knowledge for myself that I never settle down to add anything to my treatise except under duress, in order to carry out my resolution—namely, that if I live I'll have it ready to send to you by the start of 1633. I'm telling you a definite time so as to put myself under a greater obligation, and so that you can reproach me if I fail to keep to the date. You'll be surprised that I take so long to write a discourse that will be short enough—I should think—to be read in an afternoon. It's because I take more trouble, and think it more important, to learn what I need for the conduct of my life than to spend time publishing the little I have learned. If you're wondering why I haven't persevered with some other treatises that I began in Paris, I'll tell you why: while I was working on them I acquired a little more knowledge than I'd had when I began, and trying to take account of this I was forced to start a new project, a bit bigger than the first. It's like a man who starts building a house and then acquires unexpected riches which so change his status that the building he has begun is now too small for him. No-one would blame him if he made a fresh start on a house more suitable to his wealth. I'm sure I won't change my mind again, because—whether or not I learn anything more—the knowledge I now have will serve my turn and enable me to bring my plan to completion.

[A paragraph exclaiming at Ferrier's conduct, and asking Mersenne to pass the word to Ferrier that Descartes is angry with him. Then remarks about mathematical problems: Descartes has grown tired of mathematics and can't be bothered with tackling such problems, though he is willing

to send Mersenne as many of them as he wants. He presents three geometrical problems, and adds that he could come up with harder ones if he put his mind to work on it, but he doesn't think there's any need for this. Then 'As for your questions:']

(1) The corpuscles that enter a thing during rarefaction and exit during condensation—corpuscles that can penetrate the hardest solids—are of the same substance as things we can see and touch; but don't think of them as atoms or as being *at all* hard. Think of them as an extremely fluid and subtle [see Glossary] substance filling the pores of other bodies. You must admit that even gold and diamonds have pores, very tiny ones; and if you agree also that there's no such thing as a vacuum—·a region of space with literally *nothing* in it·—as I think I can demonstrate, you're forced to admit that these pores are full of matter that can penetrate everywhere with ease.

the next sentence: Or la chaleur et la raréfaction ne sont autrechose que le mélange de cette matière.

literally meaning: Now, heat and rarefaction are simply an admixture of this matter.

what Descartes perhaps meant: When matter is heated and becomes more rare—e.g. when water turns into steam—what's happening is that subtle matter is coming to replace some of its non-subtle matter.

To convince you of this would take more space than a letter permits. I have said this about many other questions that you have put to me; but, believe me, I have never used this as an excuse to conceal from you what I'm planning to write in my treatise on physics. I assure you that I don't have any knowledge that I'm keeping secret from anyone, especially from you whom I honour and admire and owe so much to. But the difficulties of physics that I told you I had taken on

are all so linked and interdependent that I couldn't solve one without giving the solutions to all; and the quickest and simplest way I know of for doing that will be in the treatise that I am writing.

[**(2)** Metals. **(3)** Three pages on how far different sounds carry, and why. Then:]

(4) [Descartes doesn't explicitly number this or any of the remaining points.] Your question of theology is beyond my mental capacity but not, it seems to me, beyond the scope of philosophy, because it doesn't connect with anything dependent on revelation, which is what I call 'theology' in the strict sense. It's a metaphysical question, and should be examined by human reason. I think that all those to whom God has given the use of this reason ought to use it primarily in trying to know him and to know themselves. That's the task I began my studies with; and I couldn't have discovered the foundations of physics if I hadn't looked for them along that road. I have studied this topic more than any other and, thank God, I have achieved something in it. At least I think I have found how to prove metaphysical truths in a way that makes them more evident than the demonstrated propositions of geometry—in my own opinion, that is: I don't know if I can convince anyone else. During my first nine months in this country [Holland], I worked on nothing else. I think I told you once about my plan to write something on the topic; but I want to see first how my treatise on physics is received. (If the book that you mention was very well written and fell into my hands, I might feel obliged to reply to it immediately, because if the report you heard is accurate it says things that are very dangerous and, I believe, very false.) However, in my treatise on physics I shall discuss a number of metaphysical topics and especially the following. The mathematical truths that you call 'eternal' have been laid down by God and depend on him entirely, no less than the

rest of his creation. To say that these truths are independent of God is to talk of him as if he were Jupiter or Saturn and to subject him to the Styx and the Fates. Don't hesitate to assert and proclaim everywhere that it's God who has laid down these laws in nature just as a king lays down laws in his kingdom. There's not one of them that we can't grasp if we focus our mind on it. They are all inborn in our minds, just as a king would, if he could, imprint his laws on the hearts of all his subjects. God's greatness, on the other hand, is something that we can't •grasp even though we •know it. But our judging it to be beyond our grasp makes us esteem it all the more; just as a king has more majesty when he is less familiarly known by his subjects, provided they don't get the idea that they have no king—they must know him enough to be in no doubt about that.

You may say:
> •'If God had established these truths he would have been able to change them, as a king changes his laws.'

To this the answer is:
> •He can change them, if his will can change.
> •'But I understand them to be eternal and unchangeable.'
> •And so is God, in my judgment.
> •'But his will is free.'
> •Yes, but his power is beyond our grasp. In general we can say that God can do everything that we can grasp, but not that he can't do what is beyond our grasp. It would be rash to think that our imagination reaches as far as his power.

I expect to put this in writing within the next fifteen days, in my treatise on physics; but I'm not asking you to keep it secret. On the contrary, feel free to tell people whenever you have the opportunity, but don't mention my name. I'll be glad to know what objections I can expect to be made against this view. I want people to get used to speaking of God in a way that is worthier, it seems to me, than the common and almost universal way of imagining him as a finite being.

(5) With regard to infinity. . . . You said that if there were an infinite line it would have an infinite number of feet and of fathoms, so that the infinite number of feet would be six times as great as the number of fathoms. I agree entirely.
> •'Then this latter number is not infinite.'
> •That doesn't follow.
> •'But one infinity can't be bigger than another.'
> •Why not? Where is the absurdity? Especially if it is only greater by a finite ratio, as in this case, where one number is reached by dividing the other by six, which doesn't in any way affect the infinity.

Anyway, what basis do we have for judging whether one infinity can be greater than another? If we could grasp it, it would no longer be infinity.

to Mersenne, 6.v.1630:

Thank you for Gassendi's account of the corona. As for the bad book [mentioned a page back], I'm no longer asking you to send it to me, because I have decided on other projects, and it would be too late to carry out the plan that made me say that if it were a well-written book and fell into my hands I would try to reply immediately. ·The plan was this:· I thought that even if there were only thirty-five copies of the book, if it were well written it would go to a second printing and circulate widely among curious people, however much it might be prohibited. I thought of a remedy that seemed more effective than any legal prohibition. My idea was that before the book was reprinted secretly it should be printed with permission ·from the authorities·, with each paragraph or each chapter followed by arguments refuting its conclusions.

I thought that if it were sold thus publicly in its entirety with a reply, no-one would care to sell it in secret without a reply; so nobody would encounter its false doctrine without at the same time being disabused of it. . . . I expect you'll say that we don't know whether I *could* have replied to the author's arguments. I can only reply that at least I would have done my best; and since I have many arguments that convince *me* of the contrary of what you report as being in the book, I ventured to expect them to convince others as well. I trusted that •truth expounded by an undistinguished mind would be stronger than •falsehood maintained by the cleverest people in the world.

As for the eternal truths, I repeat that they are true or possible only because God knows them as true or possible; and he doesn't have this knowledge in a way that implies that they are true independently of him. If men really understood the sense of their words, they could never say without blasphemy that the truth of anything is prior to God's knowledge of it. In God, willing and knowing are a single thing in such a way that by the very fact of willing something he knows it and it is only for this reason that such a thing is true. So we mustn't say that even if God didn't exist these truths would be true; for the existence of God is the first and the most eternal of all possible truths and the sole source of all the others. What makes it easy for this to be misunderstood is that most people don't regard God as a being who is infinite and beyond our grasp, the sole author on whom everything depends; they get no further than the syllables of his name and the knowledge that 'God' means •what *Deus* means in Latin and •what is worshipped by men. Those whose thoughts go no higher than that can easily become atheists; and because they perfectly grasp mathematical truths and don't perfectly grasp the truth of God's existence, it's no wonder they don't think the former depend on the latter. But they should rather take the opposite view that because •God is a cause whose power goes beyond the limits of human understanding and •the necessity of these other truths doesn't put them out of our reach, these truths are less than, and subject to, the incomprehensible power of God. What you say about the Second Person of the Trinity being generated by the First doesn't conflict with what I'm saying, I think; but I don't want to get into theology, and I'm already afraid that you will think that my philosophy is going too far when it ventures to express an opinion on such lofty matters.

to Mersenne, 27.v.1630:

(1) You ask me by what *kind* of causality God established the eternal truths. I reply: by the same kind of causality as he created all things, namely as their efficient [see Glossary] and total cause. It is certain that he is the author of the essence of created things as well as of their existence; and this essence is just these eternal truths. I don't think of them as being given off by God as light-rays are given off by the sun; but I know that

> •God is the author of everything, and
> •these eternal truths are something, and therefore
> •he is their author.

I say that I *know* this, not that I conceive it or *grasp* it; because we can know that God is infinite and omnipotent although our soul can't grasp or conceive him because it is finite. In the same way we can touch a mountain with our hands but we can't put our arms around it as we could around (for example) a tree. To grasp something is to embrace it in your thought; to know something you need only touch it with your thought.

You also ask what *necessitated* God to create these truths; and I reply that ·nothing did·: he was as free to

make it not true that the radii of a circle are all equal

as he was to

not create the world.

And it's certain that •these truths are no more necessarily attached to his essence than are •other created things. You ask what God *did* in order to produce them. I reply that from all eternity he willed and understood them to be, and by that very fact he created them. Or, if you restrict the word 'created' to the existence of *things*, then he established them and made them. Willing, understanding and creating are all the same thing in God, no one of them is prior to the others even conceptually.

(2) As for the question 'Is it in accord with God's goodness to damn men for eternity?', that's a theological question, so please allow me to say nothing about it. It's not that the arguments of free thinkers on this topic have any force—indeed they strike me as frivolous and ridiculous—but I think that when truths depend on faith and can't be proved by natural demonstration it's not doing them justice to want to support them by human reasoning and mere probabilities.

(3) As for God's freedom, I entirely agree with what you report Father Gibieuf to be maintaining. I didn't know that he had published anything, but I'll try to have his treatise sent from Paris as soon as possible so that I can see it. I'm delighted that my opinions coincide with his, because that assures me that they are, at least, not too extravagant to be maintained by very able men.

Topics **(4)**, **(5)**, **(6)**, **(8)**, **(9)** and **(11)** in your letter are all theological matters, so if you please I'll say nothing about them. As for **(7)** the point about birth-marks caused on children by their mothers' imagination, I quite agree it is worth examination, but I'm not yet convinced.

(10) Given that •God leads everything to its perfect state and that •nothing is annihilated, you ask 'Then what is the perfect state of a dumb animal? and what becomes of its soul after death?'. These questions are within my field (·rather than being outside it in theology·), and I reply that God leads everything to perfection collectively but not individually. The very fact that particular things perish and others appear in their place is one of the principal perfections of the universe. As for animals' souls and other forms and qualities, don't worry about what becomes of them. I'm about to explain all this in my treatise, and I expect to make it all so clearly understood that no-one will be able to doubt it.

⊕ [ix or x 1630: Descartes writes to Beeckman saying that he has been reliably informed that Beeckman has been publicly boasting about how much he has taught Descartes. He advises him that such conduct will get him laughed at rather than admired, and. . . .on and on it goes.]

to Beeckmann, 17.x.1630:

[Two pages in which Descartes again addresses the question of what he has learned from his former friend, and the morality of Beeckman's boasting about it. Eventually he works his way around to this:] But I can see from your latest letters that in all this you weren't sinning out of malice but were in the grip of some kind of illness. So from now on I'll be sending you sympathy rather than complaints. And now—because of our former friendship—I'd like to advise you of certain remedies that may help you to recover.

Consider first what are the things that one person can teach another: you'll find they are languages, history, observational data, and clear and certain demonstrations (like those of geometers) that bring conviction to the mind. As for mere opinions and received doctrines like those of the philosophers, simply repeating them isn't teaching them.

Plato says one thing, Aristotle another, Epicurus another, Telesio, Campanella, Bruno, Basson, Vanini, and the innovators all say something different. Which of these people do you think has anything to teach (I won't say *me*, but) anyone else who cares about wisdom? Doubtless it's the man who can first convince someone by his arguments, or at least by his authority. But if someone *comes to believe* something without being brought to this by any authority or argument, having merely heard many people say it, this doesn't mean that anyone has *taught* him anything. It may even happen that •he really knows it, being led to believe it by true reasons, and that •no-one before him has ever known it—those who believed it had inferred it from false principles, so that they didn't *know* it. If you think carefully about this you'll easily see that I have never learned anything but idle fancies from your *Mathematical Physics*, any more than I have learned anything from the ·comic-verse parody· *Batrachomyomachia*. Have I ever been influenced by your authority or convinced by your arguments? You have said that I believed some of your views as soon as I understood them. But my accepting them at once doesn't show that I learned them from you; I accepted them because I had already arrived at them for myself. Don't make your sickness worse by dwelling on the fact—which I here openly acknowledge—that I have sometimes accepted what you said; because in discussing philosophy even the most incompetent person can't help saying things that *happen* to coincide with the truth. Many people can know something that none of them learned from the others; and it's ridiculous to fuss as you do about distinguishing the items of knowledge that are yours from the ones that aren't—as if items of knowledge were pieces of land or sums of money. If you know something, it is completely yours, even if you have learned it from someone else.

[Descartes devotes about six pages to hammering away at the idea of 'ownership' of propositions or of sciences, suggesting different ways of taking this, and mocking them all. Of Beeckman's claim to have discovered something about the vocal cords, Descartes asks 'Then did Aristotle steal it from you?' He then addresses Beeckman's complaint that Descartes has never praised him for his discoveries, although he has often publicly praised Descartes. Reply: that praise wasn't the act of a friend, because Descartes—longing for solitude and quiet—had asked Beeckman not to talk about him to others. Eventually:]

You accuse me of having sometimes put myself on a level with the angels. There's no reason or basis for this—can you really be *so* out of your mind that you believe it? But I realise that your sickness may be at an advanced stage, so ·I ought to be patient, and in that spirit· I'll explain what may have led you to make this complaint. When philosophers and theologians want to say that P is in conflict with reason, they often express this by saying that *not even God* could make it the case that P. This turn of phrase has always struck me as too bold; so on occasions when others might use it I prefer the more modest statement that *not even an angel* could do it. If that's why you say I put myself on a level with the angels, you could as well say that the wisest people in the world put themselves on a level with God! It's hard on me to suspect me of vanity because of conduct that displays extraordinary modesty.

[A final page is spent saying that Descartes is not writing in anger but purely in a sympathetic attempt to help a sick friend.]

⊕ [4.xi.30: Descartes writes to Mersenne about his personal relations with Beeckman, Ferrier, Mydorge and others. A paragraph on the vibration of taut strings. A message of good will to Gibieuf.]

to Mersenne, 25.xi.1630:

[Descartes tries to head off damage to his relations with Mersenne caused by things Beeckman has said. Then:]

I'm sorry for Ferrier's troubles, though he has brought them on himself. As for my letter to you about him [we don't now have that letter], since you have thought it proper to show it to Mydorge I won't make a fuss about that; but I'd have preferred you not to put it actually in his hands. For one thing, my letters are usually written with too little care to be fit to be seen by anyone except the addressee. Also, I'm afraid that he may have inferred from the letter that I'm planning to have my *Optics* printed, because I think I mentioned it in some parts of the letter other than the last paragraph which you say you cut off. I would like this project to remain unknown, because at my rate of work it won't be ready for a long time. I want to include an account of the nature of colours and light, which has held me up for six months and still isn't half finished; but it will be longer than I expected and will contain something like a complete physics. I think it will serve to keep my promise to you to have my *World* finished in three years, because the *Optics* will be something like an abridged version of that. After that I don't think I'll ever have anything else printed, at least in my lifetime. I'm too much in love with the fable of my *World* to give it up if God lets me live long enough to finish it; but I can't answer for the future. [The 'fable' referred to here is a many-chapter account of a 'new world', an imagined possible world, in terms of which Descartes discusses the physics and cosmology of the actual world.] I think I'll send you this discourse on light as soon as I've finished it, before sending you the rest of the *Optics*; ·I'm in hurry about the former·, because in it I aim to give *my* account of colours, which requires me to explain how the whiteness of the bread remains in the Blessed Sacrament; and I want to

have this examined first by my friends before everyone sees it. As for the rest of the *Optics*, although it won't be finished for some time, I'm not afraid of anyone's getting in ahead of me, because I'm sure that no-one will write anything that coincides with my account—unless they take it from my letters to Ferrier.

Whenever you encounter someone who thinks that I'm planning to write something, please do what you can to get him to think otherwise, convincing him that nothing could be further from my mind. In fact, once the *Optics* is finished I plan to study conscientiously, for the sake of myself and my friends, trying to discover something useful in medicine. I don't want to waste time writing for others who would mock me if I did badly, be envious of me if I did well, and show me no thanks if I produced a masterpiece. . . .

[Descartes goes on to say that he is too focussed elsewhere to be able to deal with Mersenne's questions, though he briefly answers one, concerning the vibration of the strings of a lute. Then remarks about the safest way to send letters. Then:]

I am most obliged to you for taking the trouble to send me an extract from the manuscript you mentioned [see page 15]. The shortest way I know to reply to his and other atheists' arguments against the existence of God is to find an evident demonstration that will make everyone believe that God exists. I can boast of having found one that satisfies me entirely, making me know that God exists more certainly than I know the truth of any proposition of geometry; but I don't know whether I could make everyone understand it the way I can. I think it's better not to treat this matter at all than to treat it imperfectly. The universal agreement of all nations is enough to maintain God against the atheists' insults, and no individual should argue with them unless he is very certain of convincing them.

·The reception of· my *Optics* will show whether I am capable of explaining my conceptions and convincing others of truths of which I have convinced myself. I doubt it very much! But if it turns out that I *can* do this, I don't rule out my some day completing a little *Treatise of Metaphysics* that I began when in I was Friesland. [This refers to the *Meditations*.] Its chief aim is to prove *the existence of God and of our souls apart from our bodies*, from which it follows that our souls are immortal. It makes me angry to see that there are people so bold and so impudent as to fight against God.

⊕ [2.xii.30: Descartes writes at length to Ferrier, setting forth his grounds of complaint; briefly to Condren about his relations with Ferrier, and to Mersenne mainly on the same topic.]

to Mersenne, 23.xii.1630:

[In an intense first paragraph, Descartes assures Mersenne of his unshakable affection for him, begging him not to think otherwise if there are long periods during which Descartes doesn't write to him. When he doesn't write, he says, it's because he has nothing to say. Then:]

What I'm working at now is—·metaphorically speaking·—sorting out chaos so as to get light to shine from it! This is one of the hardest and most important matters I could ever undertake, because it involves almost all of physics. I have to take into account many different things all at once if I'm to find an angle on all this that will let me tell the truth without doing violence to anyone's imagination or shocking received opinion. That's why I want to spend a month or two thinking solely about this topic. [And then two more pages touching on about eight minor topics.]

to Balzac, 15.iv.1631:

[Descartes explains at length why he hasn't written during the past eighteen months, insisting that this didn't express any disregard for Balzac, and saying that now that Balzac is in Paris, Descartes would like to be there too so as to have conversations with him, if he weren't kept in Amsterdam by 'the most important work I could ever devote myself to'. He continues:]

Please don't ask me what this task is, for it would embarrass me to tell you. I've become so philosophical that I despise most of the things that are ordinarily valued, and I value others that are usually disregarded. Still, I'll tell you about it more openly some day, if you wish; for your own views are far removed from those of the majority, and you have often shown that you regard me more highly than I deserve. For the time being I'll settle for telling you that I'm no longer of a mind to commit things to paper as you've seen that I used to do. It's not that I wouldn't set great store by reputation if I could be sure of getting an illustrious one like yours, but as for a middling and uncertain reputation, which is all I could look forward to, I value that much less than the peace of mind that I have now. [Descartes gives details: ten hours sleep each night, and fruitful inter-mingling of day-dreams with night-dreams. Then on to further compliments.]

⊕ [25.iv.31: A lavishly friendly letter from Balzac to Descartes, whose letter •reached Balzac when he was in 'the blackest mood I have ever been in', and •somewhat reduced his sadness. He announces his intention to go to Amsterdam because Descartes is there.]

to Balzac, 5.v.1631:

When I read that you are planning to come here, I rubbed my eyes to see whether I was awake. . . . But I don't find it so strange that a mind as great and generous as yours should be unable to adapt itself to the constraints of service that one is subject to at Court; and since you seriously assure me that God has inspired you to retire from the world, I would think it a sin against the Holy Ghost if I tried to deflect you from such a pious resolution. [The idea is that going to Amsterdam would be one way of 'retiring from the world'. There's no mention of divine inspiration or retiring from the world in Balzac's letter of 25.iv.31.]

    You must excuse my enthusiasm if I invite you to choose Amsterdam for your retreat, and to prefer it not only to the monasteries of the Franciscans and the Carthusians that many good folk retire to, but also to the finest houses in France and Italy, and even to the famous Hermitage where you spent the past year. No matter how polished a country house may be, it always lacks countless conveniences that are found only in towns, and even the solitude one hopes to find there turns out never to be quite perfect. There, I agree, you'll find a stream that would make the greatest talkers start day-dreaming, and a valley so secluded that it could make them ecstatic; but it can easily happen that you also have neighbours who will bother you at times, and their visits will be even more of a nuisance than the ones you receive in Paris. In this large town where I live [Amsterdam], by contrast, everyone but myself is engaged in trade, and thus is so focussed on his own profit that I could live here all my life without ever being noticed by anyone. I take a walk each day amid the bustle of the crowd, with as much freedom and repose as you could get in your avenues, and I don't *attend to* the people I see, any more than I would to the trees in your

woods or the animals grazing there. [More to the same effect, and then:] I don't know how you can be so fond of the Italian air, through which one often breathes in diseases—Italy where the heat of the day is always unbearable, the cool of the evening is unhealthy, and the darkness of night is a cover for thieves and murderers. If you're afraid of the northern winters, tell me what shades or fans or fountains could shield you from the burning heat in Rome as a stove or a roaring fire would protect you from the cold here? . . . .

⊕ [2.vi.31: Descartes writes to Reneri, dealing with a problem Reneri had put to him regarding barometers, the weight of air, etc.]

to Villebressieu, summer 1631:

You saw these two results of my fine rule—my natural method—in the discussion I was obliged to have in the presence of Cardinal de Berulle, Father Mersenne and all that great and learned company assembled at the Cardinal's 's palace to hear Chandoux lecture about his new philosophy. I made the whole company recognise what the art [see Glossary] of right reasoning can do for the minds of those who aren't very learned, and how much better founded, more true and more natural my principles are than any others that are currently accepted in the learned world. You were as convinced as any of those who took the trouble to beg me to write them up and publish them.

    I read through and examined most of the things in your memoir during my recent trip to Dordrecht, from which I have returned to await you at Amsterdam, where you'll find me in good health. . . . There I'll tell you what I think about all these things. I'll advise you to put most of your ideas in the form of propositions, problems and theorems, and to publish them so that someone else will feel obliged to provided the needed research and observations. That's

what I would like everybody to do, so that many people's experiments would help to discover the finest things in nature, and to build a physics that is clear, certain, based on demonstrative proof, and more useful than what is commonly taught. You for your part could greatly help to disabuse poor sick minds concerning the adulteration of metals that you have worked on so hard—twelve years of assiduous work and many experiments—without having found any truths. Your work would be generally useful as a warning to individuals of their errors.

It seems to me too that you have already discovered some general principles of nature, such as that •there is only one material substance, which gets from an external cause its movements or ability to move from place to place, and that •from this it acquires the different shapes or modes that make it into the kind of thing we see in the primary compounds that are called 'the elements', namely earth, water, air and fire. And you have pointed out •that what marks off these elements or primary compounds from one another consists only in differences ·of size and shape· between the fragments—the small and large particles—of this matter; and that the matter often changes from one element into another when heat and movement change the larger particles into smaller ones, or the absence of heat and movement changes them back again; and the mingling of these four compounds results in a mixture that can be called 'the fifth element'. You call this the 'principle' [see Glossary] or the most noble preparation of the elements, because it is (you say) a productive seed. . . .which takes a specific form in all the noble particular individuals that are for everyone an object of wonder. I'm quite in agreement with your view that the four elements that constitute matter and the fifth that results from them can jointly constitute an animal or plant or mineral, and that when this happens all five are

so changed that none of them continues to be what it was. All this suits my style of philosophising very well, and it accords admirably with all the mechanical experiments I have conducted in this field.

⊕ [x.31: Descartes writes to Mersenne on personal matters (including messages to others) and the physics of falling bodies and of sounds.]

to Mersenne, x or xi 1631:

[Descartes says that most of Mersenne's latest questions are ones he has already answered in earlier letters. He says a little about musical intervals, going on to compare 'what your musicians say about dissonances being agreeable' with 'someone who says that olives, though bitter, sometimes taste better than sugar'. Then:]

I'm not retracting what I said about the speed of bodies falling in a vacuum. If we (mistakenly) suppose a vacuum, which everyone does, the rest follows demonstratively. I'll try to explain what heaviness, lightness, hardness, etc. are in the two chapters that I promised to send you by the end of this year. . . .

[Some personal matters, explaining a gap in correspondence, asking about Ferrier's whereabouts and activities, and reporting on Reneri's recent professorship. Mersenne's last letter asked a question about whether and why certain musical intervals are 'better' than certain others. Descartes replies that he answered that in the book on music that he wrote a dozen years earlier, quotes the relevant passage, and then comments: 'This can be proved not only by reason but also by experience—with the voice and with several other instruments.' Then:] You ask me to reply to your earlier question as to whether 120 is the only number that has the property that you noticed in it, [Namely, the property of being equal to twice the sum of its aliquot parts, i.e. the sum of its whole-number

divisors (including 1 but excluding 120).] My response is that I don't know and have never wanted to know. Investigating questions like this usually requires more patience than intelligence, and the answers are not useful. . . . [Then something about falling bodies—Mersenne has asked a question but hasn't made clear what question it is. Then:] I think I could now determine the rate at which the speed of a falling body increases, not only in a vacuum but also in real air. But my mind is now full of other thoughts, so I don't have time to investigate this, and there wouldn't be much profit in it. *Please* •excuse me for writing to you in such a scrappy way, and •accept that my letters would be shorter if they were they composed with greater care.

⊕ [i.32: Descartes writes to Golius expressing pleasure that Golius has agreed to read something [apparently a lengthy discussion/solution of a mathematical problem] that Descartes has sent him, and insisting that Golius must frankly report on all his dissatisfactions. Descartes himself already knows of some imperfections in the work, and is sure there must be others.]

to Golius, 2.ii.1632:

I'm much obliged to you for your favourable judgement on my analysis, for I know very well that it is mostly an expression of your courtesy. Still, it gives me a somewhat better opinion of myself because I see that you examined the analysis thoroughly before passing final judgement on it. I'm very pleased that you would like examine in the same way the question of refraction. I'll tell you how I would want to go about it if it were my research project. I'm hoping that this will be a help, if only in strengthening your motivation to push through all the experimental difficulties.

[Descartes now offers an abstract picture of a proposed apparatus, accompanied by an account of what it all means

and how he would use it. We can spare ourselves these details. The aim of the apparatus is to enable the experimenter to know at exactly what angle a ray of light meets a water-surface, and at exactly what angle it leaves it. (The hope is that when enough readings for different angles have been collected it will be possible to devise a general rule covering them all.) Descartes presents all this with a great deal of careful practical detail. He ends the letter thus:]

No doubt if you hunt for them you'll find other inventions better suited to the present experiment than the one I have described; but I know that you're engaged in many other activities, and it occurred to me that if you haven't yet given the matter any thought, I might lighten your burden a little by telling you about this apparatus.

to Mersenne, 5.iv.1632:

It's too long since I heard from you, and I'll start to worry about your health if you don't write to me soon. I expect that you've been waiting for me to send you the treatise that I promised you for this Easter. It's almost finished, and I could keep my promise if I thought you would hold me to the letter of it; but I would prefer to keep the treatise for a few months, to re-read it and tidy it up and also to draw some needed diagrams. They are a burden, because I am, as you know, a very poor draughtsman and careless about matters that don't help me to learn anything. If you blame me for having so often broken my promise, my defence is that I have put off writing the little I know simply because I hoped to learn more that I could add to the book. For instance, in the version of the treatise that I now have in hand, after generally describing the stars, the heavens [see Glossary] and the earth, I didn't intend to give an account of particular bodies on the earth but only to treat of their various qualities; but now I am

including also some of their substantial forms [see Glossary], and trying to clear the path to eventually discovering them all through reasoning and experience. That's what I have been busy with recently—conducting a variety of experiments to discover the essential differences between oils, alcohols, ordinary water and acidic liquids, salts, etc. The only reason I'm delaying the payment of my debt is that I want to pay it with interest. But it will be for you to decide whether what I send you is worth anything. I'm afraid that it may fall so far short of your expectations that you won't be willing to accept it in payment!

In your last letter you wrote to me about a man who boasted of being able to solve mathematical problems of all kinds. I would be glad to know if you have set him the problem of Pappus that I sent to you. I admit that I took five or six weeks to find the solution, and that if anyone else discovers it I'll acquit him of being ignorant of algebra!

to Mersenne, 10.v.1632:

You tell me that you have Scheiner's description of the phenomenon of parhelia [see Glossary] at Rome. If it's more detailed than the one you sent me before, I'd be most obliged if you would take the trouble to send me a copy.

Do you know of any author who has made a special collection of the various accounts of comets? If so, I would be grateful to be told of him; because for the past two or three months I have been quite caught up in the heavens. I have discovered their nature and the nature of the stars we see there and many other things that a few years ago I wouldn't have been optimistic about discovering; and now I have become so bold that I'm trying to explain the position of each fixed star. Although the stars seem very irregularly scattered through the heavens, I'm sure that they are ordered in a way

that is natural, regular, and determinate. Discovering this order is the key to, and foundation of, the highest and most perfect science of material things that men are capable of. If we had it, we could discover *a priori* [see Glossary] all the different forms and essences of terrestrial bodies, whereas without it we have to content ourselves with guessing them *a posteriori* [see Glossary] from their effects. I don't know of anything that could give me more help in discovering this order than the empirical study of many comets. As you know, I have no books, and even if I had I would begrudge the time spent in reading them; so I would be very glad to find someone who has collected the things that I couldn't easily get from the literature—consulting individual authors each writing about only one or two comets.

You once told me that you knew some people who were so dedicated to the advancement of science that they were willing to make every kind of experiment at their own expense. I would like it if someone with this attitude were to present the natural history of celestial phenomena, doing this in Bacon's way,

- •describing the present appearances of the heavens without any explanations or hypotheses,
- •reporting the position of each fixed star in relation to its neighbours,
- •listing their differences in size, colour, visibility and brilliance etc.,
- •reporting on how far this account squares with what ancient astronomers have written, and what differences are to be found, and
- •including all the data we have on comets, with a table of the path of each of them, like the tables Tycho Brahe made of the three or four that he observed, and
- •including the variations in the ecliptic and apogee of the planets.

[Descartes follows the fourth of those items with an explanation of his confidence that there will be differences between modern observations and ancient ones: 'I'm sure that the supposedly "fixed" stars are constantly changing their relative positions.' Now back to what he says about the whole list:] Such a work would be more useful to •scientists in general than might seem possible at first sight, and it would save •me from a great deal of trouble. But there's no hope of anyone's doing this, just as there's no hope of finding the answers to my present questions about the stars. The science I'm describing is beyond the reach of the human mind, I believe, and yet I'm so foolish that I can't help dreaming of it, though I know that this will only make me waste my time as it has been doing for the past two months. In that time I have made no progress with my treatise; still, I'll finish it by the date I told you. . . .

to Mersenne, vi.1632:

Thank you for the letters you kindly sent me. I am now at Deventer, and I'm not going to leave here until the *Optics* has been completed. For the last month I have been wondering whether to include in *The World* an account of how animals are generated, and have decided not to because it would take me too long. I have finished all I had planned to cover regarding inanimate bodies. All I have left to do now is to add something about the nature of man, and then to make a fair copy of the work and send it to you. But I shan't risk saying when that will be—I have already failed to keep my promises so often that I'm ashamed.

As for your questions, I do not believe that sound is reflected at a point as light is, since it is not propagated like light in rays which are all straight; rather, it spreads out in all directions in a circle. [He says a little more about this, accompanying it with a sketch showing this difference between light and sound. Then one sentence on a mathematical point about sounds. Then:]

The refraction of sounds can't be measured exactly, any more than their reflection can. . . . As for my method of measuring the refractions of light, **I introduce a correspondence between the sines of the angles of incidence and the angles of refraction**; but I'd be glad if this was not yet made public, because the first part of my *Optics* will be devoted entirely to this topic. It's not easy to determine what shape a line under water will be seen to have, because the image's location is not fixed either in reflection or in refraction, as optics theorists are commonly convinced. [The passage in bold type is Descartes's first announcement of his 'sine law' about how angles of refraction relate to angles of incidence—the problem he implied, four months earlier (see page 24), that he wasn't working on. This important result was achieved first by Willebrord Snell and then *independently* by Descartes; it is still commonly referred to as 'Descartes's Law' (in France) or as 'the Snell-Descartes law' (elsewhere).]

[The letter concludes with a paragraph about some mathematical problems, with mentions of Golius and Mydorge.]

⊕ [Summer 32: Descartes writes to Mersenne a letter of which we have only a fragment, date uncertain. Annoyance with Ferrier; more about the physics of sound; brief suggested explanations of events reported by Mersenne—a one-stringed instrument that sounds like a trumpet, and an experiment in which a bullet from a musket penetrated the target further when it was 100 paces away than when it was 20 paces away.]

to Mersenne, xi or xii 1632:

. . . .As for what you tell me about Galileo's calculation concerning the speed at which falling bodies move, it has no relation to my philosophy [here = 'physics']. According to my philosophy the relation between

two spheres of lead, one weighing 1lb and the other
weighing 100lb

will be different from the relation between

two wooden spheres, one weighing 1lb and the other
weighing 100lb

and indeed different from the relation between

two spheres of lead, one weighing 2lb and the other
weighing 200lb.

Galileo doesn't distinguish amongst these cases, which makes me think that he can't have hit on the truth.

I would like to know what he says about the ebb and flow of the tides, that being one of the things I have had the greatest trouble in understanding, and though I think I have mainly succeeded, some of the details still aren't clear to me.

[A paragraph each on •Ferrier's doings, •the placing of the holes in a wind-instrument, and •the physics of sounds. Then]

In *The World* I'll be saying rather more about *man* than I had intended; I'm now aiming to explain *all* the main functions in man. I have already written about the vital functions—digestion, heart-beat, the distribution of nourishment, etc.—and about the five senses. I'm now dissecting the heads of various animals, as an aid to explaining what imagination, memory, etc. consist in. I have seen the book *De Motu Cordis* [= 'The Movement of the Heart'], by the English physiologist William Harvey, which you previously spoke to me about. I find that it differs slightly from my own view, although I didn't see it until I had finished writing on this topic.

⊕ [7.ii.33: Descartes writes to Wilhelm about a student of Wilhelm's in whom Descartes has taken an interest.]

⊕ [22.vii.33: Descartes writes to Mersenne about •letters of Mersenne's that have gone astray, •the physics of sounds, •a report of an *horloge*

sans soleil (= 'clock that doesn't use the sun') which Descartes greets with scepticism but not outright rejection, and •his news that his treatise is almost finished.]

to Mersenne, late xi.1633:

[This fragment of a letter is all we have of it.] This is the point I had reached when your letter of 11.xi arrived. I was inclined to act like a bad debtor who asks his creditor for 'a little more time' when he sees the day of reckoning approaching. In fact I had intended to send you my *World* as a New Year gift, and only two weeks ago I was determined to send you at least a part of it, if the whole work couldn't be copied in time. But I have to say that I inquired in Leiden and Amsterdam whether Galileo's *World System* was available, for I thought I'd heard that it was published in Italy last year. I was told that it had indeed been published but that all the copies had immediately been burnt at Rome, and that Galileo had been convicted and fined. I was so astonished at this that I almost decided to burn all my papers or at least to let no-one see them. For I couldn't imagine that he—an Italian and, as I understand, in the good graces of the Pope—could have been made a criminal for any reason except than that he tried, as he no doubt did, to establish that the earth moves. I know that some Cardinals had already censured this view, but I thought I'd heard it said that it was nevertheless being taught publicly even in Rome. I must admit that if the view is false then so are the foundations of my philosophy, for it clearly follows from them; and it's so closely interwoven in every part of my treatise that I couldn't remove it without damaging the whole work. But I utterly didn't want to publish a discourse in which a single word would be disapproved of by the Church; so I preferred to suppress it rather than to publish it in a mutilated form.

I've never had an inclination to produce books, and I would never have completed this one if I hadn't been bound by a promise to you and some of my other friends. . . . But after all I am sure you won't send a bailiff to force me to pay my debt! And perhaps you'll be quite glad to be spared the trouble of reading wicked doctrines. There are already so many views in philosophy that are plausible and can be maintained in debate that if my views aren't more certain than that and can't be approved of without controversy, I don't want to publish them—ever. But having promised you the whole work for so long, I would be ashamed to try to buy you off with trifling pieces; so as soon as I can I shall, after all, let you see what I have written, but please allow me a year's grace so that I can revise and polish it. . . . Please also tell me what you know about the Galileo affair. [Then a final paragraph about the physics of vibrating strings.]

⊕ [12.xii.33: Descartes writes to Wilhelm expressing humble gratitude for Wilhelm's good opinion of him.]

⊕ [end of 33: Descartes writes to Stampioen offering his solution of a geometrical problem that Stampioen had sent him and, at the latter's request, sending him a geometrical problem in return.]

to Mersenne, ii.1634:

[He opens with assurances that he doesn't infer, either from Mersenne's two-month silence or from his own failure to send Mersenne the promised 'something of my philosophy', that Mersenne's affection for him has waned. Then:] I have decided wholly to suppress the treatise I have written, and to forfeit almost all my work of the last four years, in order to obey the Church's ban on the view that the earth moves. But I haven't yet seen that the ban has been ratified by the Pope or the Council—only by the Congregation of Cardinals set up for the censorship of books—and I would like to know whether the authority of that Congregation is sufficient to make the ban an article of faith; I would also be glad to hear what people in France think about this affair. The Jesuits have helped to get Galileo convicted: Scheiner's book ·*Rosa Ursina*· clearly shows that they are no friends of Galileo's. But the observations in the book provide such good evidence that the sun *doesn't* move that I can't believe that Father Scheiner himself doesn't—in his heart of hearts—share the Copernican view [that the earth moves and the sun doesn't]; and this astonishes me so much that I don't trust myself to write down what I think about it.

As for myself, I seek only repose and peace of mind—goods that can't be possessed by anyone who is angry or ambitious. I'll still have things to do, but for the time being I intend only to instruct myself. I don't think I can instruct others, especially those who would feel threatened by the truth, fearing that if it were known it would deprive them of the reputation they have already acquired through views that are false.

to Mersenne, iv.1634:

It seems that my last letter to you has been lost. . . . In it I told you at length my reason for not sending you my treatise. I'm sure you would find it so just that, far from blaming me for deciding never to show it to anyone, you would be the first to urge me to make that decision if I hadn't already done so.

Doubtless you know that Galileo was recently censured by the Inquisition and that his views about the movement of the earth were condemned as heretical. Now, all the things I explained in my treatise, including the thesis that the earth moves, were so interdependent that the discovery that one of them is false shows that all the arguments

I was using are unsound. I *thought* they were based on very certain and evident proofs, but I wouldn't wish, for anything in the world, to maintain them against the Church's authority. 'Not everything that the Roman Inquisitors decide is automatically an article of faith, but must first be approved by a General Council'—well, perhaps, but I'm not so fond of my own opinions as to want to maintain them by splitting hairs. I want to live in peace and to continue the life I have begun under the motto *Bene vixit, bene qui latuit* [Latin, by Ovid, meaning 'He lives well who is well hidden']. So I'm more •happy to be delivered from the fear that this work would make my social circle larger than I wanted it to be than I am •unhappy at having lost the time and trouble I spent on its composition.

[Mersenne has reported that musicians of his acquaintance had disagreed with certain of Descartes's views about musical intervals. Descartes finds their views 'so absurd that I hardly know how to respond'. Then he responds. Then]

What causes a stone one has thrown to stop moving? Clearly, it is air resistance—something one can easily feel. But the reason why a bent bow springs back is more difficult, and I can't explain it without referring to the principles of my philosophy, which I'm apparently obliged to keep quiet about from now on.

There has been a rumour around here that not long ago a comet was seen; if you have heard anything about this, please let me know. Also: you told me in a previous letter that some people you knew could help to perform the experiments that I wanted done; so let me tell you about an experiment that was published not long ago in ·Leurechon's· *Mathematical Games*. It involves a large cannon placed on flat ground, pointing straight up at the sky, ·and fired·. I would like this experiment performed by people who are interested and have the means. The author of the book says that the experiment has already been performed many times,

and the cannon-ball didn't once fall back to the ground. Many might think this quite incredible, but I don't judge it to be impossible, and I think it's well worth looking into. [This is referred to again on page 79.]

As for the outcomes of Galileo's experiments that you report to me, I deny them all; but I don't infer that the motion of the earth is any less probable. I do indeed agree that if you throw a stone forward from a moving chariot the stone will in some manner retain the motion from the chariot ·in addition to the motion from the throw·, but there are other factors that prevent it from retaining *all* the chariot's motion. As for a cannon ball shot ·horizontally· off a high tower, it must take much longer to reach the ground than one that is simply *dropped* from that height; that's because it meets more air on its way, which resists its vertical motion as well as its horizontal motion.

I'm astonished that an ecclesiastic should dare to write about the earth's motion, whatever excuses he may give. For I have seen official documents about Galileo's condemnation, printed at Liège on 20.ix.1633, which contained the words '. . . even if he pretended he was putting his view forward only hypothetically. . . '; thus they seem to forbid even the use of this ·as a· *hypothesis* in astronomy. So I don't dare to tell anyone any of my thoughts on the topic. Moreover, I don't see that this censure has been endorsed by the Pope or by any Council, but only by a single congregation of the Cardinals of the Inquisition; so I don't entirely lose hope that this case may turn out like that of the Antipodes, which were similarly condemned long ago. So in time my *World* may yet see the light of day; and in that case I'll need my arguments for my own use.

[In a final paragraph, Descartes returns to Mersenne's musicians, saying that either they know nothing about music or they have been merely teasing Mersenne.]

⊕ [13.v.34: Descartes writes to Mersenne about missed letters, the physics of falling bodies (dropped or shot), the physics of a bent bow's return to straightness, the perception of differences between musical tones (more scorn aimed at 'your musicians').]

⊕ [2.vii.34: Descartes writes to Reneri addressing a question about some aspect of the behaviour of siphons. Descartes's answer deploys his doctrine that all motion is 'circular' [see Glossary].]

to Mersenne, 14.viii.1634:

I was beginning to be troubled at not getting your news, but then it occurred to me that you would have been so preoccupied by the printing of the book you recently told me about [probably Mersenne's translation of Galileo's *Mechanics*] that you'd have had no time left for writing. Beeckman came here the other day and lent me Galileo's *Dialogue Concerning the Two Chief World Systems*; but he took it away with him to Dordrecht this morning, so that I've had it in my hands for only thirty hours. Still, I was able to leaf through the whole book, and I find that he philosophises pretty well on motion, though very little of what he says about it is entirely true. As far as I could see, he goes wrong more often when following accepted opinion than when striking out for himself, with the exception of his ·original· treatment of the rise and fall of the tide, which is rather forced. In my *World* I had also explained the tides in terms of the motion of the earth, but in a quite different way from his. But I must admit that in his book I have come across some of my own thoughts, including (among others) two that I think I wrote to you about some time ago. **(1)** The first is that the distance covered by a falling heavy body is proportional to the square of the time the body takes to fall. [Note in CSMK: Descartes is mistaken here: in his law the distance travelled is proportional, not to the square of the time, but to another power of the time, namely $\frac{log 2}{log \frac{1}{3}}$.] For example, if a ball takes

three seconds to fall the first three feet it will take only one second to fall the next three, and so on. I said that this holds only with many qualifications, for it's never exactly true ·just as it stands·, as Galileo thinks he has demonstrated that it is. **(2)** The second idea is that the up-and-down vibrations of a cord ·under tension· take practically the same amount of time, even though some cords are very much longer than others.

The arguments he uses to demonstrate the movement of the earth are very good; but it seems to me that he doesn't present them in a way that will make them convincing. He keeps introducing digressions that make the reader forget the earlier arguments when he is engaged in reading the later ones.

As for what he says about a cannon that is fired horizontally: if you perform that experiment precisely, I believe you'll find observable differences ·between your results and what Galileo says will happen·.

As for the other things you write about, I haven't time to reply if I'm to catch the next post. Anyway, I can't thoroughly answer any question in physics without first setting out all my principles, and the only way I can do that is by presenting the treatise that I have decided to suppress.

Here's the text of the document printed at Liège:

'The said Galileo, therefore, who had confessed at an earlier interrogation, was summoned to the Sacred Tribunal of the Inquisition, interrogated and detained in custody. He clearly showed himself once again to be still of the same opinion, though he pretended that he put forward his view only hypothetically. The outcome is that the authorities of the Inquisition, after discussing the matter thoroughly, have declared that the said Galileo is under strong suspicion of heresy, because he seems to have followed a doctrine that is false and contrary to Scripture, namely that

•the sun is the centre of the universe and does not rise
  from sunrise to sunset, whereas
•the earth moves and isn't the centre of the universe,
or to have been of the opinion that this doctrine could be
defended as a probability, although it has been declared to
be contrary to Holy Scripture.'

to Beeckman, 22.viii.1634:

I'm glad that you still remember the disagreement we had
recently. I see that you are still not satisfied with the
argument I used then, so I'll write frankly about your reply.
But first I'll give a brief account of the whole matter, so that
we can be clear about what we are arguing about.

I didn't say at our meeting that **(i)** light instantaneously
moves to the eye from the light-emitting body, but that **(ii)** it
instantaneously **arrives at the eye** from the light-emitting
body. (In your letter you attribute **(i)** to me; you also say that
there's no difference between the two. You're wrong on both
points.) And I also said that I was so certain of this that if it
were shown to be false I would admit that I knew absolutely
nothing in philosophy.

You, on the other hand, maintained that light can move
only in time, and you added that you had thought up an
experiment that would show which of us was mistaken. . . .
It goes like this:

> In the night someone holds a torch in his hand and
> waves it around while watching the reflection in a
> mirror a quarter of a mile away. He'll be able to tell
> whether he feels the movement in his hands *before* he
> sees it in the mirror.

You were so sure of the outcome of this experiment that
you admitted that your entire philosophy would have to be
regarded as false if there was no observable time-lag between
the instant when the movement was felt by the hand and the
instant it was seen in the mirror. And if such a time-lag was
detected, *my* philosophy would, I admitted, be completely
overturned. What was at issue between us was not so much

> Does light travel instantaneously or does it take time
> to get anywhere?

but rather

> •What will the outcome be of this experiment?

But the next day, wanting to be done with this dispute
and to save you from pointless labour, I told you of an-
other experiment—already carefully done by many attentive
observers—which shows clearly that there is no time-lag
between the instant the light is emitted from the luminous
body and the instant it enters the eye.

[Descartes's account of his experiment is interspersed with bits of
his reason for regarding Beeckman's as useless. The present version
separates the two, but doesn't alter the content of either.] First, there
was the issue of your experiment. I asked you to settle what
the smallest observable interval would be between t_1 when
the torch is moved and t_2 when the movement appears in
the mirror a quarter of a mile away. The day before, you
stipulated that this time-interval would have to be at least
as short as a single pulse beat; but then more liberally you
allowed that it could be as short as I liked. So, to show that I
didn't want to take advantage of your concession, I assumed
that the interval was no longer than one-twenty-fourth of
a pulse beat; and you agreed that that interval would be
undetectable in your experiment.

But it would be perfectly detectable in mine. To explain
this experiment to you, I first asked whether you thought
•that the moon gets its light from the sun and •that eclipses
occur because the earth comes between the sun and the
moon or the moon comes between the sun and the earth.
You answered Yes to both. I then asked how you suppose

the light from ·the sun and· the stars reaches us, and you replied 'in straight lines'. On your view, therefore, we never see the sun in its true position but only in the position it had at the moment when the light we're seeing now was emitted from it. Let us now suppose that the distance between the moon and the earth is fifty times the radius of the earth, and astronomy and geometry together imply that that radius is at least 600 miles long. Now if light takes $\frac{1}{24}$ of the interval of a pulse beat to cross a quarter of a mile twice, it will take an interval of 5,000 pulse beats, i.e at least one hour, to cross the space between the moon and the earth twice, as is obvious when you work it out.

I'm arguing here on the basis of the points that you conceded. [Descartes's description of the 'experiment' is needlessly hard to follow, but it's basic point is simple. Let T be a time at which we on earth see the moon starting to undergo an eclipse; we may think that the sun, earth and moon are coming to be on a straight line at that time, but according to Beeckman's thesis that light travels at a velocity not greater than 12 miles per pulse-beat they aren't. The start of the eclipse as we see it represents where the sun was *an hour earlier*, when its position relative to the earth was different (Descartes is careful to say that it doesn't matter whether it's the sun or the earth that is moving!) Descartes continues:] The careful and painstaking observations of every astronomer testify, and countless experiments confirm, that when the moon is seen from earth to be undergoing an eclipse, the sun and earth and moon *are* in a straight line. This shows that light takes no detectable time to travel a vast distance, whereas your experiment doesn't show anything either way. I claimed that this argument is conclusive; you called it fallacious and question-begging.

[Descartes devotes more than a further page to arguing against Beeckman's accusation.]

to Morin, ix or x 1634:

The fine book that you did me the honour of sending to me has arrived. I am grateful—especially since I have done nothing to deserve it, having never had occasion to do you any favour that could put you in mind of me. The work you have put into finding longitudes certainly deserves a public reward; but because scientific discoveries are too valuable to be rewarded with money, God seems to have arranged things so that monetary rewards usually go to those who achieve large-scale mechanical things or to those whose actions are low and servile. So I'm sure that an artisan who made fine lenses would make more money from them than I would from all the thoughts in my *Optics* if I planned to sell them. . . .

⊕ [15.iv.35: Descartes writes to Huygens: Friendly and apologetic for perhaps writing to Huygens at a time when military goings-on are pre-occupying him. He is sending his drawings of certain machines, to have improved versions made of them.]

⊕ [6.v.35: Huygens writes to Descartes: Apologies for delay in reading things Descartes has sent to him.]

to Golius, 16.iv.1635:

[Descartes thanks Golius for telling him about a particular maker of optical lenses; as soon as he can he'll visit 'that town' in order to see him. Then:] But what counts for more than all the lathe-operators in the world is that I've had the opportunity here in Amsterdam to meet Constantijn Huygens. After putting up with hearing a reading of part of my *Optics*, he offered to run some tests on my behalf. This relieved me of all worry on that score, because I'm sure that if the experiment *can* be carried out Huygens will find out how to do it sooner than anyone else could. He really is above all praise that I know how to give, and I have heard him praised

extremely highly by people who should know. His example shows that a single mind can occupy itself with many things and perform splendidly in all of them, and remain cleanly focussed when all sorts of other thoughts are clamouring to be let in, yet also retaining a freedom that isn't spoiled by the constraints of the ·royal· court. There are personal qualities that make a person admired but not loved, and others that make him loved without that adding to one's admiration; but I find that Huygens has perfections for which he is lovable and admirable. And I'm more than a little proud of the fact that whenever he said anything I pretty well understood it before he had finished explaining it. If the Socratic theory of metempsychosis [see Glossary] and reminiscence [see Glossary] were true, I would believe that in an earlier life he had the body of a man who had all the thoughts that I have now.

[Descartes closes with remarks about •how he is confirmed in the reasonableness of his own views by seeing them held so perfectly by Huygens, •his gratitude to Golius for making this meeting possible, and •sympathy for Golius in his current illness.

to Golius, 19.v.1635:

[Descartes explains his delay in replying to Golius's last. Then:] I changed my lodgings recently, and haven't yet had the time to interrogate sea-water to see if I could discover the cause of phosphorescence.

The observations by you and Schichardus on coronas and parhelia completely confirm the view I had; so that I won't want anything more on that topic. . . . Let me tell you about another observation I made one night about a week ago when I was on the Zuider Zee on my way from Friesland to Amsterdam. Resting my head on my right hand for quite some time, I covered my right eye with my hand, keeping the other eye open. The room I was in was rather dark until someone brought in a candle. As soon as I opened both eyes, I saw two coronas around the candle, with more perfect colours than I thought ever possible, just as you see in the drawing here. [Reddish brown on the outer circle, blue inside that, and the other 'rainbow colours' sandwiched between those two. We don't need the drawing or the further minor details. The account ends:] I had plenty of time to observe these things, for they lasted right up until I fell asleep some two or three hours later.

This showed me that the coronas were arranged in exactly the opposite way to those that appear around stars, i.e. red at the outside; and I also found they formed not in the air but in the water of one of my eyes, for when I closed my right eye and opened my left I didn't see them at all; and when I then closed my left eye and ·again· opened my right, I still couldn't see them. I think I can explain this quite well. I am so by this observation that I mustn't forget to include it in my *Meteorology*. . . .

to Mersenne, vi or vii 1635:

. . . .As for the lenses, I have to tell you that after Galileo's condemnation I revised and completed the treatise that I began some time ago [the *Optics*]. I have detached it completely from *The World*, and am planning to have it published separately quite soon. . . .

[Descartes now addresses six numbered items in the letter of Mersenne's that he is answering: about **(1)** the weight of an extremely light kind of wood; **(2)**–**(3)** the sonic properties of that wood; **(4)**–**(5)** other aspects of sound-production; then finally:]

(6) I don't think that heat is the same thing as light or as the rarefaction of air. I think of it as something quite

different, which can often arise from light and give rise to rarefaction. I no longer believe that heavy bodies fall because of some real quality [see Glossary] called heaviness, as philosophers imagine, or because of some attraction of the earth. But I couldn't explain my views on all these topics without publishing my *World* (with the forbidden movement ·of the earth·), and the time isn't ripe for that—I'm very surprised that you're planning to attack the book *Against the Movement of the Earth*, but I leave this to your own discretion. [He means: I leave it to you to decide whether this is a risk worth taking.]

⊕ [28.x.35: Huygens writes to Descartes, encouraging him not to be dissuaded from publishing his *Optics* by a fear of rejection by the public. He offers •a recommendation of a trustworthy printer; •suggestions about typography and page layout; •news about a supposedly forthcoming machine for shaping lenses; and •remarks about someone who claims to have produced a perfectly circular lens through which one could read a letter at a distance of over three miles—'If it's true, I'll pay him a good price for one'. Apologising for the seeming extravagance but insisting that it's the sober truth, he says 'You have left me with a strong impression of something superhuman about you'.]

to Huygens, 1.xi.1635:

I am obliged to you beyond words, and am amazed that having so many important tasks you're willing to see to all the details of the printing of my *Optics*. That is an excess of courtesy and sincerity that's going to cause you more trouble than you expect. I'll try to follow the detailed instructions that you kindly gave me on these external matters; and—by way of repayment!—I shall be so bold as to ask you to correct the content of the book before I let it go to the printer. At least that's what I shall do if this winter you live somewhere more accessible than your present abode, so that I'll be able to discuss things with you. The three mornings I had

the honour of spending in conversation with you left me with such an impression of the excellence of your intellect and the soundness of your judgement that—I mean this literally—I don't know of anyone else in all the world who could be so confidently entrusted to discover my errors as you. and. . . .I'm sure that you would rather I knew my own errors and removed them than that they should be seen by the public.

I plan to add the *Meteorology* to the *Optics*, and I worked pretty hard at this during the first two or three months of this year, because I found many difficulties that I hadn't yet tackled and that it was a pleasure to resolve. But. . . .as soon as I had lost hope of learning anything more about this subject, having nothing more to do in it except to tidy up what I had written, I *couldn't* make myself •do that work or •write a preface that I'd be satisfied with. So I'll need another two or three months before speaking to the publisher.

You are unique in how much you combine promptness with patience, and manual dexterity with intellectual skill. [This leads into some remarks about the shaping of lenses; it seems that Huygens favours the hyperbola, Descartes the circle.]

from Huygens, 3.xii.35:

The lens-maker in Amsterdam slowed down in his work for me, but now here he is at the end of my hyperbola—though in saying that I am exaggerating a little. His first attempt seems to have gone well. Other lens-makers who have seen the model of what he wants to do have said that if he succeeds they'll eat dirt; but that's to be expected—if he succeeds *their* trade will be ruined. [The letter continues on this theme: what a skilled artisan can do if he has the right instruments; where Descartes's work fits into this; and so on.]

⊕ [8.xii.35: Descartes writes to Huygens, saying that he has been to test the lens that Huygens had sent him, and found that the lens-maker hadn't properly followed Huygens's prescription. He writes at length about his own work on designing and making lenses, and thinks it is on a more promising path than Huygens's.]

to Mersenne, iii.1636:

About five weeks ago I received your most recent letter, dated 18.1.36; I hadn't received its predecessor until four or five days before that. I postponed replying to you because I reckoned on being able to tell you soon that I had sent ·my work· to the printer. [This refers to *Discourse on the Method*, with the *Optics*, *Meteorology*, and *Geometry*, published by Jean le Maire of Leiden in June 1637.] That's why I came here to Leiden, because the Elzevirs have said they would like to be my publishers. But now that they've seen me here they seem to think that they have *caught* me, which has led them to make difficulties; so I have decided to drop them. I could find several other publishers here, but I shan't settle with any of them until I have news from you, provided I don't have to wait too long. If you think that my manuscripts could be printed in Paris more conveniently than here, and if you would be willing to take charge of that as you once kindly offered to do, I could send them to you as soon as you gave the word. However, there are two difficulties. •My manuscript is no better written than this letter; the spelling and punctuation are equally careless; and the diagrams are drawn by me, i.e. very badly (so that the engraver won't understand them unless you explain them on the basis of your understanding of the text). •I would like to have the whole thing printed in a handsome font on handsome paper, and I would like the publisher to give me at least 200 copies because I want to distribute them to a number of people.

You'll want to know what I am planning to have printed. There will be four treatises, all in French, and the general title will be as follows:

> The Plan of a Universal Science that can raise our Nature to its Highest Degree of Perfection. And the *Optics*, the *Meteorology* and the *Geometry*, in which the Author supports his proposed universal Science by explaining the most abstruse Topics he could find, doing this in such a way that even beginners can understand them.

In this *plan* [which is of course the *Discourse on the Method*] I reveal a part of my method, try to prove the existence of God and of the soul apart from the body, and add many other things that I don't expect to displease the reader. In the *Optics*, besides treating of refraction and the manufacture of lenses, I give detailed descriptions of the eye, of light, of vision, and of everything belonging to catoptrics [see Glossary] and optics. In the *Meteorology* I dwell principally on the nature of salt, the causes of winds and thunder, the shapes of snowflakes, the colours of the rainbow—here I try also to show the nature of each colour—and the coronas or haloes and the mock suns or parhelia [see Glossary] like the ones that appeared at Rome six or seven years ago. Finally, in the *Geometry* I try to give a general method for solving all the so-far-unsolved problems. All this I think will make a volume no bigger than fifty or sixty sheets. I am sticking to my long-held resolve not to put my name to it; please don't mention it to anybody except when you think it proper to mention it to some publisher to see whether he would like to have the job. But don't make any contract for me, please, until you hear my reply; I'll decide on the basis of what you tell me. I would prefer to use a publisher who isn't in contact with Elzevir. . . .

I have used up all my paper in telling you this. There's just enough space left for me to say that examining what Galileo says about motion would take me more time than I can spare just now.

•I think that the experiment showing that sounds travel no faster with the wind than against the wind is correct, at least so far as the senses are concerned; for the movement of sound is a quite different thing from the movement of wind. •Thank you for the account of the ball shot vertically that doesn't drop back; it is very remarkable. •As for the subtle [see Glossary] matter of which I have often spoken, I think it's the same matter as terrestrial bodies; but just as air is more fluid than water, so I suppose that this matter is much more fluid = liquid, and more penetrating, than air. •A bow bends back because when the shape of its pores is distorted the subtle matter that passes through tends to restore them, whichever side it enters from.

⊕ [31.iii.36: Huygens writes to Descartes about arrangements for them to meet at the country home of Huygens's sister and her husband, not far from Leiden, where Descartes is now visiting to supervise the printing of his book. He hopes to be allowed to see some of Descartes's *World*, and jokes that 'my avarice ·is like a lake that· has no bottom and no shores'.]

⊕ [31.iii.36: Descartes (in Leiden) tells Huygens that he will call on him the next day, bringing with him all his writings that are fit to be seen, for Huygens to choose the ones he will read and criticise.]

⊕ [11.vi.36: Descartes writes to Huygens: 'I am sending you a masterpiece from my hand, the model of a hyperbola that you asked me to have done by someone else', and the letter continues joking, e.g. about Descartes's well-known skill as a portrait-painter.]

⊕ [15.vi.36: Huygens (now back in The Hague) writes to Descartes: a little about lens-making, encased in a thick crust of jokes.]

⊕ [Huygens writes to Descartes, sending a new sample lens. He says that his lens-maker says that on this second attempt he has done 'everything that the mind can ask of the hand'; Descartes's reaction and further instructions are awaited.]

⊕ [13.vii.37: Descartes writes to Huygens: The lens-maker has done what was asked of him, but this second attempt is scarcely better than the first because of tiny irregularities on the surface of the glass. More about this, and some personal matters.]

⊕ [23.x.36: Huygens writes to Descartes expressing extreme impatience to see Descartes's published book.]

⊕ [30.x.36: Descartes writes to Huygens bringing him up to date on how the publishing of the book is going, including the information that illustrations are being handled in exactly the way Huygens has suggested. He reports that he isn't doing any serious work. 'I would have been seriously upset about this if I didn't know that my mind is like infertile ground that won't be productive unless it is first allowed to lie fallow for a few years.']

⊕ [1.i.37: Descartes writes to Huygens: Greetings for the New Year, and a request that Huygens send the enclosed offprints to Paris in order to obtain a royal 'privilege' (*privilège*, a kind of license to publish that is needed to protect the interests of Descartes's Dutch publisher in Paris).]

from Huygens, 5.i.1637:

Your packet [containing offprints of the works Descartes is having published] will leave here today in company with something you would be very upset to lose, and there's no doubt that the person by whom I am sending it will very punctually do his part. . . . In leaving my hands the material has finished the dangerous part of its voyage, because my hands have been sorely tempted to snatch it from Mersenne's! But my greed gave way to your interests, and I shall now wait, armed with Stoic patience, for the opportunity to read one of the works when the whole thing is published. Actually, I'm lying; I *have* flipped through it; but that was under the pressure of so many different occupations—none of them anywhere near true wisdom—that I took in hardly anything except the quality of the printing and of the diagrams, which seemed to me equally satisfactory. As regards formal features: I'd have

liked the paper to be a bit less shiny, and the pages—quarto but nearly as long as small-folio—to be correspondingly wider by having more generous margins. But narrow margins are just one of the ways in which printers show their greed! Anyway, we aren't going to learn anything from the *form*; it's the *matter*, the content, that will occupy us. . . . I'm delighted to see how well the proof-reading has been done. If your own (evidently considerable) labour on this has left you tired, I'll present myself at Leiden to do what remains to be done. Rather that than see us spending an extra day waiting for this excellent work.

⊕ [25.ii.37: Huygens writes to Descartes enclosing •a certificate of delivery of the material Descartes asked him to send to Paris, and (asking for comments) •something Mersenne has sent him relating to a work that Mersenne has just finished.]

⊕ [27.ii.37: Descartes writes to Huygens in warmly appreciative terms. He declines to comment on the Mersenne material Huygens had sent him because 'he's an extremely good friend' and also because he has read very little of Mersenne's writings. Golius had told Descartes that Huygens thought that the title *Discourse on the Method* should have the word 'Discourse' removed, analogously to the titles of the other works. Descartes replies that in the other works he aimed to •cover the whole of optics, geometry, etc. whereas he is merely •'saying something' about his method.]

to Mersenne, iii.1637:

You must have a very poor opinion of me, regarding me as very inconstant and irresolute, since you think that what you tell me should lead me to change my plan and attach my opening Discourse to my Physics. . . . I couldn't help laughing at your suggestion that I'm forcing the public to kill me so as to see my writings sooner. I can only reply •that the writings are now in such a place and condition that someone who

killed me would never lay his hands on them; and •that if I don't die in my own good time and on good terms with the survivors no-one will see my works for more than a hundred years after my death.

Thank you for objections that you have sent me, and I beg you to continue to tell me all those you hear. Make them as unfavourable to me as you can; you couldn't please me more. I'm not in the habit of wailing while my wounds are being treated, and anyone kind enough to instruct and inform me will always find me very teachable.

But I don't understand your objection to the title. I didn't put *Treatise on the Method* but *Discourse on the Method*, which means 'preface to the Method' or 'announcement of the Method', to show that I'm not trying to teach the method but only to discuss it. Any reader can see that it's practical rather than theoretical. I call the other treatises *Essays in this Method* because I claim that what they contain could never have been discovered without it, so that they show what it's worth. And I put into the first *Discourse* a bit of metaphysics, physics and medicine, to show that my method extends to topics of all kinds.

Your second objection is that I haven't explained fully enough how I know that •the soul is a substance distinct from the body and that •its nature is solely to think. This, you say, is the only thing that makes my proof of God's existence hard to understand. I admit it. But my best way of dealing with this topic was to explain in detail the falsehood or uncertainty of all judgements that depend on the senses and the imagination, in order then to show which judgements depend only on the pure understanding, and what evidentness and certainty they have. But I deliberately chose not to go that way, mainly because I was writing in the vernacular, and was afraid that readers who weren't very bright might •embrace the doubts and scruples that

I'd have had to propound, and •not be able to follow as fully the arguments by which I would have tried to remove them—setting them on a false path and not being able to bring them back off it. But about eight years ago I wrote a fairly full presentation of that argument in Latin (in the beginnings of a treatise of metaphysics); I could have that included in my present book if a Latin version of it is made, as is planned. But I do think that readers who study my arguments for God's existence will find that the more they try to fault them the more compelling they are. I claim that they are clearer in themselves than any of the demonstrations of geometers; in my view they're obscure only to those who can't withdraw their minds from their senses. . . .

I'm extremely grateful for your offer of help with the printing of my manuscripts; but if any expenses are involved they must be met by me, and I'll make sure of sending to you whatever is necessary. I don't think in fact that there will be any great expense; some publishers have offered me gifts to get me to engage them, even before I had left Paris or begun writing. So there may still be publishers foolish enough to print my works at their own expense, and readers gullible enough to buy copies and save the publishers from their folly. I shall want to lie low, not distancing myself from my works as though they were crimes, but merely wanting to avoid being disturbed and to keep the liberty I have enjoyed up to now. I won't be very alarmed if some people know my name; but for the present I prefer that no-one says anything about my forthcoming work—that way my work won't fall short of expectations because no expectations will have been raised. . . .

⊕ [1.iii.37: Huygens writes to Descartes apologising for having criticised, ignorantly, the title of *Discourse on the Method*. (See 25.ii.37 above.) He gently complains that Mersenne 'has this time served not as my intermediary but as my third hand, without alerting me'.]

to Huygens, 3.iii.37:

[The letter opens with a complaint that Mersenne has messed up the matter of the royal 'privilege' (see 1.i.37), making Descartes's name known and having it appear in the 'privilege'. (He had wanted the book to be published anonymously.) Then an explanation of why he can't yet send the *Discourse on the Method* to Huygens. Then:]

At this time I'll send you only the *Meteorology* and the *Optics*. I'll be infinitely obliged if you will take the trouble to read them, and mark (or get someone to mark) your corrections in the margin, and then let me see them. If your wife was willing to add hers, I would regard that as an inestimable favour. I think much more highly of her judgment—she who is by **nature** excellent—than I do of the judgment of many philosophers, whose **art** or training often makes them judge badly. I am already very proud of the fact that she condescended to listen to a reading of a part of the *Meteorology*.

⊕ [22.iii.37: Descartes writes to Huygens sending the *Discourse on the Method* for him to forward to France 'along with yours'. (Huygens's high position in the Dutch government made it easier for him than for Descartes to get things safely to Paris.) In a PS: 'This letter will be presented to you by the young Schooten. Don't judge him by how he looks; there's more to him than appears on the surface.' (Franz Schooten jr. did all the drawings for Descartes's current publication and, later, for the *Principles of Philosophy*.)]

⊕ [24.iii.37: Huygens writes to Descartes with glowing praise for the *Discourse on the Method*, which he has read. He isn't competent to read the *Geometry*, but young Schooten has tutored him in this ('I took time off from my work'), and 'I can learn to see a little into this mystery'.]

⊕ [29.iii.37: Descartes writes to Huygens expressing rapturous pleasure at Huygens's opinion of the *Discourse on the Method*, and begging him to express any criticisms he has of the work. He is sending a copy of

the *Discourse* for Huygens to keep, and of the *Geometry*, to add to the *Meteorology* and *Optics*, which he already has. They are not bound like proper books. 'I am sending you two naked infants. . . .with two or three sheets missing, which are needed to clothe these babies when they first enter the world. Women know more about these things than men, so with your permission I shall commend these two to the care of Madame your wife and Madame your sister.']

to Silhon, iii.1637:

You are right that in the work you have seen there is a great defect, concerning the arguments by which I think I prove that there's *nothing* more evident and certain than the existence of God and of the human soul. The defect is that I haven't presented those arguments in a way that would make them easy for anyone to grasp. I didn't want to run the risk of doing that: I'd have had to present at length the sceptics' strongest arguments to show that there is no material thing of whose existence one can be certain. That would have accustomed the reader to detach his thought from things that are perceived by the senses; then I'd have shown that a man who doubts everything material still can't have any doubt about his own existence. From this it follows that he—i.e. the soul—is a being or substance that isn't at all corporeal, whose nature is solely to think, and that it's the first thing that can be known with certainty. Anyone who spends enough time on this meditation will gradually acquire a very clear—I would even say *intuitive*—knowledge of intellectual nature in general [= 'knowledge of what it is to be a thinking thing']. This idea when taken without any limitation represents God to us, and when limited it's the idea of an angel or a human soul. Now, a reader can't fully understand what I say later about God's existence of God unless he comes at it in this way, as I hinted in the *Discourse on the Method.*

But I was afraid that this introduction would look at first as if it were designed to bring in scepticism, and would disturb weaker minds, especially as I was writing in the vernacular. So I didn't dare to put in even the little I said about this without some words of warning. But more intelligent people like you, Sir, if they take the trouble not only to read but also to follow me in meditating on the various topics, spending long enough on each point to check whether I have gone wrong, I think they'll come to the same conclusions as I did. I'll be glad to try to explain this further, when I have time.

⊕ [20.iv.37: Descartes writes to Huygens expressing relief at the news that Huygens's wife seems to be recovering from a recent illness (in fact she died three weeks later); and accepting a correction to one of Descartes's diagrams—'I admit that Schooten and I went astray'.]

Fermat to Mersenne, iv or v 1637:

You ask for my judgment on Descartes's *Optics*. De Beau-grand lent me the work but didn't give me long to read it, and that seems to excuse me from providing exactly and in detail what you want; and the subtlety and complexity of the work tells me that you want more than informal half-considered thoughts. [He goes on to say that nevertheless he *will* say what he thinks about the *Optics*: it's good to search for the truth, and we often find it by groping in the dark, so perhaps his offerings may be useful and perhaps some day he'll be able to build them into something good. He then sets the scene: the study of refractions [see Glossary] has so far led nowhere; what is needed is a general formula by which, given one refraction-angle for a given medium we can then find all the others. Then:] So now it remains for Descartes to exercise his intelligence and reveal to us some new insights into translucent bodies that have so far produced such obscurities.

The first two parts of the *Optics*—about light and refraction—seem to me to be the main ones, because they contain the foundations of the science from which we then see Descartes drawing beautiful conclusions.

Here, in brief, is his reasoning. Light is simply bodies' inclination to move; and •the inclination to move probably follows the same laws as •actual movement does. So we can get the rules governing the effects of light from our knowledge of the rules governing movement.

He considers the movement of a ball when it is reflected and when it is refracted. I shan't here repeat his whole treatment of these matters—that would be useless and boring—so I'll settle for giving you my comments on it.

Firstly, I am not convinced that •the inclination to move should follow the same laws as •movement, because they are *different*—as different as •potentiality and •actuality. And the gap seems especially large in this case, because the movement of a ball can be faster or slower, depending on the forces acting on it, whereas light goes through a translucent body in an instant, apparently with no succession involved [i.e. with no facts about when it was at one point, when it was an inch further on, and so on]. But geometry doesn't get involved in going deeper into these issues in physics.

[Fermat then has two pages of technical criticisms of Descartes's purported proof of a 'law' about how angles of incidence relate to angles of reflection; concluding that the same criticisms apply to Descartes's treatment of incidence and refraction, which is (he says) based on the same faulty reasoning. He continues:]

That's my view of these new propositions from which Descartes draws splendid conclusions about the right way to shape optical lenses. So splendid that I wish the premises had been better supported than they are. But I think that they lack not only support but truth.

I had been going to reveal to you my own thoughts on this topic; but •I am not yet perfectly satisfied with them, and anyway •I would rather wait until I know the outcomes of the experiments you have done, or are going to do at my request, regarding the relations between angles of incidence and angles of refraction. I'll be most grateful if you would send me all that as soon as you can, and I promise that in return I'll tell you some new things about this matter.

What I have said here doesn't prevent me from greatly admiring Descartes's intellect and ingenuity; but it needs a communal effort to get the truth—which I think is still hidden from us—about this subject. . . .

⊕ [27.iv.37: Descartes writes to Mersenne complaining that Mersenne is •making a mess of the application for a royal 'privilege' and •needlessly showing the forthcoming book to too many people.]

to Huygens, 20.v.1637:

Although I have withdrawn to a very secluded place, the sad news of your affliction has reached me even here. If I measured you by the standards of ordinary souls, the sorrow you have experienced since your wife fell ill would lead me to fear that you would find her death quite unendurable; but I'm sure that your life is governed entirely in accordance with reason, so I'm convinced that you'll find that consoling yourself and regaining your former peace of mind is easier now that all hope of remedy has gone than it was when you still had cause to fear and hope. Once hope is gone, desire ceases or at least grows weaker, and the sense of loss can't be very pressing when one has little or no desire to recover what has been lost. It's true that ordinary minds don't appreciate this argument; they imagine (without knowing it) that whatever was the case once can be the case again, and that God's love for them *obliges* him (as it were) to do

whatever they wish. But a soul as strong and noble as yours knows the condition God has given us from birth and accepts the necessity of his law. This does involve some pain, but I value love so highly that I think that anything we endure for the sake of it is pleasant—so that even those who are about to die for the good of those they love seem to be happy to their last breath. While you were going without food or sleep so as to care for your invalid, I feared for *your* health, but I'd have thought it sacrilegious of me to try to divert you from such a devoted and tender task. But your grief, now that it can't be of any use to her, can no longer be so appropriate, and hence can't now be accompanied by the joy and inner contentment that follows virtuous actions and makes wise people happy in all the vicissitudes of fortune. So if I thought that your reason wasn't able to overcome your grief, I would visit you and do what I could to distract you, that being the only cure I know for such distress.

I'm taking no account here of your own personal loss in being deprived of a companion whom you dearly cherished, for it seems to me that our own troubles can't be compared with those of our friends—it's a virtue to feel pity at the slightest afflictions of others, to grieve over our own is a kind of feebleness. Besides, you have so many close relations who are devoted to you that you could have no cause to complain on that score; and although you have only one sister, Madame de Wilhelm, I think she alone is all you need to rescue you from the solitude and household cares that anyone but you would dread after losing his partner. Please excuse the liberty I have taken here in expressing my thoughts as a philosopher.

I have just received a parcel from your part of the country. I can't understand what Mersenne is up to: he still hasn't sent me any licence to publish, and seems intent on obliging me by doing the very opposite of what I ask.

to Mersenne, late v.1637:

You argue that if the nature of man is solely to think, then he has no will. I don't see that this follows; for willing, understanding, imagining, and sensing and so on are just different ways of thinking, and all belong to the soul.

You reject my statement that *In order to act well it is sufficient to judge well*; yet it seems to me that the common scholastic doctrine is that *The will doesn't tend towards evil except when evil is presented to it by the intellect as some kind of good*—which generates the slogan *Whoever sins does so in ignorance*—so that if the intellect never represented anything to the will as good without its actually being so, the will could never choose wrongly. But the intellect often represents different things to the will at the same time, and that is the source of *I see and praise the better, but I follow the worse* [Latin *video meliora proboque deteriora sequor*, by Ovid; Descartes also gives the other two emphasised statements in Latin]. This applies only to weak minds, as I said in the *Discourse on the Method*. The well-doing I'm talking about can't be understood in a theological sense—for *there* grace comes into the picture—but simply in the sense of moral and natural philosophy, where no account is taken of grace. So I can't be accused here of the error of the Pelagians [see Glossary]. Analogously: if I said that *To be a man of honour you need only good sense*, it would obviously be irrelevant to object that you need to be a man and not a woman.

Similarly, when I said that 'The world was created just as it should be' is probable, I meant •probable according to human reason; I wasn't denying that perfect faith can make it •certain. . . .

I don't find in your two letters anything else that needs a reply, except that you seem to be afraid that the publication of my opening *Discourse* may commit me to never publishing

my *Physics*. You needn't be afraid of that, because I don't anywhere promise never to publish it during my lifetime. I merely say that I did once plan to publish it but (for reasons that I give) I have decided not to do so during my lifetime. . . . That implies that if the reasons that prevent me from publishing should change, I could reasonably make a fresh decision, because 'When the cause is removed, the effect is removed' [Descartes gives it in Latin: *sublata causa tollitur effectus*]. You say also that people may think I am boasting when I say things about my *Physics* without actually presenting any of it. Well, perhaps; but I won't be accused of that by anyone who •reads not only my opening *Discourse* but the whole book, or by anyone who •knows me. And such a person won't reproach me, as you do, for despising my fellow men because I don't press on them a gift that I'm not yet sure they want. I spoke of my *Physics* as I did solely in order to urge those who want to see it to put an end to the causes that prevent me from publishing it.

Once more, I ask you to send us either the licence to publish or the refusal of it, as promptly as possible. I would rather have it in the simplest form than have it in the most ample form one day later.

to ***, late v.1637:

[This was written to some friend of Mersenne's.]

In revealing my name, Mersenne has done the very opposite of what I asked, but I can't hold it against him because his action had given me the honour of being acquainted with someone of your merit. But I have good reason to dissociate myself from his application for the licence to publish that he says he wants to try to obtain for me; for he introduces me as praising myself, describing myself as the discoverer of many fine things, and as saying that I intend to publish

treatises other than those already in print. This contradicts what I wrote both at the beginning of the *Discourse on the Method* and in other places. But I'm sure he will let you see the letter I am sending him, since I learn from your very kind letter that it was you who obliged me by suggesting to him some of the objections that I deal with.

As for the treatise on physics that you have been so kind as to urge me to publish, I wouldn't have been so rash as to speak of it in the way I did if I hadn't been anxious to publish it if •the public wanted it and if •it would be safe, and also profitable, for me to do so ['profitable' is based on reading AT's *j'y trouve mon conte* as slip for *j'y trouve mon compte*]. But I want you to know that my whole purpose in the present publication is to prepare the way and to test the waters [*sonder le gué*, literally 'to find out how deep the shallows are']. To this end I am proposing a general method. I'm not actually *following* the method, but trying to let it show its paces in the three treatises that follow the *Discourse ·on the Method·* in which I describe it. [In what follows, 'philosophy' = 'natural science'.]

> •The first treatise [*Optics*] is a mixture of philosophy and mathematics.
> •The second [*Meteorology*] is entirely pure philosophy.
> •The third [*Geometry*] is entirely pure mathematics.

In these treatises I can state that I didn't refrain from discussing anything (at least anything knowable by the power of reasoning) because I lacked knowledge of it. So I believe that I am using a method that could be used to explain any other subject just as well, provided I had done the required experiments and taken time to think about them. Also, to show that the method can be applied to everything I have included brief remarks on metaphysics, physics and medicine in the opening discourse. If I can get the public to view my method in this way, I don't think I'll have any reason to fear that the principles of my physics will be ill

received; and if I encountered only critics who are as well disposed towards me as you are, I would have no fear of it from now on.

to Mersenne, 25.v.1637:

[Descartes apologises for having offended Mersenne by what he wrote in a recent letter. Some of Mersenne's conduct had seemed to him to increase the risk of not getting permission to publish, and Mersenne had gone against his wishes by showing Descartes's writings to other people and telling them who wrote them. But he is sure that this was all well-meant, and he is grateful for Mersenne's unceasing friendship and support. Then:]

I have received all the packets that you mention in your last letter; but I didn't comment on the list of printing errors, because they had already been printed; or on the passage from St Augustine ·that you sent me·, because he seems to me to be using it [i.e. the inference from *I think* to *I exist*] quite differently from how I do. [He reports that Huygens has received the books that Mersenne sent him; and if he hasn't written to acknowledge them, that's because of the illness and death of his wife. He mentions two 'small books' that Mersenne has sent him, and expresses approval of one and contempt for the other. Then:] You also sent me a proposition [here = 'problem'?] by the geometer Fermat; it is very fine and has given me great pleasure, because it is easy to resolve through what I have written in my *Geometry*, where I present the general method ·for dealing with such problems· for three- as well as for two-dimensional figures. I expect that if Fermat is honest and open, he will be one of those •who give my work the best reception and •who can actually understand it—because I have to say that I don't think many *will* be able to understand it.

As for the physician who denies that the valves of the heart close tightly, he is going against the anatomists (who all say the opposite in their writings) rather than going against me, for I don't need that thesis to demonstrate that the movement of the heart is as I describe it in my book. Even if the valves let through half the contents of each blood vessel, the Automaton would still move necessarily, as I have said. Besides, observation makes it clear to the naked eye that the six valves in the aorta and the pulmonary artery close these vessels tightly. . . .

As for his further comment that I considered the brain and eye of an animal rather than that of a human being, I don't see where he gets that from. Perhaps he thinks that since I'm not a professional medical man I haven't had the opportunity to observe human organs, which I readily admit; or perhaps he is going by the fact that the diagram of a brain given in the *Optics* was based on a sheep's brain, the ventricles and internal parts of which are, I know, much larger in relation to the brain as a whole than they are in the human brain. But I thought the sheep's brain was more suitable for making clear what I had to say, which applies both to animals and to human beings. And that can't be held against me, because nothing that I said relating to anatomy is original or in any way disputed by those who write on that subject.

Lastly, I am not in the least bit surprised that my explanations of refraction and of the nature of colours don't satisfy everyone, for no-one has yet had time to read and think about them thoroughly. When they *do* have the time, those who take the trouble to alert me to any mistakes they notice will be doing me a great favour, especially if they consent to my reply being published along with their comments, so that my reply to one may serve as a reply to all. To conclude, I thank you for all your trouble.

⊕ [2.vi.37: Huygens writes to Descartes, with thanks for Descartes's recent letter of condolence, and telling him that the long-sought-after 'privilege' is on the way.]

⊕ [14:vi:37: Descartes writes to Colvius thanking him for taking the trouble to inform Descartes of the recent death of Beeckman.]

⊕ [14.vi.37: Descartes writes to Balzac telling him of the publication (six days earlier) of his *Discourse on the Method and Essays*, saying that the work isn't well enough written or thought-out to deserve Balzac's attention but that nevertheless criticisms will be gratefully received. He speaks respectfully of a recently published volume of Balzac's letters, and says that his silence toward Balzac for several years arose not from any lack of friendship and admiration but from a sense that he hadn't anything to say that was worth Balzac's attention.]

to Noël, 14.vi.1637:

I am sure that you don't remember the names of all the disciples you had during your 20-odd years of teaching at La Flèche, and that mine is one of the names that have been erased from your memory. But that hasn't erased from my memory my obligations to you; and I have wanted to recognise them, though my only occasion for doing so has been the publication last week of the volume that you will receive with this letter. I am happy to offer it to you as a fruit that belongs to you because it was you who sowed the first seeds of it in my mind, just as I owe to members of your Order [the Jesuits] such knowledge as I have of literature. [He goes on to say that he will be glad to hear of any faults that are found in the book by Noël or by colleagues of his who have time to read the book.]

to Huygens, 12.vi.1637:

At last I have received from France the licence to publish that we were waiting for, and which caused the publisher to delay the printing of the last sheet of the book, which I am sending you with a request that you present it to His Highness the Prince of Orange. I won't venture to say 'present it in the name of the author', because the author's name isn't given and I don't think my name is worthy of his attention, but present it as something written by an acquaintance of yours who is warmly devoted to the Prince's service. In fact I can say that ever since I decided to leave my native land and all my friends order to lead a quieter and more tranquil life than I had before, it wouldn't have occurred to me to retire to this country—preferring it to all the other places where no war was going on and where the purity and freshness of the air seemed better suited to intellectual work—if my high opinion of His Highness hadn't made me utterly confident of his protection and government. And because I have since enjoyed to the full the peace and leisure I had looked to find here under the shelter of his military power, I am deeply in his debt and think that this book, which contains nothing but the fruits of his peace, should be offered to him above all others. . . .

⊕ [14.vi.37: Descartes writes to Huygens, asking him to give two copies of Descartes's recent book to Charnacé, the French ambassador to Holland, one for the French King and the other for Cardinal Richelieu. He doesn't think that either of those two grand people will lower his thoughts to Descartes's level, but the gift of the volume is something he *owes*. Although the book was published anonymously, Descartes's name has come to be publicly linked with it, and he accepts that.]

⊕ [22.vi.37: Descartes writes to Mersenne expressing annoyance with the conduct of Delaunay, who had written to Descartes with challenges and questions, and told Mersenne that Descartes's slowness to reply

was caused by his not being sure of his ground. Descartes swats this down, and says that he isn't answering Delaunay's questions because he doesn't have a relationship with Delaunay that would make that appropriate. Annoyance also with de Beaugrand, who isn't apologetic about having kept the *Optics* for so long (see Fermat on page 39) and whose works Descartes hasn't the slightest desire to see.]

⊕ [27.vi.37: Huygens writes to Descartes with an apology for his delay in doing a favour for a third person that Descartes had asked for. Also reporting that 'the offer of your book has been received, as it deserves, by His Highness' the Prince of Orange; and that de Charnacé is coming up with some strong objections to the recently published book.]

⊕ [5.vii.37: Descartes writes to Huygens with thanks for doing the favour at a difficult time. He is honoured that Charnacé is taking trouble over his work, and is sure—given the excellence of Charnacé's mind—that his objections will be strong. But Descartes isn't in the least *afraid* of them: 'I'll be more •happy to learn my faults than •ashamed of having made them.']

⊕ [30.viii.37: Descartes writes to someone about domestic arrangements in his new home in Egmond, and arrangements for him to be joined there by his 'niece'—actually his daughter—and the girl's mother. AT (see Glossary) has a charming note on this letter. 'Where were his child and her mother? And to which faithful friend (a physician?) is this letter addressed? *Autant d'énigmes*—so many puzzles.']

⊕ [8.ix.37: Huygens writes to Descartes, writing from 'before Breda' (the army of Huygens's employer the Prince of Orange is laying siege to Breda, a Dutch town that has been seized by Spain). Some remarks about the ongoing pursuit of satisfactory magnifying lenses, and then a paragraph about the study of music: he deplores the incompetence in this field of his own friends, and anxiously declares his desire not to waste Descartes's time with such matters.]

⊕ [13.ix.37: Plempius writes to Descartes, forwarding eighteen numbered critical comments on Descartes's work that he had just received from Fromondus.]

to Plempius, 3.x.1637:

I received your letter with Fromondus's comments, which were very welcome, though I was surprised at their arriving so soon ·after the publication of the book they are commenting on·. A few weeks ago I heard that the book hadn't yet been sent to you, and many who did have it have told me that they can't judge it until they have read it several times. I am the more grateful to you both—to you for your over-generous praise and to Fromondus for •his care in reading my book and •his taking the trouble to send me his opinion of it. The judgement of such a gifted and learned man enables me, I think, to discern the view that many other readers will take. But because in many places I see that he hasn't understood my meaning, I can't yet tell what he and others will say after a closer reading.

I can't agree with your judgement that my explanations can be •rejected and ignored but not •refuted or disproved. By using only very evident principles and (like a mathematician) taking account of nothing but sizes, shapes and motions, I cut myself off from all the evasive tricks of philosophers. So the slightest error will be easy to detect and refute by a mathematical demonstration. On the other hand, if something is so true and solid that no such demonstration can overthrow it, then no-one can afford to ignore it—or at least no-one who claims to be a teacher. It's true that on the surface I expounded my opinions without proving them; but it's not hard to extract from my explanations syllogisms that destroy the rival accounts of the same topics; doing this so evidently that anyone who sides with one of the rivals and tries to defend it against people who have understood what I say will find it hard to do this without making himself a laughing stock.

I'm aware that my geometry won't have many readers. I left out things that I thought others knew, and tried to cover or at least touch on many things in very few words—*many* things, indeed everything that can ever be discovered in that science. So it demands readers who are skilled in the whole of what is so far known in geometry and algebra and also industrious, intelligent and focussed. I have heard that in your university [Louvain] there are two such men, Wendel and van der Waegen. I will be very pleased to hear from you what they, or any others, judge of it.

I'm anxious to see what you write about the motion of the heart. Send me it as soon as possible, please, and tell me how Fromondus takes my replies. Greet him warmly in my name. As for the philosophers of Leiden: I left there before the book was published, and so far as I know *conticuere omnes* [Latin, Virgil, 'they have all fallen silent'], which is what you predicted of others also.

to **Plempius for Fromondus,** 3.x.1637:

The learned and distinguished Fromondus starts his objections with a reminder of the fable of Ixion, ·who made love to a cloud, mistaking it for the goddess Juno·. This is apt, for two reasons. •He does well to warn me to avoid accepting empty cloudy speculations as though they were the truth (which I will always do my best to do, and have always done until now). •The Ixion story fits him too: he thinks he is attacking my philosophy, but all he refutes are empty theories that have *nothing* to do with me—ones based on the system of atoms and empty space that is attributed to Democritus and Epicurus and their like.

(1) Concerning the *Discourse on the Method* [Part 5, about a third of the way through] he comments that 'noble actions like *seeing* can't result from such a low-down and brutish cause

as *heat*'. He is assuming that I think that animals see just as we do, i.e. sensing or thinking that they see, which Epicurus is said to have thought and that even today is accepted by almost everyone. But in the whole of Part 5 I show openly that my view is that animals don't see as we do when •we're aware that we are seeing, but only as we do when •our mind is elsewhere. When that happens the images of external objects are depicted on our retinas, and the impressions they make in the optic nerves may cause our limbs to make various movements of which we are entirely unaware. In such a case we're moving just like automata, and no-one thinks that heat doesn't have enough power to cause *their* movements.

(2) Concerning the *Discourse* late in Part 5, he asks what need there is to attribute substantial souls to animals, and remarks that my views might open the way for atheists to deny the presence of a rational soul even in the human body. I'm the last person to deserve this criticism, because like the Bible I believe—and I thought I had clearly explained this—that the souls of animals are nothing but their blood when it is warmed by the heart, converted into *spirit*, and sent through the arteries to the brain and from there to the nerves and muscles. [Descartes is here using 'spirit' to mean 'animal spirits' (see Glossary).] This theory makes animal souls so different from human ones that it provides an argument—the *best* argument yet thought of—to refute the atheists and establish that human minds can't be drawn out of the powers of matter. As for those who credit animals with some sort of substantial soul distinct from blood, heat and spirits, ·they are in trouble in at least two ways·. **(i)** I don't see how they can respond to these biblical texts:

> •'The soul of all flesh is in its blood, and you shall not eat the blood of any flesh, because the soul of flesh is in its blood' [Leviticus 17:14];

•'Only take care not to eat their blood, for their blood is their soul, and you must not eat their soul with their flesh' [Deuteronomy 12:23]

which strike me as much clearer than others that have been quoted against certain other opinions that some people condemn solely because they appear to contradict the Bible. **(ii)** Given that these people see so little difference between the operations of a man and of an animal, I don't see how they can convince themselves that there's so much difference in nature between the •rational and •sensitive souls that (on their view)

> •when the sensitive soul is alone its nature is corporeal and mortal, and
>
> •when it is joined to the rational soul it is spiritual and immortal.

How do they think sensation is distinguished from reason? Sense-cognition, they say, is a matter of simple sensory intake and therefore can't be false, whereas the cognition of reason is a little more complex, and can be carried along lengthy chains of syllogisms. This doesn't seem to show any superiority in cognition of reason, especially given that these same people say that God's cognition, and that of the angels, is utterly simple and intuitive, a sheer intake that isn't bound up in wrappings of theory. So it seems that on their view •sensation in animals is closer to cognition in God and the angels than •human reasoning is! ·In my book· I could have said many things like this to support my theses about the soul and about almost everything else discussed ·there·. I didn't do so partly for fear of teaching some falsehoods while refuting others, and partly for fear of being seen as aiming to ridicule received scholastic opinions.

(3) Concerning Part 5 [about half-way through] he says: 'It would take the heat of a furnace to rarefy the drops of blood fast enough to make the heart expand.' Apparently he hasn't noticed how milk, oil and most other liquids, when placed on a fire, expand gradually and slowly at first, then suddenly burst into flame when they reach a certain temperature, so that most of the liquid overflows and pours out onto the ashes unless •it is removed from the fire at once or at least the lid is removed from the pot containing so to let out the vapours that are the main cause of the liquid's being rarefied. What the crucial temperature is depends on the nature of the liquid; some liquids are rarefied and expand in this way when they are barely lukewarm. If Fromondus had noted these points, he would easily have reached the conclusion that the blood in the veins of any animal comes very close to the temperature that it must have in the heart if it is to be rarefied there instantaneously.

(4) [In the next sentence, the Juno/clouds contrast echoes the first sentence of this letter.] But nowhere does he show more clearly that he has embraced •the clouds of Democritus's philosophy instead of •the Juno of mine than in his comment on page 4 of the *Optics*, where he maintains that

> my example of a blind man's stick isn't analogous to the instantaneous transmission of light rays, because a ray that shoots out from the sun should be compared with an arrow shot from a bow, which flies through the air not instantaneously but through a series of instants.

Is he confusing me with Leucippus or Epicurus....? I nowhere suppose that there's a vacuum anywhere; indeed I explicitly say the very opposite, namely that all the space between us and the sun is filled with a body that is extremely fluid yet even smoother ·than other fluids· (I call it 'subtle matter'). So I don't see how anyone can object to the two analogies—of •the stick and of •the vat of pressed grapes— that I used to explain the instantaneous transmission of light rays. And if Fromondus says that my philosophy is

'crude and thick' because ·it overlooks the fact that· some body can easily get through the pores of glass, he must allow me to reply that I consider it an even cruder (though less solid) philosophy which holds that there are no pores in glass because sound doesn't pass through them; for we know that sound is wholly deadened or at least greatly diminished and dulled by a curtain placed in its path. This shows that sound can't pass easily through any sort of aperture, but only through apertures that are sufficiently wide and open. Indeed, given that sound. . . .depends on a movement of the air, no-one should be surprised that it can't pass through apertures that don't let through a breath of air let alone a whole mass of air.

[**(5)–(8)** These four short sections seek to correct various misunderstandings in Fromondus's comments—three concerning light and movement, one concerning colours.]

(9) He expresses surprise that on page 30 of the *Optics* I recognise no sensation except what occurs in the brain. But I expect all physicians and surgeons will help me to persuade him; for they know that amputees often think they still feel pain in the parts they no longer possess. [He gives a detailed story about a girl he used to know who had an arm amputated, didn't know this, and for weeks complained about pains in parts of that arm and hand. Conclusion:] This was obviously due to the condition of the nerves that used to lead from her brain to her arm. This certainly wouldn't have happened if the feeling—or as he says, the sensation—of pain had occurred outside the brain.

(10) I don't understand his objections to pages 159 and 163 of *Meteorology*. If my philosophy seems too 'crass' for him because it deals only with shapes and sizes and motions (like mechanics), he is condemning the aspect of my philosophy

•that seems to me its most praiseworthy feature,
•that is the main reason I prefer it to all its rivals, and
•that I am especially proud of.

I mean the fact that **(i)** all the reasoning in my kind of philosophy is mathematical and evident, and **(ii)** all the conclusions are confirmed by true observational data. Whatever I concluded to be possible from the principles of my philosophy actually happens whenever the appropriate agents are applied to the appropriate matter. I'm surprised that he doesn't realise that the mechanics now current is nothing but a part of the true physics which, not being welcomed by supporters of the common ·scholastic· sort of philosophy, took refuge with the mathematicians. This part of philosophy has in fact remained truer and less corrupt than the others because it has useful and practical consequences, so that any mistakes in it result in financial loss. So if he despises my style of philosophy because it is like mechanics, to me that's the same as despising it for being true.

If he doesn't agree that water and other bodies are made up of parts that are actually distinct, he should observe that we can often *see* such parts with the naked eye: specks of dust in stones, fibres in wood,. . . . It is perfectly reasonable to base our views about things that are too small for the senses to perceive on the model of the bigger things that we do see. . . . Perhaps the reason why he won't agree that terrestrial bodies are composed of actually divided parts is that he's worried about his 'integral union' and the other shadowy entities that a subtle philosophy packs into its continuum. If so, he should reread page 164 of *Meteorology* and he'll find that I conceive each of these particles as a continuous infinitely divisible body about which could be said everything that he has proved in his most subtle treatise *On the Composition of the Continuum*. He'll also find that I don't explicitly *deny* in bodies any of the things that

others admit in addition to the elements—·of my approach to natural science, namely shapes and sizes and motions·—but that these, few as they are, are all that my 'crass unsubtle' philosophy needs.

(11) If he's convinced that my supposition that the parts of water are oblong like eels is rash and baseless, he should remember what is said near the end of the *Discourse on the Method*. If he would favour the *Meteorology* and the *Optics* with an attentive reading of everything I wrote there, he would find countless reasons from which countless syllogisms could be constructed to prove what I say. They would go like this.

- If water is more fluid and harder to freeze than oil, that's a sign that oil is made of parts that stick together easily, like the branches of trees, while water is made of more slippery parts, like those with the shape of eels. But experience shows that water is more fluid and harder to freeze than oil. Ergo, etc.
- If cloths soaked in water are easier to dry than cloths soaked in oil, that's a sign that the parts of water have the shapes of eels, and can thus easily come out through the holes in the cloth, and that the parts of oil have the shapes of branches, and thus get entangled in the same holes. But experience shows, etc.
- If water is heavier than oil, that's a sign that the parts of oil are branch-shaped, and so leave many spaces around them, and that the parts of water are like eels, and therefore are satisfied with less space. But, etc.
- If water is easier to turn into vapour than oil, that's a sign that it is made up of parts that can easily be separated from each other like eels; and that oil is made up of branch-like parts that are more closely intertwined. But, etc.

Although each of these points taken by itself gives only probability to the conclusion, taken together they amount to a demonstration of it. But if I had set out to derive all these conclusions in the manner of a dialectician [= 'an expert in scholastic logic'], the sheer size of what I produced would have worn out the type-setters' hands the readers' eyes.

(12) What I say on page 162 of *Meteorology* seems paradoxical to him: that a slow motion produces the sensation of cold, and a fast one the sensation of heat. So he should find it paradoxical that a gentle rubbing on the hand produces a sensation of pleasure, and a harder rubbing produces pain; because pleasure and pain are at least as different from each other as are heat and cold.

[There are six more numbered items: **(13)** about cold in relation to rarefaction; **(14)** about evaporation as caused by the sun; **(15)** about why the surface of an undisturbed body of water is smooth; **(16)** about what rarefaction is; **(17)** about the taste of salt; and **(18)** about the movements of winds.]

to Huygens, 5.x.1637:

[This letter begins with two pages about lens-making. Then:] As for your request for something on mechanics, I've never been less in the mood to write than I am at present. For one thing, I don't have as much free time as I had when I was living in Breda; also, I regret—*daily*—the time that my recent publication has cost me. White hairs are rapidly appearing on my head, which brings it home to me that the only thing I should be devoting myself to is ways of slowing down their growth. That's what I am doing now, trying energetically to make up for my lack of experimental data. This task needs all my time—needs it so badly that I have decided to concentrate on this alone and have even laid aside all work on my *World* so that I'm not seduced into ·spending time on·

putting the finishing touches to it. Still, I am sending you the lines you requested, seeing that you asked for only three sheets.

An account of devices that enable us to use a small force to raise a heavy weight

The single underlying principle of all these devices is that a force that can raise a 100lb weight two feet can raise a 200lb weight one foot, or a 400lb weight six inches, and so on.

You'll accept this principle if you consider that an effect must always be proportional to the action needed to produce it. Thus, if what we need to lift a certain weight x one foot is a force that can raise a 100lb weight two feet, then x must weigh 200lb. For •lifting 100lb one foot twice over is the same as •lifting 200lb one foot or 100 pounds two feet.

Now, mechanical devices can rely on this principle to move a weight over a shorter distance by applying a force over a longer distance. They include
- •the pulley,
- •the slope,
- •the wedge,
- •the cog-wheel,
- •the screw,
- •the lever.

There are some others, but they don't relate to these six as closely and clearly as these relate to one another. [Descartes offers diagrams (except for the screw); they will be omitted here, which has required a good many changes to details of the wording.]

The pulley
◇Take a 200lb box of pebbles, attach two ropes to it, and give one rope to each of two men. To support or raise it, each man will need to exert only as much force as is needed to hold up or raise 100lb, since each bears only half of the weight. ◇Now run a single rope through a pulley and give each man

one end of it; and attach the pulley to the 200lb box. Same upshot, because this differs only trivially from the previous case. ◇As before except that one end of the rope through the pulley is nailed to a beam: the situation of the man holding the other end is the same: he can still support the 200lb box with force that would be exactly enough for him to support 100lb unaided. ◇Same setup as before but now the man wants to *raise* the 200lb box; it's obvious that to raise it one foot he will have to raise his end of the rope two feet, which he can do using the amount of force that would be exactly enough for him to raise 100lb unaided. (We always need a little more force to lift a weight than to support it; that's why I have treated supporting and lifting separately.)

For perfect precision we would have to take into account •the weight of the pulley and •the friction of the rope against the pulley, but these are small in comparison with the weight of the box.

[Descartes goes on to speak of cases using two or three or more pulleys. Each time a pulley is added, the box is easier to lift to the desired height, because each addition of a pulley adds to the length of rope involved in lifting the box to that height. 'So if we add more pulleys, we can raise the heaviest of loads with the smallest of forces.' He offers all that as illustrating the fact that what is reducing the man's task is not the pulley but the lengthening of the needed rope-pull.]

The slope
If we have only enough power to lift a weight of 100lb and we want to raise a 200lb body to a height of four feet, all we need do is to pull it or roll it up an eight-foot sloping surface that ends at a height of four feet. To get the body up to that height in this way we'll employ as much force as is required to raise 100lb eight feet. And the less steep we make the slope, the less force we'll need to do the job.

[Descartes adds a warning that here again friction makes a difference. If the force needed to get the body up the slope were *entirely* devoted to weight-lifting, with none of it needed to overcome friction, then pushing it along at ground level would require no force. And another diminutive error would arise from the fact that 'flat ground' isn't perfectly flat because of the shape of the earth.]

The wedge
[Descartes explains briefly that when you raise a weight by driving a wedge under it, what you are basically doing is to force the weight up a slope; so that this isn't significantly different from the preceding device. And the same to possibilities of small errors are present here too.]

The cog-wheel
Consider a cog-wheel with a six-inch circumference, fixed to a cylinder with a one-inch circumference around which is wound a cord attached to a weight that we want to raise. To raise it one inch we must make the cylinder rotate once, which we do by making the cog-wheel rotate once; the latter rotation involves moving any given cog six inches; so the distance through which we exert force is six times the distance through which the weight rises, this proportion being exactly the proportion between the two circumferences.

[Descartes adds that we can have a cog-wheel driving another which drives another etc., each wheel smaller than the one driving it, so that there's no limit to how much weight we can lift with any given force. Except, he adds, that we have to allow for the friction involved in making the wheels turn.]

The screw
Once we know the power of the cog-wheel and the slope, it's easy to calculate the power of the screw; for the screw consists simply of a steeply sloping surface that turns on a

cylinder. Say the slope of the surface is such that it takes ten turns of the screw to move it one foot into the wood, and the circumference of the circle described by the turning force is ten inches long. Since $10 \times 10 = 100$, with such a screw a single man could press as hard as a hundred men could without it, provided we make due allowance for the force that would be needed to turn it ·even if it weren't doing any other work·.

I put that in terms of 'pressing' rather than 'raising' or 'moving', because that's what a screw is most often used for. But if we want to use the screw to lift a weight, as distinct from driving it down into something, we attach a cog-wheel to it. [Descartes whips through the arithmetic of this, in terms of an apparatus by which 'one man will be able to lift as heavy a weight as 300 men could lift without it; but the details of *how* the screw is to be 'attached' to the cog-wheel are not clear. He goes on to say:] Again we have to allow for the difficulty there may be in turning the screw, which strictly speaking is due not to the weight of the load but to the form or matter of the apparatus; and since a greater force is involved in this case, the difficulty is inherently more conspicuous.

The lever
I have left the lever to the last, because it's the weight-lifting device that is hardest to explain. [Descartes's explanation (accompanied by a complex diagram) is hard to follow, and its details won't be given here. The core idea is fairly simple. Consider a lever three feet long and name three points on it:
   •A is one end;
   •F is a fulcrum to which the lever is attached in a way that lets it freely swing one way and the other;
   •B is the other end;
and the distance A–F is two feet, leaving one foot for F–B. Suspend a 10lb box from B, and lift it by pushing down

on A. This is clearly a case where raising the box n inches will require the A end of the lever to be pushed down **more** than n inches, and the force needed to do this will be **less** than would be needed to lift 10lb n inches just by pulling it up directly. Descartes's complications all have to do with working out **how much more** distance and **how much less** force for any given position of the lever. To see that lever-position does count in this, consult your own experience: you know that the lever's force-saving power is much greater when it is horizontal than when it is almost vertical. [For the details, consult CSMK.] Descartes then adds warnings about how perfect precision would require us to take account of the curvature of the earth's surface, but says 'These points, however, have no practical significance', and then:]

It would be useful if would-be inventors of new devices ·for lifting weights etc.· knew the things I have written here *and no more*. If they did bring anything else into their plans and calculations, it would have a good chance of being wrong and leading them into error.

The devices that I have explained can be applied in many different ways. There are countless other things to consider in mechanics that I'm saying nothing about, as I have filled up my three sheets of paper, and that's all you asked for.

to Mersenne, 5.x.1637:

You tell me that your friend Fermat saw the *Optics* and had certain objections to make [they are presented starting on page 40 above]. The first was that he doubts that •the inclination to move should follow the same laws as •movement, because they are as different as potentiality and actuality. I think he acquired this doubt because he imagined that I had it too, inferring this from what I wrote early in the *Optics*: 'It's

very easy to believe that in this respect the tendency to move must follow the same laws as does the movement itself.' He thought I was equating 'It is easy to believe that P' with 'It is no more than probable that P'—which is far from being the case. If P is merely probable I count it as almost false; and when I say that P is 'easy to believe' what I mean is not •that it is only probable but •that it is so clear and so evident that there's no need for me to spend time and page-space proving it. As in fact it can't reasonably be doubted that the laws governing movement (which Fermat rightly says is the actuality) must also govern the tendency to move (which is the potentiality of that same actuality). Although not it's true that anything in potentiality is later in actuality, ·the converse does hold·: nothing can possibly be in actuality without first being in potentiality.

As for his further remark that 'the gap seems especially large in this case, because the movement of a ball can be faster or slower, depending on the forces acting on it, whereas light goes through a translucent body in an instant, apparently with no succession involved', I don't understand his reasoning here. He can't say that the two are unalike because the motion of a ball can be more or less forceful, because the action I think light consists in can also be more or less strong. And it can't be because the movement of bodies is sequential whereas the transfer of light is not; because I think I have made it sufficiently clear (through the analogies with a blind man's stick and wine sinking to the bottom of a vat) that while the inclination to move is transmitted instantaneously from one place to another it still *follows the same path* as sequential motions would have done, and that's all that is at issue here.

[Two pages disagreeing with Fermat about the concept of divisibility in geometry, and then:] When you encounter objections to anything I have written, please send them to

me, however good or bad they are, and I shall respond to them. At least, I'll respond to any that are worth troubling about if their authors are willing to have them published. . . .

to Noël, x.1637:

I'm extremely glad to learn from the letter you kindly wrote me that I am still so fortunate as to have a place in your memory and affections. Thank you also •for promising to have the book I sent you examined by those of your fellow-Jesuits who most enjoy ·thinking about· such matters, and •for being so kind as to send me their criticisms. I only wish that you would also send me your own criticisms, because—I assure you—you have more authority over me than any of the others, and I more willingly defer to you than to any of them. Friends of mine who have already seen the book tell me that a lot of time and study is required if one is to asses it properly, because
> •the introductory parts (at least in the *Optics* and the
> *Meteorology*) can't be wholly persuasive unless one
> knows everything that comes after them, and
> •the later parts can't be understood unless one remem-
> bers everything that came before.
So I'll be *very* much obliged to you if you'll give it your attention or get others give it theirs. The fact is that I'm simply trying to get instruction for myself; and those who bring errors to my attention will always please me more than those who give praise. Besides, I think it's more in the interests of the Jesuits than of anyone else to examine this book. I see already that so many people are going to accept the book's contents (especially the *Meteorology* part of it) that I don't know how anyone can go on teaching these subjects in the way they been taught down through the years in most of your Colleges unless he first disproves what I have

written. [Descartes adds 'or unless he follows it', but of course someone who follows Descartes's theories wouldn't 'go on teaching these subjects in the way they been taught' etc.] I know that the main reason why your people take such care to reject all sorts of innovations in philosophy is the fear that these innovations may force some change in theology as well; so I assure you that you have nothing to fear on this score so far as my writings are concerned, and that I have reason to thank God that
> the views that my reflection on natural causes has led
> me to regard as the most true in physics have always
> been the ones that are the most compatible with the
> mysteries of religion,
as I hope to show clearly when I have the opportunity.

⊕ [x.37: Descartes writes to a Jesuit priest—who is a teacher of a nephew of Descartes's—thanking him for his willingness to read and criticise Descartes's recently published book. He gives advice on how to tackle the work, and says there's no need to hurry: 'The later your comments come, the more favourable they will be.']

⊕ [x.37: Descartes writes to someone who is a gentleman, a soldier, an amateur mathematician, and a writer of beautiful Latin, expressing joy and admiration for a discovery that this man has made in geometry ('your rule couldn't be better'); expressing surprise that a soldier should write Latin so well; and responding to something he has said about presence of mind in battles.]

⊕ [23.ix.37: Huygens writes to Descartes with an elaborate explanation for his delay in replying to Descartes's letter of 5.x.37 and the accompanying short course in mechanics; lengthy praise of Descartes; and a question about what he should do if he 'can't keep quiet' about having received writings from Descartes and people clamour to see them.]

⊕ [ix.37: Fermat writes to Mersenne with a ten-page response to Descartes's 5.x.37 letter to Mersenne replying to Fermat's first comments. The topic is, again, Descartes's supposed 'proof' of the Descartes-Snell law relating angles of incidence to angles of refraction. Fermat opens

with an earnest declaration that what motivates him to persist with 'this little dispute' is neither envy nor ambition but a desire to know the truth.]

to Huygens, **4.xii.1637:**

The three sheets that I sent you [the Account starting on page 50] don't in the least deserve the good words in the letter which you kindly wrote me, and I assure you that I'm ashamed to have sent you such a meagre offering. In fact my fear of getting into something much longer than you had asked for led me to omit the finest parts of my topic, such as
> •the treatment of velocity,
> •problems concerning the balance, and
> •several ways of increasing motive force other than the ones I explained.

But so that you won't think that I'm trying to get you to invite me to add these topics to the treatise, I'll reply to the last part of your letter and tell you what I'm busy with.

I have never taken greater care in looking after myself than I'm doing now. I used to think that death couldn't deprive me of more than 30 or 40 years, but now I wouldn't be surprised if it were to rob me of more than 100 years. It seems obvious to me that if we merely guard ourselves against certain habitual errors in our way of life we'll be able to reach a much longer and happier old age than we could otherwise—and to do this without any further medical discoveries. But I need more time and more observational data if I'm to investigate everything relevant to this topic, so I'm now working on a compendium of medicine, basing it partly on books and partly on my own reasoning. I count on being able to use this as a provisional means of obtaining from nature a stay of execution, and of being better able from now on to carry out my plan. . . .

to Plempius, **20.xii.1637:**

I'm glad that my answers to Fromondus's objections [see page 46] have at last reached you. I am surprised that they have led him to think that I was annoyed by his paper. I was not at all; and I don't think that I uttered the slightest word against him without his having said similar or harder things against me first. I concluded that he liked that style of writing, and so against my own inclinations I followed him in it because I thought he might enjoy the game less if I received his attack too gently and softly. Friends don't stop being friends when they play chess against each other; indeed their very skill in the game often creates and strengthens their friendship between them. I was only trying by my reply to earn his goodwill. . . .

I don't expect to have a sufficiently ripe judgement on my book from anyone who merely whips through a borrowed copy. The points near the end of each treatise can't be understood unless everything that goes before is remembered; and the proofs of the propositions at the beginning depend on everything that follows. What I say in the first chapters about the nature of light, and about the shape of the particles of salt water and fresh water, are not my *principles*, as you seem to object, but rather *conclusions* that are proved by everything that comes after. Sizes, shapes, positions and motions are my •formal object (in philosophers' jargon), and the physical things that I explain are my •material object. The principles or premises from which I derive these conclusions are only the axioms that geometers base their demonstrations on—' The whole is greater than the part', 'If equals are taken from equals the remainders are equal', and so on—but they aren't abstracted from all sensible matter, as in geometry; rather, they are linked with various indubitable empirical data. For instance, from **(i)** the oblong and inflexible shape

of the particles of salt I deduced **(ii)** the square shape of its grains, and many other things that are also obvious to the senses; I wanted to explain **(ii)** by **(i)**—explaining effects by their cause. I wasn't trying •to prove things that are already well enough known, but rather •to demonstrate the cause by the effects *a posteriori* [see Glossary], as I remember I wrote at length in my reply to Fromondus's objection **(11)** [see page 49].

I'll be glad if the Jesuit to whom you recommended my book writes to me about it; anything that comes from the men of that Society is likely to be well thought out, and the stronger the objections he puts forward the more pleased I'll be with them. For the same reason I eagerly await your objections about the movement of the heart.

to Mersenne, end of xii.1637:

The judgment of my writings by ·de Beaugrand·, the author of *Geostatics*, doesn't bother me. I don't like having to speak well of myself, but because few people can understand my Geometry, and you ask me what my own view of it is, I think it is appropriate that I should tell you:

> I couldn't wish it to be better. In the *Optics* and the *Meteorology* I merely tried to •convince the reader that my method is better than the usual one; but in my *Geometry* I claim to have •demonstrated this.

Right at the beginning I solve a problem that Pappus says none of the ancients managed to solve; and it can be said that none of the moderns has been able to solve it either, since none of them has written about it, even though the ablest of them have tried to solve the other problems that Pappus says were tackled by the ancients. These modern writers include Ghetaldi, Snell, and others among whom ought to be counted that Counsellor of yours, Fermat—yet none of these knew how to solve a problem that had defeated

the ancients. [Descartes refers to each of these moderns not by name but by the title of one of his books.]

Moreover, my discussion in Book II of the nature and properties of curved lines and how to study them seems to me to be as far removed from ordinary geometry as Cicero's rhetoric is from a child's ABC. And when your geostatician promises to provide better methods than mine for finding the tangents to all curved lines, I am so far from believing him that I see him as making a fool of himself like the strutting captains in Italian comedies. As for the claim that the things I have written could easily have been taken from Viète—the fact is that I tried to include only things that I thought were not known to him or to anyone else. That's what makes my *Geometry* hard to understand.

[Descartes invites Mersenne to compare his treatment of problems about the number of roots in each equation with Viète's treatment of them. The difference is that Descartes presents general rules that solve all these problems, whereas Viète only gives particular examples. He adds 'between ourselves' a disparaging remark about Viète's level of knowledge.]

For each type of problem I have •determined what solutions are possible and •shown how to find them; so I claim that people should not only believe that I have accomplished more than my predecessors but should also be convinced that posterity will never discover anything in this subject that I couldn't have discovered just as well if I had troubled to look for it. Please keep all this to yourself. I would be very embarrassed if others knew that I have written you as much as I have on this topic.

I am not so anxious to see Fermat's disproof of what I had written on refraction as to ask you to send it to me by post, but when it's convenient to send it to me by sea with some cargo I'll be pleased enough to see it, along with the

Geostatics and de la Chambre's book on light, and anything else of that sort. I would in fact be glad to see at once what others write for or against my views or about their own discoveries, but the cost of sending letters ·by post· is too high.