CARTESIAN MECHANICS*

Sophie Roux
(Centre Alexandre Koyré, EHESS, Paris)

Introduction

For many historians, the elaboration of modern science relies on a tension between two trends: on the one hand the search for a mathematical treatment of phenomena, on the other hand the demand for mechanical explanations. And, while everybody agrees on the importance of Descartes' *Principia philosophiae* for natural philosophy, we also know that they could hardly have been called *Principia mathematica*: quantitative expressions are scarce, with nary an equation to be found. Consequently, Descartes is often taken as the typical mechanical philosopher, and contrasted as such to the founder of mechanics as a science, namely Galileo.[1]

The purpose of this paper is not to refute this big picture, but to qualify it from a Cartesian point of view, which means neither to contrast Descartes to a hypothetically clear paradigm of a Galilean science of motion nor to reduce his enterprise right away to that of a mechanical philosopher. To put it bluntly, I would like to understand Cartesian mechanics in its own terms. But these terms are at least twofolds: on the one hand, you have what I call the general idea of mechanics, namely the famous Cartesian thesis according to which there is no difference between physics and mechanics; on the other hand, you find answers to some mechanical questions, which were usual at the time. Thus, before asking if Descartes crosses the border between the two worlds, or how he put each of this world in touch with the other, I will show that he inhabited the two.

The metaphor of the world should however be taken seriously: to inhabit a world means first to be in relationship with its inhabitants. Although Descartes imposed to his readers, in science as well in

---

* I modernize french quotations. I use the following abbreviations, complete references being given in the final bibliography:

A.T. = Descartes, *Oeuvres de Descartes*.

T.H. = Fermat, *Oeuvres de Fermat*.

E.N. = Galileo, *Le Opere di Galileo Galilei*.

T.W.B. = Mersenne, *Correspondance du P. Marin Mersenne*.

I thank Egidio Festa and Carla Rita Palmerino for their thoughtful and inspiring comments on earlier versions of this paper.

[1] See, among many other examples, Koyré, *Etudes d'histoire de la pensée scientifique*, pp. 166-170. Westfall, *The Construction of Modern Science*, p. 1, p. 36, p. 42, pp. 49-50, p. 120, p. 138.

philosophy, a very singular protocol of reading, , as if they could tackle his works independently from any context except the one furnished by his own system, we must take into account the parallel enterprises of his contemporaries, as well of his immediate predecessors or followers. In other words, we should not go from the Charybdis of seeing Descartes through the prism of Galilean science to the Sylla of thinking that his works have created from nothing the norms, according to which they should be read.

With that in mind, I begin with a study of what the word “mechanics” meant by Descartes and of how the general idea of mechanics developed in his works, namely the thesis according to which there is no difference between physics and mechanics. In the second part, I analyse a sample of Cartesian mechanics, namely Descartes’ answer, presented in a letter of July 1638, to the so-called geostatical question, that is, the question of whether a body is heavier when it is closer to the centre of the Earth or when it is farther away. In the third and last part I grasp the singularity of the Cartesian practice of mechanics and address the question of the relationship between the general idea of mechanics and the practice of mechanics, in so far as it may be revealed in this piece of Descartes' correspondence.

1. Descartes’ general idea of mechanics

Mechanics underwent indeed profound conceptual changes in the sixteenth and seventeenth centuries; but we should not forget that the very meaning of the word “mechanics” also changes during this period. It used to refer to treatises on simple machines or to apply to every kind of empirical or approximate practice; since Antiquity, there had indeed been a science of mechanics, but it has been sharply distinguished from natural philosophy. Around the middle of the seventeenth century, however, the word “mechanics” began to designate the science of motion and, in so far as motion is the first object of natural philosophy, the very core of natural philosophy.[2] It would be exaggerated to pretend that this terminological shift had immediate or radical consequences for the evolution of mechanical concepts; inversely however, it would be idle talk to disqualify the question on the pretext that it relates only to a matter of disciplinary boundaries: if we want to ask seriously the question of the relationship between mechanical science and mechanical philosophy in the seventeenth century, we have to know what these categories meant.

In that respect, Descartes is an interesting and inevitable figure, and he obviously played a major part in the terminological shift of the word “mechanical.” To understand how this happened, I shall proceed mostly in a chronological way. The chronology in itself would be of little interest, yet if one forgets chronology, one is doomed to confuse different themes, which may play out simultaneously, but are nevertheless distinct.[3] First, I say a few words about the “physico-mathematics” Descartes and Beeckman

---

[2] The semantical transformation of the word “mechanics” has been pointed out by Gabbey, “Newton’s *Mathematical Principles of Natural Philosophy*”, and by Gabbey, “Between *Ars* and *Philosophia Naturalis*”. In what follows I use “physics” and “natural philosophy” as synonyms, as they were used in the seventeenth century.

[3] Forgeting chronology is the only reproach one could address to the otherwise extremely complete and profound paper by Séris, “Descartes et la mécanique”.

were practising together. Next, I show that Descartes' break with this practice was inspired by the desire to make physics as certain as geometry. This ideal of certainty makes it all the odder that at some point, he decided to present his physics as "mechanical". My third point amounts to unravel the circumstances in which he did so. Lastly I give a few general indications about the Cartesian claim that his physics is nothing else but mechanics.

1.1. Physico-mathematics

The starting point is the short and fruitful period where Descartes and Beeckman exchanged questions, problems and answers. To give a full account of this period in a few lines would be a difficult task, because it involves the question of the relationship of Beeckman and Descartes — a question which obviously have been obscured by a certain nationalism (as if establishing that Beeckman had been of some moment in the unfolding of the Cartesian system would undermine the French Genius' glory), but also, by the confusion of at least two of the many elements which are sheltered under the umbrella of "the mechanical philosophy", namely the corpuscularian and the mathematical trends.[4] But I do not pretend to give such a full account: my only concern is to understand what "mechanics" meant for Descartes at the time when he was working with Beeckman, without drawing up a balance sheet of their mutual debts and credits. That is why I shall not confront globally the *Journal* of Beeckman with the Cartesian works, but focus the few passages, which have beyond doubt been written at four hands.

At the time when he first met Descartes, Beeckman was indeed interested in problems of mixed mathematics; he was also a convinced atomist, denying the possibility of actions at distance like magnetism and heaviness, and trying to explain them by corpuscularian mechanisms.[5] All the ingredients were there in order that he establishes perhaps the first system of mechanical philosophy; however, he did not, probably because he prefers the resolution of singular problems to the building of overarching systems, but also because he maintains a distinction between mathematics and physics.[6] The intellectual friendship between Beeckman and Descartes in 1618-1619, renewed in 1629, was founded on the exchange of such singular problems, to the exclusion of any exchange related to corpuscularism. The

[4] For short and recent presentations of the relationship between Beeckman and Descartes, see Berkel, "Beeckman, Descartes, et la philosophie physico-mathématique". Berkel, "Descartes' debt to Beeckman". Garber, *Descartes' metaphysical physics*, 9-12.

[5] See for example, about mechanical problems: 18 July-[November] 1612, in *Journal*, I pp. 1-4. [April 1614], in *ibid.*, p. 30. About atomism: 23 December 1616-16 March 1618, in *ibid.*, I p. 152. 9 July-August 1618, in *ibid.*, I pp. 201-203. About mechanical explanations: July 1613-April 1614, in *ibid.*, I pp. 25-26. April 1614-January 1615, in *ibid.*, I p. 26. 6 February-23 December 1616, in *ibid.*, I pp. 101-102.

[6] As far as I can see, there is no link between atomist considerations and the practice of physico-mathematics. Moreover, in at least a passage he very clearly makes a distinction between "physics" which deals with the essence of things (that is body and void) and "mathematics" and "mechanics", which deal with proportions between things (23 December 1616-16 March 1618, in *Journal*, I p. 131).

questions they touched upon were many; most of them were concerned with musics, but some with mechanics, for example why a spinning top stays erect, what is the increase of the motion of a falling stone, if the motion is conserved, how much does water weigh in different vessels.

When a name is given to these questions, it is sometimes "mechanics" and sometimes "mathematico-physics."[7] Whatever the origin and the diffusion of this last expression is, it was becoming usual at this time to refer to the practice of mixed mathematics.[8] Already by the time of Aristotle, mathematical reasoning having been introduced in certain parts of natural philosophy, like astronomy, music, optics and mechanics, he described them as partly physical and partcly mechanical.[9] These sciences, later on known as "mixed sciences", were highly esteemed in the 16th century, even by the so-called "scholastics"; nevertheless, they entail neither a criticism of the traditional notions of physics nor a modification of its status, simply because they were sharply distinguished from it.[10] More specifically, the reappropriation of the pseudo-Aristotelian *Mechanical Questions* raised many commentaries, the aim of which was to insert mechanics in the Aristotelian classification of the sciences and to differentiate it from physics. To put it bluntly, mechanics and natural philosophy are both contemplative sciences dealing with natural matter (that is with *mobilia* and *ponderosa*), but they differ in at least two respects: mechanics treats its object in a mathematical way, natural philosophy does not; mechanics concerns primarily violent motions, natural philosophy natural ones.[11]

---

[7] There are three occurrences of the word "mechanics" in the correspondence between Descartes and Beeckman. Descartes explains that he should state the first principles of his mechanics before comparing the weights of water in different vessels: "multa ex meis mechanicae fundamentis [sunt] praemittenda" (A.T. X p. 67). He asks Beeckman to give his opinion about their mechanics: "de mechanicis nostris mitte quid sentias" (To Beeckman, 26 March 1619, in A.T. X p. 159). He promises to put in order his mechanics, that is, his geometry: "tibi polliceor me mechanicas vel geometriam digerendam suscepturum" (To Beeckman, 23 April 1619, in A.T. X p. 162). To what exactly these two last occurences are referring is not obvious. As for the occurence of "physico-mathematics", see the piece called " Physico-mathematici paucissimi", where Beeckman notes that Descartes praises him for being the only one "qui hoc modo (…) studendi utatur, accurateque cum mathematica physicam jungat" (*Journal*, 23 November-26 December 1618, I p. 244 and A.T. X p. 52). Later on, Beeckman will revendicate as his own the idea of "mathematico-physics" (To Beeckman, 17 October 1630, in A.T. I p. 159 et p. 164); a few excerpts of his *Journal* were posthumously published by his brother Abraham under the name *D. Isaaci Beeckmanni Mathematico-Physicarum, Meditationum, Quaestionum, Solutionum Centuria* (Trajecti ad Rhenum: Daniel Sloot, 1644).

[8] Dear, *Discipline and Experience*, pp. 170 sqq., gathered some fifteen occurrences of the expression "physico-mathematics" in titles of books of the early seventeenth century.

[9] Aristotle's texts are: *Metaphysics*, 1078 a24 sqq.; *Physics* 194 a9 sqq.; *Second Analytics*, 76 a24 sqq. et 78 b37 sqq. On this point, see McKirahan, R. D., Jr, "Aristotle's Subordinate Sciences".

[10] On the status of mechanics in the sixteenth century, see Rossi, *I Filosofi e le macchine*; Laird, "The Scope of Renaissance Mechanics".

[11] See for example Alessandro Piccolomini, *In Mechanicas quaestiones Aristotelis paraphrasis*, Venitiis, 1565 (2nd ed.), fols. 8r-v and fols. 55r-56r, analysed in Laird, "The Scope of Renaissance Mechanics", pp. 50-51; the lectures of Giuseppe Moletti,

The question is how Descartes, at the time he worked with Beeckman, can be situated in respect with this tradition, which certainly made a niche for mathematics in physics, but a carefully marked off niche. It is true that he deals with motions without making any difference if they are natural or violent, and that he assumes that natural phenomena may be explained by artificial devices. But he does not pronounce explicitly against the distinctions between natural and violent motions or between natural and artificial things; neither does he infer that physics should be built up on new foundations. His general attitude is to bother neither with the establishment of a system, nor ontological significance of the notions he uses. This is especially striking concerning points, about which he will later on hold strong theses: he supposes the existence of a vacuum or of a gravity inherent to bodies, without enquiring if there is really something like vacuum or gravity; he speaks incidentally of "atoms of water" without specifying if these atoms are hypothetical entities used for the sake of mathematical reasoning or actual components of water, and, in this last case, of specifying how this composition works.[12] Considering that, at this time, Beeckman had already acquired, not only the conviction that mathematics can help the understanding of nature, but a corpuscularian view of the world, this reserve should be attributed to Descartes himself. In other words, he conformed to the practice of mechanics, which were usual at his time, in particular by the Jesuits, and that he may have known when he studied at La Flèche.[13]

1.2. Physics, geometry and metaphysics

Between 1619 and 1629 however, Descartes wrote the *Regulae ad directioneml ingenium*, the ambition of which should be quickly summarize with respect to our concern with mechanics. What prompted Descartes to write the *Regulae* was the discovery of the unity of all the sciences: hence the project of a universal knowledge, which would resume all the existing scientific practices (geometry, algebra, but also physico-mathematics) but also cover new scientific practices not yet discovered. What makes the unity of the sciences and the universality of knowledge is however a process of abstraction, which amounts to consider everything *as if* it was a mathematical quantity. In other words, the abstraction described in the *Regulae* is symbolic and does not entail any ontological reform of physics.[14]

---

analysed in *ibid.*, pp. 60-62; Giovanni di Guevara, *In Aristotelis Mechanicas commentarii*, Roma, 1627, pp. 7-11 and pp. 18-26 analysed in *ibid.*, pp. 65-66. Seee also Laird, *The Unifinished mechanics of Giueeppe Moletti.*

[12] For gravity, see: A.T. X pp. 58-60. A.T. X pp. 75-76. For tha atom of water, see: A.T. X p. 68.

[13] In the piece called "Physico-mathematici paucissimi", quoted above, note 7, Beeckman presents Descartes as "cum multis Jesuitis aliisque studiosis vriisque doctis versatus". Even if it is difficult to specify what Descartes learnt at La Flèche (on the possible conjectures about Desacrets' formation at La Flèche, see Romano, "L'Enseignement des mathématiques à La Flèche dans les années de la formation de Descartes"), he has probaly been initiated to the mixed sciences.

[14] The literrature on the signification of the *Regulae* is prolific. Considering that my point is mainly to situate the *Regulae* in the developpment of Descartes' thiughts about mechanics, suffice here to refer, in the recent litterature, to Fichant, "L'*ingenium* selon Descartes et le chiffre universel des *Règles pour la direction de l'esprit*", and "La "Fable du monde" et la signification métaphysique de la science cartésienne", in *Science et métaphysique*, resp. p. 1-28, p. 59-84.

Between the late twenties and the early thirties however, Descartes decided to reform natural philosophy from top to bottom. As in the *Regulae*, the problem for him was to make physics certain: “Je souhaiterais (…) bâtir une physique claire, certaine, démontrée, et plus utile que celle qui s’enseigne d’ordinaire.”[15] To make physics certain amounts, he continues, to introduce mathematics in physics, to cultivate, besides the usual abstract geometry, a geometry which deals with natural phenomena, or to reduce physics to geometry.[16] We should not believe that such formulas refer to the introduction of mathematical proportions, or to some kind of reasoning *more geometrico*, that is of formal procedures supposed to guarantee blindly the certainty of the outcome; neither do they mean the resort to abstraction and symbolisation, as it was the case in the *Regulae*. They refer rather to the emergence of a sound ontology. Physics is now uncertain, because ages of philosophy have added to its object all kinds of superfluous and obscure entities; a certain physics would be easy to build if we contemplated this object naked and pure. We would then realise that this object is basically the same as the object of mathematics, namely matter or extension, indefinitely extended and always the same, although it may be diversified in a variety of shapes, sizes and motions.[17] And consequently, we would have the same relationship to both the object of physics and the object of mathematics, that is a relationship of intuitive evidence, which would let us easily notice any error.[18]

Although I do not wish to insist here on this point, another change is worthnoting at this crucial period at the beginning of the thirties. The identity of physics and mathematics received also metaphysical fundations, because of the doctrine of eternal truths formulated in the letters to Mersenne of 1630. According to this doctrine, even mathematical truths are laws of nature established by God; with respect

---

[15] To Villebressieu, summer 1631, in A.T. I p. 216.

[16] “(…) introduire la certitude et l’évidence des démonstrations mathématiques dans des matières de philosophie, telles que sont le son et la lumière” (To Huygens, 1 November 1635, in A.T. I p. 331). “Je n’ai résolu de quitter que la géométrie abstraite (…) ; et ce afin d’avoir d’autant plus de loisir de cultiver une autre sorte de géométrie, qui se propose pour questions l’explication des phénomènes de la nature. Car s’il lui plaît [to Desargues] de considérer ce que j’ai écrit du sel, de la neige, de l’arc-en-ciel, etc., il connaîtra bien que toute ma physique n’est autre chose que géométrie” (To Mersenne, 27 July 1638, in A.T. II p. 268). “Pour la physique, je croirais n’y rien savoir si je ne savais que dire comment les choses peuvent être sans démontrer qu’elles ne peuvent être autrement ; car l’ayant réduite aux lois des mathématiques, c’est chose possible” (To Mersenne, 11 march 1640, in A.T. III p. 40).

[17] Motion was at the time considered as a part of pure mathematics, because curves can be generated by motions; it explains an otherwise cryptic remark: “Nihil etiam docui de motu, in quo tamen examinando mathematica pura, ea saltem quam excolui, praecioue versatur” (To Ciermans, 23 March 1638, in A.T. II p. 71).

[18] See the introduction of matter and motion in *Le Monde*, in A.T. XI, esp. p. 33, p. 36, p. 39. This theme continues later; see for example: “Cum nulla nisi valde manifesta principia admittam, nihilque praeter magnitudines, figuras et motus, mathematicorum more considerem, omnia mihi philosophorum subterfugia interclusi, et quicunque vel minimus error occuret, ab aliquo deprehendetur, et mathematica demonstratione refelletur” (To Plempius, 3 October 1637, in A.T. I p. 411).

to our problem, it amounts to deny any difference between the object of physics and the objects of mathematics, as well as between their respective degrees of certainty.[19]

Now, Descartes could as easily have asserted the identity of physics and mechanics in so far as he assumed that the object of physics is matter diversified by motion, without any distinction between violent and natural motions.[20] He did not do this, however, and I surmise that this was precisely because the most important thing for him at this point was to claim certainty for natural philosophy. And indeed, geometry had a better reputation concerning certainty than mechanics, which was, for Descartes as for most of his contemporaries, associated with various pejorative connotations despite its staut of mixed science[21]. "Mechanical" connotes empiricity.[22] It refers to artisans making instruments and marvellous machines.[23] It is opposed to the exactitude of geometry.[24] It may even imply meanness and vileness.[25] Considering on the one hand this ideal of certainty and, on the other hand, the pejorative connotations

---

[19] See Fichant, "La "Fable du Monde" et la signification métaphysique de la science cartésienne".

[20] Descartes insists that there is no distinction between natural and violent motions: "Je ne connais rien de violent dans la nature, sinon au respect de l'entendement humain, qui nomme violent ce qui n'est pas selon sa volonté, ou selon ce qu'il juge devoir être ; (…) et c'est aussi bien le naturel de l'eau d'être glacée lorsqu'elle est fort froide que d'être liquide lorsqu'elle l'est moins" (To Mersenne, January 1638, in A.T. I p. 485). "Je ne mets aucune différence entre les mouvements violents et les naturels, car qu'importe si une pierre est poussée par un homme ou bien par la matière subtile ?" (To Mersenne, 11 march 1640, in A.T. III p. 39). "Neque ista translatio magis violenta est materiae, quam quies : quippe nomen violenti non refertur nisi ad nostram voluntatem, quae vim pati dicitur, cum aliquid sit quod ei repugnat" (To Morus, 30 August 1649, in A.T. V p. 404).

[21] For mechanics as a mixed science, see: "Quaesivi (…) quare non modo jam dictae (arithmetica et geometria), sed astronomia etiam, musica, optivca, mechnanica, aliaeque complures, mathematicae partes dicantur" (*Regulae ad directionem ingenii*, reg. 4, in A.T. X p. 377).

[22] "Ce (…) qui revient merveilleusement à toutes les expériences mécaniques que j'ai faites de la nature sur ce sujet" (To Villebressieu, summer 1631, in A.T. I p. 217). "C'est une expérience fort vulgaire. Et il y en a une infinité de semblables, dans les mécaniques" (To Mersenne, 29 January 1640, in A.T. III p. 10).

[23] "{…) quaedam illorum machinae, quae apud Historicaos celebrantur" (*Regulae ad directionem ingenii*, reg. 4, in A.T. X p. 376). Those who "mechanicis student absque physica, et nocva ad motus ciendos intrumenta fabricant temere" are criticiez ((*Regulae ad directionem ingenii*, reg. 5, in A.T. X p. 380). "S'étant fort adonné à l'étude des mécaniques, il aurait fabriqué ou aidé à fabriquer plusieurs automates" ([March 1638], in A.T. II p. 39).

[24] "Prenant, comme on fait, pour géométrique ce qui est précis et exact, et pour mécanique ce qui ne l'est pas" (*La Géométrie*, Discours second, in A.T. VI p. 389). "Si on ne veut nommer démonstrations que les preuves des géomètres, il faut donc dire qu'Archimède n'a jamais rien démontré dans les mécaniques" (To Mersenne, 27 May 1638, in A.T. II p. 142). "Ses deux façons pour décrire la parabole sont du tout mécaniques, et en bonne géométrie sont fausses" (To Mersenne, 11 October 1638, in A.T. II p. 386).

[25] "des ouvrages mécaniques et grossiers, ou (…) des actions basses et serviles" (To Morin, September or October 1634, in A.T. I p. 314).

attached to the word "mechanics", it may be surprising that, at some point in the late thirties, Descartes introduces a new theme, namely that physics is mechanics.[26]

1.3. A mechanical physics

The chronology makes me think that this shift was mainly prompted by an accidental circumstance, namely a letter which Liber Fromondus, professor of philosophy in Louvain, addressed to Descartes in September 1637 after he had read the *Discours de la méthode* and the *Essais*. Fromondus blames Descartes' philosophy for being crude, gross, rude — in a word "mechanical."[27] In his answer, Descartes admits all these qualifications; at the same time he makes clear that his crude philosophy succeeds where other more subtle philosophies failed.[28] Moreover, he reverses the negative value of the term mechanical:

> "Si nimis *crassa* mea philosophia ipsi videtur, ex eo quod figuras, et magnitudines, et motus, ut mechanica consideret, illud damnat quod supra omnia existimo esse landandum, et in quo me praecipue effero et glorior : nempe, quod eo philosophandi genere utar, in quo nulla ratio est, quae non sit mathematica et evidens, cujusque conclusiones veris experimentis confirmantur ; adeo ut quicquid ex ejus principia fieri posse concludi, fiat revera, quoties activa passivis, id par est, applicantur. Miror ipsum non advertere illam, que

---

[26] This new theme does not cancel the theme that physics is geometry. See for example: "Talia sunt ea quae scripsi, ut, cum non aliis quam mathematicis rationibus, aut certa experientia nitantur, nihil falsi possint continere (…) Egi de multis quae soli philosophiae tribui solent, (…) et nihil optabilius esse puto in materia philosophica, quam ut mathematica probatio habeatur" (To Mersenne, 30 August 1640, in A.T. III p. 173). "On joint ici ma physique avec les pures mathématiques, auxquelles je souhaite surtout qu'elle ressemble" (*Sur les cinquièmes objections*, in A.T. IV p. 212-213). "Je ne connais point d'autre matière des choses corporelles que (…) celle que les géomètres nomment la quantité, et qu'ils prennent pour l'objet de leurs démonstrations ; et (…) je ne considère en cette matière que ses divisions, ses figures et ses mouvements ; et enfin (…) touchant cela, je ne veux rien recevoir pour vrai, sinon ce qui en sera déduit avec tant d'évidence qu'il pourra tenir lieu d'une démonstration mathématique" (*Principes de la philosophie*, II 64, in A.T. IX-2 p. 101-102. On this article, see De Buzon, "La *mathesis* des *Principia*").

[27] "rudem et pinguisculam", "nimis crassa et mechanica" (Fromondus to Plempius, 13 September 1637, in A.T. I p. 402 and p. 406 resp.).

[28] "Si rudem et pingusculam philosophiam meam esse dicat" (To Plempius for Fromondus, 3 October 1637, in A.T. I p. 417). "Si nimis crassa mea philosophia ipsi videtur" (*ibid.*, p. 420). "inveniet (…) crassam et rudem meam philosophiam pauculis istis esse contentam" (*ibid.*, p. 422). "Crassa mea philosophia talem augmentationem quantitatis non capit" (*ibid.*, pp. 428-429). "Confido ipum non adeo magnam occasionem reperturum pinguisculam et mechanicam philosophiam meam contemnendi" (*ibid.*, p. 430. The expression "mechanica philosophia mea" is analysed by Gabbey in this volume). The procedure here at stake is a constant feature of this letter: Descartes accepts Aristotelian words and ways of thinking and turns them to his own advantage; this is for example the case when he insists that one can infer almost certain conclusions from the addition of probable signs ("indicia"), or that one should rely on experiences.

hactenus in usu fuit, Mechanicam, nihil aliud esse quam verae Physicae particulam, quae cum apud vulgaris philosophiae cultores nullum locum reperiret, apud Mathematicos se recepit. Mansit autem haec pars Philosophiae verior et minus corrupta, quam caeterae, quia cum ad usum et praxin referatur, quicumque in eam peccant, suptuum jactura plecti solent, adeo ut si contemnat meam philosophandi rationem ex eo, quod sit similis Mechanicae, idem mihi esse videtur, ac si eamdem contemneret ex eo, quod sit vera."[29]

As we have seen, Descartes perfectly knew the pejorative connotations attached to the word "mechanical."[30] Here however he brings quite other connotations into play. As a mixed science, mechanics is connected with mathematics, and partakes in its certainty.[31] As a science related to practical ends, it is sanctioned if its results are disastrous: sooner or later the bad mechanician is doomed to ruin. Moreover, Descartes gives an account of the historical relationship between natural philosophy, mechanics and mathematics.[32] Mechanics, he says, is a small part of the true physics, which has been taken up by mathematicians because common philosophy rejected it. To have a better physics, a physics as certain as mathematics and controlled by experience, we should give back their belongings to philosophers and restore mechanics among them. One could however doubt that the procedures allowed in mechanics may be extended to the whole of physics: what is the element which makes this extension legitimate?

At this point one should notice that the ironical answer to Fromondus has not been completely dictated by the circumstances, but was also a very serious answer, which met a crucial point in Cartesian physics, namely the very notion of laws of motion, or principles according to which motion is conserved. According to Descartes, the core of physics as well as the core of mechanics are "rules of motion", "laws of mechanics", "laws of physics" and "laws of nature" — very often, he presented these expressions by pairs, in order to show that they are equivalent.[33] My point here is not to examine thoroughly the physical

---

[29] To Plempius for Fromondus, 3 October 1637, in A.T. I p. 420-421. This letter is discussed

[30] See the quotations given above, notes 22 to 25. On the medieval roots of these pejorative connotations, see Allard, "Les Arts mécaniques aux yeux de l'idéologie médiévale". On the meaning of "mechanics" in the seventeenth century, see Gabbey's paper in this volume.

[31] Somewhat differently, the *Discours de la méthode* insists on a negative consequence of the mixed sciences — it makes one forget pure mathematics: "je me plaisais surtout aux mathématiques, (…) mais je ne remarquai point encore leur vrai usage, et pensant qu'elles ne servaient qu'aux arts mécaniques (…)" (*Discours de la Méthode*, Discours 1, in A.T. VI p. 7).

[32] Gabbey made this point in "Descartes' Physics and Descartes' Mechanics: Chicken and Egg", esp. pp. 314-317.

[33] "Les règles suivant lesquelles se font ces changements, je les nomme les lois de la nature" (*Le Monde*, chap. 7, in A.T. XI p. 37). "Selon les règles des mécaniques, qui sont les mêmes que celles de la nature, lorsque plusieurs choses tendent ensemble à se mouvoir vers un même côté, [the strongest prevails over the others]" (*Discours de la Méthode*, cinquième partie, in A.T. VI p. 54). "mechanicae meae, hoc est physicae, leges docent [how the blood flows through a ligatured artery]" (To Plempius, 15 February 1638, in A.T. I p. 524). "Vous ne voudrez pas rejeter les règles des mécaniques et de la vraie physique, pour alléguer

concepts set up in these laws; neither it is to unravel their metaphysical foundations. I just want to point out two consequences of the identity of laws of nature and rules of motions. First, insofar as these rules bear on motion in general, without any distinction between violent and natural motions,[34] Descartes made possible the definition of mechanics as the science of motion, despite the fact that this definition does not appear explicitly in his works.[35]

1.4. "My physics is nothing else but mechanics"

The second consequence is well known: Descartes' agenda was not only to find laws of motion; because these laws of motions are laws of nature, he pretended that they were sufficient to explain all the natural phenomena:

> "Je pourrais mettre encore ici plusieurs règles, pour déterminer, en particulier, quand, comment, et de combien, le mouvement de chaque corps peut être détourné, augmenté ou diminué, par la rencontre des autres ; ce qui comprend sommairement tous les effets de la nature."[36]

These effects, that a few rules of motion should explain, concern in particular the later on called secondary qualities — according to Descartes, not only colours, odours, sounds and flavours, but also qualities like heaviness, liquidity or hardness. The thesis goes as far as including the motions which were

---

ici que toute la matière a de soi résistance au mouvement local" (To Morin, 13 July 1638, in A.T. II p. 212). "On peut par les règles des mécaniques démontrer [that the wind does not modify the action of subtle matter, which constitutes light]" (To Morin, 13 July 1638, in A.T. II p. 217). "Toute ma physique n'[est] autre chose que mécanique" (To Debeaune, 30 April 1639, in A.T. II p. 542).

[34] See the quotations given above, note 20.

[35] See Séris, "Descartes et la mécanique", p. 33, p. 36. According to Gabbey, "Newton's *Mathematical Principles of Natural Philosophy*", and to Gabbey, "Between *Ars* and *Philosophia Naturalis*", an explicit definition of mechanics emerges in late 1660s England by Barrow, Wallis and Boyle. In France I have found it in Poisson and Pardies — the latter could have been influenced on that point by Wallis, whose *Mechanica* he read. "(…) il faut prendre garde à ne pas se tromper touchant le mot de mécanique, qui ne signifie pas seulement cette science qui apprend à composer des machines, ou à en connaître les parties, mais sous ce mot on renferme aussi toutes les différentes manières dont un corps se meut pas rapport à certaines lois de la nature qu'on ne peut jamais contester" (Poisson, *Traité de la Mécanique,* p. 18). "(…) il n'est pas possible de pénétrer dans les secrets de la physique, ni de réussir dans l'invention et la pratique des arts, sans le secours des mécaniques, c'est-à-dire sans la connaissance des lois du mouvement" (Pardies, *Discours du mouvement local*, Préface [1670], in *Oeuvres du R. P. Ign. Gaston Pardies de la Compagnie de Jésus*, p. 135).

[36] *Le Monde*, chap. 7, in A.T. XI p. 47.

traditionally assigned to animal souls.[37] It is usually referred to by the expressions "mechanical philosophy", "mechanicism" or "mechanism": a sound theory of perception, as well as a sound mechanics or a sound biology, should be rooted in a physics which has got rid of all what is not matter and motion; to put it otherwise, matter (or extension) and (local) motion are the basic magnitudes, according to which everything ever seen under the sun should be explained.

It is obvious that, with respect to this general agenda, Descartes drew back in two ways. First, in his physical treatises, he explained most of the phenomena not with universal laws of motion, but with corpuscles of certain sizes and certain shapes — in that case a mechanical explanation is just a pseudo-atomical explanation.[38] Second, in his biological treatises, he almost never talk of laws of motion, or even of corpuscles.[39] At best he talks of machines, but in a very weak acception, explanations in this context amounting to the evocation of artificial designs (organs, clocks, automata), to comparisons with known phenomena, be they natural ones (eels, branches, sponges), or, simply, to the representation of necessary chains of causes and effects.[40]

This brief chronological survey of the successive meaning of "mechanics" in Descartes' works leads to the following conclusions. At the very beginning, Descartes, like some of his contemporaries, practised mechanics as one of the mixed sciences. The desire for a certain physics made him break with this practice, and it is only some time after this break that he realised that his new physics could be called "mechanical" as well as "geometrical". The reasons for this new appellation are twofold: it was a provocative answer to the reproach that his new physics considers only material things; it corresponds to the fact that the core of the new physics is a set of laws of motion, which are as well laws of nature. Descartes' agenda, namely first to find these laws of motion, then to use them to explain all natural phenomena, would later be modified and some weaker senses of "mechanical" emerge.

---

[37] "[Nature is able to form the parts of any animal] pourvu qu'on suppose que cette nature agit en tout selon les lois exactes des mécaniques, et que c'est Dieu qui lui a imposées ces lois" (To Mersenne, [20 February 1639], in A.T. II p. 525). "J'explique (…) leurs appétits naturels ou inclinaisons [des animaux] par les seules règles des mécaniques" (To Mersenne, 28 October 1640, in A.T. III p. 213). "il ne faut pas concevoir que cette séparation se fasse autrement que *mechaniche"* (To Mersenne, 24 December 1640, in A.T. III p. 264). "Ea enim est fabrica istarum valvularum, ut necessario juxta leges mechanicae (…) hae aperiantur et illae claudantur. (…) Quae omnia revera sunt mechanica, ut etiam mechanica sunt experimenta, quibus probatur esse varias anastomoses venarum et arteriarum" (To Beverwick, 5 July 1643, in A.T. IV p. 4-5).

[38] "Pseudo-atomical", because Descartes refutes atomism; on this refutation, see Roux, "Descartes atomiste ?". However, not all Cartesian explanations are pseudo-atomical: the explanations of lightness and of heaviness do not rely on corpuscules, but on universal laws of motion.

[39] Here are the exceptions which prove the rule: "la seule inclinaison qu'ils [animal spirits] ont à continuer leur mouvement, suivant les lois de la nature" (*Traité de l'homme, in* A.T. XI p. 137). "suivant les règles des mécaniques" (*La Description du corps humain, in* A.T. XI p. 279).

[40] On this point, see Séris, "La mécanique cartésienne", esp. pp. 44-51.

The general and the systematic character of this agenda may have blurred the different layers, that the word "mechanical" presents by Descartes, and obscured the fact that, even once he had established the general idea of mechanics, he went on practising mechanics as a specific mixed science — mostly, it must be said, because his correspondents requested him to do so. But we must, so to speak, held both ends and explain how the two elements, that were later on labelled "mechanical philosophy" and "mechanical science" have effectively interacted. I have elsewhere tried to show that some explanations of the Cartesian "mechanical philosophy" involve concepts, usually reputed to belong to his "mechanical science."[41] Here I would like to do the opposite: I shall take a rather technical piece of "mechanical science", and ask to what degree it has been moulded by the mechanical philosophy.

2. An exercise of mechanics: the letter to Mersenne of the 13th of July, 1638

There are three letters in which Descartes answered what he himself called a mechanical question in such a fashion that, at first sight, they may be considered as small treatises:[42] the first is the *Explication des engins par l'aide desquels on peut avec une petite force lever un fardeau fort pesant*, sent to Constantin Huygens on October the 5th, 1637; the second is the *Examen de la question savoir si un corps pèse plus ou moins, étant proche du centre de la terre qu'en étant loin*, presented in the letter to Mersenne of July the 13th, 1638; the third is the determination of how far a jet of water goes according to the height of the vessel it was contained in, written in mid-February 1643, at both Huygens' and Mersenne's request.[43] I have chosen the second of these letters, that is the answer to the so-called geostatical question, to illustrate the Cartesian practice of mechanics for three reasons: it has not received much attention; it includes most of the first letter; it offers glimpses on the state of French mechanics in the thirties. As I just explained, my aim is to ask to this particular letter the question of the relationship between mechanical philosophy and mechanical science; beforehand however, I would like to say a few words about the historical context in which it was written; next, I shall follow its development step by step, without forgetting to highlight some connections with other writings on mechanics.

2.1. The historical context

We know little about mechanics in France in the first third of the seventeenth century; writing on mechanics was at the time mostly an Italian affair. Of course, the readership of Italian books on mechanics was European; it is however significant that the only commentary on the *Mechanical*

---

[41] See Roux, *La Philosophie mécanique*, II chap. 1, 2 and 3, esp. the conclusions p. 438; pp. 520-521, pp. 557-558; pp. 720-721.

[42] "Ce que vous désirez des mécaniques" (To Huygens, 5 October 1637, in A.T. I p. 434). "Le petit traité de mécaniques que j'envoyai" (To Pollot, 12 February 1638, in A.T. I p. 518). "Le petit écrit de mécaniques que je vous ai envoyé" (To Mersenne, 15 November 1638, in A.T. II p. 431). "Quelques questions que vous me faites touchant le jet des eaux, et autres mécaniques" (To Mersenne, 15 sept. 1640, in A.T. III p. 176).

[43] On this letter, see Nardi, "Poids et vitesse, Descartes "presque" galiléen, 18 février 1643".

Questions was that of Henri de Monantheuil (1599), and that, except for Mersenne, the only French book devoted to mechanics before 1630 was a translation of Cardan, *Les livres de Hierôme Cardan (…) intitulés de la Subtilité et subtiles inventions, ensemble les causes occultes et raisons d'icelles, traduits de latin en français par Richard Le Blanc* (1566). Around the thirties a shift was initiated in Mersenne's circle, or at least made felt through Mersenne's commitment to imagine questions for others and to publish their results. In this regard, 1634 may be considered as a landmark: Albert Girard translated in French Simon Stevin's complete works, and, among them, *La Statique ou Art pondéraire*; Mersenne himself translated in French Galileo's *Mecaniche* and published it for the first time; Pierre Hérigone a bilingual (French-Latin) *Cours de mathématique*, including a part on the mechanics in the third tome, which remained a reference for teaching in France for the rest of the century. Two years later, Gilles Personne de Roberval wrote a short *Traité des mécaniques*, which was published independently and included in Mersenne's *Harmonie universelle*.

In 1635, Jean de Beaugrand, a former pupil of Viète at the time travelling in Italy, announced to the world and to the mathematicians close to Galileo that he was able to prove that a body is less heavy when it is closer to the centre of the Earth than when it is farther away.[44] The question, as well as the proof given by Beaugrand, was not new, having been first raised by Blasius of Parma, and one can keep track of it in the works of Benedetti, Guidobaldo and Stevin.[45] However, when Beaugrand returned in Paris

---

[44] In the *Geostatice*, p. 10, Beaugrand claimed that his proposition had received the approbation of Galileo and Castelli; the reality is somehow less glorious. Indeed, Beaugrand mentioned his proposition to Galileo at the end of a letter mostly devoted to the examination of the method for calculating longitudes proposed by Jean-Baptiste Morin — Beaugrand was one of the experts officially chosen to judge this method (Beaugrand to Galileo, 3 November 1635, in E.N. XVI pp. 336-337 and T.W.B. V pp. 454-456), but Galileo's answer does not say a word about geostatics (11 November 1635, in E.N. XVI p. 340 sqq.). At the end of November 1635, Beaugrand arranged a first meeting with Benedetto Castelli and exposed to him the geostatical proposition; Castelli demonstrated it and, when they met again a few days later, he gave to Beaugrand "a bone to gnaw", namely the proposition that, if one accepts the supposition that weights converge towards the centre of Earth, there is no centre of gravity in a sphere (Castelli to Galileo, 30 November 1635, in E.N. XVI pp. 351-352). Castelli sent also his own demonstration to Bonaventura Cavalieri, who appreciated it (Cavalieri to Castelli, 19 December 1635, in T.W.B. V p. 548). A frank negative reaction came from Raffaello Magiotti, who explained that Beaugrand's demonstration receals a "manifest petition of principle" (Magiotti to [Michelini], 25 January 1636, in E.N. XVI pp. 382-383 and T.W.B VI pp. 13-14). A few months later Castelli received, probably from Carcavy, one of Fermat's first writings on geostatics; he explained that at some point his enthousiasm for this question cooled down, because of the difficulties and paradoxes it raises (Castelli to [Carcavy], July or August 1636, in T.W.B. VI pp. 128-129).

[45] As far as I understand the matter, geostatical problems proceed from the mixture of statical questions, be they considered in the Archimedean tradition or in the Aristotelian tradition, with the assertion that heavy bodies tend towards the centre of the world, or that verticals are convergent lines. More precisely, the first traditional component of Beaugrand's question is Blasius of Parma's proposition that the more a scale is elevated, the heavier is the weight suspended to it — a result which is deduced from the proposition that "one body is heavier than another by the amount that its movement towards the centre [of the

and published his *Geostatice* in 1636,[46] it immediately became the pretext of hot debates in Mersenne's circle. Pierre de Fermat was the first to enter in the debate, probably not so much out of interest for the subject than to introduce himself to the *Academia parisiensis*: when Carcavy put him in relationship with Mersenne in 1636, the geostatical question was one of the first he was asked to answer.[47] In the summer 1636, Gilles Personne de Roberval read Fermat's *Nova in mechanicis theoremata* ; an intense correspondence between Roberval and Fermat followed during the next months, in which Etienne Pascal in August — in December 1636 however, it had run its course, each one having exposed his own principles and expressed his disagreement with the principles of the other.[48] Even the physician Guy de la Brosse, founder of the Jardin des Plantes and ardent defensor of the use of chemical principles in medicine, wrote in 1636 an *Eclaircissement d'une partie des paralogismes (…) de la première partie de la quatrième proposition (…) de la Géostatique*[49] René Descartes and Girard Desargues. The final outcome was not very positive for Beaugrand: he ended up on bad terms with everybody, even those like Fermat and Desargues who beforehand held him in high esteem.

---

world] is straighter" (*Tractatus Blasii de Parma de ponderibus*, prop. 7, in Moody and Clagett, *The Medieval Science of Weights*, pp. 242-243). Although he used quite other principles, Benedetti maintained the same proposition with similar argument and figure (*Diversarum speculationum mathematicarum, et physicarum liber*, cap. 3, in Drake and Drabkin, pp. 169-170). The other component of Beaugrand's question comes from Jordanus Nemorarius, and following his steps, Tartaglia: to prove that two equal weights at equal distances in a scale are in a stable equilibrium (so that, if deplaced, they will come back), they happen to assert that the highest weight is positionally heavier (*Elementa Jordani super demonstrationem ponderum*, prop. 2, in Moody and Clagett, pp. 130-132. *Quesiti, et inventioni diverse*, VIII 32, prop. 5, in Moody and Clagett, p. 125 sqq. On positional gravity, see below, note 52). Guidobaldo put these two components together: starting with a criticism of Jordanus and Tartaglia, he ended up defending a proposition similar to the one of Blasius and Benedetti, but founded on different principles (*Mechanicorum liber*, "On the balance", prop. 4, in Drake and Drabkin, pp. 261-272). As far as I know, Stevin is the first one to show that the convergence of parallels implies that two equal weights at equal distances in a scale are in equilibrium only if they are at equal distances from the centre of the Earth, and consequently that there is no centre of gravity — an absurde consequence, which he refuses (*La Statique*, livre I, I[ère] partie, pétition 5, p. 436).

[46] The thirteen propositions of the *Geostatice* are given in A.T. II pp. 645-46; the geostatical question is answered in the fourth proposition: "omne grave prope Terrae centrum minus ponderat quam procul, et ejusdem gravis varia pondera eandem habebunt rationem quam a Terrae centro distantiae".

[47] As he said: "J'aime mieux paraître ignorant en vous répondant mal qu'indiscret en ne vous répondant point du tout" (Fermat to Mersenne, in T.H. II p. 3). On Fermat's arguments, see Costabel, "Les enseignements d'une notion controversée : Le centre de gravité" and the brief "Appendix I" of Mahoney, *The Mathematical Career of Pierre de Fermat*, pp. 372-384.

[48] This correspondence has been published in T.H. II.

[49] I found no clew on the reason why he did so in the classical studies on de la Brosse (Guerlac, "Guy de la Brosse: botanist, chemist, and libertine" and entry "Guy de la Brosse" in *Dictionary of scientifc biographies*, VII, pp. 536-546, and Howard, "Guy de la Brosse: botanique et chimie au début de la Révolution scientifique"). I suspect that it may be related to the censorship Beaugrand exerced as secretary of the Chancellor Séguier (see below, note 51).

With the exception of Desargues, Descartes was the last to enter the debate in 1638, whereas a few months earlier he had written at Huygens' request a short treatise on the five simple machines, where he formulated for the first time what he calls the general principle of statics.[50] For a long time he resisted Mersenne's reiterated and insistent questions; what finally made him examine thoroughly Beaugrand's book and put his own propositions down in writing was obviously the desire to keep up his position in Mersenne's circle, but also to defeat Beaugrand with whom he happened to be in a bitter enmity.[51] This historical background is important: it shows that the Cartesian letter was written as an exercise of scientific virtuosity at a period where French mathematicians were reappropriating problems coming from various traditions of the old science of weights. Consequently, even if I follow step by step this letter, I shall not hesitate to transgress the Cartesian protocol of reading and confront his text to arguments and proofs of other mechanicians.

2.2. The letter (1). Absolute heaviness: the physical answer to the question

As an introduction, Descartes distinguishes between two kinds of heaviness, true or absolute heaviness on the one hand, apparent or relative heaviness on the other. To understand this distinction, he gives the example of a stick, which appears heavier if you hold it by one of its ends than if you hold it by its middle, although, being the same stick, it has in both cases the same absolute weight.[52] The first part of

---

[50] Desargues' *Brouillon Project,* written in 1639 but unpublished at the time, contains an annex called *Atteinte aux événements des contrariétés d'entre les actions des puissances ou forces,* obviously related to the geostatical controversy. This annex has been first published by Taton, *L'œuvre mathématique de G. Desargues*, pp. 181-184, and is briefly commented by Costabel, "Centre de gravité et équivalence dynamique", pp. 14-15.

[51] As "secrétaire du Roi" under the Chancellor Séguier, Beaugrand was in charge of giving "privilèges" to scientific books. He got the first printed sheets of the *Essais* at the beginning of 1637, delayed the publication and, at the time where the *Dioptrique* had not yet been published, criticized it and sent it to Fermat; after Descartes had pointed out the faults in the *Geostatice*, Beaugrand charged the *Géométrie* with plagiarism from Harriot and Viète in three anonymous pamphlets, later published by Tannery, "La Correspondance de Descartes dans les inédits du fonds Libri" (See Descartes to Mersenne, 22 June 1637, in A.T. I pp. 390-391. To Mersenne, 1st March 1638, in A.T. II p. 25. To Mersenne, 31 March 1638, in A.T. II p. 85. Beaugrand to Mersenne, April 1638, A.T. V pp. 504-512. See also the notes of the editors A.T. I p. 355 et pp. 361-362. A.T II pp. 457-459. A.T. II p. 269. A.T. II pp. 326-328, A.T. II pp. 395-396). This growing inimity goes probably back to the early thirties, where Beaugrand and Descartes competed on mathematical questions (T.W.B. III pp. 260-262).

[52] The notion of relative heaviness is of course an avatar of the Jordanian *gravitas secundum situm* (see for example *Jordani Liber de ponderibus*, "Proemium", in Moody and Clagett, pp. 150-151, *passim*). It may recall the definition of the moment", given in Mersenne-Galileo, *Les Méchaniques de Galilée*, p. 444: "Le *moment* est l'inclinaison d'[un] corps, lorsqu'elle n'est pas seulement considérée dans ledit corps, mais conjointement avec la situation qu'il a sur le bras d'un levier ou d'une balance"; contrary to the Galilean *momento* however, the Cartesian relative heaviness has no dynamical application. On "*gravitas secundum situm*", see Galluzzi, *Momento*, pp. 70-73 ; on the different acceptions of "momento" by Galileo, see *ibid.*,

the letter, actually much shorter than the second one, is dedicated to the absolute heaviness and explains how to reach a physical answer to the geostatical question. The second part gives a mathematical answer to the question; this answer concerns only relative heaviness and seems from the very beginning fraught with doubts because it relies on a false supposition concerning absolute heaviness.

In the first part, Descartes begins by drawing the two consequences deriving from the three existing opinions concerning the nature of heaviness for the geostatical question. If heaviness is in heavy bodies or if it depends on their forms or on their quantity of matter, it is always the same and does not change according to their distance to the centre of Earth.[53] If heaviness is in the Earth which attracts bodies, they are heavier when they are closer to the Earth.[54] Mersenne on the one hand, Roberval and Pascal on the other, had similarly linked the nature of heaviness and observable phenomena. However, not only are the opinions they evoked not exactly the same, but their conclusions are different.[55] Mersenne's point is mainly that all the opinions concerning the nature of heaviness may be reconciled with the proportion observed in the fall of heavy bodies.[56] Roberval's and Pascal's conclusion is that, since one does not

---

pp. 199 sqq. In what follows, I shall quote the Galilean *Le Mecaniche* in the translation of Mersenne, at least as far as it is possible: the similitude of formulations is all the more strinking when the language is the same.

[53] "Suivant ces deux opinions, dont la première est la plus commune de toutes dans les écoles, et la seconde est la plus reçue entre ceux qui pensent savoir quelque chose de plus que le commun, il est évident que la pesanteur absolue des corps est toujours en eux une même, et qu'elle ne change point du tout à raison de leur diverse distance du centre de la terre" (To Mersenne, 13 July 1638, in A.T. II pp. 223-224). The second opinion is compatible with the definition of heaviness given in Mersenne-Galileo, *Les Méchaniques de Galilée*, p. 443: "La *pesanteur* d'un corps est l'inclination naturelle qu'il a pour se mouvoir et se porter en bas vers le centre de la terre. Cette pesanteur se rencontre dans les corps pesants à raison de la quantité des parties matérielles, dont ils sont composés ; de sorte qu'ils sont d'autant plus pesants qu'ils ont une plus grande quantité desdites parties sous un même volume". On the notion of gravity in Galileo's *Mecaniche*, see Galluzzi, *Momento*, pp. 94-95.

[54] The third opinion is for example the one of Roberval. Descartes' argument is that the activity of a natural agent decreases with its distance; for a similar argument, see Mersenne, *Traité des mouvements et de la chute des corps pesants*, prop. III, quoted in T.W.B. III p. 632. Roberval and Etienne Pascal argue however that if heaviness is an attraction, it decreases when the body goes nearer to the centre of the Earth (To Fermat, 16 August 1636, § 8, in T.H. II, pp. 40-41). This apparent contradiction comes from the fact that Descartes and Mersenne consider the body when it is outside the Earth, Pascal and Roberval when it is inside.

[55] For Mersenne, heaviness may be either "positive et réelle", or an effect of the pushing air, or an attraction; for Roberval and Pascal, heaviness is an attraction, the cause of which may be either in the attracted body, or in the attracting body, or in both.

[56] "Encore que l'on ne sache pas la vraie raison de la chute des corps terrestres (…), l'on peut néanmoins expliquer quelques raisons qui satisferont à plusieurs, soit que la pesanteur des corps les pousse en bas, que l'air les chasse hors de son lieu, que la terre les attire, ou que ces trois causes et plusieurs autres contribuent à cet effet" (Mersenne, *Traité des mouvements et de la chute des corps pesants*, prop. III, quoted in T.W.B. III p. 631). However he added a remark, which may be retrospectively linked to the geostatical question: if bodies were attracted by the Earth, they should accelerate less on a given distance when they are farther from the Earth than when they are closer (*ibid*., quoted p. 632).

know the nature of heaviness, one can not make any statement concerning a question which, like the geostatical question, depends on this nature.[57] As for Descartes, he seems to have mentioned these opinions simply to show that he was well informed and had a synthetic view of the subject. Namely, he continues by asserting that his own opinion differs from the three he just mentioned, but that he shall not to express it here.[58] He however informs us that it allows him to conclude that the geostatical question is a mere question of fact:

> "par elle [mon opinion sur la pesanteur], je n'apprends rien qui appartienne à la question proposée, sinon qu'elle est purement de fait, c'est-à-dire qu'elle ne saurait être déterminée par les hommes, qu'en tant qu'ils peuvent faire quelque expérience."[59]

Consequently, he suggests experimental ways of resolving the question. First, he says, we could compare a weight at the top of a tower and at the bottom of a well, on condition that the difference between the height of the tower and the depth of the well is great.[60] We could also rely on well-known observations: planets, big birds, snow and kites have no difficulties to fly high, although they are heavy on the ground.[61] Last, Mersenne had reported that balls vertically shot do not come down.[62] These

---

[57] "Puis donc que de ces trois causes possibles de la pesanteur, nous ne savons quelle est la vraie, et que même nous ne sommes pas assurés que ce soit l'une d'icelles, se pouvant faire [que la vraie cause soit composée des deux autres ou] que ce [en] soit une [tout] autre, de laquelle on tirerait des conclusions toutes différentes, il nous semble que nous ne pouvons pas poser d'autres principes [pour raisonner] en cette matière que ceux desquels nous sommes assurés par une expérience continuelle assistée d'un bon jugement" (Roberval and Etienne Pascal to Fermat, 16 August 1636, § 9, in T.H. II p. 41).

[58] The Cartesian notion of heaviness comports one metaphysical side and one physical side. From a metaphysical point of view, Descartes criticizes the confusion of substance and accident as well as the confusion of body and mind that are embodied in all the usual opinions of heaviness (on this point, see the following letters. To ?, [August 41], in A.T. III p. 424. To Arnauld, 29 July 1648, in A.T. V pp. 222-223. To Elizabeth, 21 May 1643, in A.T. III pp. 667-668. To Mersenne, 20 April 1646, in A.T. IV p. 401). From a physical point of view, he suggests mechanisms which could explain heaviness without making such confusion, that is mainly without having to make the supposition that a body can have in himself himself towards the center of the earth.

[59] Descartes to Mersenne, 13 July 1638, in A.T. II p. 224. Among the authors I have studied, Descartes and Mersenne are the only ones who suggest that one should rely experiments to answer the geostatical question.

[60] Robert Hooke will carry out such experiments in the sixties; see Dugas, *La Mécanique au XVII^e siècle*, pp. 357-358.

[61] It was then common knowledge that birds prefer to fly high, the problem being to find why. See for example Beeckman, *Journal*, [3]-20 October 1624, II p. 306; 2 May- 7 July 1634, III p. 348; 24 September 1634, III p. 310, and the references of the editor III p. 253, note 2. Mersenne, *Harmonie universelle*, livre III : Des mouvements des cordes, prop. 20 [misnumbered as prop. 19], tome I, p. 207.

[62] In April 1634, Descartes asked Mersenne to perform this experience, about which he read in Jean Leurechon's *Récréations mathématiques*, problème 86 (To Mersenne, April 1634, in A.T. I p. 287. The text of the problem 86 in reproduced in A.T. X

observations lead Descartes to a qualitative answer to the geostatical question: a body is heavier when it is closer to the centre of the Earth than when it is farther and at some point it looses entirely its heaviness. He concludes then rather abruptly:

> "Voilà tout ce que je puisse dire ici de physique sur ce sujet. Je passe maintenant aux raisons mathématiques, lesquelles ne se peuvent étendre qu'à la pesanteur relative."[63]

2.3. The letter (2). Relative heaviness: the explanation of simple machines

Descartes begins by stressing the limitations of this mathematical way of proceeding. Not only does it concern only relative heaviness, but it relies on a conventional, local and empirical determination of heaviness:

> "Nous prendrons, s'il vous plaît, pour la pesanteur absolue de chaque corps, la force dont il tend à descendre en ligne droite, étant en notre air ordinaire à certaine distance du centre de la terre, et n'étant ni poussé ni soutenu par aucun corps, et enfin n'ayant point encore commencé à se mouvoir."[64]

Moreover, one should rely on the first or the second opinion about the absolute heaviness, which Descartes has judged false:

> "Nous supposerons que chaque partie d'un même corps pesant retient toujours en soi une même force ou inclination à descendre, nonobstant qu'on l'éloigne ou qu'on l'approche du centre de la terre, ou qu'on le mette en telle situation que ce puisse être. Car encore que,

---

p. 547). Descartes was unsatisfied with Mersenne' first experience, performed with an arquebuse (To Mersenne, 15 May 1634, in A.T. I p. 293). Two years later however, he congratulated Mersenne for having performed it again, apparently in satisfying conditions: "Je vous remercie aussi de celle [l'expérience] tirée vers le zénith, qui ne retombe point, ce qui est fort admirable" (To Mersenne, March 1636, in A.T. I p. 341). This experience is also alluded to in Mersenne, *Harmonie universelle*, livre III : Des mouvements des cordes, prop. 20 [misnumbered as prop. 19], tome I, p. 207. It is the subject of an engraving at the beginning of Pierre Varignon's *Nouvelles conjectures sur la pesanteur*, Paris: Jean Boudot, 1690: in the middle, a gun is pointing the zenith; right and left, Marin Mersenne and Pierre Petit tilt their heads backwards and look upwards; below is indicated the question in which they are wrapped up: "Retombera-t-il?".

[63] Descartes to Mersenne, 13 July 1638, in A.T. II p. 226.

[64] *ibid.*, pp. 226-227. That this supposition is a conventional postulate is clear from the way it is introduced: "Nous prendrons, s'il vous plaît…". In the sentences following the quotation I just gave, Descartes explains why he introduces all these specifications, which make this determination of heaviness local and empirical.

comme j'ai déjà dit, cela ne soit peut-être pas vrai, nous devons toutefois le supposer, pour faire plus commodément notre calcul."[65]

After this set of conventional or false suppositions, Descartes recalled the obvious principle, that he first stated in the treatise on simple machines sent to Huygens almost one year before:

"La preuve de ceci ne dépend que d'un seul principe, qui est le fondement général de toute la statique, à savoir qu'il ne faut ni plus ni moins de force pour lever un corps pesant à une certaine hauteur, que pour en lever un autre moins pesant à une hauteur d'autant plus grande qu'il est moins pesant, ou pour en lever un plus pesant à une hauteur d'autant moindre. Comme par exemple, la force qui peut lever un poids de 100 livres à la hauteur de deux pieds, en peut aussi lever un de 200 livres à la hauteur d'un pied, ou un de 50 à la hauteur de 4 pieds, et ainsi des autres, si tant est qu'elle leur soit appliquée."[66]

In other words, weights and heights compensate each others, so that the same force F can raise a weight P_1 of a height h_1 or a weight P_2 of a height h_2 if $P_1 \times h_1 = P_2 \times h_2$. Descartes was certainly not the first to introduce certain proportions in statics; two points make him stand, if not completely alone, at least out from the crowd of mechanicians: his commitment to exclude from statics considerations of time and speed on the one hand; his success in founding statics in one unique principle on the other hand. I shall develop the first point below in 3.2, I concentrate here on the second one.

It should be remembered that Renaissance statics had inherited from various traditions — the Aristotelian, the Archimedean, the Jordanian. Each of these traditions had its own principle, and none of these principles was apt by itself to account for the five simple machines. For example, it is impossible to explain the inclined plane according to the analysis of the lever developed by Archimedes, and that is why Stevin had recourse to another principle, namely the impossibility of perpetual motion.[67] In a sense, this is also the case of Galileo: contrary to what has been often said, the principle of compensation that he formulated, according to which whatever is gained in force through a machine is lost in time and speed, is not a conclusion, but a true principle set at the very beginning of the treatise, and working effectively

---

[65] *ibid.*, p. 227. That Descrates is refering to the two opinions that he has just related is clear in the very formulation; compare the text just quoted and pp. 223-224 of the letter : "suivant ces deux opinions (…) la pesanteur absolue des corps est toujours en eux une même, et qu'elle ne change point du tout à raison de leur diverse distance du centre de la terre"**.**

[66] *ibid.*, p. 228. For a good commentary on the meaning of this principle for Cartesian statics, see Séris, *Machine et communication*, esp. pp. 211-221. The question of why this principle is not one of the laws of nature of the *Principia philosophiae* is addressed in Gabbey, "Descartes's Physics and Descartes's Mechanics: Chicken and Egg?".

[67] *La Statique*, livre I, théorème 40, proposition 19, pp. 448-449.

in the explanation of every machine.[68] However, Galileo does not make a point to derive everything from this principle — in fact, he also relies on the supposition that equal bodies suspended at equal distances are in equilibrium, from which he tries to deduces the principle of the lever.[69] On the contrary Descartes undertakes to demonstrate even this supposition through his principle; in a word, his aim is here really to accomplish an axiomatization of statics: he reduces as much as possible the number of principles, even if it turns out that it obliges him to demonstrate in a most unnatural way what has been beforehand hold for obvious: the lever appears as the most difficult of the machines.[70]

The Cartesian principle is similar to an axiom not only because of its fecundity, but also because of its obviousness. Descartes insists that it is as clear as 1+1=2 and that it does not need any empirical proof.[71] It is namely justified by the proportionality between causes and effects. The equality of the forces is inferred from the equality of the effects because force, far from being a mysterious power which subsists in the body, is totally exhausted in the production of certain effects (here, raising weights).[72] As for the

---

[68] For an example of this historiographic opinion, see Clavelin, *La Philosophie naturelle de Galilée*, p. 169. For formulation of the principle, see Mersenne-Galileo, *Les Méchaniques de Galilée*, chap. 1, pp. 439-441; chap. 5, p. 454; chap. 7, pp. 464-465; chap. 8, p. 468; *passim*.

[69] *ibid*., chap. 3, pp. 445-446.

[70] "J'ai différé à parler du levier jusques à la fin, à cause que c'est l'engin pour lever des fardeaux le plus difficile de tous à expliquer" (To Huygens, 5 October 1637, in A.T. I p. 443). That the Cartesian order of reasons had something outraging for common sense statics appears in the criticism Pardies adresses to the Cartesian principle: he admits that it is "très véritable" and "indubitable", but notices that it presents a "quelque chose qui ne satisfait pas entièrement l'esprit, qu'il suffise pour faire des démonstrations" (Pardies, *La Statique ou la science des forces mouvantes*, § 62, pp. 102-103).

[71] "On s'est imaginé que j'avais apporté les exemples de la poulie, du plan incliné et du levier, afin d'en mieux persuader la vérité, comme si elle eût été douteuse, ou bien que j'eusse si mal raisonné que de vouloir prouver un principe, qui doit de soi être si clair qu'il n'ait besoin d'aucune preuve, par des choses qui sont si difficiles qu'elles n'avaient peut-être jamais ci devant été bien démontrées par personne" (To Mersenne, 12 September 1638, in A.T. II p. 358). "il ne faut que savoir compter jusqu'à deux (…)" (*ibid*., p. 360). This made me qualify Gabbey's conclusion, in "Descartes' Physics and Descartes Mechanics: Chicken and Egg", p. 320, that the GPS, as he calls it, is not a law of nature because of its empiricity: Descartes explains precisely that his principle is not a conclusion reached through experiences; in other words, his reasons for not including it in his set of laws of nature are not epistemological, but exclusively ontological (it is not formulated in terms of matter and motion). Such a conclusion is of course not without complications, because it raises the question of how epistemological evidence can be separated from its ontological reference.

[72] "L'effet doit toujours être proportionné à l'action qui est nécessaire pour le produire" (To Huygens, 5 October 1637, in A.T. I p. 438. To Mersenne, 13 July 1638, in A.T. II p. 228). Descartes distinguishes in this context "action" (which is the force corresponding exactly to the effect) and "power" (which may exceed the effect): "Lorsqu'on dit qu'il faut employer moins de force à un effet qu'à un autre, ce n'est pas dire qu'il faille avoir moins de puissance : car encore qu'on en aurait davantage, elle n'y nuit point ; mais seulement qu'il y faut moins d'action. (…) Or il n'y a point, ce me semble, d'autre moyen de connaître *a priori* la quantité d'[un effet] (…), que de mesurer la quantité de l'action qui cause cet effet, c'est-à-dire de la

equality of the effects, it is simply a question of manipulating quantities in an homogenous space: raising one unit of weight through two units of space is the same as raising two units of weight through one unit of space, or, to put it in a geometrical way, a rectangle, the sides of which are 2 and 1, is equal to a rectangle, the side of which are 1 and 2.[73]

Just after having exposed his principle of statics, Descartes explains how to apply it to the comparison between absolute heaviness (hereafter P_a) and relative heaviness (hereafter P_r):

> "Et il suit évidemment de ceci que la pesanteur relative de chaque corps, ou ce qui est le même, la force qu'il faut employer pour le soutenir et empêcher qu'il descende, lorsqu'il est dans une certaine position, se doit mesurer par le commencement du mouvement que devrait faire la puissance qui le soutient, tant pour le hausser que pour le suivre s'il s'abaissait. En sorte que la proportion qui est entre la ligne droite que décrirait ce mouvement, et celle qui marquerait de combien ce corps s'approcherait cependant du centre de la terre, est la même qui est entre la pesanteur absolue et la relative."[74]

This is a passage worth of comment. Descartes first notices that the relative heaviness is the force to sustain a body in a certain position and that it may be measured by the beginning of motion that the power which sustains it should make in order to raise it or to go down with it. [75] The assimilation of the force to sustain and the force to raise may remind of Galileo, who, contrary to Guidobaldo, had argued that any minimal heaviness added to a body in a position of equilibrium is sufficient to move it, so that one can neglect the difference between the power to sustain and the power to raise.[76] In other texts

---

force qui doit y être employée" (To Mersenne, 15 November 1638, in A.T. II pp. 432-433). On this point, see also Debeaune to Mersenne, 13 November 1638, in A.T. V p. 526.

[73] To Mersenne, 12 September 1638, in A.T. II pp. 356-357. This very abstraction of this manipulation is criticized by Lamy: a man who is able to raise a weight of 1 to a height of 1000 is not able to raise a weight of 1000 to a height of 1 (*Traités de mécanique*, p. 79). This criticism has been forestalled by Descartes: "Je ne considérais pas, en cet écrit, la puissance qu'on nomme la force d'un homme, mais seulement l'action qu'on nomme la force par laquelle un poids peut être levé (…)" (To Mersenne, 15 November 1638, in A.T. II pp. 432-433).

[74] To Mersenne, 13 July 1638, in A.T. II p. 229. Reiterated discussions with Egidio Festa helped me to understand what is at stake in this passage.

[75] As Costabel, "La démonstration cartésienne relative au centre d'équilibre de la balance", p. 94, notes, considerations on the direction of forces are of so little moment for Descartes that he assimilates the relative heaviness of a body and its opposite, that is the power to hold it.

[76] Guidobaldo del Monte, *Mechanicorum liber*, "On the lever", prop. 4, corollary, in Drake and Drabkin, p. 300, *passim*. Galileo, *Le Mecaniche*, in E.N. II p. 164: "E perchè, per fare descendere il peso B, ogni minima gravità accresciutagli è bastante, però, non tenendo noni conto di questo insensibile, non faremo differenza dal potere un peso sostenere un alto al

however, Descartes notes that the force to raise is always a little bit greater than the force to sustain.[77] This is all the more serious for him that it is not a matter of experience, but of geometry: he insists that the two-dimensions force of raising can be no more assimilated to the one-dimension force of sustaining than a surface can be assimilated to a line.[78] The least that one can say is that Descartes is not coherent on this point. As for the reason of this incoherence, it probably amounts to the fact that, notstanding the difference between a line and a surface, there is no way of measuring the force to sustain except through its geometrically measurable effects, that is through its motions.[79]

Once the assimilation between the force to raise and the force to sustain is admitted, what Descartes does is pretty simple. The absolute heaviness being linked to a displacement towards the centre of the Earth, and the relative heaviness being linked to a displacement restricted by the constraints of a given machine, he applies the principle of statics and gets an indirect evaluation of P_r by the ratio P_a/P_r. This indirect evaluation may be summarised in a formula easier for a modern reader than long sentences:

P_a/P_r = displacement according to the mechanical constraints/displacement towards the centre of the Earth

Two remarks should be made in order to precise the range and meaning of such a formula. First, the link between the convergence of verticals and the geostatical question appears here in a nutshell: if the convergence of verticals is not taken into account, the displacement towards the centre of the Earth is a constant; consequently P_a/P_r is also a constant, and there is no variation of the relative heaviness.[80] Second, Descartes has no algorithm to calculate in general what this ratio is; he evaluates it case after case, that is simple machine after simple machine. Moreover, one should not so much speak of evaluation

---

poterlo movere". This decisive passage for bridging the static-dynamic gap in the case of simple machines is not translated by Mersenne.

[77] To Huygens, 5 October 1637, in A.T. I p. 438. To Mersenne, 13 July 1638, in A.T. II p. 229.

[78] "J'ai parlé de la force qui sert pour lever un poids à quelque hauteur, laquelle force a toujours deux dimensions, et non de celle qui sert en chaque point pour le soutenir, laquelle n'a jamais qu'une dimension, en sorte que ces deux forces diffèrent autant l'une de l'autre qu'une superficie diffère d'une ligne" (To Mersenne, 12 September 1638, in A.T. II p. 353. See also *ibid.*, p. 357).

[79] Dugas, *La Mécanique au XVII^e siècle*, p. 155, note 1, goes as far as mentioning "le caractère différentiel" of the Cartesian principle of statics. Such a passage should warn us against asserting without further consideration that there is here the first statement of the principle of virtual speeds: if Bernoulli states this principle, it is also that he had a somewhat clearer idea about how to manipulate infinitely small quantities.

[80] As for the reason why one should take this convergence into account, Descartes says that it is for the sake of mathematical exactitude (To Mersenne, 13 July 1638, in A.T. II p. 232-234). For a similar position, see Fermat to Mersenne, 24 June 1636, in T.H. II p. 19; Fermat, *Nova in mechanicis theoremata*, in *ibid.*, p. 23. For an opposite position, see Stevin, *La Statique*, livre I, I^ère partie, pétition 5, p. 436.

than of diagrammatic determination: the ratios are not expressed in a formal way, but seen on peculiar diagrams.

Now, let's show how the diagrammatic determination works for each simple machine — the pulley, the inclined plane and the lever. I'll skip here the explanation of the pulley, which presents no difficulty: because the weight as well as the power act vertically, it is a direct application of the principle of statics.

In the inclined plane ABC (figure 1), according to the principle of statics the action to raise the body F along AB equals the action to raise the body F along AC. But, whereas on AB one should take into account its absolute heaviness, on AC only the relative heaviness intervenes. In other words:

$P_a/P_r = AC/AB$.[81]

In writing this equation, you consider the displacement as a whole, not how the relative heaviness of the body is modified when it is raised along the plane. But the Earth being not a plane and the verticals being not parallels, P_r is not a constant, and therefore the equation can not be true at each point F.[82]

Now, if you look for the variations of P_r (figure 2b), you have still P_a/P_r = displacement according to the mechanical constraints/displacement towards the centre of the Earth. Descartes first gives a bracket of these variations: P_r is maximal in A, for which you have $P_a/P_r = AC/AB$; it is minimal and equal to zero in K, given by KA perpendicular to KM. Then he notices that at any point D on the line AC, you can construct an inclined plane DNP for which you have:

---

[81] Descartes asserts that every mechanician knows this ratio; it was indeed accepted by Tartaglia, Galileo, Stevin, Roberval and Mersenne, even if they use different principles, but Guidobaldo had preferred to follow Pappus and Salomon de Caus had ignored the problem in the little treatise on simple machines, that he inserted in *Les Raisons des forces mouvantes*, theoreme 9-theorem 18, fol. 5 verso- fol. 9 verso. As for Descartes, he states it for the first time, without any justification, in the letter to Mersenne of 3 may 1632, in A.T. I p. 247.

[82] As Descartes writes, it would be true if CB were a part of a circle and CA a part of a spiral (To Mersenne, 13 July 1638, in A.T. II p. 233). The line according to which P_r is a constant is a spiral (figure 2a), because at each point F of the line the angle (MFK) is the same, M being the centre of the Earth, and FK the tangent to the line. The angle (MFK) is constant because P_a on FM is by definition a constant; having fixed the quantity of relative heaviness, you want also P_r on FK constant; consequently, you must have the angle (MFK) constant in the right-angle triangle.

This problem had already been solved by Mersenne, in *Harmonie universelle*, livre II des mouvements, prop. 8: *Démontrer si un point peut descendre par un plan incliné jusques au centre de la terre et la manière de décrire une ligne tellement inclinée que le poids pèse toujours dessus et la presse également en chaque point,* et prop. 9: *Expliquer une autre manière plus aisée, pour décrire le plan d'une égale inclinaison*, in tome I, pp. 113-120 (See also the notes of the editors, in T.W.B. IV pp. 264-265). Contrary to Mersenne, Descartes affirms however that this spiral reaches the centre of the Earth (To Mersenne, 11 October 1638, in A.T. II p. 390).

$P_a/P_r = DP/DN.$

N is any point on DM, M being the centre of the Earth, and P is the point on ADK given by NP perpendicular to NM; DN represents the displacement towards the centre of the Earth, and DP the displacement along the inclined place.

A last comment should be added here. Descartes insists that one should take into consideration the beginning of motion, not the motion.[83] The question is why. Obviously, it is not because the equation $P_a/P_r = DP/DN$ would hold only for a very small DP: as we have noticed, it holds for any point N on DM, DP being constructed accordingly. Three reasons can be found for this precision, the first two ones implicit, the last one explicit. First, as we have seen, the assimilation between the force to sustain and the force to raise is valid only for the beginning of motion, or for an infinitely small displacement. Second, on a displacement which would not be infinitely small, the relative heaviness would vary, and the consequent motion also; in other words, one should consider a displacement so small that the motion has no time to transform.[84] Thirdly, and this is the only explicit point in Descartes, this precision lets extend the reasoning for the point D on the inclined plane AC to any curve, the tangent of which is AC. As we shall see, this will be used by Descartes to give an account of the lever, conceived as a circularly inclined plane. It allows more generally an extension of the principle of statics to any system, independently of the variations of the forces applied to it.

The third machine considered by Descartes is the lever COH (figure 3); when it goes up or down, AO describes the semi-circumference ABCDE and OF the semi-circumference FGHIK. Let us suppose that we apply a certain power acting along the line ABCDE in order to raise a weight initially in F: it travels along FGHIK, but it is raised from the height FK. Applying the principle of statics, we consequently get the following relationship:

[83] "Notez que je dis *commencer à descendre*, non pas simplement *descendre*, à cause que ce n'est qu'au commencement de cette descente à laquelle il faut prendre garde" (To Mersenne, 13 July 1638, in A.T. II p. 233).

[84] As Costabel, "La démonstration cartésienne relative au centre d'équilibre de la balance", p. 99, nicely puts it: "On ne peut engager, au stade de l'élaboration logique où l'on se place, une structure du mouvement que l'on ne connaît pas encore. On ne peut donner des définitions simples (force à deux dimensions) et poser des proportions simples (avec les déplacemnts) qu'à l'échelon infinitésimal où l'on "saisit" le mouvement dans une durée très courte, une ébauche, où le mouvement n'a pas le temps de changer si l'on peut dire. Mais autant, alors, se contenter de considérer les petits déplacements, sans parler du temps ni des vitesses, puisqu'aussi bien ce sont ces petits déplacements qui donnent "raison des forces à considérer". I shall come back below in 3.2. on why Descartes does not consider speed.

P_a/P_r = circumference ABCDE/diameter FK.[85]

In this equation, the displacement is considered as a whole. It is however obvious that the power to raise is smaller in A or in E, where the vertical displacements are small, than in C, where the vertical displacement is maximal. To measure exactly the variations of P_r according to the position of the lever, Descartes assimilates FGHIK to a "circularly inclined plane", along which a weight would be dragged; at each point of this "circularly inclined plane", the power is measured by the inclinaison of the circumference.[86] For example, the power necessary at the point B of ABCDE is given by the inclination in the corresponding point G of FGHIK. In other words, at the point G:

Pa/ Pr = GM/GR, M being any point on the tangent in G, T being the centre of the Earth, R being on GT and given by MR perpendicular to GR.

The similitude with the Galilean *Le Mecaniche* is too striking to be hushed up. The two important steps of Descartes' demonstration consist namely 1. in assimilating a body travelling on a circularly inclined plane to a body travelling on a semi-circumference because it is suspended to the end of a lever and 2. in measuring the infinitesimal displacement on this circularly inclined plane by its inclination. But these steps are precisely the ones that Galileo took.[87] There are however two differences. First, Galileo proceed from the lever to the inclined plane, not the reverse. Second, and this the most important, his aim is to evaluate the moment of descent of a body on variously inclined planes, or, to put things more generally, to reach conclusions about the motion of bodies, whereas Descartes, as we have seen, mentions motion only to make it disappear.

---

85 "La proportion qui est entre la force qui meut ce poids et sa pesanteur, ne se mesure pas par celle qui est entre les deux diamètres de ces cercles, ou entre leurs deux circonférences, mais plutôt par celle qui est entre la circonférence du premier et le diamètre du second" (To Mersenne, 11 July 1638, in A.T. II p. 235).

86 "Pour mesurer exactement quelle doit être cette force en chaque point de la ligne courbe ABCDE, il faut penser qu'elle y agit tout de même que si elle trainait ce poids sur un plan circulairement incliné, et l'inclinaison de chacun des points de ce plan circulaire, ou sphérique, se doit mesurer par celle de la ligne droite qui touche le cercle en ce point-là" (*ibid*., p. 236).

87 "Il considerare questo grave discendente, e sostenuto dalli semidiametri BF, BL ora meno e ora più, e constretto a caminare per la circonferenza CGL, non è diverso da quello che saria imaginarsi la medesima circonferenza CFLI esser una superficie così piegata, e sottoposta al medesimo mobile, sì che, appoggiandovisi egli sopra, fosse constretto a descendere in essa ; perchè se nell'uno e nell' altro modo disegna il mobile il medesimo viaggio, niente importerà s'egli sia sospeso dal centro B e sostenuto dal semidiametro del cerchio, o pure se, levato tale sostegno, s'appoggi e camini su la circonferenza CFLI" (*Le Mecaniche*, in E.N. II p. 184). There is no equivalent of this passage is Mersenne's translation, but the other passage is translated: "Le point d'inclination F de la circonférence CI ne diffère point de l'inclination de la tangente CFH, que par l'angle insensible de contact" (Mersenne-Galileo, *Les Méchaniques de Galilée*, chap. 9, p. 485).

2.4. The letter (3). The mathematical answers to the geostatical question

In the very last part of the paper, Descartes answers the geostatical question thanks to his previous demonstrations on simple machines. He distinguishes several cases, whether the body is free or in a scale,[88] and in the first case, whether it is solid or liquid.[89] In comes to a set of three answers, that I shall here present without further comment:

— a hard body in air without any constraint is less heavy when it is closer to the centre of the Earth; the demonstration relies on the former analysis of the inclined plane. Descartes considers a hard body BCD descending from C to A along the line HFA, A being the centre of the Earth (figure 4). Since BCD is a hard body, B is forced to descend along BE and D along DG. But, Descartes says, the parts of the body between D and C hold D as an inclined plane would do, therefore DG can be mentally assimilated to an inclined plane along which D would descend.[90] In that condition, we can deduce from the previous analysis of the inclined plane that the relative heaviness is zero at G, G being the point where the perpendicular drawn from the centre of the Earth meets the inclined plane. It means that the relative heaviness of D decreases when it comes closer to the centre of the Earth and the same reasoning holds for all the points of the body, hence the conclusion.[91]

— a liquid body in air without any constraint is heavier when it is closer to the centre of the Earth; the demonstration relies on the interpretation of an observation through considerations on centers of gravity. The observation is that a liquid body in a vessel arches less when it is farther from the centre of the Earth,which has for consequence that its centre of gravity is lower in the vessel.[92] Now, this lowering of the centre of gravity being less and less when the vessel goes up, the power necessary to raise the vessel

---

[88] This distinction corresponds to the first criticism addressed to Beaugrand (To Mersenne, 29 juin 1638, in A.T. II pp. 184-185).

[89] The distinction between solid and liquid bodies, which does not appear in the other writings on geostatics, is fundamental in Descartes' physics. Suffice it to say here that in *Le Monde*, Descartes notes: "la différence qui est entre les corps durs et ceux qui sont liquides, est la première que je désire que vous remarquiez" (chap. 3, in A.T. VI p. 12). He introduces right away the distinction between liquid and hard bodies in his physics as well as in his statics: to give a mechanical explanation of these qualities does not mean to get rid of them. My (somewhat paradoxal) feeling is that, besides his famous identification of matter with motion, he had developped a great sensibility to concrete differences between materials.

[90] This assimilation may remind of the Galilean assimilation of a body sustained by a circularly inclined plane and of a body suspended from the centre of a lever. What makes the difference however (and the physically absurd character of Descartes' suggestion) is that, contrary to the Galilean body, the Cartesian body is not suspended from any fixed part; consequently, there is no equivalent to the constraint represented by the inclined plane.

[91] To Mersenne, 11 July 1638, in A.T. II pp. 238-239.

[92] According to the editors, T.W.B. pp. 372-373, this observation goes back to the Middle Ages and has been summarized in the unpublished continuation of Mersenne's *Quaestiones in Genesim*.

when it is nearer to the centre of the Earth is greater than the power necessary to raise the vessel when it is farther from to the centre of the Earth. Hence the conclusion.[93]

— a body in a scale is heavier when it is closer to the centre of the Earth; the demonstration consists in comparing the relative heaviness of two equal bodies in a symmetrical scale inclined with respect to the horizon, the convergence of verticals being once again taken into account. I shall not examine it here.[94] From this result, Descartes draws the important and paradoxical conclusion that the centre of gravity of a body in the sense of Pappus or Commandino is not always at the same point, or that there is no unique centre of gravity.[95] Namely, if the body which is closer to the centre of the Earth has a greater relative heaviness, the centre of gravity will not be in the middle of the scale, and it will change if you modify the inclination of the scale or its distance with regard to the centre of the Earth.[96] Taking the example of the sphere, Descartes extends this reasoning to any body, which he divides into two sets of parts counterbalanced one by the other, as if in a scale.[97]

3. The idea of mechanics and the practice of mechanics

We have explored two different worlds, which both answer to the name of "mechanics" according to Descartes: on the one hand, the general idea of mechanics, which amounts to the programmatic identity of physics and mechanics; on the other hand, a sample of a certain practice of mechanics, in many

---

[93] *Ibid*., pp. 240-241.

[94] On this point, see Costabel, "La démonstration cartésienne relative au centre d'équilibre de la balance", pp. 95-96.

[95] Descartes had already briefly noted this point in the letter to Huygens, 5 October 1637, in A.T. I pp. 446-447. He claimed to be the first to do so, but it had already been the case of Stevin (see above, note 44), and, in the context of the polemic raised by Beaugrand, of Castelli (see above, note 45) and Fermat (*Nova in mechanicis theoremata*, § 6, in T.H. II pp. 25-26).

[96] On this demonstration, see Costabel, "La démonstration cartésienne relative au centre d'équilibre de la balance", pp. 96-98 and "Centre de gravité et équivalence dynamique", pp. 11-13. Costabel points out that there is a contradiction in the determination of the new centre of gravity suggested here, because it relies on the supposition that the old centre of gravity still holds; curiously enough, Descartes had pointed out a similar contradiction in the criticism he made of Beaugrand's book (To Mersenne, 29 Juny 1638, in A.T. II pp. 185-186).

[97] In the very last paragraph of the letter, Descartes suggested a way to determinate the position of the centre of gravity of a sphere, which he latter on declared false (To Mersenne, 15 November 1638, in A.T. II pp. 451-452). As I shall report below in 3.1., Duhem infers from this conclusion that Descartes' contribution to the history of mechanics has been to clarify the notion of centre of gravity and to show that it is not compatible with the convergence of verticals. It is certain that Descartes insisted that the convergence of verticals leads to the paradoxal conclusion that the usual notion of centre of gravity does not hold; however, he does not pronounce on what we should conclude from this paradox. Does it mean that we should confine ourselves to practical procedures when we want to determine the center of gravity of a body (e.g. suspending the body and considering that the center of gravity is the intersection of the verticals thus obtained)? That should to neglect the convergence of verticals? That we should forge another mathematical notion of center of gravity? Or that we should stop altogether to pretend that there is such a thing as a *science* of weights?

respects close to the old science of weights. One can not help asking if there is according to Descartes any relationship between these two worlds. Did the practice of mathematical mechanics help the maturation of the general idea of mechanics? Inversely, did this general idea influence the practice of mechanics ?

As should be clear from what has been already written, the answer to the first question is simply: no. The answer to the second question is somewhat more complicated. One can not pretend that Descartes considered that his general idea of mechanics makes mechanics as a separate discipline obsolete. Namely, such a treatise of mechanics would have been necessary to complete the whole body of philosophy he was announcing in the preface of the French edition of the *Principes de la philosophie*.[98] If the whole body of philosophy is like a tree, the trunk being physics, and the branches being morals, medicine and mechanics, there is indeed some kind of organic continuity between physics and mechanics, but they remain quite distinct — in other words, a separate treatise of mechanics should have been written.

However, Descartes never considered that the letters he wrote on specific questions of mechanics offer sufficient coherence to constitute such treatises. When Huygens asked his permission for showing the *Explication des engins à l'aide desquels on peut avec une petite force lever un fardeau fort pesant*, or Mersenne, for publishing the criticism of Beaugrand's book and the examination of the geostatical question, Descartes' answer was more or less always the same: you can show them and even publish them, but they should not be presented as autonomous books, because they are neither complete nor perfect.[99] So, what about this missing treatise on mechanics? Why did Descartes never work on such a treatise as he worked on, for example, the *Description du corps humain*? [100]

---

[98] *Principes de la Philosophie*, Préface, in A.T. IX p. 14 and p. 17. On the relationship between this statement and the claim that physics and mechanics are identical, see Gabbey, "Descartes' Physics and Descartes Mechanics: Chicken and Egg", p. 315, pp. 320-321.

[99] Descartes authorized Huygens to show the *Explication des engins…* in his letter to Huygens, 4 December 1637 [wrongly given by A.T. as a letter from January, 25, 1638], in A.T. I p. 507. See also To Pollot, 12 February 1638, in A.T. I p. 518-519. To Huygens, [March 1638], in A.T. II p. 51. It has finally been published by the oratorian Nicolas Poisson in 1668. Consistent with his wish to preserve his anonymity and with the current norms of "honnêteté", Descartes let Mersenne publish the letter of June 1638 criticizing Beaugrand on the condition that his own name and certain insulting words concerning Beaugrand should be omitted (To Mersenne, 27 July 1638, in A.T. II p. 271-272). As for the *Examen de la question géostatique*, Descartes noted that it is not "assez complet ni achevé pour aller seul" and suggested its publication in a collection of objections "car aussi bien ne sera-ce qu'un ramas de toutes sortes de matières" (To Mersenne, 11 October 1638, in A.T. II p. 392). This collection was never realized, but in 1644 Mersenne published some extracts of the *Examen* in his *Cogitata physico-mathematica*, after having once again asked for Descartes' authorization (To Mersenne, 3 February 1643).

[100] Garber, "A Different Descartes: Descartes and the Program of a Mathematical Physics in the *Correspondence*", pp. 207-208, p. 211, focuses on the question of why Descartes has not published anything on mechanics. There can be many reasons

A first answer comes easily to mind: Descartes had other preoccupations, which he judged more urgent to answer than the "useless" geostatical question.[101] In the late thirties, he began to think that the most important thing to do with his time and at his age was to improve medicine. In any case, that's what he explained to Huygens who requested him many times to write a full treatise on mechanics.[102] But there are other answers as well: in order to develop them, I shall first say a few words about earlier interpretations of the letter of July 1638 and more generally of Cartesian letters on statics. Then I shall characterise the Cartesian practice of mechanics with respect to the exclusion of speed and to the problem of heaviness. This will lead me to a new appreciation of the conflict between the idea of mechanics and the practice of mechanics.

3.1. Historiography

Compared to other pieces of work from Descartes, the letter of July 1638 has not given rise to a considerable body of literature. Duhem was the first to give a general interpretation of it; he related it to the broader history of the notion of centre of gravity.[103] According to him, two contradictory definitions had been brought together by Benedetti and Guidobaldo — on the one hand, the phenomenological definition of centre of gravity from Pappus: it is the point of the body so that, if the body was suspended from this point, it would rest, or come back to its position if moved; on the other hand, the cosmological definition of Albert of Saxony: it is the point of the body which tends towards the centre of Earth and which would rest if united to it.[104] The contradiction lies in the fact that Pappus' definition does not suppose the convergence of verticals, whereas Albert does; the geostatical controversy would be the historical moment where this contradiction bursts out. Showing by a *reductio ad absurdum* that the notion of centre of gravity implies that verticals are considered as parallels, Descartes would have solved the crisis, made obvious a latent truth and clarified definitively the notion of centre of gravity.[105] As always with the continuist Duhem, the differences are blurred between the old and the new, or between a dim anticipation and a distinctly elaborated notion. Following in Duhem's footsteps, Costabel devoted

---

for not publishing (the results may be already known, the subject, too narrow, the question, useless, the readership, not prepared, the market, not developped) and they are not the same as the reasons for not working on a subject.

[101] "cette question de nul usage" (To Mersenne, 12 September 1638, in A.T. II p. 358).

[102] "Les poils blancs qui se hâtent de me venir m'avertissent que je ne dois plus étudier à autre chose qu'aux moyens de les retarder. C'est maintenant à quoi je m'occupe" (To Huygens, 5 October 1637, in A.T. I pp. 434-435). "Je travaille maintenant à composer un abrégé de médecine" (To Huygens, 4 December 1637 [given by A.T. as 25 January 1638], in A.T. I p. 506-507).

[103] This history of the notion of centre of gravity is itself included in a history, which is the core of *Les Origines de la statique*, vol. II, namely the history of Torricelli's principle, according to which two heavy bodies linked together can not move unless their common centre of gravity goes down.

[104] *Les Origines de la statique*, II, chap. 15, §§ 2-3 et § 7, *resp*. pp. 6-31 et pp. 99-104.

[105] *ibid*., II, chap. 16, § 2, pp. 156-185.

three papers to the geostatical question.[106] He is obviously much more anxious than Duhem to mark out historical differences and nuances; however, he adopts Duhem's general perspective: he examines the geostatical question in a *longue durée* history, exclusively written in the light of the modern notion of centre of gravity.[107] Thus, Duhem and Costabel cut off from their context propositions which they see as contributions to the emergence of a modern notion.[108] They skip over all what concerns so to speak the mere geostatical question, and retain only what can be ordered on the axis of a cumulative progress.[109]

This retroactive way of reading may be fruitful in certain cases, but here it makes one forget the fact that the letter of July 1638 is a knot of dead ends and contradictions. First, there is obviously a gap between the maniacal exactitude of the mathematical answers and the physical answer based upon observations, which are not only qualitative, but trivial, imprecise, and restricted to what happens in our air[110]. Moreover, the very division between these two types of answers reminds of the Aristotelian difference between physics and mechanics as a mixed science, difference that the Cartesian idea of mechanics was supposed to have abolished. Second, these two answers seem in contradiction with each other: according to the mathematical answer, hard bodies in air without any constraint are less heavy when they are closer to the centre of the Earth; according to the physical account, this is however not the case of planets, large birds, snow and kites, which on the contrary illustrate the proposition that a body is heavier when it is closer to the centre of the Earth than when it is farther.[111] Last, but not least, the

[106] "Centre de gravité et équivalence dynamique", is a broad presentation of the history of the notion of centre of gravity from Pappus to d'Alembert; "La démonstration cartésienne relative au centre d'équilibre de la balance" is about its treatment by Descartes; "Les enseignements d'une notion controversée : Le centre de gravité", about its treatment by Fermat.

[107] For nuances, see for example "Centre de gravité et équivalence dynamique", p. 13. "La démonstration cartésienne relative au centre d'équilibre de la balance", p. 98. The Duhemian inspiration is especially clear in "Centre de gravité et équivalence dynamique", which, after an ahistorical presentation of the modern notion, exposes the problem by reference to Pappus, Albert of Saxony, Guidobaldo and Benedetti, just as Duhem did.

[108] Consequently, blame and praise are constantly distributed; see for example Costabel: "Il faut reconnaître à Descartes le mérite de l'avoir compris" ("Centre de gravité et équivalence dynamique", p. 10). "On ne peut qu'admirer ces précautions d'un esprit épris de rigueur", "On ne saurait exiger plus de clarté", "Une méthodologie dont la valeur mérite d'être reconnue" ("La démonstration cartésienne relative au centre d'équilibre de la balance", resp. p. 90, p. 94, p. 100). "Le mérite d'avoir bien compris cette conclusion désastreuse revient à Fermat" ("Les enseignements d'une notion controversée : Le centre de gravité", p. 117).

[109] In the case of Descartes, Costabel does not say a word about the physical answer; in the mathematical answer, he concentrates exclusively on the explanation of the simple machines and on the proposition that there is no centre of gravity. In the case of Fermat, he ignores that most of Fermat's writings on mechanics in 1636 are devoted to something which does not make any sense in a modern perspective, namely a "lever" where weights act towards the fulcrum.

[110] To Mersenne, 11 July 1638, in A.T. II p. 225.

[111] Carla Rita Palmerino suggested that this contradiction may be a kind of *reductio ad absurdum*, by which Descartes would have shown that the assumption of an absolute heaviness is false. It is a nice interpretation, but I do not think it fits with

mathematical answer being founded upon a supposition on heaviness which is false according to Descartes, one may wonder if, and how, it could ever lead to something true.[112]

Therefore, one should find a way of reading this letter, as well as other letters on statics, which would take these contradictions into account. In two recent papers, Gabbey and Garber suggested that they result from the tension between the requirements of the practice of mechanics and what may be found in other parts of Cartesian works, namely what I called the new idea of mechanics: both of them interpret this letter in relation, not with the posterior development of science, but with Descartes' other works.[113] Gabbey's purpose is to explain why Descartes does not mention his principle of statics in the *Principia philosophiae*, and more generally to elucidate the relationship between mechanics and physics in Descartes works. To put it bluntly, his answer is that this principle is not for Descartes a law of nature, because it involves a complex and obscure notion, namely the one of gravity.[114] As for Garber, he considers the letter of July 1638 as evidence that Descartes, as other correspondents of Mersenne, knew what he calls the "Galilean paradigm".[115] To the consequent questions of why Descartes has not worked in the Galilean paradigm and of why he has not published any treatise on mechanics, Garber's answer is that he could not: mechanics as well as the Galilean paradigm rely on a conception of gravity that was contrary to the Cartesian natural philosophy, and Descartes could not elaborate on his own notion of gravity because of lack of appropriate experiments to evaluate the speed of the subtle matter.[116] In other words, Gabbey's and Garber's horizon of interpretation is constituted by Descartes' complete works,

---

Descartes' scientific psychology: usually, he is very explicit about what he wants to prove and he prefers direct demonstrations, which go from the obvious to the obvious, to *reductio ad absurdum*.

[112] Descartes compares his way of proceeding to the supposition of the equality of median, which make calculations easy for astronomers; such a comparison is not totally convincing: the astronomers make a mean between different measures; Descartes assumes in his letter to Mersenne that a local and conventional definition of heaviness can be extended everywhere, whereas his own notion stands in the way of this extension.

[113] Gabbey, "Descartes' Physics and Descartes' Mechanics". Garber, "A Different Descartes: Descartes and the Program of a Mathematical Physics in the *Correspondence*".

[114] I have argued above, note 71, that, although the principle of statics involves the ontologically obscure notion of gravity, this is not a principle known by experiment according to Descartes.

[115] Garber, "A Different Descartes: Descartes and the Program of a Mathematical Physics in the *Correspondence*", pp. 196-198, characterized the Galilean paradigm by the combination of mathematics and physics for describing the behaviour of heavy bodies. I have at least two objections to such a paradigm: it is much too gross to grasp the singularity of Galilean science, and would include as well all the mechanicians of the sixteenth century; the very recepion of Galileo shows that there is no established paradigm, but a complex tradition with different trends.

[116] *ibid.*, pp. 214-216. I do not see to what kind of workable experiments Garber is thinking of, and I show in 3.3. the many *conceptual* difficulties, that Descartes encountered about the notion of heaviness.

supposed to have such a coherence that any element can be related to another.[117] Both of them argue that the notion of heaviness used in the science of weights was false according to *Le Monde* and the *Principia philosophiae* , that the ideal would have been to found a new statics on a true notion of heaviness, but that it turns out that it was impossible, either for more conceptual reasons (Gabbey) or for more experimental ones (Garber). In sum, while Duhem and Costabel ignored the mechanical philosophy, Gabbey and Garber made it responsible for the difficulties Descartes encountered to develop a complete mechanics. In the very last paragraph, I shall qualify this interpretation; beforehand however I shall begin by examining the most obvious characteristic of Cartesian mechanics, namely the exclusion of speed.

3.2. The exclusion of speed

Through Mersenne's pen more than one objected that Descartes should have taken speed rather than space as the fundamental quantity; confronted to this objection, Descartes boasted that his cleverest point has been precisely to exclude speed, and that those who take it in consideration are totally mistaken.[118] It is interesting to have a glimpse at what these stray souls were actually doing. Indeed, with a few exceptions, it is a common trend of mechanicians of the time to formulate principles of compensation (or of equivalence between ratios) indifferently in terms of speed, space or time.[119] However, their operative notions are not times and speeds, but displacements — which is especially clear when it is only in a corollary that there is an inference from the proportionality between weights and spaces to the proportionality between weights and times.[120] The case of Roberval, whom Descartes accuses by name of having wrongly confused times and spaces, is noteworthy: except in one corollary, he never speaks of time, so that Descartes should have gone through his treatise really in detail, page after page, to find the

[117] In that respect, Gabbey's rhetoric is striking: he begins by picking up "anomalies", "intriguing remarks" and "inconsistencies" (pp. 311-314); next he "disentangles" them (pp. 315-320); last he "concludes" that they are only apparent (p. 320).

[118] "Si j'ai témoigné un tant soit peu d'adresse en quelque partie de ce petit traité de statique, je veux bien qu'on sache que c'est plus en cela seul qu'en tout le reste" (To Mersenne, 12 September 1638, in A.T. II p. 355). Roberval "parle du temps, ou de la vitesse, au lieu que je parle de l'espace ; ce qui est une très grande erreur" (To Mersenne, 11 October 1638, in A.T. II p. 391). "Pour ceux qui disent que je devais considérer la vitesse, comme Galilée, plutôt que l'espace, pour rendre raison des machines, je crois, entre nous, que ce sont des gens qui n'en parlent que par fantaisie, sans entendre rien en cette matière" (To Mersenne, 15 November 1638, in A.T. II p. 433). See also the quotations given below, note 124.

[119] This is by the way perfectly legitimate in a simple machine like the lever: because both ends are linked, the time, which one end needs to go up, is the same as the time, which the other end needs to go down, so that it is indifferent to speak of spaces or of speeds.

[120] See for example Guidobaldo, *Mechanicorum liber*, "De trochlea", prop. 28, cor. 2, in Drake and Drabkin, p. 317. Hérigone, *Cours de mathématique*, prop. 2, corollaire, p. 291. Mersenne-Galileo, *Les Méchaniques de Galilée*, chap. 1, p. 439-441.

faulty place.[121] It follows therefrom that, even if Descartes once stated that the most beautiful thing in mechanics is related to speed,[122] he wanted to eradicate it totally from his mechanics. The consequent question is why.

A common answer is that, being too much of a geometrical mind, Descartes reduced motion to space and excluded speed because such a notion would be incompatible with mathematical intelligibility or with geometrical representation.[123] This answer seems inadapted here for at least two reasons. First, if an abstruse geometrician is able to reduce motion to space, he does need anymore to exclude speed, because it is, by this very reduction, a geometrical magnitude. Second, Descartes does not exclude speed without qualification, since it is the object of his laws of motion, which just explain how the speed of a body can be conserved or modified by its meeting with other bodies. In short, before imagining reasons, which Descartes may have had for excluding speed from his statics, we should examine the reasons he explicitely gave for doing so — as far as I know, there are three.

First, he points out that speed gives only the *quod ita sit*, and not the *cur ita sit* as does space: a twice as great force does not necessarly generates a twice-as-great-speed, even if it sometimes happens by accident.[124] The principle, which is here put forward, is classical in Aristotelian logic: from false premisses it is possible to infer a true conclusion, but it will bear only on the oJvti, not on the diovti.[125] To understand better the Cartesian point, one should moreover add that certain mechanicians, for example Mersenne, effectively present the proportionality of weights and speeds as the cause of the proportionality of weights and spaces, because only a variation in speed can be the cause of a variation in

---

[121] Roberval, *Traité des mécaniques*, corollaire 5, p. 11. The inference from spaces to times is explicitely justified by the proposition that "le temps est en raison des chemins" (*ibid.*, p. 12).

[122] "J'ai omis le plus beau de mon sujet, comme en autres la considération de la vitesse, les difficultés de la balance, et plusieurs moyens qu'on peut avoir pour augmenter la force des mouvements qui diffèrent de ceux que j'ai expliqués (To Huygens, 4 December 1637, in A.T. I p. 648).

[123] See for example Koyré, *Etudes galiléennes*, p. 131, p. 331-332, p. 341. Séris, *Machine et communication*, pp. 216-218.

[124] Galilée "explique fort bien *quod ita sit*, mais non *cur ita sit*, comme je fais par mon principe" (To Mersenne, 15 November 1638, in A.T. II pp. 433-434). "Je reprends ceux qui se servent de la vitesse pour expliquer la force du levier (…) pour ce que cette vitesse ne comprend pas la raison pour laquelle la force augmente ou diminue, comme fait la quantité de l'espace" (To Mersenne, 2 February 1643, in A.T. III p. 614). "Non nego quin *materialiter* verum sit, quod mechanicis dici solet, nempe longiorem partem in vecte tanto velocius moveri quam alteram, quanto minori vi indiget, ut moveatur ; sed nego celeritatem aut tarditatem rei causam esse" ([To Boswell, 1646 ?], in A.T. IV p. 685. See also [To Boswell, 1646 ?], in A.T. IV p. 696). On the problems of datation raised by these two last letters, see below, .

[125] *First Analytics*, II 2, 53 b5 sqq. and II 4, 57 a38 sqq. This principle has been often commented by astronoms, in contexts where the Ptolemaic and Copernican hypothesis were confronted.

force (and vice-versa).[126] So, why is Descartes considering that a proposition involving speed constitutes a false premisse and may not deliver the true cause of a statical proposition? Nothing more explicit being said by him on this question, one is reduced to conjectures.[127] The first conjecture relies on the fact that, even if it is possible to substitute speeds for spaces in simple machines, where by mechanical construction the power and the weight move in the same time, this is not the case in general: Descartes' point would be thus that propositions involving speed can not pretend to general validity. The problem with this conjecture is that I have not found any text to support it. The second conjecture is that Descartes is of the opinion that virtual motions can not be the true cause of an equilibrium, that is of an absence of motion. This conjecture is more probable than the first one, insofar as the argument is attested by Stevin and by other Archimedean adversaries of the Aristotelian tradition.[128] To conclude on this argument, it should be noted that the editors of Mersenne' correspondence date the fragments where it appears as early as 1630, and that they most probably have been written between 1635 and the 1637.[129] I have no competence on these philological matters, but if it is true, it means that the first motive of

---

[126] Mersenne, *Traité de l'harmonie universelle*, livre II, théorème 10, p. 395, p. 399, p. 404. See also the texts quoted by Duhem, *Les Origines de la statique*, appendice 1, pp. 291-296. In that sense, Duhem may be right to argue that Descartes' target was the Aristotelian axiom that speeds are proportionnal to motive actions. He is however overinterpreting when he asserts that, seeing that there is no simple ratio between force and speed, Descartes' desire would have been to make statics autonomous from the false Aristotelian dynamics, waiting for the time where the true dynamics would have been elaborated (*ibid*; I, chap. 14, pp. 342-348) — not to speak with the contrast he makes between the visionary Cartesian restriction and the dullness of Galileo, who would have got stuck in the Aristotelian axiom and consequently would not deserve the honour of being called the father of a new dynamics (*ibid*., I, chap. 11, pp. 247-248, p. 253, p. 255, pp. 260-261, *passim*).

[127] It is interesting to note that the meaning of this argument was already unclear for the first editor and commentator of Descartes' mechanics, the oratorian Nicolas Poisson. Not only he affirms that there is no difference between considering speeds and displacements (*Traité de la Mécanique, composé par Monsieur Descartes*, p. 22), but he criticizes Descartes for having laught at Galileo: "On pourrait lui répondre [to Descartes]: *Quid rides ? Mutato nomine de te fabula narratur*" (*ibid*., p. 45)

[128] Before Descartes, this argument against Aristotle has been set out by Benedetti, *Diversarum speculationum mathematicarum, et physicarum liber*, cap. 11, in Drake and Drabkin, pp. 180-181 and by Baldi, *In mechanica Aristotelis problemata exercitationes*, p. 36. Stevin's formulation is especially clear-cut: "Là où il n'y a pas de circonférence, elle ne sera pas cause de ce qui advient, ainsi donc la circonférence ne sera pas la cause de l'équilibration. (…) Le mouvement et la description des circonférences n'advient là que par accident" (*La Statique*, "Appendice de la statique", chap. I, p. 501). In this context, it is interesting to note that Stevin mentions, at the beginning of his explanation of pulleys, a general rule, apparently common at the time, which does not involve time, but which is too general to be a workable principle: "On tient pour règle générale en mathématique, que *Comme l'espace de l'agent, à l'espace du patient : Ainsi la puissance du patient, à la puissance de l'agent*" (*ibid*., "Des poulies ou de la trochléostatique", proposition I, p. 509). After Descartes, the anti-Aristotelian argument appears in Lamy, *Traités de mécanique*, p. 74.

[129] A.T. IV pp. 814-818

Descartes for excluding speed has not been his idea of mechanics and consequent system of the world, but his dependence on the Archimedean tradition.

The second line of argument for excluding speed is quite different. Descartes asserts that there is an exact proportionality between height and force, in the sense that a force twice as great raises a body twice as high, but that this twice-as-great force does not raise the body exactly twice as fast. Namely, he explains, variations of speed depend on what resists speed, in particular on the medium in which the body is raised.[130] It is interesting to note that, for Stevin also, the various resistances that a body encounters constitute a good reason for excluding speed from statics, because there would be no ratio between motion and what impedes this motion.[131] But this common line of thought does not explain everything, if anything: one does not see for example why the resistance of the air would impede the speed of the body, but not its spatial displacement. Here Descartes is somewhat explicit, at least in the late thirties and forties.[132] To put an end to the reiterated questions of Mersenne, he finally gives an example:

> "Prenant un éventail en votre main, vous le pourrez hausser ou baisser, de la même vitesse qu'il pourrait descendre de soi-même dans l'air, si vous le laissiez tomber, sans qu'il vous y faille employer aucune force, excepté celle qu'il faut pour le soutenir; mais pour le hausser

---

[130] Pour lever un corps deux fois plus vite, "il n'y faut point employer une force qui soit justement double (…), mais (…) une qui soit plus ou moins grande que la double, selon la diverse proportion que peut avoir cette vitesse avec les causes qui lui résistent" (To Mersenne, 12 September 1638, in A.T. II p. 354). "Bien qu'il soit évident qu'il faut plus de force, pour lever un corps fort vite, que pour le lever fort lentement, c'est toutefois une pure imagination de dire que la force doit être justement double pour doubler la vitesse" (To Mersenne, 15 November 1638, in A.T. II pp. 433-434). "Je reprends ceux qui se servent de la vitesse pour expliquer la force du levier (…) pour ce (…) qu'il y a plusieurs autres choses à considérer touchant la vitesse, qui ne sont pas aisées à expliquer" (To Mersenne, 2 February 1643, in A.T. III p. 614). "(…) imo etiam addo celeritatem, qua per accidens reperitur, nonnihil de veritate hujus calculi minuere. Nam, exempli causa in vecte ABC, posita parte AB centum partium, qualium BC est una, et existente pondere in C centum librarum, si absque ita celeritate esset, hoc pondus centum librarum in C attolleret pondus unius librae in A; sed, propter ipsam, debebit pondus in A esse paulo levius" ([To Boswell, 1646 ?], in A.T. IV p. 685. See also To Boswell, 1646 ?], in A.T. IV p. 694-696).

[131] "La statique enseigne seulement à mettre en équilibre le mouvant avec l'ému. Et (…) touchant la pesanteur ou la puissance, que le mouvant a besoin d'avoir encore davantage, pour faire que l'ému se puisse mouvoir (…), la statique ne montre pas la manière de trouver telle pesanteur ou puissance mathématiquement, pour ce que l'ému et ses empêchements n'ont aucune proportion avec un autre ému et ses empêchements" (*La Statique*, livre III, "Au lecteur", p. 469).

[132] His earlier position on the resistance of the air is somewhat different: "Pour le *quantum* [de l'empêchement de l'air par les mouvements], je l'ignore, et encore qu'il se pût faire mille expériences pour le trouver à plus près, toutefois, pour ce qu'elles ne se peuvent justifier par raison, au moins que je puis atteindre, je ne crois pas qu'on doive prendre la peine de les faire" (To Mersenne, 18 December 1629, in A.T. I pp. 99-100).

> ou baisser deux fois plus vite, il vous y faudra employer quelque force qui sera plus que double de l'autre, puisqu'elle était nulle".[133]

Other texts may clarify what Descartes had in mind, besides the common experience of the resistance of the air, especially striking in the case of a fan. A given body in a given medium has, according to him, a "natural speed": it is the speed with which this body moves when he does not receive any supplementary force.[134] Now, this natural speed depends on two factors: the form of the body, and the speed of the small parts of the medium.[135] The first factor is obvious by common experience; the second factor depends on the Cartesian theory of liquidity and hardness. According to this theory, a body is more or less liquid, depending on whether the speed of its parts is quicker or slower; a liquid medium in respect with a certain body which moves in it, is consequently a medium, the parts of which move quicker than this body: if its parts move slower, then the medium resists, and it is not anymore liquid with respect to the body.[136] In our case, it means that the variation of the speed of a body as a function of the resistance of the air depends on the ratio between the speed of this body and the speed of the parts of the air. One understands that Descartes had preferred to avoid speed in these conditions: not only it is difficult to measure the speed of the parts of the air which make it fluid, but, even if it would have been known, to calculate the resulting speed of a body at a given time would have been a mathematically difficult task.

The third argument that Descartes put forward is that the speed of a heavy body depends on its heaviness: the variations of heaviness being, as we shall see, too complicated to measure or to evaluate, it

---

[133] To Mersenne, 2 février 1643, in A.T. III p. 614.

[134] "Encore qu'il n'y ait aucun mouvement qui n'ait quelque vitesse, toutefois il n'y a que les augmentations ou diminutions de cette vitesse qui sont considérables, et lorsque, parlant du mouvement d'un corps, on suppose qu'il se fait selon la vitesse qui lui est plus naturelle, c'est le même que si on ne la considérait pas du tout" (To Mersenne, 12 September 1638, in A.T. II p. 355).

[135] The more complet text on this question, although written in the somewhat different context of the controversy on centers of oscillation, is the letter To Mersenne, 7 September 1646, in A.T. IV p. 500: "Je n'entends pas celui [l'empêchement de l'air] qui dépend de la figure des corps qui se meuvent (…), mais j'entends principalement celui qui vient de ce que, l'air n'étant pas parfaitement fluide, quand un corps est supendu en l'air en équilibre, il faut le pousser avec plus de force, pour le mouvoir fort vite, que pour ne le faire mouvoir que lentement". See also, in the same context, To Mersenne, 30 March 1646, in A.T. IV, pp. 385-386; 20 April 1646, in A.T IV, pp. 391-392; 15 May 1646, in A.T. IV p. 417; 2 November 1646, in A.T. IV p. 547.

[136] *Principia Philosophiae* II 56-57, in A.T. VIII-1, pp. 71-75. For a general presentation of the Cartesian concepts implied here, see Roux, *La Philosophie mécanique*, p. 408-433.

is almost impossible to say anything certain about the speed of a falling body.[137] The problem of heaviness is well documented: one knows that Descartes obstinately refused Galileo's law of falling bodies because of his own conception of heaviness.[138] It is however interesting in our context to look more closely at this problem.

3.3. The problem of heaviness

We have seen that the mathematical part of the letter to Mersenne of July 1638 was threatened of being meaningless, because it relies on a hypothesis which is false from a physical point of view, namely that each body retains in itself the same heaviness in whatever circumstances. One may now wonder why Descartes never replaced this doubtful hypothesis by a sound view about heaviness, which would have opened the way to a meaningful demonstration. In the first part of this paper, I recalled incidentally that, according to Descartes, heaviness is not a principle inherent to natural bodies, but a secondary quality, which may be explained by certain rules of motion or laws of nature: in the early thirties, he convinced himself that it may be explained as an effect of the subtle matter circulating around the Earth. When he wrote the *Examen géostatique*, he was reluctant to make this explanation public, because it would have forced him to explain his whole system of the world. Yet, even if he had done so, he would have been

---

[137] "Sans cela [giving the *Monde* to the world] je ne saurais aussi achever les mécaniques dont vous m'écrivez, car elles en dépendent entièrement, principalement en ce qui concerne la vitesse des mouvements" (To Huygens, March 1638, in A. T. II p. 50). "Il est impossible de rien dire de bon et de solide touchant la vitesse sans avoir expliqué au vrai ce qu'est la pesanteur, et ensemble tout le système du monde. Or à cause que je ne le voulais pas entreprendre, j'ai trouvé moyen d'omettre cette considération, et d'en séparer tellement les autres que je les pusse expliquer sans elle" (To Mersenne, 12 September 1638, in A.T. II p. 355). "Pour la pesanteur, je n'imagine rien autre chose, sinon que toute la matière subtile qui est depuis ici jusques à la lune, chasse vers elle tous les corps qui ne se peuvent mouvoir si vite. Or elle les chasse avec plus de force, lorsqu'ils n'ont point encore commencé de descendre, que lorsqu'ils descendent déjà ; car enfin, s'il arrive qu'ils descendent aussi vite qu'elle se meut, elle ne les poussera plus du tout, et s'ils descendent plus vite, elle leur résistera. D'où vous pouvez voir qu'il y a beaucoup de choses à considérer, avant qu'on puisse rien déterminer touchant la vitesse et c'est aussi ce qui m'en a toujours détourné" (To Debeaune, 30 April 1639, in A.T. II p. 544). This justified retrospectively the somewhat cynical remark of Descartes at the beginning of this letter: "encore que toute ma physique ne soit autre chose que mécanique, toutefois je n'ai jamais examiné particulièrement les questions qui dépendent des mesures de la vitesse" (*ibid*., p. 242).

[138] Galileo is explicitely criticized in the following letters. To Mersenne, 30 August 1637, in A.T. I p. 392. To Mersenne, 11 October 1638, in A.T. II p. 385. To Mersenne, 29 January 1640, in A.T. III pp. 9-11, *passim*. On this criticism, see for example Koyré, *Etudes galiléennes*, pp. 131-134. For a rich and detailed account of the reception of Galileo's law of fall not only in Descartes, but in the French community, see Palmerino, "Infinite Degrees of Speed. Marin Mersenne and the debate over Galileo's law of free fall".

doomed to silence as well, and perhaps even more. Namely, as I shall argue in the following, the Cartesian view about heaviness was not appropriate for a law linking speed and heaviness.[139]

So how does Descartes explain heaviness? According to *Le Monde* and *Principia philosophiae*, the main mechanical model of heaviness is based on three points: 1. the existence of a centrifugal conatus in bodies circularly moved; 2. the impossibility of void, which implies that a body can not move except if another body takes his place; 3. the evaluation of the centrifugal conatus of a body by the quantity of subtle matter, which it contains.[140] Strictly speaking, this model does not imply a transfer of motion from the subtle matter to the body through discrete successive impacts. However, whenever Descartes has to justify his model, in particular when he wants to explain that a falling body does not pass through infinite degrees of speeds, he fells back on the model of impact and says that the heavy body is pushed by successive impacts of subtle matter, just as it would be pushed by successive impacts of smaller bodies.[141]

Now, whether a physical law is assumed to be a universal principle valid for all bodies or a mathematical ratio between magnitudes, or both, such an impact model of heaviness invalidates any law linking heaviness and speed. Namely, in order to determinate the heaviness of a body and its consequent effects, one should first be able to answer three questions. First, what is the speed and the size of an average part of subtle matter? For Descartes, this question belongs to the world of mere facts, totally obscure to reason.[142] Second, what are the effects of successive impacts of subtle matter, considering that each impact modifies the speed of the pushed body? Descartes' point here, grounded in the law of nature that a natural power acts more or less on the subject according to its disposition, is grossly that a decrease in the difference between the respective speed of the pushing and the pushed bodies implies a decrease in the effect of impact, so that at a certain point, the body does not accelerate anymore.[143] But the

---

[139] The remarks addressed by Carla Rita Palmerino to a former version of this paper, as well as a reading of her paper "Infinite Degrees of Speed. Marin Mersenne and the debate over Galileo's law of free fall", helped me greatly to clarify the following points.

[140] *Le Monde*, chap. 11, in A.T. XI pp. 74-76. *Principia philosophiae*, IV 23-25, in A.T. VIII-1 pp. 213-215. Another explanation is suggested in the *Principia*, but immediately forgotten; on these two explanations, see Roux, *La Philosophie mécanique*, esp. pp. 534-544.

[141] See for example to Mersenne, 29 January 1640, in A.T. III pp. 9-10. To Mersenne, 11 March 1640, in A.T. III pp. 37-38. To Mersenne, November 1640, in A.T. III p. 593.

[142] "Je ne puis déterminer la vitesse dont chaque corps descend au commencement, car c'est une question purement de fait, et cela dépend de la vitesse de la matière subtile" (To Mersenne, 11 March 1640, in A.T. III p. 36).

[143] Supposer "que la force qui faisait mouvoir cette pierre, agissait toujours également", cela "répugne apertement aux lois de la nature: car toutes les puissances naturelles agisssent plus ou moins, selon que le sujet est plus ou moins disposé à recevoir leur action ; et il est certain qu'une pierre n'est pas également disposée à recevoir un nouveau mouvement ou une nouvelle augmentation de vitesse, lorsqu'elle se meut déjà fort vite et lorsqu'elle se meut fort lentement" (To Mersenne, October-November 1631, in A.T. I p. 230). See also, to Mersenne, 15 November 1638, in A.T. II p. 443. To Debeaune, 30 April 1639,

quantitative evaluation of this decrease, as well as the time when there is no acceleration anymore, stay unknown. Last, but not least, what are the effects of the impacts of subtle matter on bodies according to the various quantities of subtle matter, that they contain? Descartes asserts in 1632 that the speeds of two leaden balls, weighing one pound the first and hundred pounds the second, stand in a different ratio than, say, the speeds of two wooden balls of one and hundred pounds, or of two leaden balls of two and two hundred pounds.[144] The first part of this assertion (the difference between leaden balls and wooden balls) is linked, I think, with the idea that the *quantum* of heaviness of a given body depends, all similar things having been subtracted, on the ratio between the quantity of terrestrial parts contained in this body and the quantity of subtle matter which would occupy the same volume as this body.[145] As for its second part (the difference between leaden balls of various magnitudes) it is according to me linked to considerations about the resistance of the air: the ratio between a volume of one pound and another of hundred pounds is the same as the ratio between a volume of two pounds and another of two hundred pounds, but the ratios between the surfaces corresponding to these volumes are not equal, and surfaces, not volumes, matter if the resistance of the air is taken into account. Once again however, Descartes gives us no clew for evaluating this ratio in a quantitative, and if I may say, effective, fashion.[146]

For all these three reasons, the notion Descartes had of the nature of heaviness stands in the way of any law relative to the speed of heavy bodies. We have thus clarified the third general reason for excluding speed; is it now possible to apply this conclusion to our case, namely that this notion of heaviness invalidates the statical results of July 1638? It is certain that the difficulties, which Descartes exposed in relation with the Galilean law of fall, are far-reaching. Indeed, they concern falling bodies, while statics concerns bodies at rest, but I do not see any indication that Descartes marked any difference between heaviness in statics and in cinematics; on the contrary, as we have seen, he goes constantly from one context to the other. And, if it is the same cause — the pushing of subtle matter — which produces

---

in A.T. II p. 544 (quoted above, note 137). To Mersenne, 27 August 1639, in A.T. II p. 571. To Mersenne, 11 June 1640, in A.T. III p. 79. To Mersenne, 30 August 1640, in A.T. III p. 164.

[144] To Mersenne, October-November 1632, in A.T. I p. 261.

[145] The link is explicit in *Principia philosophiae*, IV 24, in A.T. VIII-1 p. 214. Palmerino, “Infinite Degrees of Speed. Marin Mersenne and the debate over Galileo’s law of free fall”, p. 285, comments on the letter of 1632.

[146] Descartes’ more explicit tentative for giving an evaluation of the law of heaviness appears in a manuscript, conserved by Leibniz and published in A.T. XI p. 629-630. Descartes first notices that, for a body impelled by a uniform force in the void (the only such force is according to him a force imparted by mind), the law he formulated when he met Beeckman would be true — this law amounts to that if the body travels 1 unit of space in the first moment, it will travel 4/3 unit of space in the second. Asserting however that there is no void but a space, the resistance of which augments in a geometrical proportion with the speed of the body, and that the impuls of the force imparted to the body decreases in geometrical proportion, Descartes concludes that the “true” law should be in geometrical proportion. Then he considers different hypothese and notes in particular that a geometrical augmentation and a geometrical diminution should be combined in order that the arithmetical law in 4/3 remains.

two different effects — in cinematics, the fall of bodies, and in statics, their resistance to upwards motions —, the difficulties encountered to evaluate one effect extend to the other effect, insofar as they are linked to this cause. However, the Cartesian notion of heaviness does not invalidate everything in the results of July 1638: the ratio, which is established between P_a and P_r stands whatever the nature of heaviness is. The only consequence of the Cartesian notion of heaviness is that one can not speak in a strict sense from "absolute" heaviness, because it is varying with the place of the body in the whirling subtle matter. . In other words, even if we accept to conform to Descartes' intimation, according to which his works should be tackle as a coherent system, we should not exagerate the real consequences of his notion of heaviness on his practice of statics.

To put the letter of July 1638 in the context of Descartes' works has leaded us to examine the reasons why he had chosen to exclude speed from his statics. The main reason for this exclusion in the achieved Cartesian system was certainly that considering speed would have led to many complications, if statics was to be coherent with other parts of Cartesian physics, in particular with its theory of liquidity and with its conception of gravity. However, we should be aware of another motive, perhaps anterior, and most probably inspired by the Archimedean tradition: motion can not explain equilibrium, that is rest.More general conclusions should now be advanced.

Conclusion

It has been often said as a reproach to the Cartesian physics that it has not renounced to the search for causes, but simply substituted some causes to other ones. In that case, Cartesian statics does not belong to Cartesian physics. Force is reduced to its effects, raising weights at a certain height; once the nature of heaviness has been put aside, a local definition and an empirical determination of heaviness makes us reach some mathematical conclusions; speed is excluded because of the tricky links between speed, resistance of a milieu and heaviness. A French epistemologue of the sixties would have congratulated Descartes for having performed an epistemological "*coupure*" in statics: he has established statics as an autonomous science, with a principle of its own, from which a coherent body of propositions can be inferred. This coupure made the splendour as well as the misery of Cartesian statics. The exclusion of speed and the phenomenological reduction of heaviness may insure certainty and clarity to statics, but at the same time they widen the gap between statics and dynamics. The basic magnitudes of the laws of motion and of the principle of statics are different — on the one side, extension and motion (or size and speed), on the other hand, heaviness and displacement.[147] This state of things is not accidental: in the Cartesian system, one can not consider at the same time speed and heaviness. The laws of motion of the *Principia philosophiae* involve speed, but they are valid only in the void, where there is no heaviness, but only extension; the principle of statics involves heaviness, but excludes speed. Thus, the extension of statics to the analysis of motion, which was going to be so fruitful in the hands of Galileo, is not even considered here.

---

[147] On this point, see Séris, *Machine et communication*, pp. 219-221.

It is interesting at this point to gather what we have said about the confrontation of this Cartesian piece of statics with the Galilean *Le Mecaniche*. From Descartes' correspondence, one gathers that he read fairly early Guidobaldo and Stevin.[148] In 1638, he got directly involved in mechanics and glanced at Roberval s *Traité de mécaniques* for the first time.[149] Although Huygens drew his attention to the translation of the Galilean treatise by Mersenne, there is no testimony that he read it.[150] Whatever one think about a possible influence of Galileo on Descartes, the similitudes are however striking between the Carteisan and the Galilean pieces of works, not only concerning the basic notions and principles, but also concerning certain steps of the demonstrations. On this background of similarities, the difference of attitudes towards a common body of knowledge, namely the old science of weights including its transformations in the Middle Ages and in the Renaissance, are all the more striking. In the hands of Galileo, the inclined plane became a tool for understanding motion, the problem being either to compare the speeds of a body when it descends along various inclined planes, or to evaluate the speed of a body during its descent along an inclined plane. This did not happen by Descartes; on the contrary, he consciously avoided all the roads which would lead him to confront the problem of speed.

If the exclusion of speed properly is a characteristic of the Cartesian way, it could perhaps be linked to a more general feature of French mechanics. It is indeed striking that French mechanicians invoked neither the Pseudo-Aristotle nor Jordanus nor Tartaglia. Mersenne's *Synopsis mathematica* (1624) mentions Commandino, Guidobaldo dal Monte, Luca Valerio, Stevin.[151] Roberval's *Traité de mécaniques* (1634) mentions Archimedes, Guidobaldo, Luca Valerio, Cardano, Pappus.[152] Fermat's letters of 1636, quotes mostly Archimedes, and only once Guidobaldo.[153] Perhaps thanks to Stevin, these new Archimedeans were able to explain the inclined plane; they were good mathematicians indeed, but they do not consider mathematics as a clue for understanding real things — mathematical constructions

---

148 For Guidobaldo, see [To Boswell, 1646 ?], in A.T. IV p. 696. For Stevin, see to Huygens, 1st November 1635, in A.T. I p. 331; to Mersenne, 13 July 1638, in A.T. II p. 247; to Mersenne, 11 October 1638, in A.T. II p. 391.

149 To Mersenne, 11 October 1638, in A.T. II p. 391.

150 Huygens to Descartes, 8 September 1637, in A.T. I p. 397. My opinion is that Descartes did not read Mersenne's translation, because otherwise, he would at this point have taken the opportunity of explaining why Galileo's ways of proceeding are not as good as his own. This does not exclude however that he may have had a glimpse earlier on one of the manuscripts of *Le Mecaniche* circulating in Paris in the twenties. There is good evidence for this circulation: in 1635, Diodati wrote that a manuscript of *Le Mecaniche* was brought in France some 16 or 18 years beforehand, also in 1619-1621 (**ref**); the two existing parisian manuscripts are dated respectively from 1623 and 1627; in his first letter to Galileo, dated from the 1st February 1629, Mersenne notes "vidimus etiam mtractum Mechanicorum, quem e tua manu putant ortum" (T.W.B. II p. 175).

151 Luca Valerio's *De Centro Gravitatis*, as the homonymous later treatise by Guldin, determines in an Archimedean fashion the centre of gravity of various figures and solides. On Mersenne's *Synopsis* and its sources, see Duhem, *Les Origines de la statique*, I p. 295-299, II p. 123-129, *passim*.

152 *Traité de mécaniques*, p. 1, p. 11, p. 15, p. 21.

153 *Nova in mechanicis theoremata*, in T.H. II p. 26.

are for them only abstractions, with which it may be fun to play a little while, but which should not be confused with what happens really in nature. Moreover, with the exception of Mersenne, they are not interested in the ways and proceedings of nature concerning motion; at best, motion is for them a way of generating curves.

But this should not make me forget my original question when the letter of July 1638 first came under my scrutiny: what is the relationship between the idea of mechanics and the practice of mechanics? In the first part of this paper, I emphasised that different layers were superposed in what is usually called the Cartesian mechanical philosophy; so it is not surprising that the answer to this question depends on what is meant by "idea of mechanics." If it means the desire to make a physics as certain as geometry, there is no opposition between the idea of mechanics and the piece of statics I have been commenting upon; on the contrary, the second one proceeds from the second. If it refers to the programme of explaining everything thanks to matter and motion, the relationship between the idea of mechanics and the *Examen de la question géostatique* is at least twofold: on the one hand, the *Examen* has its own coherence and autonomy; on the other however, the idea of mechanics imposes constraints to what statics should be and closed the way to the integration of speed.

Sophie Roux
(EHESS, Centre Alexandre Koyré)

Sources

Baldi, B., *In mechanica Aristotelis problemata exercitationes*, Moguntiae: Joannis Albini, 1621.

Beaugrand, J. de, *Joannis de Beaugrand, Regi Franciae Domui Regnoque ac aerario sanctiori a consiliis secretisque Geostatice, seu de vario pondere gravium secundum varia a terrae (centro) intervalla Dissertatio mathematica*, Paris: Tussanum Du Bray, 1636.

Beeckman, I., *Journal tenu par Isaac Beeckman de 1604 à 1634*, de Waard, C., ed., 4 vol., La Haye: Martinus Nijhoff, 1939-1953.

Brosse, G. de la, *Eclaircissement d'une partie des paralogismes (…) de la première partie de la quatrième proposition (…) de la Géostatique*, Paris: Jacques Dugast, 1637. Latin translation in 1638.

Cardan, J., *Les livres de Hierôme Cardan (…) intitulés de la Subtilité et subtiles inventions, ensemble les causes occultes et raisons d'icelles, traduits de latin en français par Richard Le Blanc*, Paris: G. Le Noir, 1566.

Caus, S. de, *Les Raisons des forces mouvantes avec diverses machines tant utiles que plaisantes (…)*, Francfort: Jean Norton, 1615.

Descartes, R., *Oeuvres de Descartes*, Adam, C., and Tannery, P., eds, 11 vol., nouv. présentation, Rochot, B., and Costabel, P., Paris: J. Vrin, 1964-1974. Abridged A.T.

Drabkin, I. E. and Drake, S., *Mechanics in Sixteenth-Century Italy. Selections from Tartaglia, Benedetti, Guido Ubaldo and Galileo*, Madison-Milwaukee-London: The University of Wisconsin Press, 1969.

Fermat, P., *Oeuvres de Fermat*, Tannery, P., and Henry, C., eds, 4 vol., Paris: Gauthier-Villard et Fils, 1891-1912. Abridged T.H.

Galileo, G., *Le Opere di Galileo Galilei*, Edizione Nazionale a cura di Favaro, A., e Del Lungo I., 20 vol., Firenze: Barbèra, 1890-1909 (repr. 1929-1939 and 1964-1968). Abridged E.N.

Hérigone, P., *Cursus mathematici tomus tertius (…). Troisième tome du Cours mathématique. Contenant la construction des tables de sinus, et logarithmes, avec leurs usages aux intérêts, et en la mesure des triangles rectilignes ; la géométrie pratique ; les fortifications ; la milice ; et les méchaniques*, Paris: l'Auteur et Henry Le Gras, 1634.

Lamy, B., *Traités de mécanique*, Paris: André Pralard, 1682 (2nd ed.)

Mersenne, M., *Correspondance du P. Marin Mersenne, religieux minime*, Tannery, P., de Waard C., and Beaulieu, A., eds, 17 vol., Paris: éditions du CNRS, 1932-1988. Abridged T.W.B.

Mersenne, M., *F. Marini Mersenni Minimi Cogitata physico-mathematica, in quibus tam naturae quam artis effectus admirandi certissimis demonstrationibus explicantur*, Paris: Antoine Bertier, 1644.

Mersenne, M., *F. Marini Mersenni Ordinis Minimorum Quaestiones celeberrrimae in Genessim, cum accurata textus explicatione*, Paris: Sébastien Cramoisy, 1623.

Mersenne, M., *Harmonie universelle contenant la théorie et la pratique de la musique*, Paris: Sébastien Cramoisy, 1636, repr. Paris: CNRS, 1963.

Mersenne, M., *Les Méchaniques de Galilée*, Paris: Henri Guenon, 1634. Here quoted in *Questions inouïes*, Pessel, A., ed., Paris: Fayard, 1985.

Mersenne, M., *Traité de l'harmonie universelle, Paris: Guillaume Baudry, 1627.*

Mersenne, M., *Traité des mouvements et de la chute des corps pesants et de la proportion de leurs différentes vitesses*, Paris: Jacques Villery, 1633.

Monantheuil, H. de, *Aristotelis Mechanica Graeca*, Paris, 1599.

Moody, E. A., and Clagett, M., (eds), *The Medieval Science of Weights (Scientia de ponderibus): Treatises Ascribed to Euclid, Archimedes, Thabit ibn Qurra, Jordanus de Nemore and Blasius of Parma*, Madison-Milwaukee-London: The University of Wisconsin Press, 2nd ed., 1960.

Pardies, I. G., *La Statique ou la science des forces mouvantes*, Paris: Sébastien Mabre-Cramoisy (2nd ed.), 1674.

Pardies, I. G., *Oeuvres du R. P. Ign. Gaston Pardies de la Compagnie de Jésus*, Lyon, 1725.

Poisson, N., *Traité de la mécanique, composé par Monsieur Descartes : de plus l'Abrégé de Musique du même auteur, mis en français avec les éclaircissements nécessaires par N. P. P. D. L.*, Paris: Charles Angot, 1668.

Roberval, G. P. de, *Traité de méchaniques. Des poids soutenus par des puissances sur des plans inclinés à l'horizon. Des puissances qui soutiennent un poids suspendu à deux cordes*, in Mersenne, *Harmonie universelle*, with an independant pagination. It was also published independly, Paris: Richard Charlemagne, 1636, but I have not looked at this edition.

Stevin, S., *La Statique ou Art pondéraire*, in *Oeuvres mathématiques*, traduction et commentaires par Albert Girard, Leyde: Bonaventure et Abraham Elzevier, 1634, vol. IV.

Secondary Litterature

Allard, G. H., "Les Arts mécaniques aux yeux de l'idéologie médiévale", *Cahiers d'études médiévales, VII. Les Arts mécaniques au Moyen-Âge*, Montréal-Paris: Bellarmin-Vrin, 1982.

Armogathe, J.-R., and Belgioioso, G., (eds.), *Descartes: Principia Philosophiae (1644-1994)*, *Atti del Convegno per il 350° anniversario della pubblicazione dell'opera*, Naples: Vivarium, 1994.

Armogathe, J.-R., Belgioioso, G., and Vinti, C., (eds.), *La Biografia intellettuale di René Descartes attraverso la Correspondance. Atti del Convegno "Descartes e l'"Europe savante". Perugia, 7-10 ottobre 1996*, Napoli: Vivarium, 1996.

Berkel, K. van, "Beeckman, Descartes, et la philosophie physico-mathématique",*Archives de philosophie*, 46, 1983, 620-626.

Berkel, K. van, "Descartes' debt to Beeckman", in Gaukroger, Schuster and Sutton (eds.), *Descartes Natural Philosophy.*

Clavelin, M., *La Philosophie naturelle de Galilée*, Paris: Albin Michel, 1996 (1st edition : Armand Colin, 1968).

Costabel, P., "Centre de gravité et équivalence dynamique", in *Les Conférences du Palais de la Découverte*, série D, 34 (1954), 1-19.

Costabel, P., "La Démonstration cartésienne relative au centre d'équilibre de la balance", *Archives internationales d'histoire des sciences*, 35, 1956, 133-146, here quoted in the reprint *Démarches originales de Descartes savant*, Paris: Vrin, 1982, 87-100.

Costabel, P., "Les Enseignements d'une notion controversée : Le centre de gravité", in *Actes du Symposium international des sciences physiques et mathématiques dans la première moitié du XVII^e siècle. Pise-Vinci 16-18 juin 1958*, Paris: Hermann, 1960, 116-125.

De Buzon, F., "La *mathesis* des *Principia* : remarques sur II, 64", in Armogathe and Belgioioso (eds.), *Descartes: Principia Philosophiae (1644-1994)*, 303-320.

Dear, P., *Discipline and Experience. The Mathematical Way in the Scientific Revolution*, Chicago: Chicago U.P., 1996.

Dugas, R., *La Mécanique au XVII^e siècle*, Neuchâtel: Le Griffon, 1954.

Duhem, P., *Les Origines de la statique*, 2 vol., Paris: Hermann, 1905-1906.

Festa, E., and Gatto, R., (eds), *Atomismo e continuo nel XVII secolo. Atti del Convegno Internazionale "Atomisme et continuum au XVII^e siècle (Napoli, 28-29-30 avril 1997)*", Napoli: Vivarium, 2000.

Fichant, M.,*Science et métaphysique dans Descartes et Leibniz*, Paris: PUF, 1998.

Field, J. V., and James, F. A. J. L., (eds.), *Renaissance and Revolution: Humanists, Scholars, Craftsmen and Natural Philosophers in Early Modern Europe*, Cambridge: Cambridge U.P., 1993.

Gabbey, A., "Between *Ars* and *Philosophia Naturalis*: Reflections on the Historiography of Early Modern Mechanics", in Field and James (eds), *Renaissance and Revolution,* 133-146.

Gabbey, A., "Descartes' Physics and Descartes Mechanics: Chicken and Egg", in Voss (ed.), *Essays in the Philosophy and Science of René Descartes*, 311-323.

Gabbey, A., "Newton's *Mathematical Principles of Natural Philosophy*: a Treatise on 'Mechanics'?", in Harman and Shapiro (eds.), *An Investigation of Difficult Things*, 305-322.

Galluzzi, P., *Momento, Studi Galileiani*, Rome: Ateneo e Bizzari, 1979.

Garber, D., "A Different Descartes: Descartes and the Program of a Mathematical Physics in the *Correspondence*", in Armogathe, Belgioioso and Vinti (eds.), *La biografia intellettuale di René Descartes attraverso la Correspondance*, 193-216.

Garber, D., *Descartes' Metaphysical Physics*, Chicago: Chicago U.P., 1992.

Gaukroger, S., Schuster, J., and Sutton, J. (eds.), *Descartes Natural Philosophy*.

Guerlac, H., "Guy de la Brosse: botanist, chemist, and libertine", in *Essays and Papers in the History of Modern Science*, Baltimore, John Hopkins U.P., 1977, p. 440-450.

Harman, P. M., and Shapiro, A. E., (eds.), *An Investigation of Difficult Things: Essays on Newton and the History of the Exact Sciences*, Cambridge: Cambridge U.P., 1992.

Howard, R., "Guy de la Brosse: botanique et chimie au début de la Révolution scientifique", *Revue d'histoire des sciences*, 34, 1978, 301-326.

Koyré, A., *Etudes d'histoire de la pensée scientifique*, Paris: Gallimard, 1973.

Koyré, A., *Etudes galiléennes*, Paris: Hermann, 1966.

Laird, W. R., "The Scope of Renaissance Mechanics", *Osiris*, 2nd series, 2 (1986), 43-68.

Laird, W. R.,*The Unfinished Mechanics of Guiseppe Moletti*, University of Toronto Press, Toronto, Buffalo, London, 2000.

Mahoney, M. S.,*The Mathematical Career of Pierre de Fermat (1601-1665)*, Princeton: Princeton U.P., 2nd ed., 1994.

McKirahan, R. D., Jr, "Aristotle's Subordinate Sciences", *British Journal for the History of Science*, 11 (1978), 197-220.

Nardi, A., "Poids et vitesse, Descartes "presque" galiléen, 18 février 1643", *Revue d'histoire des sciences*, 39 (1986), 3-16.

Palmerino, C.-R., "Infinite Degrees of Speed. Marin Mersenne and the debate over Galileo's law of free fall", *Early Science and Medicine*, vol. 4, 4 (1999), 271-328.

Romano, A., "L'Enseignement des mathématiques à La Flèche dans les années de la formation de Descartes", in *Actes du colloque universitaire de La Flèche (12-13 avril 1996)*, La Flèche: Prytanée national militaire, 1997, 76-103.

Rossi, P., *I Filosofi e le macchine (1400-1700)*, Milano: Giangiacomo Feltrinelli, 1962.

Roux, S., "Descartes atomiste ?", in Festa and Gatto (eds), *Atomismo e continuo nel XVII secolo*, 211-274.

Roux, S., *La Philosophie mécanique (1630-1690)*, Thèse de Doctorat non publiée, Paris: E.H.E.S.S., 1996.

Séris, J.-P., "Descartes et la mécanique", *Bulletin de la Société française de philosophie : séance du 31 janvier 1987*, Paris: Colin, 1987, 31-66.

Séris, J.-P., *Machine et communication*, Paris: Vrin, 1987.

Tannery, P., "La Correspondance de Descartes dans les inédits du fonds Libri", in *Mémoires scientifiques,* tome VI, Paris: Gauthier-Villard, 1896.

Taton, R., *L'Oeuvre mathématique de G. Desargues*, Paris: P.U.F., 1951.

Voss, S., (ed.), *Essays in the Philosophy and Science of René Descartes*, Oxford: Oxford U.P., 1993.

Westfall, R. S., *The Construction of Modern Science. Mechanisms and Mecanics*, Cambridge: Cambridge U.P., 1971.