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The hydrostatic paradox and the origins of Cartesian dynamics

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Abstract

In the early decades of the seventeenth century, various attempts were made to develop a dynamical vocabulary on the basis of work in the practical mathematical disciplines, particularly statics and hydrostatics. The paper contrasts the *Mechanica* and Archimedean approaches, and within the latter compares conceptions of statics and hydrostatics and their possible extensions in the work of Stevin, Beeckman and Descartes. Descartes' approach to hydrostatics, a discussion of which forms the core of the paper, is shown to be quite different from that of his contemporaries, above all in its attempt to provide a natural-philosophical grounding for hydrostatics while at the same time using it to develop a range of concepts, approaches and ways of thinking through problems that would shape Descartes' mature work in optics and cosmology.

Keywords: Scientific revolution; René Descartes; Isaac Beeckman; Simon Stevin; History of mechanics; History of statics; History of optics

1. Introduction

Hydrostatics was one of a number of areas of 'practical mathematics'—made up primarily of geometrical optics, positional astronomy, harmonics, aerostatics and hydrostatics—that had been developed by Alexandrian authors in the Hellenistic era,

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and which received renewed if piecemeal study in the Middle Ages and then again in the Renaissance. Our concern in this paper is with Descartes' use of hydrostatics as a source and model for other areas of physical enquiry where mathematical and physical concerns meet. This issue had general implications for how one thinks through physical problems mathematically in the period between the growing revival of interest in statics and kinematics in the mid to late sixteenth century, and Galileo's *Two New Sciences* (1638), whose treatment of falling bodies was a watershed, providing the kinematic foundation—further developed by Huygens and Newton, amongst others—on which classical dynamics was later to be constructed.

The relation between physical or natural-philosophical concerns and mathematical ones in this period is conceptually very complex, and we will do well to avoid compounding this complexity by trying to deal with it using an unnecessarily ambiguous terminology. Four terms that will be invoked are 'practical mathematics', 'mixed mathematics', 'subordinate sciences' and 'physico-mathematics'. We need to identify an area of shared concern, namely the area of intersection of mathematical and natural-philosophical forms of understanding and techniques, and competing conceptions of how this area of shared interest should be thought through. There are two competing conceptions that we shall be concerned with. The first can readily be identified as that of 'subordinate sciences', which we associate with traditional Aristotelian natural philosophy even though some quite radical moves can be made in the genre which attempt to distance it from particular Aristotelian natural-philosophical doctrines. The second is 'physico-mathematics', which is how Descartes and Beeckman described their enterprise, and which, at least in Descartes' hands, we will argue marks an approach which is completely different from the 'subordinate sciences' conception of how mathematical and physical accounts are connected to one another. Terminologically, this is relatively clear cut. The shared area of concern, however, has been variously characterised as 'practical mathematics' and 'mixed mathematics'. The term 'mixed mathematics' has the problem that the 'mixing' is traditionally a mixing of two sciences, one subordinate to the other, so that it is not a neutral term. 'Physico-mathematics', as Descartes conceives of it, is in no sense a 'mixing' of this type, and is indeed conceived in a way that cuts across the traditional conception of 'superior' and 'subordinate' sciences, and it abandons the metaphysically driven demarcation of disciplines that provides it with its rationale.[1] The term 'practical mathematics', on the other hand, is, in the literal meanings of the words that make up the term, neutral in this regard. The drawback is that it is sometimes reserved for a set of disciplines that are picked out not in a theoretically motivated way but in terms of those practices of various mechanical and other 'arts' that employed mathematical techniques, such as cartography, surveying, perspective, music, navigation and military architecture. We shall be using the term 'practical mathematics' to describe a theoretically motivated division of disciplines that employ both physical and mathematical forms of understanding, however the relation

[1] Jesuit commentators adopted the term 'physico-mathematics' by the 1630s—as for example in Zucchi (1639)—but they used the term as synonymous with 'mixed mathematics': Baldini (1999), p. 259.

between these is conceived, and we shall reserve the term ‘mixed mathematics’ for a particular way of conceiving of how these disciplines must function.[2]

The term ‘mixed mathematics’ had been framed by Aristotle to refer to a group of disciplines intermediate between physics, which deals with those things that change and exist independently of us, and mathematics, which deals with those things that do not change but have no existence independently of us, since numbers and geometrical figures have (*contra* Plato) an existence only in our minds.[3] A physical account of something—such as why celestial bodies are spherical—is an explanation that works in terms of the fundamental principles of the subject matter of physics, that is, it captures the phenomena in terms of what is changing and has an independent existence; whereas a mathematical account of something—such as the relation between the surface area and the volume of a sphere—requires a wholly different kind of explanation, one that invokes principles commensurate with the kinds of things that mathematical entities are.[4] In the *De caelo*, for example, we are offered a *physical* proof of the sphericity of the earth,[5] not a mathematical one, because we are dealing with the properties of a *physical* object. In short, distinct subject matters require distinct principles, and physics and mathematics are distinct subject matters. However, Aristotle also recognises subordinate or mixed sciences, telling us in the *Posterior Analytics* that ‘the theorem of one science cannot be demonstrated by means of another science, except where these theorems are related as subordinate to superior: for example, as optical theorems to geometry, or harmonic theorems to arithmetic’.[6] Whereas physical optics—the investigation of the nature of light and its physical properties—falls straightforwardly under physics, for example, geometrical optics ‘investigates mathematical lines, but *qua* physical, not *qua* mathematical’.[7] The question of the relation between mixed mathematics and the ‘superior’ disciplines of mathematics and physics, which do the real explanatory work on this conception, remained a vexed one throughout the Middle Ages and the Renaissance, but so long as the former remained marginal to the enterprise of natural philosophy the problems were not especially evident.

By the beginning of the seventeenth century, the disciplines of what were conceived of as mixed mathematics were attracting a significant amount of attention, both on the question of whether they might have any explanatory force in their own

[2] It should be said that the issue is not wholly terminological, for the idea that both Aristotelians and partisans of ‘physico-mathematics’ shared some common concern with ‘mixed mathematics’ suggests a significant degree of continuity. See, for example, Dear (1995), who uses the idea of ‘mixed mathematics’—illegitimately in our view—to establish just such a continuity.

[3] See Aristotle, *Metaphysics* Book E.

[4] See Aristotle, *Posterior Analytics*, 75a28–38: ‘Since it is just those attributes within every genus which are essential and possessed by their respective subjects as such that are necessary, it is clear that both the conclusions and the premisses of demonstrations which produce scientific knowledge are essential . . . It follows that in demonstrating we cannot pass from one genus to another’. Cf. 76a23ff and *De caelo*, 306a9–12.

[5] *De caelo*, 297a9ff.

[6] *Post. Anal.*, 75b14–16.

[7] *Physics*, 194a10.

right—where the issue of 'mixing' was crucial—and in relation to their practical applications—where it was often ignored, as by Stevin. Descartes, in the course of attempts to develop a systematic mechanistic natural philosophy and cosmology, as well as a physical optics, in the 1630s and 1640s, would use concepts and techniques derived from statics, and above all hydrostatics, to try to think through questions about the nature of force. In particular, drawing upon a novel conception of hydrostatics which he had developed in 1618/19, he was to discuss the stability of planetary orbits in terms of bodies carried along in a fluid being in a state of equilibrium with the fluid, and he was to interpret the impact of light corpuscles on reflecting surfaces—where the light corpuscle cannot disturb the surface if his law of the equality of angles of incidence and reflection holds—in terms of how lighter weights can never displace heavier ones on a beam balance.[8]

Our concern in this paper is with Descartes' earliest encounter with statics and hydrostatics. We shall be exploring the concepts and techniques he developed in thinking through hydrostatical problems, and examining in what ways he thought these concepts and techniques might be extrapolated to physical theory generally. Conceived in the prevailing Aristotelian manner as a discipline of mixed mathematics, hydrostatics covered both mathematical and physical disciplines. Descartes in fact extrapolated from hydrostatics to both these areas. In the case of mathematics, his general approach to hydrostatics was problem-solving rather than demonstrative, and this helped lead him to see mathematics in terms of problem-solving as opposed to demonstrative techniques. Indeed, this is what drove his advocacy of analysis over synthesis: his claim was that a geometrical demonstration does not reveal to us how a mathematical result is generated. Algebraic proofs, by contrast, have a transparency which reveals the path by which the conclusion is produced.[9]

Although our focus here will not be upon how Descartes pursued this wider program in algebra and a general theory of 'method', we shall be concerned with the senses in which he understood his early hydrostatical work to be 'mathematical' and how such a mathematical endeavour related to the emerging interest he shared with Isaac Beeckman in formulating a corpuscular-mechanical approach to natural philosophising. His early approach to hydrostatics employed a procedure that purported to uncover the physical processes that are responsible for the phenomena that hydrostatics, of the traditional Archimedean kind, had up until that point only described in mathematical terms. This suggested to Descartes a way of providing physical content to mathematically conceived disciplines. Indeed, we shall argue that it helped Descartes to conceive how a mechanistic natural philosophy, inspired by but superior to Beeckman's efforts in this area, could be constructed, and how in turn the existing practical mathematical fields could at long last be firmly articulated to a sound—i.e. corpuscular-mechanical—natural philosophy. Descartes' early hydrostatics is particularly important because of the role it played in providing the starting point and initial resources for his complicated attempts to move between geometrical and

[8] See Gaukroger (2000a).
[9] See Gaukroger (1995) pp. 172–81, and Sepper (1996), pp. 157–208.

physical optics in the 1620s and 1630s,[10] endeavours which in turn led to the development of conceptions of force and causation that would be central to later versions of his mechanistic natural philosophy, and deployed in his mechanics of celestial motion and in his cosmologically inspired physical optics.

2. Mathematics, matter theory and natural philosophy

At the end of 1618, Descartes, under the guidance of Isaac Beeckman, turned his attention to a number of closely related problems in hydrostatics, all of which hinged in one way or another on the 'hydrostatic paradox', as this had been brought to light and demonstrated in Simon Stevin's work. We shall be looking at the way in which Stevin deals with the paradox (Section 3), and at how Descartes, guided by Beeckman's micro-corpuscularian natural philosophy, transforms this treatment (Sections 4 and 5). But before we do this, we need first to understand the complicated and problematic standing that statics and hydrostatics had in the complex mathematics/practical mathematics/natural philosophy nexus, for it is a shift in this standing on which Descartes' project turns.

Descartes' treatment of the hydrostatic paradox is given in a report from Beeckman which he entitles *Aquae comprimentis in vase ratio reddita à D. Des Cartes*.[11] The paradox derives from Stevin,[12] who had demonstrated that a fluid can exert a total pressure on the bottom of its container that is many times greater than its weight. In particular, he showed that a fluid filling two vessels of equal base area and height exerts the same pressure on the base, irrespective of the shape of the vessel—one might be conical, for example, the other cylindrical—and hence independently of the amount of (weight of) fluid contained in the vessel. His proof of this theorem works through the idea that fluid can be replaced by a solid body of the same density without affecting the pressure it exerts, and given this, the demonstration proceeds geometrically.

Descartes' approach is quite different, although there has been a tendency among those few commentators who mention his hydrostatical exercises to assimilate his treatment to that of Stevin. Milhaud, for example, maintains that Descartes proceeds geometrically, starting with definitions or postulates and demonstrating results from these in a syllogistic way,[13] and Rodis-Lewis also mentions his syllogistic path, noting his 'remarkable formal rigour'.[14] But actually what is remarkable is the absence of formal rigour. Descartes substitutes, for Stevin's formally rigorous and

[10] See Schuster (2000).

[11] The text, which derives from Beeckman's diary, is given in Vol. 10 of Descartes (1974–86), which we refer to by the standard abbreviation AT: AT, Vol. x, pp. 67–74, as the first part of the *Physico-Mathematica*. See also the related manuscript in the *Cogitationes Privatae*, AT, Vol. x, p. 228, introduced with 'Petijt e Stevino Isaacus Midlleburgensis quomodo aqua in funda vasis *b*. . .'.

[12] Stevin's statical works are translated in Stevin (1955–66), Vol. i., pp 375–501.

[13] Milhaud (1921), pp. 34–7.

[14] Rodis-Lewis (1971), p. 30–1.

conclusive geometrical demonstration, a very different kind of account which is exploratory and quite inconclusive, at least by the standards of Archimedean statics used by Stevin and familiar to modern readers. Descartes did not and could not have denied the rigour of Stevin's account. If correctness or rigour in the accepted Euclidean or Archimedean sense was not at issue, what was? As we learn to answer this question, we will discover the keys to Descartes' natural philosophical and 'mathematical' program, as it was crystallising in 1619, partly in concert with Beeckman's goals and concerns. Above all, what we might initially be tempted to call an 'inconclusive' account actually bespeaks a style of conceptualisation and a loose protocol for problem-solving that together form the kernel of much of Descartes' later thinking about physical theory.

Stevin's explanation falls within the domain of practical mathematics.[15] The account Descartes substitutes for it falls within the domain of natural philosophy: the concern is to identify what causes material bodies to behave in the way they do. The geometrical account does not provide an *explanation* of the phenomenon, because it does not identify what causes the phenomenon. Fluids are physical entities made up, on Descartes' account, of microscopic corpuscles the behaviour of which determines the macroscopic behaviour of the fluid, and we need to understand the physical behaviour of the constituent corpuscles if we are to understand the behaviour of the fluid, because this is what is causally responsible for its behaviour. As we shall see, Descartes speaks in terms of microscopic corpuscles whose movements or tendencies to movement are understood in terms of an emergent but still largely tacit theory of forces and tendencies, a causal discourse which he identifies as part of a 'Mechanics' upon which he was working. In broad terms, this would have accorded with the traditional view of the scope and aims of natural philosophy: physical explanation involves the identification of what causes material bodies to behave in particular ways. This was understood to be the case whether, as in Aristotelianism, natural processes were explained primarily on the basis of causes identified with the nature or essence of the matter in question, or, as in neo-Platonic natural philosophies, brute matter was seen as being worked upon from the outside by various types of non-material causal agents. Theorising about matter and an associated 'causal register' was traditionally taken as constitutive of natural philosophy. Whatever disputes there might have been amongst Platonists, Aristotelians, Stoics and atomists, there was consensus on what kind of theory provided the ultimate explanation of macroscopic physical phenomena, namely a theory of matter and causation. And it was such a conception, reflected through Aristotle's categorisation of the mixed mathematical sciences as subordinate to given, previously established explanatory physical principles of matter and cause, that had effectively marginalised, or at least rendered problematic, mathematical approaches to natural phenomena within natural philosophy.

[15] As was the case with many master practitioners of the practical mathematical disciplines, Stevin also envisioned the application of these results to more properly practical ends; that is, a key mathematical result would command a wide domain of application in a number of practical fields.

It looks, therefore, as if Descartes is rejecting a mathematical treatment of a physical question in favour of a traditional understanding of natural philosophy; moreover, that he is rejecting a precise and rigorous quantitative account in favour of an almost wholly speculative qualitative one. If this were the case, we might put this down to the fact that the exercise in hydrostatics was a first attempt at dealing with mechanical problems, and note that Descartes was to transcend this approach in his later writings, becoming one of the most vigorous advocates of a 'mathematical' approach to natural philosophy. But the issue of his treatment of the hydrostatic paradox is far more complex than this. Certain concepts and modes of argument appear in the hydrostatics manuscript which were to constitute the essence of Cartesian micro-mechanism in optics, cosmology, physiology and natural philosophy generally. They would be refined, revised, recast and presented in a variety of different ways, but they would remain clearly recognisable descendants of techniques and ways of thinking developed in the course of his treatment of the hydrostatic paradox. Moreover, we shall see that Descartes (and Beeckman) conceived of this enterprise as a case of what they then termed 'physico-mathematics', and this work seemed to Descartes to be 'mathematical' in some sense, with an emphasis on an analytical, problem-solving orientation. Additionally, there was embedded in the concept of 'physico-mathematics', as applied to these hydrostatical problems, a radically non-Aristotelian vision of the relation of the practical mathematical sciences to this emergent form of corpuscular-mechanical natural philosophising—a relation Descartes was to pursue very successfully in his early optical researches.

Descartes' initial aim, under the rubric of 'physico-mathematics', seems to have been to shift hydrostatics from the realm of practical mathematics unambiguously into the realm of natural philosophy. This he tries to achieve by redescribing, in terms of his matter theory, what it is that causes the pressure exerted by a fluid on the floor of the vessel containing it: he redescribes what causes the pressure in terms of the cumulative behaviour of postulated microscopic corpuscles making up the fluid. Substituting for the formal synthetic part of Stevin's proof a more exploratory problem-solving approach, he does two things. First, he shows the value of a problem-solving approach over formal demonstration, something that was to shape his work not only in physical theory, but also, most spectacularly, in mathematics; and indeed he would attempt to generalise this to a general theory of 'method' in the first half of the *Regulae*, which was drafted in the two years after his work on hydrostatics. Secondly, in the course of trying to provide a physical account of something that Stevin had treated largely as a mathematical problem, Descartes develops a number of physical concepts and techniques which he would later attempt to generalise to the whole of physical theory, despite the fact that they bear very distinctive hallmarks of their statical origins, such as reference to tendencies to motion rather than motions proper, and, most strikingly of all, the idea that the behaviour of bodies is to be thought through in terms of their interaction with the medium surrounding them, or in which they are embedded. In being articulated and applied these concepts would be developed and reshaped, in part by being given natural philosophical explications and legitimations. Part of that same process was to be a drift away from the traditional notion of centrality of matter theory as constitutive of natural philosophy to a much more developed 'dynamics' grounded in hydrostatics and statics via optics. Nevertheless, although matter theory would later cease to be a motivation for Descartes in the way that it is in *Aquae comprimentis*, because his dynamics is conceived in terms of the interaction between bodies and their material surroundings—as opposed to the behaviour of atoms or point masses in a void, for example—it would continue to play a residual role in his thinking, and it would be developed into a prototypical dynamics in which body/medium relations would become central to thinking through the nature of the forces that shape a body's dynamical behaviour.

3. Stevin's treatment of the hydrostatic paradox

Fluids manifest compressive forces, which are always perpendicular to the surface of the fluid, and they are equal to the pressure exerted multiplied by the area on which that pressure is exerted. Differences in pressure in the parts of a fluid at the same level and adjacent to one another would cause those parts to behave differently: those subject to less pressure would cede in favour of those subject to greater pressure. In this case, the fluid would not be in static equilibrium. For there to be static equilibrium, the pressure exerted at all points at the same level in the fluid must be the same, and hydrostatics investigates the properties of a fluid under these conditions. These are the fundamentals of hydrostatics as laid out in Book 1 of Archimedes' *On Floating Bodies*, which initiated the discipline.

The hydrostatic paradox, as Stevin generates it, relies on a simple and ingenious principle, that of solidification. He uses the principle in a number of ways, in its simplest application to account for Archimedes' buoyancy theorem, which tells us that when a body is partially or totally submerged in a fluid, the resultant of all the compressive forces acting on the body is equal and opposite to the weight of fluid displaced by the body, and passes through the centre of gravity of the body displacing the fluid. In proving this theorem, Stevin imagines a series of modifications to an initial case of a fluid in equilibrium, an ocean at rest. In the first modification, we introduce an imaginary surface—the simplest case is a hemispherical surface—into the water. Next we imagine the water inside that surface being frozen into ice, so that we have an immersed hemisphere of ice whose flat surface is flush with the surface of the ocean. This ice has (Stevin assumes) the same weight as the water it replaces, so the physical state of the water is unaffected. Finally, we imagine replacing the ice with a thin shell of steel—the hull of a boat—with a weight placed in it, in such a way that the centre of gravity has not changed. This acts in exactly the same way as the ice, and so in exactly the same way as water.

The principle of solidification introduced here is used in a more ingenious way in generating the hydrostatic paradox. In his *Elements of Hydrostatics*, Stevin proves that the weight of a fluid upon a horizontal bottom of its container is equal to the weight of the fluid contained in a volume given by the area of the bottom and the height of the fluid measured by a normal from the bottom to the upper surface.[16]

[16] Stevin (1586); reprinted and translated in Stevin (1955–66), Vol. i, p. 415.

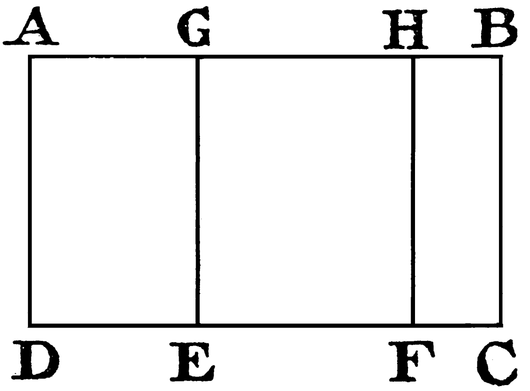

Fig. 1.

He employs a *reductio ad absurdum* argument applied to the gross statical properties of fluids: ABCD is a container filled with water (Fig. 1). GE and HF are normals dropped from the surface AB to the bottom DC, notionally dividing the water into three portions, AGED, GHFE and HBCF.

> We have to prove that on the bottom EF there rests a weight equal to the gravity of the water of the prism GHFE. If there rests on the bottom EF more weight than that of the water GHFE, this will have to be due to the water beside it. Let this, if it were possible, be due to the water AGED and HBCF. But this being assumed, there will also rest on the bottom DE, owing to the water GHFE, because the reason is the same, more weight than that of the water AGED; and on the bottom FC also more weight than that of the water HBCF; and consequently on the entire bottom DC there will rest more weight than that of the whole water ABCD, which (in view of ABCD being a corporeal rectangle) would be absurd. In the same way it can also be shown that on the bottom EF there does not rest less than the water GHFE. Therefore, on it there necessarily rests a weight equal to the gravity of the water of the prism GHFE.[17]

Stevin then argues that various portions of the water can be notionally solidified, or replaced by a solid of the same density as water. This permits the construction of irregularly shaped volumes of water, to which, paradoxically, the theorem can still be applied. Take, for example, Stevin's Corollary II (Fig. 2):

> Let there again be put in the water ABCD a solid body, or several solid bodies of equal specific gravity to the water. I take this to be done in such a way that the only water left is that enclosed by IKFELM. This being so, these bodies do not weight or lighten the base EF any more than the water first did. Therefore

[17] Ibid. Vol. i, p. 415.

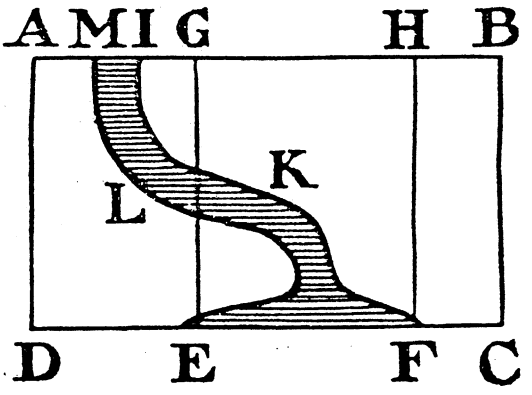

Fig. 2.

> we still say, according to the proposition, that against the bottom EF there rests a weight equal to the gravity of the water having the same volume as the prism whose base is EF and whose height is the vertical GE, from the plane AB through the water's upper surface MI to the base EF.[18]

Stevin goes on to apply these findings to oblique bottoms and thus to the sides of containing vessels.

Stevin's line of argument in establishing the hydrostatic paradox proceeds entirely on the macroscopic level of gross weights and volumes. The mathematical character of his proof depends upon his insistence on the maintenance of a condition of static equilibrium, understood in terms of the fundamentals of Archimedes' hydrostatics. This purely mathematical approach stands in contrast to the tradition deriving from the pseudo-Aristotelian *Mechanica problemata*.[19] The contrast is most evident in their treatments of the principle of the lever. Archimedes' treatment abstracts from physical quantities, treating bodies placed on the lever as points, lying at the centres of gravity of the bodies, along a line. Once the initial abstractions have been made and a (geometrical) procedure for determining centres of gravity established, it is all a matter of geometry. By contrast, the earlier treatment of the principle of the lever in the *Mechanica* works in terms of proportionality between weight and velocity:

[18] Ibid. Vol. i, p. 417.

[19] On the *Mechanica* see Carteron (1923). On the influence of the *Mechanica* in the sixteenth and seventeenth centuries, see Duhem (1905–6), Rose & Drake (1971) and Laird (1986). The *Mechanica*, which is probably the work of Strato or Theophrastus, was traditionally attributed to Aristotle, an attribution which Duhem and Carteron follow. The work is Aristotelian in tenor, but has the peculiar feature that whereas Aristotelian natural philosophy confines itself to natural processes (for it is these that follow from the nature of things) the subject matter of the *Mechanica*, as is explained in the opening sentence of the work, is 'those phenomena that are produced by art despite nature, for the benefit of mankind'.

> As the weight moved is to the weight moving it, so inversely is the length of the arm bearing the weight to the length of the other arm. The further away one is from the point of support, the easier will the weight move. We have already mentioned the cause of this: the weight furthest from the point of support describes a larger circle. So applying the same force, the weight will describe a greater path the further it is from the point of support.[20]

The basic principle behind this, set out in a number of passages in Aristotle,[21] is that the same force will move two bodies of different weights, but it will move the heavier body more slowly, so that the velocities of the two bodies are inversely proportional to their weights. When these weights are suspended from the ends of a lever, we have two forces acting in contrary directions, and each body moves in an arc with a force proportional to its weight times the length of the arm from which it suspended. The one with the greater product will descend in a circular arc, but if the products are equal, they will remain in equilibrium.

Whereas Archimedes' approach makes statics a mathematical discipline independent of any general theory of motion, that of the *Mechanica* makes statics simply a limiting case of a general dynamical theory of motion, a theory which is resolutely physical. In other words, the *Mechanica* account comes as part of a package which is driven by Aristotelian dynamics, above all by the principle of the proportionality of weight and velocity. This did not stop a number of mathematicians, such as Benedetti, Tartaglia and Galileo, from trying to revise the package, hoping they could salvage the dynamical interpretation of the beam balance and simple machines while jettisoning the natural philosophy that lay behind it; but the pivotal role this natural philosophy had played meant that such a revision could never be successful, as we shall see below when we consider Galileo's attempt to realise this program. The Archimedean account, by contrast, comes without any dynamical, or more broadly speaking physical, commitments: put more strongly, it comes without any physical content.

Stevin pursues an ultra-Archimedean program. He rejects the basic principle on which the *Mechanica* rests:

> *That the cause of bodies being in equilibrium does not reside in the circles described by the extremities of the arms.*
> The reason why equal weights at equal arms are in equilibrium is known by common knowledge, but not so the cause of the equilibrium of unequal weights at [inversely] proportional unequal arms. Thereto, which cause the Ancients, when they inquired into it, considered to reside in the circles described by the extremities of the arms, as appears in Aristotle's *In Mechanicis* and his successors. This we deny and give the following reason therefor

[20] *Mechanica*, 850a39–850b6.
[21] The most important is *De caelo*, 301b4ff.

> That which hangs still does not describe a circle;
> Two weights in equilibrium hang still;
> Therefore two weights in equlibrium do not describe circles.
>
> . . . As regards (in order to explain the second proposition of the syllogism) the motion or description of the circles which may appear to present itself, this is not peculiar to bodies in equilibrium, but accidental, as by wind, pushing, or some other movement by which not only these, but bodies not in equilibrium, can describe circles. It is therefore manifest that this cause does not reside in circles, but in that which has been proved about it mathematically in the 1st proposition of the 1st book of the elements of the *Art of Weighing*.[22]

But he also rejects the whole approach of the *Mechanica* to the discipline of statics:

> Because in several propositions of the *Practice of Weighing* the motions of bodies will be dealt with, I thought it advisable, before coming to the matter, to explain something of it to the reader. To wit, that the *Art of Weighing* only teaches us to bring the moving body into equilibrium with the body being moved. As to the additional weight or force which the moving body requires in order to set in motion the body to be moved (which weight or force has to overcome the impediment of the body to be moved, which is an inseparable attribute of every body to be moved), the *Art of Weighing* does not teach us to find that weight or force mathematically; the cause of this is that the one moved body and its impediment are not proportional to the other moved body and its impediment.[23]

What Stevin is advocating is a not just a reduction of statics to mathematics, but a severing of statics from kinetics, that is, from those parts of mechanics that study motion—kinematics and dynamics—and indeed a severing of statics from natural philosophising, howsoever conceived. The *Mechanica* tradition relied not just on an elementary mechanics, however, but also on Aristotelian natural philosophy, above all upon a matter theory that motivated the idea of the proportionality between weight and speed.

The advantage of this isolation of statics from the rest of mechanics means that it is freed from any commitment to a particular natural philosophy. The disadvantage is that it ceases to become part of physical theory: or rather, it gives the impression of being a piece of pure mathematics, yet it purports to describe the behaviour of physical bodies. Stevin was a leading figure in a number of the practical mathematical sciences, as well as the practical mathematical arts. However, he always eschewed systematic natural philosophical questions or inquiries. He began his career as an accountant, and his approach to statics and hydrostatics might seem very much that of an accountant: so long as one balances the books, it does not matter what the

[22] Stevin (1955–66), Vol. i, pp. 507–9, Appendix to the *Art of Weighing*.
[23] Stevin (1955–66), Vol. i, p. 297, *The Practice of Weighing*, 'To the Reader'.

numbers are numbers of. This is not just problematic in its own right, but it also undermines any transition from statics or hydrostatics to a dynamics, with its associated move from a mathematical to a causal register, that is, to natural philosophy proper. In other words, there are two questions at stake. First, how is it possible for statics and hydrostatics to describe an empirical state of affairs, *qua* empirical state of affairs, i.e. not merely *qua* abstraction? And second, how do we connect those physical features of this empirical state of affairs that are picked out by statics and hydrostatics with those physical features dealt with in other parts of physical theory; in particular, how do we connect the description of the conditions under which equilibrium occurs with an account of the forces acting on or within bodies which combine in such a way as to give rise to an equilibrium state?

Broadly speaking, there are two options open to Descartes. First, he could take the *Mechanica*-type analysis forward on the basis of the lever and simple machines, treated dynamically, to underpin a new natural philosophy that would replace Aristotle's, and then proceed to pursue physical theory generally on this basis, taking advantage of the already-established connections between statics and the rest of physical theory. We shall see that in a quite specialised way, this is largely what Beeckman had done at the heart of his own corpuscular mechanical natural philosophy. Or, second, he could start from Stevin's Archimedean approach in hydrostatics and try, from scratch as it were, to provide it with physical content.

On Duhem's reading, Galileo's *De Motu*, which dates from around 1590 (but which was not published, and remained unknown, in his lifetime), does something like the former.[24] The principle underlying the *Mechanica* tradition is that of the proportionality between the force of a moving body and the speed of the body. Galileo—who by the time he wrote *De Motu* was beginning to think that rate of fall was proportional to the specific weight of the falling body—rejects the idea that rate of fall is simply proportional to absolute weight, and interprets the proportionality of weight and velocity in such a way that bodies of the same specific weight fall at the same rate, on the grounds (which Benedetti had proposed) that a body ten times heavier than another requires ten times the force to move it, so that increased absolute weight is always balanced by increased force needed to displace that absolute weight.[25] But Duhem's claim that this represents an extension of the Aristotelian proportionality principle could not be further from the truth. Quite the contrary, it undermines the proportionality principle. In Aristotelian natural philosophy, what moves a falling body is not something external to the body which acts against a resistance provided by the body's weight. The weight of a body is what provides its motive force, and this is why its speed is proportional to its weight. Duhem's construal only makes sense if we think of the body being pulled to the earth: in this case we can think of the external force pulling the body downwards being checked

[24] There is an English translation of *De Motu* in Drabkin & Drake (1960).
[25] See the discussion in Duhem (1905−6), Vol. i, Ch. 11.

by an internal force which resists this motion.[26] But this is completely at odds with the most fundamental tenets of Aristotelian natural philosophy.

This is not to deny, however, that in *De Motu* Galileo has some conception of statics as a limiting case of kinetics, or that in some sense this is pursued as an extension of the *Mechanica* programme. In *De Motu*, he tries to use hydrostatics as a basis for dynamics, most notably in his account of free fall. Believing that the key to the problem is to treat heavy bodies falling in air on a par with light bodies rising in water, he attempts to extrapolate from the treatment of the rise and fall of bodies in fluids studied in hydrostatics to cover the dynamical problem of the cause of differences in speeds of bodies moving through different media, and in doing so to render the dynamical problem amenable to the same kinds of geometrical treatment as a statical one. What he does is to equate the buoyant effect of the medium and the artificial lightness that an impressed force induces in a body. When a body is thrown upwards a force is impressed on it which endows the body with an artificial lightness: this is an effect which alters the effective weight of the body immersed in the medium. Weight is relativised to effective weight, which is equal to the specific weight of the body minus the specific weight of the medium: when the effective weight takes a positive value the body will fall; when it takes a negative one, the body will rise. Because Galileo thinks of statics as a limiting case of dynamics, namely that case where forces are in equilibrium, his move between hydrostatics and dynamics is a natural one.

It looks, then, as if one can retain formal features of the *Mechanica* account—statics as a physical discipline, which deals with the limiting case of equilibrium—while at the same time dispensing with the content of the *Mechanica*, rejecting the Aristotelian account of motive force that provided its initial rationale. But it is far from clear that this is in fact possible, even judging the matter on criteria Galileo himself would have applied. If one rejects the Aristotelian account of motive force, the explanation of equilibrium in terms of bodies' tendencies to move in arcs at speeds proportional to their weights must surely come into question, and in that case what is one left with? These principles provide the connections between statics and moving bodies. One cannot simply abandon them and assume that the connections will remain: any such connections will have lost their physical rationale. Galileo makes a valiant effort in *De Motu* to redirect physical theory by taking the shell of the *Mechanica* and trying to give it a new content. He failed, and eventually abandoned the project, giving up the idea of using statics as a basis for physical theory,

[26] This is how Newton saw things. Note the classical Newtonian explanation of why bodies of different masses which fall to the earth from the same distance fall at the same rate. More massive bodies experience greater gravitational attraction, so we would expect their rate of acceleration to be greater. The reason why it isn't, on the Newtonian account, is that the increased gravitational attraction to which more massive bodies are subject is exactly counterbalanced by their greater inertia, manifested as resistance to acceleration. Classical mechanics never had any explanation of this equivalence of gravitational and inertial mass, and General Relativity reveals that it was one of the greatest lacunae in classical mechanics, for the equivalence held the key to the understanding of the connection between gravitation and inertia.

and replacing motions with moments in his account of the law of the lever in his *Mechanics* of 1602, thus severing the direct ties between statics and kinetics.[27]

Descartes takes the alternative approach. What he does is to ask what natural philosophy—in effect, what theory of matter and what dynamical theory of the action of causes—enables one to make sense of the results of Stevin's analysis. By ignoring the physical interpretation of statics central to the *Mechanica* tradition and opting for an Archimedean approach, he cannot rely on any natural connection between statics and the rest of physical theory: any connections have to be forged anew. But nor is he stuck with the Aristotelian natural philosophy that comes as part of the *Mechanica* package. Far from being guided by a *Mechanica*-type approach, with its grounding in Aristotelian natural philosophy, he does not even consider Aristotelian natural philosophy. Quite the contrary, he starts with Stevin's mathematical account and attempts to 'physicalize' it, by fleshing it out in terms of a micro-corpuscularian theory of the material constituents of fluids. This is for him an example of what he and Beeckman were calling 'physico-mathematics'. Far from being something in the tradition of the Aristotelian 'subordinate sciences',[28] this 'physico-mathematics' pursues a completely different route to the quantitative understanding of physical processes, attempting not to 'mix' mathematics and physics, but to translate physical problems into the quantitatively characterisable behaviour of microscopic corpuscles making up material things (bodies and fluids), and then invoking a causal register of forces, tendencies, components and (later) 'determinations', which are completely different from Aristotelian 'principles'. What Descartes ultimately hopes to achieve by this is, at the most general level, much the same as what Galileo had hoped to achieve in his *De Motu*: the extrapolation from statics—pursued in a precise, quantitative, geometrical fashion—to the whole of physical theory. But they attempt to realise this aim in quite different ways: in *De Motu* Galileo had attempted a radical revision of the 'subordinate sciences' tradition, whereas Descartes is now in effect abandoning this tradition in favour of something wholly new.

Descartes' hope is that by exploring what he takes to be the latent natural-philosophical underpinnings of statics, and specifically the hydrostatic paradox, he will uncover and render explicit a wholly general natural philosophy. It was the underlying natural philosophy of Aristotle that provided the unification of mechanical disciplines offered in the *Mechanica*. If one removes that natural philosophy, then, as Galileo learned, the unification does not remain intact. The unification has to be produced by the natural philosophy, rather than grafted on to it. We shall see that the young Descartes pursued precisely this programme; that he wanted to reduce Stevin's hydrostatics to an embryonic corpuscular mechanism in which discourse concerning causes or 'forces'—which we term his 'dynamics'—would provide the basis for unifying the practical mathematical sciences (now construed no longer in terms of 'subordinate sciences' or 'mixed mathematics') and practical mathematical

[27] See Drake (1978), Ch. 2 on *De Motu* and Chs. 3 and 4 on Galileo's *Mechanics*. There is an English translation of the *Mechanics* in Drabkin & Drake (1960).

[28] *Contra* Dear (1995), who treats mathematical physics as coming out of the tradition of mixed mathematics.

arts under a set of analytical and representational procedures which he termed 'mechanics'.

4. Beeckman and Descartes

The document entitled 'Physico-Mathematica' which contains Descartes' hydrostatics manuscript dates from the end of 1618 or beginning of 1619.[29] It derives from the initial period in which Descartes and Beeckman went through various sorts of mechanical and mathematical questions. We have to ask why Beeckman introduced Descartes to problems in hydrostatics in the first place, what they hoped to achieve, and why they described this work under the rubric of 'physico-mathematics'. There is, however, a deeper and prior question: we already know that the hydrostatics manuscript betrays an anti-Aristotelian posture in natural philosophy, attempting to link the practical mathematical field of hydrostatics to an embryonic corpuscularian philosophy. So, our first task must be to explore why Descartes rejected an Aristotelian natural philosophy and why he put a corpuscularian natural philosophy in its place. Here, the influence of Beeckman is crucial. We need to understand his natural-philosophical agenda, as well as what he thought practical mathematics had to do with it. Grasping the answers to these questions will provide key clues to what the young Descartes and Beeckman thought 'physico-mathematics' was and what they took themselves to be doing with, or to, Stevin's hydrostatics.

In November 1618, while garrisoned in Breda, the twenty-two-year-old Descartes had the good fortune to make the acquaintance of Isaac Beeckman, a thirty-year-old Dutch scholar, who had recently taken his medical degree at Caen in Normandy.[30] This was to be the beginning of a decisive period in the development of Descartes' views about natural philosophy—as Descartes soon afterward confessed, Beeckman had 'recalled' him to 'erudition' and led him back to 'serious occupations'.[31] For

[29] As is the case with all the early writings, no exact date can be assigned to the hydrostatics manuscript. Some internal evidence suggests that Descartes composed it shortly before Beeckman left Breda at the beginning of 1619: see AT, Vol. x, p. 69, l. 15, and p. 74, l. 23 which seem to imply that Beeckman and Descartes had recently discussed these problems in person. Adam and Tannery note that the 'Physico-Mathematica' were misplaced in Beeckman's *Journal*, having been transcribed along with the *Compendium Musicae* between two entries for 20 April 1620 (AT., Vol. x, pp. 26–7). By that time Descartes himself was in Germany and no longer in contact with Beeckman. If, as seems to be the case, the 'Physico-Mathematica' were composed around the same time as the *Compendium*, which was a New Year's gift to Beeckman, then it again seems very likely that the hydrostatics manuscript dates from late 1618 or early 1619.

[30] Beeckman was born in Middelbourg on 10 December 1588. He was first intended for the reformed ministry and studied theology at Leiden between 1607 and 1610. There he also came in contact with Rudolph Snel, the Ramist practical mathematician and pedagogue. This connection is of potentially great significance for the interpretation of Beeckman's career, for Snel offers a prime example of the tendency of late sixteenth-century Ramism to concern itself with problems of the practice and pedagogy of the mechanical arts and applied mathematics. See Hooykaas (1981) and the biographical note by C. de Waard in Mersenne (1932–88), Vol. ii, p. 217; Mahoney (1981); Ong (1958), p. 305; Vollgraff (1913).

[31] Descartes to Beeckman, 23 April 1619, AT, Vol. x, pp. 162–3.

two months at the end of 1618 Descartes and Beeckman worked together, speculating upon and resolving various problems in natural philosophy, mechanics, theory of music, mathematics, and of course hydrostatics. After Beeckman's departure for Middelbourg early in 1619 the two men continued to correspond at least until Descartes set off on his travels in Germany and the east in late April.[32] In effect, Descartes served a second natural philosophical apprenticeship with Beeckman, an apprenticeship which fortified him with a new vision of the aims and content of natural philosophy, and which displaced the Scholastic vision purveyed during his schooldays at La Flèche. Beeckman also apparently stimulated Descartes' return to the study of mathematics.[33]

Beeckman was one of the very first individuals in Europe to pursue consistently the idea of a micro-mechanical approach to natural philosophy. He conceived of a redescription of all natural phenomena in terms of the shape, size, configuration and motion of corpuscles, and he insisted that the causal register of this account, that is, the principles of all natural change, had to be derived from the transdiction of the presumed mechanical principles of macro-phenomena, in particular the behaviour of the simple machines. Beeckman offered on a first-hand basis an approach to natural philosophy which was not available to Descartes from any other contemporary source.[34] Beeckman's natural-philosophical inquiries have a disorganised, almost random character, bespeaking more the humanist commonplace book than the Ramist attention to methodical textuality he surely learned from the elder Snel. Still, at the end of the 1620s he edited his notes on mechanics and cosmology into the form of a reasonably systematic account with a view to publication.[35] However, Descartes, who was beginning to put together the material for *Le Monde* at this time and was evidently disconcerted to learn that Beeckman had a similar project in mind, directed a barrage of abuse against Beeckman, calling into question his abilities and his originality, and as a result Beeckman abandoned plans for the book.[36] Beckman's *Journal* is filled with questions ranging from embryology to celestial mechanics and from logic to applied mathematics, all addressed in short entries rarely as much as a page in length. He prided himself on the spontaneous character of his inscriptions, which he thought offered a more genuine insight into the questions posed than any pre-

[32] The last extant letter from this period dates from 29 April 1619, AT, Vol. x, p. 164.

[33] Descartes alludes to the study of 'mechanics' and 'geometry' in the correspondence with Beeckman: 26 March 1619, AT, Vol. x, p. 159, l. 13; 23 April 1619, AT, Vol. x, p. 162, l. 15. But here Beeckman's influence was less decisive, for Descartes' earliest recorded work in mathematics already shows deep conceptual concerns which did not form an important part of Beeckman's intellectual armory.

[34] Of course, in his own natural philosophising Descartes would eventually employ a very different notion of just what are the principles of mechanics which provide the causal dimension of his mechanical philosophy. In addition, unlike Beeckman, Descartes would later be drawn into serious concern about the metaphysical grounding of his natural philosophy and the epistemic status of his claims. Nevertheless, from Beeckman came the inspiration for a new species of natural philosophy, as well as a considerable portion of its content.

[35] It was edited by his brother Abraham after Beeckman's death and appeared as Beeckman (1644).

[36] For the details of this episode, see Berkel (2000).

arranged program of scholarship.[37] In fact he may have had a point, because his random speculations did focus his attention on troublesome details of applying micro-mechanical principles to specific questions without the baggage of textual systematisation and metaphysical or theological legitimation. This makes the *Journal* a unique source of insight into the values, aspirations and presuppositions constitutive of the mechanistic world-view. Descartes' hydrostatic manuscript, emerging in this natural-philosophical milieu, arguably displays in the case of Descartes a similar 'naïve' stage in the early formulation of a corpuscular mechanical approach.

Beeckman's views on natural-philosophical explanation seem to stem from his unexamined faith in the truth and relevance of the theory and practice of the mechanical arts, as he had learnt them working with his father laying water conduits, and reading the works of Stevin and the Snels. In the *Journal*, with its hundreds of pages of natural-philosophical speculations, interleaved with practical questions drawn from the mechanical arts, one can detect the merger of natural philosophy with the re-evaluation of the aims and limits of knowledge which had emerged in discussions of technology and the practical arts in the later sixteenth century.[38] He consistently held Aristotelian and neo-Platonic notions of immaterial causes and agencies to be 'unintelligible' and hence useless in natural philosophy,[39] and he frequently insisted that natural philosophy speak in terms of imaginable things and processes, rather than entities of the pure understanding.[40] No doubt Beeckman conceived himself to be attacking traditional modes of philosophical discourse in the name of common sense; but, of course, his 'common sense' was precisely the educated, and to that degree sophisticated, common sense of the theory, practice and ideology of the mechanical arts. No mechanic would appeal to teleological processes, occult virtues or immaterial causes to account for the functioning of a simple mechanical device. Explanations in the mechanical arts rested on the appeal to a clear picture of the structure and interaction of the constitutive parts of the apparatus. As simple mechanical and hydrodynamical devices showed, only motion or pressure can produce the rearrangement of parts and hence produce work, and for theoretical purposes the causes of motions and pressures are other motions and pressures.

What Beeckman was demanding in natural philosophy was the application of the criteria of meaningful communication between mechanical artisans—the appeal to a picturable or imaginable structure of parts whose motions are controlled within a putative theory of mechanics. His central contention was that there is no point in talking about effects if you cannot imagine how they are produced, and the exemplar of imaginatively controlled efficacy resides in the mechanical arts where one can

[37] Beeckman (1939–53), Vol. ii, p. 99.

[38] See Rossi (1970), pp. 1–62.

[39] Beeckman (1939-53), Vol. i, p. 25.

[40] Beeckman to Mersenne, 1 October 1629, Mersenne (1932–88), Vol. ii, p. 283, 'nihil enim in philosophia admitto quam quod imaginationi velut sensile representatur'. Cf. the demands that Descartes was to place on mathematics and 'mathematical' natural philosophy in the latter portions of the *Regulae*, as well as his insistence on the 'figurate' representation of problems to be solved, both in mathematics and in optics and natural philosophy generally. On the latter see Sepper (2000).

command nature at a macroscopic level. Hence it was characteristic of Beeckman's translation of the imperatives of the mechanical arts into the terms of natural philosophy that he was not overly concerned with metaphysical objections to his doctrines. Transdiction from the macroscopic to microscopic realms did not pose epistemological difficulties for Beeckman as it would later for Descartes and other mechanists. The only constraint he placed on transdiction was the eminently 'mechanical' one of observing whether the widely differing surface to volume ratios of macroscopic bodies and corpuscles would entail any differences in their mechanical behaviour in various systems.[41] Clearly Beeckman did not pursue such a natural philosophy because he had read Stevin, studied with Rudolph Snel, and made an early career in the mechanical arts. Rather, it was Beeckman's education and pedagogical vocation, and his objectively correct image of himself as a man of learning and polite interests, which instilled in him the cultural value of the pursuit of natural philosophy.[42] But what helped make him unique was the way his desire to be a natural philosopher was refracted by his early experience in the theory and practice of the mechanical arts. The *Journal* testifies to his private goal of reforming natural philosophy in the name of values of mechanical intelligibility and utility.

Beeckman held a fundamentally atomistic view of nature. His atoms possess only the geometrical-mechanical properties of size, shape and impenetrability (being absolutely hard, incompressible and non-elastic). Motion is conceived as a simple state of bodies, rather than an end-directed process which they undergo. Moreover, the possession of motion is not mediated by any metaphysical conception of an internal moving force, *impetus*, or virtue. All other qualities, including of course the four elemental qualities of Aristotle, arise from the diverse ways in which various atomic structures constituting bodies impinge upon our sense organs.[43] Indeed, Beeckman devoted much of his speculation about matter to devising a four-element theory within the assumptions of his atomic doctrine.[44] Beside answering traditional

[41] Beeckman (1939–53), Vol. ii, pp. 77–8. Similarly, Aristotelian 'philosophical' arguments against the existence of the void carried less weight against his atomism than the transdiction of the 'metaphysical' objection that perfectly hard atoms lacking pores cannot undergo rebound (ibid, p. 100). He was obviously disturbed by his inability to conceive of a convincing macroscopic model for hard body rebound. Mechanical common sense seemed to indicate atoms do not exist.

[42] Prior to 1616 Beeckman had spent a few years in the trade of candle-making and also followed his father's craft of laying water conduits, especially for breweries. Many of the notes in his *Journal* reveal that Beeckman saw connections between practical questions raised in relation to his craft activities and the teachings he had received from the elder Snel, as well as the writings of Willebrord Snel and Simon Stevin. Beeckman, however, did not plan on remaining a practitioner of the mechanical arts, even a highly educated and philosophically literate one. In 1618 he took an M.D. degree at Caen. From November 1619 he was Conrector of the Latin School at Utrecht, and in December 1620 he moved to Rotterdam, where his brother was Rector of the Latin School. Beeckman gave lessons and became Conrector in 1624. He also founded a 'collegium mechanicum', or society for craftsmen and scholars interested in natural philosophical questions with technical import. In 1627 he became Rector of the Latin School at Dordrecht, a position he held until his death in 1637.

[43] Beeckman (1939–53), Vol. ii, p. 86.

[44] Ibid., pp. 86, 96; cf. Beeckman (1939–53), Vol. iii, p. 138, 'Ignis minimum non est atomus sed homogeneum ex atomis compositum.'

modes of explanation still very much alive in Aristotelianism and Galenic medicine, Beeckman's element theory allowed him to deemphasise atoms as explanatory elements in certain contexts. This was important, because he was impressed by arguments showing the impossibility of rebound after collision of perfectly hard atoms, and because he had difficulty reconciling atomic theory with the phenomena of elasticity.[45] Accordingly, he built his traditional elements out of congeries of atoms and manipulated the elements as functional units of explanation,[46] without, however, explaining what structural features the congeries had that enabled them to possess the required property of elasticity that their constituent parts lacked.

Unlike previous advocates of atomism and prior to any of the great mechanists of the later seventeenth century, Beeckman sought to explain the behaviour of his atoms by applying to them a causal discourse modelled on the principles of mechanics. It is here that very precise bearings emerge for the meaning of 'physico-mathematics', and for the content of Beeckman's influence upon Descartes, as well as a set of markers relating, ultimately, to differences between their two projects. By 1613 or 1614 Beeckman formulated a concept of inertia holding for both rectilinear and curved motions.[47] He insisted that motion, once imparted to a body, is maintained at the same speed, unless destroyed by external resistances. In the absence of external constraints there is no reason why the state of motion of the body should alter:

> Everything once moved never comes to rest unless due to an external impediment. Moreover, the weaker the impediment, the longer the moving body moves . . . A stone thrown in a vacuum is perpetually moved; but the air hinders it by always striking it anew and thus acts to diminish its motion. Indeed, what the philosophers say, that a force is impressed in the stone, seems without reason. For who can conceive in his mind what that force would be, or how it would continue to move the stone, or in what part of the stone it would find its seat? But someone can easily conceive in his mind that motion in a vacuum never comes to rest, because no cause changing the motion is present; for nothing is changed without some cause of change.[48]

Combining his principle of inertia with his atomic ontology, Beeckman was led to conclude that the only possible mode of external constraint or resistance that can be exerted on an inertially moving body is corpuscular impact. Conversely, only corpuscular colli-

[45] Beeckman (1939–53), Vol. ii, pp. 100–1.

[46] Beeckman (1939–53), Vol. iii, p. 31. Beeckman's theory of light provides a good example: he held light to be corporeal and to consist in the finest particles of elemental heat or fire. Because light can be reflected and refracted (to Beeckman refraction was a form of internal reflection), it cannot consist in isolated atoms; therefore, light, heat and fire had to be conceived as second-order homogenous composites made up of numerous atoms and void space.

[47] Beeckman (1939–53), Vol. i, pp. 24–5. We have employed the typescript translation by Michael S. Mahoney.

[48] Ibid.

sion and transfer of motion can account for the initiation of motion of resting bodies which have resisted the passage of inertially moving bodies. Ultimately, therefore, only the transfer of motion can account for change in the position, arrangement and disposition of atoms, and hence furnish the principle of all natural change.[49]

Beeckman eschewed metaphysical elaboration of concepts of internal moving forces or *impetus* as the cause of the continuation of inertial motion.[50] His attitude seems to have been that the idea of motion is sufficiently well understood, and that it is motion *per se*, the state of traversing space in time, which is imparted to bodies at the beginning of their movements. All this again points to the hard-headed 'common sense' of macro-mechanics which controls Beeckman's conceptualisations. His 'mechanics' of atoms—the causal register of his natural philosophy—was constructed within the limits of a mechanical artisan's belief in the priority of explanations appealing to the motions, resistances and displacements of parts, with no further verbal explication required.

Central to such a mechanics was the problem of furnishing rules of collision specifying the outcomes of exchanges of motion on the atomic level.[51] Since his atoms are perfectly hard, he formulated rules applicable to what we would term perfect inelastic collisions. He measured the quantity of motion of corpuscles by taking the product of their quantity of matter and their speed. Significantly, Beeckman linked his measure of motion to a dynamic interpretation of the behaviour of the balance beam. He evaluated the effective force of a body on a balance beam by taking the product of its weight and the speed of its real or potential displacement, measured by the arc length swept out in unit times during real or imaginable motions of the beam: the classic *Mechanica*-based procedure, but with his own dynamical gloss. Beeckman was able to build up a set of rules of impact by combining certain intuitively symmetrical cases of collisions with the dictates of the inertial principle and an implicit concept of the conservation of the directional quantity of motion in a system. His treatment of symmetrical cases of collision and his notion of the conservation of motion owed their form and their putative legitimacy to the model of the balance beam, interpreted in a dynamic rather than static fashion.[52] Indeed, Beeck-

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[49] Beeckman even tried to explain the centrifugal tendency of bodies moving in circular motion in resisting media as the result of the combination of circular inertia and differential resistance of the medium on different parts of the body. See Beeckman (1939–53), Vol. i, p. 253.

[50] Ibid., p. 25.

[51] See Appendix I in Mersenne (1932–88), Vol. ii, pp. 632–44, which includes de Waard's notes.

[52] Beeckman's rules fall into two broad categories: (1) cases in which one body is actually at rest prior to collision and (2) cases which are notionally reduced to category (1). The concept of inertia and the stipulation that only external impacts can change the state of motion of a body provide the keys to interpreting instances of the first category. The resting body is a cause of the change of speed of the impacting body and it brings about this effect by absorbing some of the quantity of motion of the moving body. Beeckman invokes an implicit principle of the directional conservation of quantity of motion to control the actual transfer of motion. In each case the two bodies are conceived to move off together after collision at a speed calculated by distributing the quantity of motion of the impinging body over the combined masses of the two bodies. For example, in the simplest case, in which one body strikes an identical body at rest, 'each body will be moved twice as slowly as the first body was moved . . . since the same impetus must sustain twice as much matter as before, they must proceed twice as slowly'. And

man's commitment to a dynamical interpretation of the principles of the simple machines and his belief in a correspondence between these principles and the rules of corpuscular collision run right through his work.[53] Beeckman consistently demanded a dynamical approach to statics, the theory of simple machines and mechanics in general, including hydrostatics. This dynamical approach inheres in a set of rules or principles about the motion or tendencies to motion of bodies, which may also be read into the behaviour of fundamental corpuscles and atoms, to provide the causal register for our explanatory discourse about them.

To state this more generally, one can say that the style of Beeckman's natural philosophy demanded that macroscopic phenomena be explained through reduction to corpuscular-mechanical models. The *Journal* offers hundreds of examples of this

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Beeckman adds, analogising the situation to the mechanics of the simple machines, 'it is observed in all machines that a double weight raised by the same force which previously raised a single weight, ascends twice as slowly' (Beeckman, 1939–53, Vol. i, pp. 265–6). Instances of the second category of collision are assessed in relation to the fundamental case of collision of equal speeds in opposite directions (ibid., p. 266). Being perfectly hard and hence lacking the capacity to deform and rebound, the two atoms annul each other's motion, leaving no efficacious residue to be redistributed to cause subsequent motion. This symmetrical case, which was also generalised to cases of equal and opposite quantities of motion arising from unequal bodies moving with compensating reciprocally proportional speeds, derives from a dynamical interpretation of the equilibrium conditions of the simple machines. Instances in which the quantities of motion of the bodies are not equal are handled by annulling as much motion of the larger and/or faster moving body as the smaller and/or slower body possesses (Beeckman, 1939–53, Vol. i, p. 266). This in effect reduces the smaller and/or slower body to rest. The outcome of the collision is then calculated by distributing the remaining unannulled motion of the larger and/or swifter body over the combined mass of the two bodies (ibid.). It is obvious that Beeckman viewed this case through a two-fold reference to the simple machines; for he first extracts as much motion as can conduce to the equilibrium condition for symmetrical cases, and then he invokes the principle cited just above in this note to determine the final outcome.

[53] Beeckman (1939–53), Vol. i, p. 266 and Vol. iii, pp. 133–4. Consider, for example, his commentary upon a remark made in Mersenne (1627) to the effect that 'Vitesse ou tardivité du mouvement cause de tout ce qui se fait par bilances'. As Beeckman's entry shows, he fundamentally agreed with this dynamicist interpretation of the principles of the simple machines: 'The reason for this fact can be rendered very easily by those things which I wrote a little before concerning motion. For it follows from them that a sphere twice as heavy [as another sphere], that is, having twice as much matter, but moving twice as slowly [as the other sphere], will be stopped after colliding with it, that is, both spheres will be at rest. For I specified that mass and motion compensate for one another [se reciprocari]. The same thing must also be concluded concerning the balance.' Despite some confusions Beeckman introduced in the explication of this point, his central contention is clear enough: even macroscopic equilibrium is a consequence of the laws of motion and impact, because it can be explained through a dynamical interpretation of countervailing motions on the model of the laws of collision. He closes his account with a clear statement of this point: 'One should not doubt how an account is given here of the theory of equilibrium [in isorhopicis] by means of motion. For even if there is no motion when bodies hang in equilibrium, motions would however take place immediately if an external force, a weight, etc were to displace these weights from equilibrium. Moreover, all bodies that return to their own places as soon as they are moved from them never change their places of their own accord. Thus stones never ascend spontaneously and in the absence of an external force. Bodies which are at rest in our vicinity never spontaneously move . . . The cause of equilibrium therefore can be motion, even if the bodies in equilibrium are not moved. For the cause of equilibrium is past and future motion. During the present, to be sure, the body is at rest because past and future motions occasion rest' (ibid., Vol. iii, pp. 133–4).

sort of enterprise. In many cases merely qualitative reports of phenomena are so reduced; but in other cases one is dealing with quantitative representations of phenomena which had already been achieved in the practical mathematical sciences, as is the case in Beeckman's reading of laws of collision out of exemplary findings in the (dynamical) interpretation of the simple machines. Beeckman's questioning of Descartes about Stevin's 'paradoxical' hydrostatical findings arguably sits squarely within this practice. This was what Beeckman and Descartes were envisioning when in 1618 they congratulated themselves on being virtually the only '*physico-mathematici*' in Europe. What they meant was that only they unified the mathematical study of nature with the search for true corpuscular-mechanical causes.[54] Beeckman wanted to see what his new friend and fellow 'physico-mathematicus' could do about reducing Stevin's work to corpuscular mechanical terms, thereby fundamentally explaining it.

The hallmark of Descartes' natural-philosophical work, even in the early days of 1619, was to assign causes to phenomena by postulating the underlying mechanical behaviour of corpuscles. This is precisely what is occuring in the hydrostatics manuscript. Descartes had learned from Beeckman that when you explain a machine by its parts and their motions you simultaneously deal with it mechanically and in terms of its matter and the properties of that matter. In the hydrostatics manuscript, which we shall be analysing in the next section, we shall see Descartes reducing Stevin's macro-analysis in descriptive geometry to the underlying 'machinery'—the material parts, their arrangements and motions, or the kind of matter involved and its properties, how it 'naturally' behaves. The idea of 'underlying machinery' takes Descartes from mechanics as a general, dynamically interpreted science of machines, which falls within practical mathematics, to mechanics as a general causal account of underlying corpuscular machinery, that is, of matter and motion. Moreover we shall see in Section 7 that this marked many of the key moments in his work at the interface of mixed mathematics and mechanistic natural philosophy. In 1620, he attempted precisely the same move in unpacking what he took to be a great insight of Kepler, who had suggested that light moves with more force in denser optical media and 'hence' is bent toward the normal in moving from a less to a more dense medium. Moreover, the principal step in Descartes' constitution of a physical optics, which would have an exemplary role in his mature natural philosophising, occurred directly after his discovery of the law of refraction in 1626/7 in a simple geometrical form (as a law of cosecants), when he literally read out of his key geometrical diagram the principles of a micro-mechanical theory of light which would then subsume the new macro-geometrical law which had prompted them in the first place.[55] In short, after his initial interaction with Beeckman, Descartes always interpreted the search

[54] AT, Vol. x, p. 52. In this regard Beeckman was to note in 1628 that his own work was deeper than that of Bacon on the one hand and Stevin on the other just for this very reason: Beeckman (1939–53), Vol. iii, pp. 51–2, 'Crediderim enim Verulamium [Francis Bacon] in mathesi cum physica conjugenda non satis exercitatum fuisse; Simon Stevin vero meo judico nimis addictus fuit mathematicae ac rarius physicam ei adjunxit.'

[55] For details see Schuster (2000).

for causes in natural philosophy as the search for real corpuscular models worked according to principles of a mechanics, indeed a dynamics, specifying the causal principles at work in the microscopic realm. Those like Galileo, who theorised at the level of macroscopic geometrical regularities, would be accused of 'building without foundation' in much the same way that Beeckman identified 'physico-mathematics' with a proper balance of the mathematical and the physical (natural philosophical), using Stevin and Bacon respectively as examples of those who cleaved too much to the erroneous extremes of this continuum.[56]

However, granting all this, we are not saying that Descartes was slavishly following Beeckman—not in 1618 and certainly not later. Even when he was pursuing his first corpuscular-mechanical researches with Beeckman in 1618, Descartes was dissatisfied with details of Beeckman's speculations. What Descartes asserts in the hydrostatics manuscript does not map directly onto Beeckman's detailed conceptualisation. Right from the start he proceeds not via a dynamical interpretation of the *Mechanica* account of the lever, but rather via an Archimedian account which he fleshes out in terms of the micro-corpuscularian model he learned from Beeckman, albeit with the details significantly revised. Indeed he ultimately did not accept Beeckman's formulation of the principles of mechanics, or causal register of corpuscular mechanism. Moreover, by the early 1630s and quite possibly even earlier, Descartes had invented a system of mechanics, applied to corpuscles as the causal dimension of his discourse, which was based on concepts owing little if anything to the teachings of Beeckman, and largely grounded in his struggles over issues in geometrical and physical optics.[57] Nevertheless we should remember that these and other differences can only be assessed from within a perspective which recognises the influence of Beeckman on the original formation of Descartes' view of micro-mechanical natural philosophy, and its relation to the practical mathematical sciences, in particular as evidenced in the work on hydrostatics.

5. Descartes' micro-corpuscular reduction

What Descartes learnt above all from Beeckman in 1619 was that laws or generalisations expressed in macroscopic terms required deeper 'natural-philosophical' explanation by reference to the mechanical behaviour of micro-corpuscles. But the way in which he fills out this requirement goes beyond this point, and generates some very distinctive Cartesian ideas, such as the extensive use of 'tendencies to motion' rather than motions proper.

The hydrostatics manuscript is concerned with four problems, of which we shall only need to discuss one in detail. It is clear that the problems were culled from the work of Stevin and that Beeckman set the problems to Descartes, probably as an

[56] AT, Vol. ii, p. 385.
[57] See Schuster (2000).

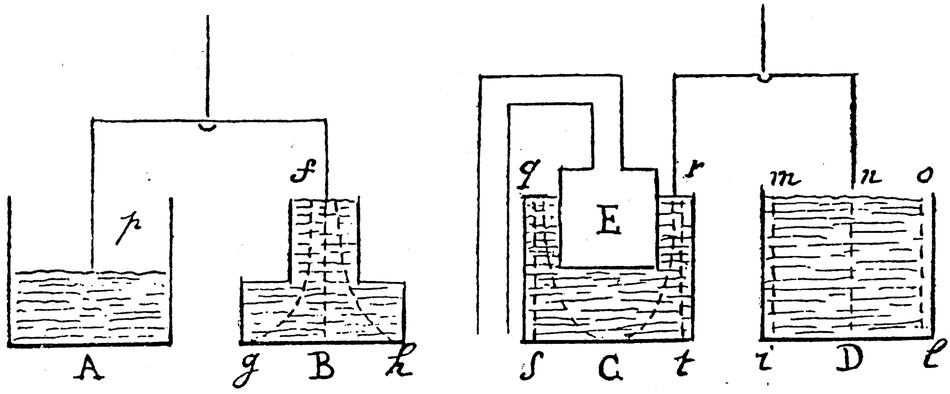

Fig. 3.

exercise in their style of 'physico-mathematics'. Descartes takes as given the following conditions (Fig. 3):

> . . . let there be four vases of equal width at the base, of the same weight when empty and of the same height. Let A be filled with only as much water as B can contain, and let the remaining three be filled as much as possible.

It is proposed to show that

> . . . the water in base B will weigh equally upon the base of the vase as does the water in D upon its base, and consequently each will weigh more heavily upon their bases than the water in A upon its base, and equally as much as the water in C upon its base.[58]

This is the key problem, because it refers to Stevin's important findings about the hydrostatic paradox. Beeckman, with his theoretical and practical concerns in hydrostatics and his corpuscular-mechanical aims in natural philosophy, was probably curious about how Descartes would explain this fundamental but strange result.

While Stevin's approach is geometrical, Descartes' analysis and explanation are by contrast based on an attempt to reduce the phenomenon to micro-mechanical terms. The hydrostatics manuscript implies the judgment that Stevin's macro-geometrical arguments and results can only be truly explained in terms of the corpuscular-mechanics of fluids. Thus, it reflects the opinion of the two 'physico-mathemat-

[58] AT, Vol. x, pp. 68–9. This is the second of the four puzzles posed in the text, the others are: '(First), the vase A along with the water it contains will weigh as much as vase B with the water it contains. . . . Third, vase D and its water together weigh neither more nor less than C and its water together, into which *embolus* E has been fixed. Fourth, vase C and its water together will weigh more than B and its water. Yesterday I was deceived on this point.'

ici', cited earlier, that Stevin was too given to mathematics and insufficiently concerned with the physical causes residing behind his mathematical representations of macroscopic bodies and phenomena.[59] Yet while the judgement of the two 'physico-mathematici' corresponded, the understandings of 'physico-mathematics' on which this judgement was reached differed very significantly.

Descartes' intention of reducing the problem to micro-mechanical terms emerges in the opening four paragraphs of the report. He complains that a full account would require a good deal of explanation of the 'foundations' of his 'mechanics'. It soon becomes apparent that this would have been a system of corpuscular mechanics justifying the sorts of arguments employed in the remainder of the manuscript. Descartes asks us to accept a series of assumptions. First, he claims that of the various ways in which bodies may 'weigh-down' [*gravitare*] only two need be discussed: the weight of water on the bottom of a vessel which contains it, and the weight of the entire vessel and the water it contains.[60] Descartes' later discussion shows that by the weight of the water on the bottom of the vessel he does not intend the gross weight of the quantity of water measured by weighing the filled vessel and subtracting the weight of the container itself. He means instead the total force of the water on the bottom arising from the sum of the pressures exerted by the water on each unit area of the bottom. Secondly, the term 'to weigh down' is explicated as 'the force of motion by which a body is impelled in the first instant of its motion'. Descartes insists that this force of motion is not the same as the force of motion which 'bears the body downward' during the actual course of its fall.[61] Finally, one must, Descartes contends, attend to both the 'speed' and the 'quantity of the body', since both factors contribute to the measure of the 'weight' or force of motion exerted in the first instant of fall. He explains that

> if one atom of water about to descend would be twice as fast as two other atoms, the one atom alone will weigh as much as the other two together.[62]

These three suppositions mark the first appearance of some fundamental notions of Cartesian natural philosophy, at least in forms from which later, mature versions are clearly descended. Weight or heaviness reduces to the mechanical force exerted by a particle in its tendency to motion of descent. Moreover, as will be more clear later in the text, the 'weight' of a body is now affected by the mechanical constraints and conditions of its surroundings. Far from being an essential and invariable quality of bodies, weight is now a purely derived mechanical quality jointly determined by the size of the body and its tendency to motion as conditioned by a given configuration of neighbouring bodies. To be sure, Descartes does not offer anything like his mature version of these ideas. For example, there is little hint of the systematic

[59] Beeckman (1939–53), Vol. iii, pp. 51–2.

[60] AT, Vol. x, p. 68.

[61] AT, Vol. x, p. 68. In the *Cogitationes Privatae* (AT, Vol. x, p. 228) the inclination to motion is described as being evaluated 'in ultimo instanti ante motum'.

[62] AT, Vol. x, p. 68.

composition and resolution of tendencies to motion which he would employ later, especially in his mechanistic optics. Nor does Descartes generalise from ‘weight’ considered as a tendency to motion to the decomposition of real motion into momentary states of tendency to motion. This move, which would be the key element in his mechanistic optics and general system of dynamics (i.e. what in 1619 in its embryonic form he called his ‘mechanics’), only emerged in the 1620s.[63] On the other hand, Descartes does develop and articulate the concept of tendency to motion to a certain extent as he struggles to apply it consistently throughout the manuscript. We should also note that Descartes’ measure of the force of motion shows the imprint of Beeckman’s ideas and their common source in a dynamical interpretation of the simple machines and ultimately therefore in the *Mechanica* tradition. As Milhaud long ago observed, this is apparent both in Descartes’ concern with the first instant of descent, which is, so to speak, permanently maintained in dynamical equilibrium, and in the evaluation of force as the product of quantity of matter and potential or nascent velocity.[64]

Descartes next solves the problem of accounting for the hydrostatic paradox. But, whereas Stevin had offered an argument from macroscopic conditions of equilibrium, Descartes manufactures a curious exercise in *ad hoc* micro-mechanical reductionism. He proposes to demonstrate the statement by showing that the force on each ‘point’ or part of the bottoms of the basins B and D is equal, so that the total force is equal over the two equal areas.[65] He does this by claiming that each ‘point’ on the bottom of B is, as it were, serviced by a unique line of ‘tendency to motion’ propagated by contact pressure from a point (particle) on the surface of the water through the intervening particles. (See Fig. 3.)

> For example, let there be determined in one base the points g, B, h; in the base of the other, i, D, l. I contend that all these points are pressed by an equal force, because to be sure, they are each pressed by imaginable lines of water of the same length; that is, from the top part of the vase [water level] to the bottom. For line fg is not to be reckoned longer than fB or [any] other line. It does not press point g in respect to the parts by which it is curved and longer, but only in respect to those parts by which it tends downward, in which respect it is equal to all the others.[66]

At least the latter portion of this passage is initially plausible. Assuming that the points on the bottoms are indeed served by unique lines of tendency transmitted from points on the surface, then, in so far as we are only concerned with the tendency

[63] On Descartes’ optics and its connection to his mature dynamical conceptions, see Schuster (2000).

[64] Milhaud (1921), p. 34.

[65] Descartes consistently fails to distinguish between ‘points’ and finite parts. But he does tend to assimilate ‘points’ to the finite spaces occupied by atoms or corpuscles. Throughout we shall assume that Descartes intended his points to be finite and did not want his ‘proofs’ to succumb to the paradoxes of the infinitesimal.

[66] AT, Vol. x, p. 70.

to descend, we may compare the lines of tendency in respect to their vertical ‘components’. However, the procedure of mapping the lines of tendency is quite curious. Descartes can perhaps be taken to imply that when the upper and lower surfaces of the water are similar, equal and posed one directly above the other then unique normal lines of tendency will be mapped from each point on the surface to a corresponding point directly below on the bottom. But, when these conditions do not hold, i.e. when the upper surface of the water differs from the lower in respect of size and/or shape, or when it is not directly posed above the bottom, then some other unstated rules of mapping come into play. It would seem that in the present case the area of the surface at f in the basin B is precisely one third that of the bottom, so that each point or part on f must be taken to service three points or parts of the bottom. The problem, of course, is that no explicit criteria or rules for mapping are, or can be, given. Descartes makes no attempt to justify the three-fold mapping from f. He merely slips it into the discussion as an ‘example’ and then proceeds to argue that *given the mapping*, f can indeed provide a three-fold force to g, B and h.

In fact, the demonstration continues solely as a justification of the three-fold efficacy of f, rather than as a general demonstration of the problem, such as we might expect:

> It must be demonstrated, however, that point f alone presses g, B, h with a force equal to that by which m, n, o press the other three i, D, l. This is done by means of this syllogism. Heavy bodies press with an equal force all neighbouring bodies, by the removal of which the heavy body would be allowed to occupy a lower position with equal ease. But, if the three points g, B, h could be expelled, point f alone would occupy a lower position with as equal a facility as would the three points m, n, o if the three other points i, D, l were expelled. Therefore, point f alone presses the three points simultaneously with a force equal to that by which the three discrete points press the other three i, D, l. Therefore, the force by which point f alone presses the lower [points] is equal to the force of the points m, n, o taken together.[67]

Let us note the structure of this argument, for it is of some significance in understanding important aspects of Descartes’ later natural-philosophical views. The demonstration depends on taking the mapping as given and then imagining g, B and h to be removed or the spaces below them opened. Descartes then asks whether it is not obvious that f would descend with equal ease toward each one of the three points and that it thus exerts a tendency to descend upon each one of them. In addition, it is implied that in working out the hypothetical case of descent, Descartes imagines away the rest of the fluid, *qua* fluid. That is, it is in effect hypothetically solidified, so that its behaviour does not complicate the postulated mechanical relations between f and g, B and h. There is thus a three-fold displacement away from what one might consider the original terms of the problem: Descartes assumes an *ad hoc* mapping;

[67] AT, Vol. x, pp. 70–1.

invokes a hypothetical voiding and consequent motion; and, finally, implicitly solidifies parts of the fluid not involved in the first two steps. The proof of this 'example' is then taken as a general demonstration without any indication as to how the procedure is to be generalised to all the points or parts in the surfaces.

This idiosyncratic mode of argument has a much greater significance than might appear at first sight, because Descartes would make precisely the same moves in important areas of his later natural philosophy. In fact, the three-fold technique by which Descartes evades the problem forms, as it were, a fairly consistent motif or style of explanation in his later work. Much of his aerostatics and cosmological mechanistic optics employs these sorts of arguments. The key to this later style of natural philosophical argument resides in his propensity for explanations based on the attribution of tendencies to motion to corpuscles in various states of rest, motion and spatial relation. Often, tendencies to motion are represented by geometrical lines which in turn are analysed in order to yield the required explanation. But Descartes was never to make explicit the rules guiding the attribution of 'lines of tendency'; just as, in the present problem, he baldly presents his mapping of tendencies, yet cannot justify or even rationalise his choice. In his later work he would typically try to establish the mechanical efficacy of the lines of tendency chosen for the problem, granted their existence and precise configuration in the first place. As we have just seen, this *post facto* justification proceeds by means of the hypothetical voiding of the region toward which the relevant particles are said to tend. An analysis of the resulting hypothetical motion is used to buttress the claim for the efficacy of the particular configuration of lines in question. All this in turn renders comprehensible the *de facto* 'solidification' of parts of the medium which he employs in this problem and would use again in his theory of light. The solidification is the conceptual corollary of mapping lines of tendency between specially chosen 'privileged surfaces', and those privileged surfaces would also reappear in the theory of light in *Le Monde*.

This mode of explanation haunts so much of Descartes' physical thought that we would venture to suggest that it goes a long way toward accounting for the curiously tendentious and idiosyncratic character of much of his natural philosophy. After all, the style of explanation we have been analysing really consists in an ongoing series of *ad hoc* manipulations. The manipulations masquerade as clarifications, while in fact they condition a progressive loss of contact with the original aims of the problem. They close in on themselves, forming a superficially tidy universe of discourse increasingly irrelevant to the problem at hand and insulated from any fruitful return to new empirical information.

6. Force of motion

A different set of conceptual problems was raised by a part of Descartes' argument which we have not yet examined. Descartes did resolve these problems with a degree of success and in so doing was able to clarify some of the central ideas of his mature system of dynamics: that is, what in 1619 he terms his mechanics—the causal register in his nascent mechanistic natural philosophy. The difficulty surrounds an ambiguity or tension in the formulation of the concept of ‘tendency to motion’. Descartes had two concepts through which to express the ‘tendency to motion’ of a body. On the one hand, in his second ‘assumption’ he speaks of the ‘force of motion by which [a body] is impelled in the first instant of motion’. Here ‘force of motion’ is used in a manner similar to that in which it would later be employed in Descartes’ mechanistic optics or in *Le Monde*. It bears the connotation of an efficacy or force characterising the body during an instant (specifically the first instant) of its motion. By contrast, when Descartes specifies the measure of ‘tendency to motion’ in his third ‘assumption’, he introduces the notion of speed:

> in that first imaginable instant of motion, we must take note also of the imaginable beginning of the speed by which the parts of the heavy body descend.[68]

Hence it turns out that one dimension of the instantaneous efficacy or ‘force of motion’ is constituted by the speed of the body. The ambiguity begins to appear at this point because, in order to assimilate speed to instantaneous force, Descartes tries to introduce the notion of an ‘imaginable beginning of speed’. This phrase in effect deflects the kinematic connotation of speed over a finite interval of space or time toward an idea of instantaneous speed. However, the manoeuvre leads to a degree of ambiguity when Descartes later tries to evaluate real instantaneous tendencies (i.e. forces of motion) by reference to a set of hypothetical but ‘kinematic’ speeds. The kinematic connotation then reasserts itself, and Descartes is left saying that the body has a tendency to a triple speed when, in fact, it can attain only ‘one’ speed in case of a finite translation being actualised.

We see this issue played out in Descartes’ explanation of the three-fold force of motion of the point f. He first evaluates the total tendency to motion of f by attributing to it three units of instantaneous speed arising from the three paths of descent caused by hypothetically voiding g, B and h:

> . . . let all the lower points g, B, h and i, D, l be imagined to be opened at the same instant by the force of gravity of the superposed bodies. Certainly it will have to be conceived that in the same instant point f alone will move three times more quickly than each of the points m, n, o. For in that instant three places will have to be filled by the former [f], while only one place will have to be occupied by each of the points, m, n, o.[69]

Then he translates the result into a total force of motion, as we have already seen:

> Therefore, the force by which point f alone presses the lower [points] is equal to the force of the points m, n, o taken together.[70]

[68] AT, Vol. x, p. 68.
[69] AT, Vol. x, pp. 70–1.
[70] AT, Vol. x, p. 71.

Descartes' argument can be rendered as follows. Point f will descend along all lines fg, fB and fh with the same 'natural' speed of descent. Since all three lines materialise at once, f must have three units of speed at once. But three speeds implies a three-fold force of motion and hence f can have as much 'weight' as m, n and o put together. The term 'speed' can mediate between the consequences of the three cases of hypothetical voiding and the reckoning of the total force of motion, because it signifies both the finite but hypothetical translations and a dimension of the measure of instantaneous force of motion.

Descartes quickly realised that the multiple speeds calculated for the hypothetical voiding are difficult to reconcile with the intuitively plausible idea that a body should be able to actualise its instantaneous force of motion as a commensurable real speed of descent. He saw that the dual role of 'speed' was to blame, for it allows one to slide easily between tendencies expressed as 'speeds' and actualised tendencies measured by 'speeds'. Viewed in terms of the triple voiding, f has a three-fold instantaneous speed at the first moment of descent, but if any real translation were to occur, it would obviously occur in one direction and at one speed only. We might say that f cannot really fall in three directions at once; or that its triple 'potential' speed can only be realised as a single unit of 'actual' speed. As Descartes put it,

> . . . an objection can be offered, which in my opinion is not to be disregarded, and the solution of which will confirm the foregoing. All bodies of equal magnitude and weight, if they should be borne downwards, have some certain equal mode of speed, which they do not exceed unless they are impelled by some extraneous force. Thus it is wrongly assumed above that point f is inclined to move three times more quickly than any one of the points m, n, o, since it cannot be said to be impelled by any external force.[71]

To his credit, Descartes perceived that the difficulty is a conceptual one requiring a more precise notion of the relation between 'tendency to motion' and 'motion', as well as the avoidance of loose talk about multiple instantaneous speeds:

> I respond in this way to the objection. The antecedent is quite true; however, it is erroneously deduced from it that the point f is not able to incline to a triple velocity. For there are two different considerations in relation to weight which must be distinguished: inclination to motion and motion itself. For bodies which tend downwards are not inclined to move to the lower place with this or that speed, but rather they are inclined to move there as quickly as possible. Whence it happens that point f is able to have a triple inclination, since there are three points through which it is able to descend. The points m, n, o each have a unitary inclination, since there is only one point through which each can move respectively.[72]

[71] AT, Vol. x, p. 71.
[72] AT, Vol. x, p. 72.

Through a conceptual reshuffling, Descartes is prepared to accept both horns of the dilemma. He grants that only one real speed can possibly be actualised and he still insists on the triple inclination. However, he has altered his understanding of inclination. It is now obvious that multiple inclinations are not and need not be translatable into multiple real motions. Clarification is achieved by insisting on a consistent dualism between 'motion' and 'tendency to motion', or 'speed' and 'inclination to speed': Descartes' phrase is *ad triplicem celeritatem propendere*. The real translation—motion or velocity—of a body cannot be evaluated in terms of the manifold tendencies to motion it may possess at any moment owing to the mechanical conditions in which it is placed. Conversely, the fact that only one real translation can be attributed to a body does not alter the truth of mechanics that bodies such as f can press down on several bodies at once in several different directions.

The most striking thing about the passages just discussed is that they show Descartes in the very act of reformulating some of the concepts of his dynamics of corpuscles, his 'mechanics', as he struggles to solve the problem at hand. Descartes' mechanistic optics, as it developed in the later 1620s, and his general system of dynamics in *Le Monde*, are based on the configuration of concepts which begins to emerge in these passages. Cartesian mechanistic optics and natural philosophy will mainly depend on the analysis of instantaneous tendencies to motion, rather than finite translations. Indeed, Descartes dissolves real translation into a series of inclinations to motion exercised in consecutive instants of time at consecutive points in space. Moreover, many of Descartes' explanations will require the consideration of multiple tendencies to motion which a body may possess at any given instant, depending on its mechanical circumstances. In such cases Descartes will be careful to employ the terms 'tendency to motion' or instantaneous 'force of motion', rather than 'motion' or 'speed', so that he may avoid the consequence that the real speed of a body varies with the number of different tendencies to motion one attributes to it at any given instant. In short, Descartes will insist that instantaneous tendency to motion can be resolved into various configurations of its 'components', but that real motion cannot be so analysed, lest different sums result for the total quantity of motion of the body, the system to which it belongs, or the cosmos as a whole.

After responding to the objection that he has confused real motion with tendency to motion, Descartes adds that he described lines fg, fB, mi and so on 'not because' he wanted 'a mathematical line of water to descend', but rather for the 'easier comprehension of the demonstration'. He then closes the paragraph by remarking,

> For, since I speak here about things which are new and my own work, much must of necessity be supposed, unless they are to be explained in a complete treatise; therefore I judge that it is sufficient that I demonstrate that which I have undertaken.[73]

This treatise was presumably to deal with the 'mechanics' mentioned at the begin-

[73] AT, Vol. x, p. 72.

ning of the manuscript. Hence it would have contained the principles for a complete justification of the hydrostatic argument. Such a treatise on 'mechanics' is also mentioned twice in the early correspondence between Descartes and Beeckman in the spring of 1619.[74] It may be conjectured that Descartes' planned treatise of mechanics would have had to have been quite different from the classical model of treatises in statics or hydrostatics, such as those of Archimedes or Stevin. Unlike the latter thinkers, Descartes was not primarily interested in a macro-geometrical mechanics in which mathematical rigour was achieved by arguing through cases of static equilibrium. In order to legitimate the approach taken in the hydrostatics manuscript, which was, after all, only a special exercise in Beeckman's kind of micro-mechanism, Descartes' treatise would have had to have dealt with the mechanics of corpuscles. This could have included a micro-mechanics of moving particles concerned with the laws of collision, as already pursued by Beeckman, as well as a mechanics of tendencies to motion, including, of course, a discussion of the representation of tendencies through geometrical lines—a style of mechanics more typically Cartesian, as evidenced in the manuscript and throughout his subsequent work This entire undertaking, in its embryonic and somewhat disjoint state in 1619, represented what Beeckman and Descartes then termed 'physico-mathematics'.

The fact that the hydrostatics manuscript depends on the geometrical representation of underlying patterns of tendencies to motion raises one final issue about the early gestation of Descartes' programs which we can only mention briefly here, although it is part of any larger account of Descartes' early struggles. The solving of natural-philosophical problems by the analysis of corpuscular tendencies to motion represented as geometrical lines may have provided Descartes with one of the first hints of that grandiose notion of a universal mathematical science of nature which was to take hold of him during the course of 1619. Descartes expressed the idea of such a general science in a part of Rule 4 of the *Regulae ad directionem ingenii,* written some time between March and November 1619.[75] If the hydrostatics manuscript is any guide to the original meaning of the term 'universal mathematics', we see that it was to be a general mathematical approach to natural philosophy: a science amenable to rigorous modes of argument because the objects of its analyses are geometrical lines; and yet a science of nature, because the lines represent and objectify an underlying micro-mechanical realm.[76]

[74] AT, Vol. x, p. 159, l. 11–12; p. 162, l. 15.

[75] See Weber (1964); Schuster (1980) and Gaukroger (1995), Ch. 4.

[76] This conjecture concerning the hydrostatics manuscript points to still another related issue in the development of Cartesian mechanism. Schuster (1980) shows that it eventually evidenced a tension between two visions of a science of nature. On the methodological plane Descartes came to advocate in the *Regulae* a macro-geometrical science founded on the orderly resolution of problems involving the relations of the macro-mathematical properties of bodies. Yet, there is implicit in the *Regulae* an undercurrent of interest in a more properly micro-mechanical science based on the imputation of properties and relations to unobservable corpuscles. This science eludes the methodological framework Descartes tried to set up in the *Regulae*, but, paradoxically, it is directly implicated in Descartes' explanation of how knowledge of macro-geometrical properties is possible. This problem is perhaps one of the reasons why Descartes left the *Regulae* unfinished and soon turned his attention to micro-mechanical natural philosophy

In sum, it should be realised that the hydrostatics manuscript does not contain a preliminary sketch of Cartesian mechanistic natural philosophy, nor *a fortiori* even a summary of what he was calling his ‘mechanics’, the core of the dynamics, or causal register of that mechanistic natural philosophy to come. But, as we have already shown, the hydrostatics manuscript does reveal fundamental elements of Descartes’ approach to mechanistic explanation against a background of matter-theoretical concerns. In 1619 Descartes had already learnt from Beeckman that laws or generalisations expressed in macroscopic terms, including those generated in the mixed mathematical sciences, required deeper ‘natural-philosophical’ explanation by reference to the mechanical behaviour of micro-corpuscles.

7. The hydrostatical model in the 1620s

At the end of Section 3, we noted that Descartes hoped that by exploring what he took to be the latent natural philosophical underpinnings of statics he could uncover a wholly general, but non-Aristotelian, natural philosophy. We also argued that the underlying natural philosophy of Aristotle had provided the unification of mechanical disciplines offered in the *Mechanica*, so that if one removed that natural philosophy, then, as Galileo learned, the unification did not remain intact; that is, the unification had to be produced by the natural philosophy, rather than grafted on to it. We have now established that this was precisely Descartes’ agenda in 1619. He attempted to reduce Stevin’s hydrostatics to an embryonic corpuscular mechanism in which discourse concerning causes or ‘forces’ provided the basis for pursuing the practical mathematical sciences, no longer construed as ‘subordinate sciences’ or ‘mixed mathematics’. Then in Section 6 we established precisely how, within the text of the hydrostatics manuscript, Descartes moved in the direction of formulating an early version of a dynamics of corpuscles, and we foreshadowed the relation of those early concepts to the mature dynamics he developed by the time he composed *Le Monde*.

In Descartes’ later mechanistic optics and corpuscular-mechanical natural philosophy the behaviour of micro-particles was governed by a carefully articulated theory of dynamics.[77] Descartes held that bodies in motion, or even merely tending to motion, can be characterised from moment to moment by the possession of two sorts of dynamical quantity: (1) the absolute quantity of the ‘force of motion’; and (2) the directional modes of that quantity of force, which Descartes termed ‘determi-

in *Le Monde*. The hydrostatics manuscript provides us with the earliest expression of this tension, because it involves the juxtaposition of the same two positions which reappear in the *Regulae*. We have in the hydrostatics manuscript the attempt to integrate the methodological clarity of geometrical representation and argument with a fundamental commitment to an ontology of mechanical corpuscles. And the manuscript is all the more interesting for revealing this juxtaposition before Descartes had crystallised either his methodological or natural philosophical aspirations. Descartes’ life-long claim that his micro-mechanical natural philosophy was ‘geometrical’ may have had one of its sources in the attempt to re-write the work of Stevin into Beeckman’s terms in the hydrostatics manuscript.

[77] See for example Gabbey (1980); Garber (1992); Gaukroger (1995, 2000a); McLaughlin (2000); Schuster (2000).

nations'. As corpuscles undergo instantaneous collisions with each other, their quantities of force of motion and determinations are adjusted according to certain universal laws of nature, rules of collision. Descartes' emphasis therefore falls on the analysis of instantaneous tendencies to motion, rather than finite translations in space and time. Indeed, Descartes offers a metaphysical account of real translation which dissolves it into a series of inclinations to motion exercised in consecutive instants of time at consecutive points in space. As we have noted, many of Descartes' explanations require the consideration of multiple tendencies to motion which a body may possess at any given instant, depending on its mechanical circumstances. In such cases Descartes employs the terms 'tendency to motion' or instantaneous 'force of motion', rather than 'motion' or 'speed', avoiding, as in 1619, the consequence that the real speed of a body varies with the number of different tendencies to motion one attributes to it at any given instant. Thus everything now points to the origins of this mature dynamics residing in the statical and hydrostatical themes (and results) of his early physico-mathematics.

It is important to realise, however, that although the dynamics of *Le Monde* and the *Dioptrique* bear the direct marks of descent from the embryonic dynamical considerations of 1619, there is a considerable degree of refinement and elaboration involved. The path between these two points was not marked out in statical or hydrostatical research, but rather in Descartes' long and tortuous course of research in geometrical and physical optics between 1620 and the composition of the *Dioptrique* by 1633. This path has been documented in detail elsewhere,[78] but it can be briefly summarised here to tease out the connections we are discussing.

Around 1620 Descartes explored Kepler's speculations about the refraction of light, using his newly acquired physico-mathematical style of 'reading' geometrical diagrams representing phenomena for their underlying message about the causal principles at work. This yielded notions about the dynamics of light that actually blocked his access to the law of refraction. It was only in 1626/7 that Descartes, in collaboration with Claude Mydorge, discovered the law of refraction. This discovery took place entirely with the confines of traditional geometrical optics, without the benefit of dynamical or corpuscular-mechanical theorising. It was based on the analysis of data on angles of incidence and refraction, taken in conjunction with a traditional geometrical optical assumption concerning the location of the refracted images of point sources. But, after the discovery of the law of refraction by purely geometrical optical means, Descartes looked for better conceptions of the dynamics of light by which to explain the law. These he found by deploying the physico-mathematical style he and Beeckman had pioneered in 1619: that is, he literally transcribed into dynamical terms some of the geometrical parameters embodied in his initial diagrammatic representation of the law. The resulting dynamical principles concerning the mechanical nature of light foreshadowed, and indeed suggested, the form of the two central tenets of his mature dynamics mentioned above—after all, what could be more revealing of the underlying principles of the punctiform dynamics of micro-

[78] See Schuster (2000).

corpuscles than the basic laws of light, itself an instantaneously transmitted mechanical impulse?

These principles were then put back to work in their site of discovery, in Descartes' derivation of the laws of reflection and refraction of light in the *Dioptrique*. The underlying dynamical structure of these proofs was occluded, however, by the fact that in the *Dioptrique* he tried to model his theory of light as an instantaneously propagated mechanical impulse by means of a kinematic model involving the motion of tennis balls. The model allowed Descartes to appeal to the translation and rotation of the balls, this then being used in his theory of colour as applied to the rainbow and other phenomena. Nevertheless, the proofs in the *Dioptrique* can be decoded to expose the underlying pattern of dynamical explanation, based on consideration of the instantaneously exerted quantity and directional quantity of 'force' of a light ray, and its instantaneous alteration at the moment of reflection or refraction.

In sum, by 1633 Descartes had arrived at general principles of a statically based dynamics, and applied them to an exemplary achievement in optics, thus vindicating the broader agenda and thrust of his earliest physico-mathematical explorations with Beeckman in 1619. He also had elaborated the first version of a complete system of corpuscular-mechanical natural philosophy on the basis of these findings. In Descartes' own mind, at least, the ambitious but rather vague and embryonic physico-mathematical project of 1619 had borne profound and vast dividends. His youthful aim of building a novel corpuscular-mechanical natural philosophy which would entrain new, non-Aristotelian, productive and physically revealing relations between natural philosophising and the mathematically based physical disciplines had been realised.

8. Conclusion

Descartes' natural philosophy came to fruition in the mid-1620s and early 1630s, as he developed a mechanistic optics and then a mechanistic cosmology. The rudimentary dynamical concepts that he formulated in dealing with these areas clearly show their origins in his early studies in statics and hydrostatics. The case of cosmology is particularly striking in this regard. In the cosmos of *Le Monde*, physical effects are produced by means of the vortical motions in an all-encompassing fluid. It is this motion that results in the breaking up of matter into three orders of size, with centrifugal force pushing the bulkiest matter (which makes up the planets and their satellites) outwards from the centre towards the periphery, and squeezing the lightest matter back into the centre, where it forms a sun in each of the indefinite number of solar systems that make up the cosmos. Pressure in this system, caused by the effects of centrifugal force and inertia in a contained fluid system, accounts for everything from the stability of planetary orbits to the propagation and transmission of light from the sun. There were of course fluid cosmologies before Descartes, and a number of writers on cosmology had interpreted the crystalline spheres as being regions of fluid, rather than crystal or ice, for example.[79] But, unlike those writers who liquified the crystalline spheres, Descartes' approach was not at all motivated by what he considers the celestial fluids to be made of. Rather, what drove it was the fact that looking at the behaviour of bodies in terms of the fluids that surround them, and with which they interact, was his canonical mode of thinking through physical problems. It was in this context that he developed his conceptions of force, action and tendency to motion: these concepts brought with them, even fifteen or twenty years later, a picture of an archetypical physical situation that derived from the hydrostatical context in which they were first worked out.

The hydrostatical model was initially developed in the context of overcoming a dichotomy between practical mathematics, which included mechanics, and which was conceived to work in terms of mathematical idealisations and hypotheses, and natural philosophy, which had traditionally been pursued via matter theory, and which was designed to provide an account of the physical world in terms of an idea of a fundamental reality underlying natural phenomena. The thought was that, by bridging this gulf, a quantitative natural philosophy might be possible; and this was the most pressing problem for those in the vanguard of physical theory in the early decades of the seventeenth century. There were many and varied attempts to deal with it, and the complexity of the issues raised by this problem has not always been fully appreciated. We have tried to show that an understanding of how mechanics can be articulated in natural-philosophical terms shapes the whole project of pursuing a physical theory, because it shapes the kind of understanding of physical processes one operates with, and the conceptual apparatus one is able to deploy in dealing with them. The formulation of a viable dynamics was, at bottom, nothing less than the question of how one pursues a quantitative natural philosophy, and, at least in the early decades of the seventeenth century, this hinged on the ability to flesh out mechanics in terms of matter theory, a project in which Descartes was the greatest pioneer.

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[79] See Gaukroger (2000b) and, more generally, Grant (1996).

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