THE CAMBRIDGE HISTORY OF PHILOSOPHY OF THE SCIENTIFIC REVOLUTION

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Mechanics in Newton's Wake

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Introduction: Received and New Historiographies

In his influential *Die Mechanik in ihrer Entwickelung* (Mechanics and its evolution) (1883), Ernst Mach asserted,

> The principles of Newton suffice by themselves, without the introduction of any new laws, to explore thoroughly every mechanical phenomenon practically occurring, whether it belongs to statics or to dynamics. If difficulties arise in any such considerations, they are invariably of a mathematical, or formal, character, and in no respect concerned with questions of principle.[1]

This view of "Newtonian" or "classical" mechanics had become orthodoxy by the middle of the twentieth century. For example, A. Rupert Hall wrote that

> the refinements of the French mathematicians in no way modified the essential principles of mechanics, which were already fixed, although their more penetrating analysis enabled some new problems to be solved, and some old errors to be corrected.[2]

Implicit in both Mach's and Hall's assessments was the belief that Isaac Newton had reached the zenith of mathematical-mechanical thinking in the long Scientific Revolution. As a corollary, both suggested that Newton's achievement went unchallenged by later generations and that the system of thinking attributed to him was preserved with only minor modifications. Well-known eighteenth-century developments in mathematical formalism were consequently portrayed as *merely* formal, with no significantly novel semantic content. The possibility that changes in the mathematical language of physics could alter the physics it expressed was given relatively little attention.

[1] Mach (1919, 256). [2] Hall (1956, 340).

The mid-twentieth century turn to externalism in the history and philosophy of science pushed the question even further into the background. Investigations of the internal logic of physical theories were increasingly abandoned in favor of detailed studies of external determinants. In addition, historians rejected the traditional, physics-centered narrative of the Scientific Revolution. They replaced it with a set of more complicated views, but insofar as the development of mechanics in the eighteenth century was concerned, the (much needed) historiographical change simply preserved the received view in amber.[3]

At the same time, however, scattered historians of mathematics began looking intently at technical issues in eighteenth-century mathematics.[4] Their work has since taken root, and is now being expanded in multiple directions, with greater attention being paid both to the technical detail of post-Newtonian mathematization and to the conceptual work it accompanied. As a result, historians have found significant differences between Newtonian and post-Newtonian mathematization,[5] as well as significant differences in conceptual frameworks.[6] A more plausible thesis has emerged: The technical and formal developments of mechanics were conceptually rich. New mechanical concepts were embodied in new formalisms, and their articulation altered, sometimes subtly, often radically and foundationally, Newton's original framework. Thus, Sandro Caparrini and Craig Fraser note in a review article that

> the appearance in the eighteenth century of new physical principles and modes of description was organically linked to the mathematical elaboration of a coherent theory of mechanics.[7]

[3] For the intricate development of Scientific Revolution narratives, see Cohen (1994). For a sampling of the newer views, see Shapin (1996); Henry (2008b), as well as this volume.

[4] Among the best are Boyer (1959); Truesdell (1966); Bos (1993).

[5] E.g., Hankins (1970); Fraser (1989); Blay (1994); Guicciardini (1994); Fraser (1997); Guicciardini (1999).

[6] Thackray (1970); Blay (1994); Blay (1998); Boudri (2002); Maglo (2003); Ahnert (2004). We cannot address the question of whether the changes being described in the secondary literature mark the eighteenth century as "revolutionary" or not, in a Kuhnian sense (Kuhn 1970). We believe, however, that revolution and continuity are most appropriately ascribed to small-scale changes rather than to large historical periods that are virtually always comprised of patterns of small-scale revolution and continuity. For example, the increasingly refined articulation in the eighteenth, nineteenth, and twentieth centuries of the positions and masses of solar-system bodies is clearly continuous with the research program developed by Newton (Smith 2014). This development may best be viewed through the Kuhnian notion of "paradigm articulation" (e.g., Kuhn 1970, 23). However, many of the changes in dynamical theory for which this refined articulation provided evidence (some of which are discussed below) are more revolutionary in character, and were presented as such at the time.

[7] Caparrini and Fraser (2013, 359).

The historiographical challenge is now to construct coherent narratives that articulate more precisely this organic link and its evolution. In this chapter, we hope to contribute to these narratives.

In keeping with a theme of this volume, our aim is to describe the emergence of the new discipline of analytical mechanics as a response to specific problems, both technical and conceptual, found in the lacunae of Newton's original framework. We also describe it as a profound, if partial, departure from what came to be known as Newtonian natural philosophy. The transition from seventeenth-century natural philosophy to eighteenth-century analytic mechanics thus represents an inflection point in the history of science.

Analytical Mechanics, the Mechanical Philosophy, Transformation, and Continuity

But what changed? At the end of the inflection we trace, Joseph-Louis Lagrange declared mechanics to be a branch of analysis. He claimed his *Mécanique analytique* (1788) contained no geometrical or mechanical reasoning. And, in fact, other than historical summaries that prefaced each of the work's two major sections, it contained little or no reasoning of the kind Lagrange's predecessors would have recognized as mechanical – no discussions of machines, mechanical analogies, the fundamental properties of bodies, or, especially, causes. Yet *Mécanique analytique* is full of what, today, we call "mechanics," and which we recognize as physics. (The work presents much of what current textbooks call "Lagrangian mechanics.") Newton's *Principia* was criticized upon publication as "merely" a work of *mathematique*, not *physique*,[8] primarily for not identifying true causes. A century later, receptivity to the idea of a work of natural philosophy that was almost entirely mathematical had changed.[9]

The sea change in mechanics was brought about by three more specific developments: the adoption of the analytic calculus, a growing technical independence from Newton's *Principia* (including its three laws), and the subsequent explosion of solved problems. The three developments are linked in the following ways. The new formalism, the analytic calculus, afforded increased uniformity and intelligibility to the presentation of problems

8 In a review (probably by Pierre-Sylvain Régis) in the *Journal des sçavans* (August 2, 1688). See Koyré (1965, 115).

9 For the ups and downs of mathematics in physical theory in the seventeenth and eighteenth centuries, see Mancosu (1996); Warwick (2003); Guicciardini (2013).

compared to Newton's geometrical presentation. Problems were solved by seeking functional relations that remained the same despite the complex changes that might be involved in variable forces acting on moving bodies. These fixed functional relations led to the discovery of higher-order invariant quantities, ones that measured parameters at levels above the observable changes themselves. They also led to the discovery of higher-order equilibrium conditions, e.g., those embodied in minimization techniques or Lagrange's "general formula of the movement of any system." In this way, the analytic formalism allowed all first- and higher-order changes (changes of changes, or changes of combinations of quantities) to be represented and treated in uniform ways. This was a profound advance, as the earlier geometric formalism required different representations, and thus different solution methods, for each type of physical problem.

In many ways, the new mechanics of the eighteenth century stayed true to, but also expanded and completed, a project begun in the middle of the seventeenth century. In *Some Specimens of an Attempt to Make Chymical Experiments Useful to Illustrate the Notions of the Corpuscular Philosophy* (1661), Robert Boyle conceptualized the attempt to "deduc[e] all Phenomena of Nature from Matter and Local Motion" as the "mechanical" philosophy.[10] In so doing, he gathered together a host of competing philosophies that nevertheless shared a broadly anti-Aristotelian and anti-magical approach to natural philosophy, characterized by explanatory minimalism and ontological reductionism. In various ways and to varying degrees, they rejected final causes and promoted only material and efficient causes in explanation. They also deflated various rich conceptions of matter and its qualities and endorsed instead a sparer view, on which matter is possessed of (and perhaps defined by) a truncated list of privileged affections.[11] The list differed from thinker to thinker, but included extension, magnitude, figure, motion, impenetrability, and hardness (sometimes it also included fundamental powers like resistance or even life).

Two features of this "original" mechanical philosophy are relevant for its later development. Both stem from the fact that mechanical philosophers regularly held that their approaches to natural explanations were more intelligible than those of non-mechanical competitors. First, increased intelligibility was often attributed to the use of mathematics, since mathematical

[10] Boyle makes the point even more famously in *Origine of Formes and Qualities* (1666). See Jalobeanu (2006); Garber (2013c).

[11] The literature here is vast. See, for example, Dobre (2011), Jalobeanu (2011), Jalobeanu (2013), and references therein.

principles were recognizably intersubjective, certain, and inferentially transparent.[12] In fact, mechanics itself was seen as a branch of mathematics, a so-called mixed mathematics.[13] Whether mechanics was *merely* a branch of mathematics was subject to debate, of course, with many practitioners arguing that their new sciences should also be considered as significant contributions to physics. Importantly, the same type of negotiation also took place in the eighteenth century, but often in the opposite direction. The presumption was that mechanics was physical, but many practitioners hoped to imbue it with the kind of absolute certainty and clarity more appropriate to pure mathematics.[14] The details of these debates would take us too far afield, but our point is that the status of mechanics as physics and/or mathematics was not a settled question in either the seventeenth or the eighteenth century.

Second, increased intelligibility was also attributed to the use of machine analogies in explanation. Analogies to machines and their visible, moving parts were touted for their contrast to explanations by obscure forms, occult qualities, and metaphysically-complex entities. The "structural explanations" embodied in these analogies were premised on the idea that manipulation of parts according to simple laws is uniquely understandable.[15] The analogies also suggested an inherently reductive program for natural investigation: to understand a whole, break it into parts, break each part into its constituents parts, break each part again, etc. Taking this reductionism to its logical conclusion suggested that physical explanations ultimately had to appeal to the very smallest parts of machines; that is, to the very smallest parts of matter. Mechanical philosophers were overwhelmingly committed to the idea that, at the fundamental level, all matter is essentially the same and follows the same laws. From Boyle's "catholick" matter, to the generic unity of René Descartes's matter, to the atomism Newton expressed in the *Opticks*, the uniformity of matter became a working, and often unquestioned, hypothesis.[16] In practice, however, higher-level categories – such as Descartes's three kinds of body, or Boyle's solids, liquors, and gasses – proved

[12] See the chapters by Roux (ch. 4) and Van Dyck (ch. 14) in this volume.
[13] See Mancosu (1996); Cormack, et al. (2017).
[14] E.g., Euler (1736, 10). Of course, the idea of imbuing mechanics with mathematical certainty and clarity was not new to the eighteenth century. What was new was the desire to do so given the presumption that mechanics, as rendered in the language of mathematics, was unproblematically physical. For earlier efforts, see, e.g., Garber (2002b), Babeș (2018), among many others.
[15] Anstey (2000b, 55), following McMullin (1978).
[16] Boyle (1772a, 15); Newton (1952, Query 31); Descartes (1982, II.10).

indispensable for many mechanical explanations. These explanations required the interaction of different types of bodies, as, for example, in aetherial accounts of gravity or mechanical explanations of cohesion.[17]

Mechanics in the eighteenth century inherited the idea that intelligible explanations were reductive and structural, as well as the tension between the theoretical commitment to fundamental homogeneity and the practical need for distinctions between types of bodies. Taking apart bodies was only the first step, as it were, in rendering natural phenomena intelligible.[18] The following step was putting bodies back together, in mathematically tractable ways. This is precisely what eighteenth-century mechanical philosophers had to do in order to solve problems dealing with bodies like strings, columns, membranes, gasses, or fluids. This was a broadly mechanico-philosophical endeavor, *not* a Newtonian one. Newton's principles were of little help, especially when compounded with their non-analytic presentation. As I. B. Cohen puts it,

> in the *Principia*, largely, problems are investigated which depend wholly on the masses, motions, and positions of bodies and the medium in which they move: problems which lend themselves to treatment in independence of the state (save for motion or rest) of a body, its internal constitution, and the kind of material or materials of which it may be made. The *Principia*, in other words, presents a kind of physics of mass abstracted from real bodies . . . .[19]

Although Cohen here contrasts *Principia* with the more ecumenical *Opticks*, he points to exactly those problems left open by Newton that became the launching point for analytical mechanics. Practitioners of the analytic calculus had to address problems requiring consideration of the internal mechanisms – as Cohen puts it, "states" – of various types of bodies, in both their dynamic (that is, variable) and invariant aspects. This was a task to which the analytic formalism was well suited, but which was all but intractable through geometric representation. In treating these problems, eighteenth-century mechanists echoed their seventeenth-century predecessors (albeit with unprecedented precision and mathematical sophistication) more than they echoed Newton. Or, more precisely, they did not echo the Newton of *Principia* – the Newton the Machian narrative claimed they merely completed – but the more traditionally mechanical Newton of the *Opticks*. This is not to say that eighteenth-century rational mechanists operated in the tradition of the *Opticks*. Rather, unlike their more Baconian

[17] See Glanvill (1661); Huygens (1950). [18] Dijksterhuis (1961). [19] Cohen (1966, 118).

counterparts in Britain, continental mechanists relied on mathematics over exploratory experimentation, and insisted on providing mechanics with a conceptually coherent and necessary foundation. Although the requirement of necessity seemed incompatible with an empirical grounding, it was nevertheless the "rational" mechanists who proved far more successful than their British counterparts in solving the open empirical problems of their time.

In fact, it is important to note that the demand for secure conceptual foundations in rational mechanics was *not* on par with the demand for secure metaphysical foundations in seventeenth-century rationalist, speculative philosophy. One of the deepest methodological debts owed to Newton by his eighteenth-century successors was an inversion of the traditional, pre-Newtonian ordering of knowledge-generating disciplines. Whereas metaphysics was often seen as a prolegomenon to physics (e.g., by Gottfried Wilhelm Leibniz and Descartes), Newton insisted that physics itself was a guide to metaphysics. For example, he wrote that

> [in the *Principia*] we are concerned only with sensible things and their parts, for it is in these things alone that the inductive argument has its place. The other things which cannot be perceived, but yet are hypothetically termed bodies by some people, these things are more properly treated of in metaphysics and hypothetical philosophy . . . . Philosophy begins with phenomena. Experimental philosophy consists in treating of such things. One must pass from experimental philosophy to the efficient and final causes of things and from all these to the nature of imperceptible things and finally to hypothetical [i.e., metaphysical] philosophy.[20]

This attitude animated Newton's most famous public proclamation – that he does not feign hypotheses. His claim was not simply that speculation is off-limits. Rather, it was that truths about fundamental ontological levels are not required for establishing truths about less fundamental levels; and, moreover, truths about these "higher" levels serve as constraints for discovering truths about the "lower" levels.[21]

We see the same attitude in, for example, Euler's insistence that

> as it is Metaphysics which is occupied in the investigation of the nature and the properties of bodies, the knowledge of the truths [of mechanics] can serve as a guide in these thorny investigations.[22]

[20] Draft materials for *Principia*, Book III, quoted in McGuire (1995, 116).
[21] See, e.g., Gaukroger (2014, 20); Smith (2012, 371). [22] Euler (1750, 324).

Crucially, Euler held that only mechanics in its analytic form – because of its increased intelligibility – could serve this role. But adopting the idea that metaphysics is informed by physics is perhaps the most crucial way in which eighteenth-century mechanical thinkers were fundamentally aligned with *Newtonians*. At the same time, and precisely because mechanics would now take only what it needed from metaphysics, a mechanics that represented the world in a fundamentally new way – through functions rather than curves – would require changes in metaphysical commitments. Along with reshaping mechanics through the articulation of the new mathematics, one might also look to the forms of conceptual clarification which accompanied that transformation. Concepts like force and mass and action were informed by adopting a mathematics of infinitesimals and of functions. The influence could go in both directions, resulting in new, mathematically motivated concepts, such as Jean le Rond D'Alembert's virtual work or Euler's understanding of inertia.[23]

Taken together, the inversion of the order of disciplines, the emphasis on the rational intelligibility of abstract quantities and functional relations, the reliance on formalism as a vehicle for this intelligibility, as well as the recognition that different bodies required different mathematical techniques, constituted the emergence of a new discipline, though one with clear roots in the previous century's mechanical philosophy. It was a discipline with its own methods and standards of problem solving, as well as a shared set of problems to which to apply them. This new discipline would foster a blurring of the distinction between mechanical and mathematical problem solving. Mechanical reasoning would become mathematical reasoning.

A common theme of some rejections of the standard narratives of the Scientific Revolution is that those narratives have overemphasized mathematization and mechanics, at the cost of underemphasizing broader cultural and conceptual developments.[24] Others, like Johann Christiaan Boudri, argue that the overemphasis on *mere* mathematization has underemphasized the important conceptual work done within mechanics itself.[25] Our narrative seeks to complement both of these literatures by stressing the importance of mathematical developments to the new concepts in mechanics and hence the influence of mechanics more broadly.

This narrative is programmatic, of course. We cannot hope to provide here the historical and technical detail the full story requires. We provide

[23] For D'Alembert's principle, see below. For Euler on inertia see Hepburn (2010) and Stan (ch. 21) in this volume.

[24] For instance, Osler (2000a); Zinsser (2005c); Hagengruber (2012).

[25] Boudri (2002).

scattered evidence, however, by providing a series of examples leading up to Lagrange's declaration that he has rendered mechanics a branch of analysis. To understand these, we first examine the set of problems eighteenth-century mechanics set out to solve.

Open Questions of the *Principia*

The fundamental successes of Books I and III of the *Principia* depend on treating only centripetal (or approximately centripetal), conservative forces.[26] The limitation allows Newton to employ his geometric devices built around a central point: the area law, the linear sagitta, and the "linear dynamics" solid.[27] Anachronistically put, the area law provides a conservation principle and a measure of time; the sagitta a geometric representation of the curvature of a trajectory (an indirect representation of the magnitude of force, but dependent on velocity); and the solid a geometric representation of the magnitude of a force at any point of a curve (after velocity and curvature are taken into account). These, coupled with Newton's method of first and last ratios, allow Newton to overcome a major challenge of mechanics; namely, to represent the complexity of a force whose direction and strength is changing because it is acting on a body whose motion is changing in consequence of the force itself. The method of first and last ratios allows Newton to represent such continuous changes by a series of similar, finite representations. From these, he can discover mathematical relations that remain invariant as the finite representation nears a continuous change; i.e., in the infinitesimal limit. These geometric devices, however, are valid representations only for conservative centripetal forces. (The method of first and last ratios itself is not limited in this way.)

In Book II of the *Principia*, Newton aims to solve a broader set of problems, ones that are not solely dependent on conservative centripetal forces. These are problems of fluid dynamics, of bodies moving through resistive media, and bodies constrained to various non-orbital trajectories. Losing the centripetal constraint dramatically affects the lengths Newton must go to, and the strategies he must employ, in order to solve these problems. For example, in

[26] Newton also replicates earlier Huygensian and Galilean results, which take gravitation to act in parallel lines. These cases can be subsumed under the treatment of centripetal force by assuming that the center of force is infinitely distant.

[27] See Lemmae and Propositions 1, 6. The solid in question is inversely proportional to the strength of a centripetal force. Brackenridge (1995, 7) calls the inverse quantity "the linear dynamics ratio."

order to construct the trajectory of a projectile moving in a resistive medium, Newton employs a hyperbola to represent the relations among speed, time, and distance. But how those quantities relate to the hyperbola varies according to the resistance force function; for instance, whether the resistance is proportional to velocity or velocity squared. That the hyperbola works at all follows from a particular geometrical relation to the exponential: the area between the hyperbola and one of its asymptotes grows exponentially as you approach the other asymptote. Thus, it is useful for problems in which quantities change in proportion to themselves. Newton puts this to ingenious use at Propositions I–IV, in Section I of Book II, for media where the resistance is directly proportional to velocity. But he then has to find another, equally ingenious way, in Section II, to use the hyperbola for the case of a resistance proportional to the square of the velocity.[28]

Euler took this as a sign that treating mechanics geometrically does not provide true understanding – one's ability to solve a problem should not be upended by a minor change to the problem. Both above problems are solved by Euler – using analysis – as part of the general problem of "media of whatever resistance."[29] Analysis, Euler thought, far better than geometry could provide the understanding needed to generalize solutions to problems in mechanics. As Euler would point out later, the chief difficulty of a general science of motion – anything worthy, in Euler's estimation, of the name *mechanics* – is that forces are applied to moving bodies in ever-changing relations of direction and magnitude. To solve such problems *in a generalized way* requires discovering some underlying and unchanging rules governing the interactions between force and body. Euler's insight was that these rules might be discovered in the natures of the various types of bodies.

In the next section we look briefly at some of the ways that this was done in the eighteenth century, telling the story, through some few examples, of the development of mechanical principles in tandem with developments in conceptions of physical quantities and formal developments. This part of our narrative is that the development of analytic mechanics was an evolution from a natural philosophy concerned more narrowly with the generation of motions by forces, to a physics concerned with representing invariant mechanical relations among physical quantities (especially in analogy with the balance or equilibrium relations). In this way, the need for a reduction to point masses to which Newton's laws could be applied was obviated. It was

[28] Newton (1687, 236). [29] Euler (1736, Prop. 105).

replaced, instead, by systems of bodies, linked by mechanical constraints expressed through functional relations. The evolution took place in an anti-reductionist way, from point masses "upwards" to a mechanics of integrated bodies.

New Programs in Mechanics

In 1736, ten years after the publication of the last edition of *Principia*, Euler described his view of the program by which the science of mechanics should proceed, based upon the composition of bodies. Mechanics, for Euler, is firstly to be contrasted with statics. Statics assumes an equilibrium of forces, so that there are no changes to the states of motion of the bodies involved. Mechanics, on the other hand, is the science of moving bodies. Forces are not at equilibrium, resulting in changes to the states of motion of the bodies. Of particular difficulty for the science of mechanics, then, is to calculate the effects of forces on moving bodies.

> In Statics, where all things are assumed to remain at rest, all *potentiae* [state-changing powers] are set up to perpetually keep their same directions. But in Mechanics, as a body perpetually comes to other positions, the directions in which the *potentiae* act on [the body] continually change. According to the different places of bodies or directions of *potentiae*, which will be parallel to one another or converging to a fixed point, or holding to some other rule, from that variety of *potentiae* in Mechanics, the discussion arises.[30]

The orientations between the forces, the bodies, the direction of their motions, and the resultant changes, are all continuously varying. Newton's laws dictate that the changes in the motions are proportional to, and in the direction of, the forces. But when those directions are continuously changing, the application of the laws would be continuously changing. Newton's laws simply cannot be applied to all but the simplest of problems, or those where some simplifying constraint or invariance can be found. The prime example of this are problems with a centripetal force, as discussed above.

On the face of it, then, the generalized problem seems intractable. Any continuously varying trajectory would require an infinite number of discrete applications of Newton's laws to correspond to the infinite number of directions in which the forces act on the body. This problem holds for the motion of points considered on their own, but is compounded for points aggregated into bodies. Bodies are systems of points, each interacting with

[30] Euler (1736, 41).

one another, adding another layer of complexity to the calculation of forces and effects.

Euler's proposed strategy was to work his way "up" through bodies of different natures, beginning with forces on free point masses (not limited to centripetal forces), then points constrained to motions on surfaces or moving in media, then rigid bodies, elastic solids, non-rigid bodies of several types, and, lastly, fluids.

> The diversity of bodies therefore will supply the primary division of our work. First indeed we shall consider infinitely small bodies . . . . Then we shall attack bodies of finite magnitude which are rigid . . . . Thirdly, we shall treat of flexible bodies. Fourthly, of those which admit extension and contraction. Fifthly, we shall subject to examination the motions of several separate bodies, some of which hinder [each other] from executing their motions as they attempt them. Sixthly, at last, the motion of fluids will have to be treated.[31]

The point of the program is that each kind of body represents a different set of constraints on the motion of the points of that body. What makes the latter bodies more difficult is that the constraints themselves change in more and more complicated ways. Rigid bodies have fixed constraints, fixed relations among the parts. Elastic bodies have changing relations, but the changes can be described in fixed ways. With extension and contraction, the constraints can change, and so can the rules by which the constraints change. Fluids are the most complex of all. Surprisingly, it is here that Euler has the most success, though hard won. (This can be contrasted with Newton's attempts at treating fluids in *Principia*, Book II.)

Several other strategies emerged to deal with the difficulties of change (and of changing change, and changing change of change, etc.). Commonly, these began with attempts to extend equilibrium in static cases to dynamic ones. Pierre Varignon, for instance, sought to reduce the understanding of all machines to a principle which could be ascribed to the lever. He explained, in the Preface to *Projet d'un nouvelle mechanique*, how he came across a remark of Descartes's that it seemed ridiculous to apply the principle of the lever to a pulley. Varignon demonstrated nevertheless how one could first apply the principle to a body on an inclined plane, and then to the pulley. The link was to consider the equilibrium of forces.[32] Although Varignon was one of the prominent figures to work on translating *Principia* into analytic form, his work was overshadowed by that of Jakob Hermann and Johann Bernoulli.[33]

[31] Euler (1736, 8–9). [32] Varignon (1687). [33] See Guicciardini (1999, §8.4).

His greater contribution was through his systematization of equilibrium conditions in statics and the simple machines.

D'Alembert was even better known for extending the concept of equilibrium to dynamic problems. D'Alembert's principle, as it would come to be known, requires introducing fictitious forces which, were they present, would result in the equilibrium of the body under consideration. Non-static quantities, in other words, could be treated through an invariant, equilibrium relation. This approach allowed for the successful solving of problems of vibrating strings, and of fluid mechanics, including bodies moving in a frictional medium.

In D'Alembert's 1743 treatise on solid bodies, he claimed his Laws were founded on metaphysical principles independent of experience. When the principles are metaphysical one can "determine exactly those of the Principles which must serve to found the others."[34] Moreover, "[t]he Laws of the Mechanics of ordinary Bodies . . . can be reduced to three; known as the force of inertia, the composition of movements, and the equilibrium of two equal masses moved in contrary directions by two equal virtual velocities."[35]

However, when it came to fluids, D'Alembert could no longer found the project on metaphysical principles independent of experience. He expressed the intent to do so in his 1744 *Traité de l'équilibre et du mouvement des fluides*:

> In the *Traité de Dynamique* . . . I aimed to reduce to the smallest number possible the Laws of the equilibrium and movement of solid Bodies; I have tried to do here the same thing for Fluids.[36]

And D'Alembert argues that, since fluids are composed of material components, all three of his Laws of Mechanics for ordinary bodies will apply to them. However, since "we do not know the form and the disposition of fluid particles," the third law, concerning individual parts and their virtual velocities, is of no use in discovering the equilibrium conditions of fluids. We must appeal to experience: "Experience alone can instruct us on the fundamental Laws of Hydrostatics."[37]

The "experiences" D'Alembert describes are what we might call phenomena. But they are also very close to principles in and of themselves. As an example, D'Alembert asks us to consider a cylindrical tube bent in a sort of U-shape, and containing a liquid. That is, there are two vertical parts joined

34 D'Alembert (1743, vi).
35 D'Alembert (1743, viii).
36 D'Alembert (1744, vi).
37 D'Alembert (1744, ix).

by a horizontal one, with an opening at the top of each side. The fluid in the tube will only be at equilibrium when the two vertical columns of water are of equal height. At equilibrium, then, the fluid everywhere in the horizontal part will be subjected to equal pressures in both directions – the more general property being that fluids are pressed, and press, equally in all directions.

> This general property, shown by an Experience so simple, is the foundation of all that one can demonstrate on the equilibrium of Fluids.[38]

The point is that the foundations of any domain of mechanics, as D'Alembert sees it, are principles (laws) about equilibrium conditions.[39] In simple domains you can read those principles right off your metaphysics. For more complicated bodies, like fluids, you have to look to experiences. But the required experiences will be those that tell you about the equilibrium conditions for bodies of that type. You have to observe how they behave *as systems* because of their complexity. The right experiences will provide you with equilibrium principles on which to build the rest of the science.

Even within the domain of fluids we see D'Alembert proceeding through a series of bodies of ever-increasing complexity. Book I, on the equilibrium of fluids, proceeds by chapters through fluids only under their own weight, then under pressure from another weight but in a constant direction, fluids in vessels, with floating solids, in various shapes of tubes, different combinations of these conditions, and finally up to elastic fluids. Then, in Book II, D'Alembert does essentially the same thing all over again but for fluid systems *not* at equilibrium. In a remark at the beginning of Book II, D'Alembert makes explicit the connection between the first two books.

> It is evident that this Theorem is nothing other than the application of our general Principle of Dynamics to the movement of Fluids. As all the Laws of the movement of solid bodies . . . have been reduced by this Principle to the equilibrium of these same bodies, the Laws of the movement of Fluids can also be reduced in the same way to the Law of Equilibrium of Fluids.[40]

"Our" general Principle of Dynamics to which D'Alembert is referring is roughly the one we now call D'Alembert's principle, alluded to earlier.

38 D'Alembert (1744, x).
39 If D'Alembert has a principled distinction between the terms 'Principle' (*Principe*) and 'Law' (*Loi*), we cannot detect it. For example, he says that the "famous Law of Mechanics, called the conservation of living forces" is a "Principle today recognized as true by all Mechanicians" (D'Alembert 1744, xvi).
40 D'Alembert (1744, 70–71).

Lagrange introduced yet another invariant relation among changing quantities, his "general formula for the movement of any system of bodies," in 1788. Lagrange's treatise has the same two sections that D'Alembert employed in his two treatises on solid bodies and fluids. That is, first one treats the equilibrium conditions and cases, and then the non-equilibrium cases. Lagrange called this statics and then dynamics, rather than equilibrium and then movement. The books are united by a common mechanical principle, functionally represented, but given in two versions in the two sections. First, at the introduction of the statics section, where the formula was a true equilibrium condition, the principle contained no accelerations, and only virtual velocities which sum to zero. At the beginning of the dynamics section, the same principle was given in the more general form to include accelerations, and given as a sum or integration over all the bodies of the system. The accelerations were, of course, implicit in the static version of the principle, except each acceleration was individually equal to zero. The virtual velocities, on the other hand, only equated to zero when summed. "From which," Lagrange comments, "one sees that the laws of movement of a system are the same as those of its equilibrium, in simply adding the new accelerative forces."[41]

There is one more apparent and significant difference between the static and dynamic versions of the "general formula." Namely, the dynamic version requires what Lagrange calls *variations*, rather than differentials, as in the equilibrium case. Variations are required because Lagrange is considering *systems* of bodies in which the bodies constrain each other's motions. To find the actual moments of the forces in the dynamic case, one multiplies the forces for each direction by the variations for that direction. These are the actual mechanical *moments*, because the variations are the actual mechanical *movements*. But the analytical, functional representation makes clear the connection between the equilibrium and dynamic cases.

> For the general formula of equilibrium consisted in that the sum of the *moments* of all the forces of the system must be null . . . thus one has the formula sought after by equaling to zero the sum of all the quantities relative to each of the bodies of the proposed system.[42]

These two cases illustrate how invariant relations could be used to render problems in mechanics intelligible and tractable, even while expanding the set of solvable problems to more and more general kinds of bodies,

[41] Lagrange (1788, 193). [42] Lagrange (1788, 195).

understood as mechanical systems. The analytic approach made it easier, even straightforward, to formulate relations among continuously varying quantities, but also the rates of change of those quantities, the rates of change of their rates of change, etc., and combinations of those with other quantities. Euler remarked explicitly on the importance of analysis for setting up, solving, and understanding problems in a now well-known and oft-quoted passage from the Preface to his *Mechanica*:

> But in all writings which are written without analysis it happens most in Mechanics that the reader, although convinced of the truth of those things which are put forward, nevertheless does not achieve clear and distinct knowledge of them, and so can barely solve the same questions by his own devices when they are altered even a little, unless he engages in analysis and explicates the same propositions using an analytical method. This often happened to me when I began to read through Newton's *Principia* and Hermann's *Phoronomia*: although I seemed to myself to have understood the solutions to many problems, still I could not solve other problems that differed even a little.[43]

A new discipline rose up around this strategy of searching for *unchanging* relationships, analytically expressed, which would allow mechanics to be extended beyond problems in celestial dynamics.[44] Invariant relationships were needed, like the area law, but for situations involving forces, motions, and constraints which were changing in complex ways – situations which arose outside central body problems, and for bodies whose internal structure could not be reduced to a mathematical point mass. These unchanging relationships were sought either in higher orders of the changes (i.e., dynamic equilibrium conditions such as D'Alembert's or Lagrange's) or in looking for conserved quantities, such as *vis viva* or action.[45]

Mechanics Has Become Analysis

In the *Avertissement* to the *Mécanique analytique*, Lagrange remarks that "Those who love Analysis, will be happy to see Mechanics become a new branch, and thank me for having extended the domain."[46] Given the emphasis on analysis, it is not shocking that the work contains no figures, nor do the methods require constructions or geometrical reasoning (as he

[43] Euler (1736, 9). [44] Hepburn (2010); Caparrini and Fraser (2013).
[45] On least action see, e.g., Lagrange (1788, 188–189). [46] Lagrange (1788, ii).

remarks in the same paragraph). But very puzzling is the rest of the sentence, denying that any *mechanical* reasoning is required. Analytic mechanics, for Lagrange, is mechanics without mechanical reasoning. What analytic mechanics does require are "only algebraic operations, subject to a regular and uniform procedure [*marche*]."[47]

Two important points relevant to our narrative can be highlighted here. Firstly, far from Lagrange being solely responsible for this new arrangement, and despite his proclamation, he is a signal of the inflection that had been taking place: the transformation of mechanical problems from problems of natural philosophy (including metaphysics) into problems of mathematics.[48] D'Alembert had earlier remarked on the important connection between mathematical and mechanical advance:

> Although the Physics of the Ancients was neither so unreasonable, nor so limited as some modern philosophers think or say, it appears, however, that they were not well versed in the sciences that we call Physico-Mathématiques, and which consist in the application of calculation to phenomena of nature.[49]

Because of the ready availability of mathematical techniques and solutions, mechanics adopted the standards of success of the mathematical domain. This inflection and discipline-merging was not entirely one-sided. Mechanics became mathematics, but not purely. Mechanical intuitions like equilibrium and balance and symmetry were still a part of the discovery and justifications of principles.

The new analytic mechanics was conceptually unified, but unified in a clear formalism, technique, and syntax, as well – far less obscure in its explanations and solutions than was Newton's *Principia*. Newton feigned no hypotheses, but nevertheless, or even for that very reason, there is much in his explanations we must simply take for granted: the *vis insita*, the strength and action at a distance of the gravitational attraction, the equivalence of inertial and gravitational mass, the law of equal and opposite action. These are powerful explanans, but they are not explained. Most magical of all is Newton's approach to the solving of problems. He seems to pull out of thin air the constraints and quantities he needs to make physical phenomena tractable. The linear dynamics solid, for instance, is not an obvious measure of centripetal force, and it is not clear how one arrives at a hyperbola to represent motion through

[47] Lagrange (1788). [48] See Stan in this volume (ch. 21). [49] D'Alembert (1752, vii).

a resistive media. The solutions were beyond impressive but hardly instructive.

In Lagrange, the mechanical reasoning seems less magical. We recognize it there on the page as parts of equations: functional relations. The equations look like machines. You can follow the moving parts, watch the quantities being manipulated. The equations express equilibrium interactions, with quantities, actions, activities, on both sides of the equation literally being equal to one another. In fact, Lagrange's preferred form of expressing his mechanical principles was with all factors on the left side of the equation and nothing but zero on the other. The solutions to many problems offered by Lagrange, or by Euler, remain standard in courses now called "classical mechanics."

Conclusion

We have attempted to sketch a narrative for understanding developments in eighteenth-century mechanics. Our narrative weaves together these elements: the role of the analytic formalism; the need for new principles to solve problems in mechanics for phenomena that do not involve centripetal forces, particularly kinds of bodies characterized by more complex internal interactions; and the functional conception and representation of mechanical phenomena. Our main conjecture is that the third point links the first two. It is an affordance of the analytic formalism that it allows for the functional representation of the interactions among the parts of complex systems. This was because not only could you straightforwardly represent changes to system quantities in the analytic formalism through differentials, interactions among those quantities could then be straightforwardly represented as functions once the relevant dynamic equilibrium principles were grasped. Our narrative sketch describes developments in mechanics as largely a search for those equilibrium principles through investigating the physical conditions that characterize more complex types of bodies, and physical systems more generally. By making mechanics a part of analytic mathematics in this way, mechanical understanding was greatly advanced at the same time as mathematical problem-solving techniques of the new formalism were exploding. The result was a new discipline, a hybrid of mathematical and mechanical reasoning, treating bodies as systems – as machines – reduced to simple mechanisms, rather than points.

It is true that mechanics in the eighteenth century was mechanics in a post-Newton world. This was a period in which the Continent became enamored with the Enlightenment image of Newton as a paragon mathematical natural philosopher. This image was inescapably felt through its influence on the spirit of the age. But the real work done in mechanics in the eighteenth century, when attended to closely, does not usually begin with Newton, nor follow Newton. It was merely done in Newton's wake.