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The Axiomatic Tradition in Seventeenth-Century Mechanics

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Over the last few years several scholars have devoted increasing attention to the changing roles of contrived experiments in the seventeenth century, an age when they gained a significant if problematic role as a component of our knowledge about nature. Special attention has been devoted to the transition from generalized everyday experiences, at times associated with an Aristotelian or peripatetic tradition, to the contrived experiments performed at special places and times by the new natural philosophers. Historians have studied how experiments pertinent to several disciplines were conceived, performed, privately recorded, witnessed, and reported in print.[2]

There is no question that for some scholars, such as the French Minim Marin Mersenne, the nobleman Robert Boyle, or the Paris academician Edme Mariotte, for example, experiments were the key source of knowledge about nature and they made no secret of it. Other scholars in the mathematical disciplines, however, did not wish to rely on contrived experiments at all in the formal presentation of their sciences. Rather, they sought either principles to which the mind naturally consents, such as symmetry, or principles based on generalized experiences describing the normal course of nature. Mental operations or thought experiments would be more appropriate terms to describe how they proceeded.

This search for new principles is a significant feature of seventeenth-century investigations and formulations of the mathematical disciplines that has attracted less attention than experiments. One root of this approach can be found in classical Greece and especially in Archimedes, who was among the first to try to formulate a science dealing with nature—mechanics or the science of the balance—in a mathematical fashion resembling Euclid's *Elements*.[3] My essay focuses on mechanics both because this was Archimedes's subject in *On the Equilibrium of Planes*, and also because mechanics provides ample material to reflect on the way mathematicians like Galileo and Christiaan Huygens maneuvered in the transition from the science of equilibrium to the new science of motion.

Recently Michael Mahoney has identified the engineering tradition as a source of rules or principles in mechanics, namely:

1. You cannot build a perpetual-motion machine.
2. You cannot get more out of a machine than you put into it.
3. What holds an object at rest is just about enough to get it moving.
4. Things, whether solid or liquid, do not go up by themselves.
5. When you press on water or some other liquid, it pushes out equally in all directions.

According to Mahoney, starting in the 1580s with the Dutch mathematician and engineer Simon Stevin and with Galileo, engineers aspiring to become natural philosophers began transforming such maxims into formal mathematical principles of mechanics.[4]

In this essay I explore this Archimedean tradition and the attempts to reformulate classical mathematical disciplines, notably the doctrine of the equilibrium of the balance, or formulate new ones, such as the science of motion or of the collision of bodies, relying on generalized experience and principles to which the mind consents, rather than contrived experiments. While Mahoney is certainly right in identifying a significant source in the engineering tradition, mathematicians appealed also to abstract principles, such as symmetry, and progressively reformulated and expanded all principles regardless of their provenance in ways bearing only at best a vague relation to engineering.[5] Moreover, often mathematicians were only too keen to suppress their engineering background to address a more philosophical audience; therefore, engineering connections were more likely to be suppressed than emphasized. Several mathematicians adopted various axioms and postulates in their works on mechanics and motion. Since it would be impossible to provide an exhaustive account, I will discuss only a few of the protagonists of this approach besides Stevin and Galileo, namely the latter's successor and follower, Evangelista Torricelli, who formulated a new principle of the science of motion, and Huygens, who provided an axiomatic formulation of the science of collision among bodies. It is Galileo, however, that is going to attract most of the attention and whose project and concerns I examine in greater detail.

Starting from Descartes and his laws of motion, several philosophers and mathematicians, such as Leibniz, appealed to God, and to broader theological and philosophical reasons, in order to establish the foundations of several sciences, especially with regard to conservation principles. At times, the boundaries between theologically and not-theologically based principles were quite blurred, with principles introduced on theological grounds being later posed on non-theological ones. Descartes, for example, justified his laws of motion having recourse to God's immutability, but Huygens used the first two laws, claiming that bodies left to themselves tend to move with a rectilinear uniform motion, dropping God out of the picture. Those theological and philosophical considerations add factors about God and his relations to the world going beyond the scope of my contribution.[6]

It goes without saying that specific rhetorical techniques of mentioning witnesses of contrived experiments, such as those mentioned by Simon Schaffer, Steven Shapin or Peter Dear, do not apply here. But one may still wonder what counted to late 16th- and 17th century mathematicians as an acceptable axiom or principle, how they were presented in print and justified, and how the perception of what was acceptable changed with time.[7]

1. Archimedes and Axiomatic Foundations

A key exemplar from Antiquity is Archimedes's work in mechanics, especially his treatise *On the equilibrium of planes*, which established the doctrine of the balance and determined centers of gravity of different geometrical figures. His other work on mechanics, *On floating bodies*, has a more problematic axiomatic structure, so much so that the meaning of its only postulate is unclear.[8] As we are going to see below, in his 1612 work on bodies in water Galileo tried to provide foundations for hydrostatics based on the balance, thus underscoring its primacy over other areas of mechanics.

In *On the equilibrium of planes* Archimedes relied on an axiomatic style derived from mathematics in a work about nature, starting from seven postulates. The first two, reproduced below, give us a sense of his enterprise:[9]

1. We postulate that equal weights at equal distances are in equilibrium, and that equal weights at unequal distances are not in equilibrium, but incline towards the weight which is at the greater distance.

2. If, when weights at certain distances are in equilibrium, something be added to one of the weights, they are not in equilibrium, but incline towards that weight to which something has been added.

Archimedes did not take as a postulate the statement that "magnitudes are in equilibrium at distances reciprocally proportional to their weights," but rather he tried to prove it as a theorem. That statement lacked the required criteria to be naturally accepted by our mind. If some generalized experiences may have guided him in his choice of some postulates, others must have appeared less suitable and contrived experiments were certainly not a basis on which to proceed; even such relatively common experiences as those provided by a *statera*, or balance with unequal arms, did not look sufficient to him. I am unsure whether we have reflected with sufficient care on which statements could be chosen as postulates at different locations and times. I often paused when reading Peter Dear's claim: "Recent research has shown that Galileo aimed at developing scientific knowledge, whether of moving bodies or of the motion of the earth, according to the Aristotelian (or Archimedean) deductive formal structure of the mixed mathematical sciences" (Dear 1995, 125–26). My sense is that Archimedes would have required stricter criteria for what can be accepted as a postulate and would have used common experience more sparingly than Aristotle. But this is a claim needing a more extensive elaboration than can be given here.

The actual proof of equilibrium provided by Archimedes when the distances are inversely as the weights was seen as problematic or at least improvable by several commentators, such as Galileo, and has attracted attention to the present day.[10] In propositions 6 and 7 of *On the equilibrium of planes*, Archimedes tried to provide somewhat cumbersome proofs of the equilibrium of balances with unequal arms and unequal weights in two cases, first with commensurable and then with incommensurable magnitudes. Nonetheless, Archimedes's work constituted an example of how to formulate a mathematical science dealing with nature that was especially influential in the late sixteenth and seventeenth centuries.

2. Stevin and Galileo: From the Balance to Falling Bodies

Much as Archimedes in his study of the equilibrium of the balance, Stevin and Galileo did not wish to rely on contrived experiments in their formal presentation of the science of mechanics, hydrostatics, the science of the resistance of materials, and the science of motion. Stevin provided axiomatic formulations of his theory. In addition, both in his celebrated study of the inclined plane and of the equilibrium of water in a container, he relied on a principle such as the denial of perpetual motion (see Illustration 1).[11]

Galileo gave the balance pride of place in his work on mechanics. The balance was both the device that had been treated and formalized by Archimedes, and the basis for understanding other machines and problems, such as the other simple machines, the siphon, and the loaded beam.

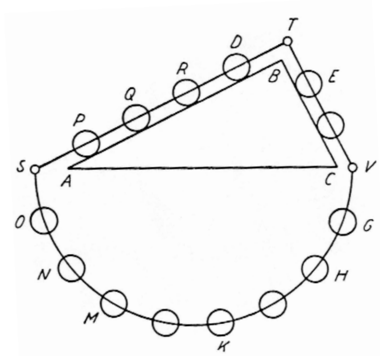

ILLUSTRATION 1: Stevin and the Inclined Plane.

According to Stevin the chain of spheres on the inclined plane will not move on its own accord, lest we have perpetual motion. The lower part SON . . . GV can be removed with symmetry consideration. The remaining portion STV shows that the weights are in equilibrium when they are proportional to the lengths of the inclined planes.

In some cases, such as the science of resistance of materials, Galileo believed he could extend the doctrine of the balance to new domains by seeking to show visually how a beam could be conceptualized as a balance (see Illustration 2). Similarly, in the case of the siphon he tried to provide foundations based on a balance with unequal arms by relying on the analogy

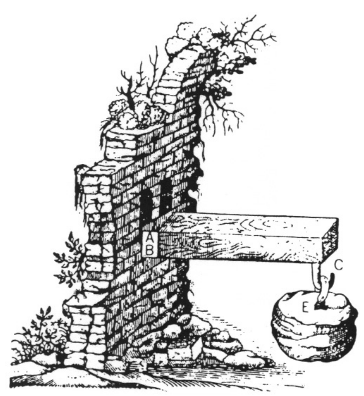

ILLUSTRATION 2: Galileo's beam.

Galileo argued that a loaded beam could be seen as a balance or lever. One arm is CB, the fulcrum is B, and the other arm is AB, namely the cross section of the beam attached to the wall. The "weights" in this case would be represented by the resistance offered by the beam's fibers.

between the speed of the two weights and of water in the two branches of the siphon (see Illustration 3).[12]

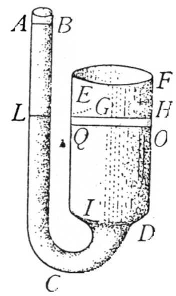

ILLUSTRATION 3: Galileo's siphon.

Here Galileo argued that if water in the larger arm is pressed down from GH to QO, water in the smaller arm will rise from L to AB. The speed of water in the two arms is proportional to GQ and LA and is inversely as the cross sections. Therefore the siphon works like a balance where the weights are as the water surfaces in the two arms and the speeds are as the distances from the fulcrum.

Although Galileo's reformulation of the doctrine of the balance does not tell us much that is new about the contents of mechanics, in my interpretation it is the key to understanding his formal axiomatic presentation strategy in other areas, especially the science of motion. Here I am going to outline his reformulation and its implications.

Both in his Padua lecture notes, *Le mecaniche*, and later in the *Discorsi*, Galileo presented a new proof of the key condition for the equilibrium of a balance, namely, that the weights are inversely as the distances of their suspension points from the fulcrum. Nobody doubted the truth of the proposition; the trouble was how to prove it starting from indubitable assumptions. If Galileo had sought to ground his science on experience, the claim that the balance is in equilibrium if the weights are inversely as the distances of their suspension points from the fulcrum would have looked like a perfect candidate. Such balances had been commonly used for centuries on every market square and nobody doubted the principle on which they operated. However, Galileo wished to prove that principle by means of a series of operations that he thought would be accepted as legitimate by the mind or our intuition (see Illustration 4). Much as for Archimedes, the starting point was a variant of the perfectly symmetrical case of equal weights hanging from equal distances.

Galileo attempted to simplify Archimedes's proofs, but followed a rather similar approach. The main idea consists in attaching a homogeneous bar, such as a cylinder or a prism, parallel to a horizontal balance;

then by a series of operations that do not appear to our mind to alter the equilibrium conditions, like cutting the prism at some point and suspending it in a different fashion, one is left with a configuration whereby the weights are inversely to their respective distances from the center. Much as in Archimedes, intuition and reason are invoked rather than actual experiments.[13]

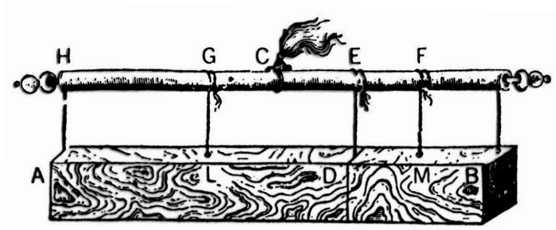

ILLUSTRATION 4: Picture of Galileo's balance.

The prism AB is attached at A and B to the balance with fulcrum C. Cutting the prism in D and attaching the two parts to the balance in E leaves the equilibrium conditions unchanged. Next Galileo suspends the two parts from their middle points L at G and M at F, again without altering the equilibrium conditions. The distances CG and CF are found with some calculations to be inversely as the weights of AD and DB of the two portions of the prism.

From the perspective adopted here, a distinction between private manuscripts and publication in Galileo is insufficient. Within Galileo's published works we have to distinguish between informal and formal presentation. I have no doubt that privately Galileo performed important experiments either heuristically or to confirm results he had found by calculation.[14] In print Galileo referred to experiments in the *Dialogo*, for example, but in the second and third days of the *Discorsi* his main concern was with establishing an axiomatic science of motion on the example of Archimedes.

In *De motu antiquiora* Galileo tried to establish foundations for the science of motion from the balance, but he soon realized that he needed a different starting point and therefore attempted to find suitable foundations elsewhere. Galileo performed several heuristic experiments privately, and discussed some of them in print in a variety of texts, but my main concern here is with the formal axiomatic presentation of his sciences. Whereas Galileo's experimental forays have been frequently commented upon, his foundational efforts have attracted less attention, yet they constitute a major episode in the history of science. Galileo's letters to Paolo Sarpi in 1604 and Luca Valerio in 1609 show him desperately seeking principles to which the mind naturally consents, in order to construct an axiomatic science based on definitions and propositions. It is somewhat ironic to notice how hard Galileo was struggling to find axioms that looked natural. Indeed, this is often a feature of axioms, to be contrived in the extreme in order to look perfectly natural and straightforward, much like some experiments. Both in *Le mecaniche* and in a letter to Sarpi, Galileo used the word "undoubted" or "indubitable" to qualify an axiom or postulate. In *Le mecaniche* he stated: "We can take as an undoubted axiom this conclusion: heavy bodies, once we have removed all external and adventitious impediments, can be moved on the plane of the horizon by a whatsoever smallest force." Similarly in the letter to Sarpi he stated that thinking over the problem of motion, he realized he lacked a "totally indubitable principle to be put as an axiom." His "indubitable" principle turned out to be the erroneous proportionality between speeds and distances, but this does not affect my point.[15] The rhetorical strategy and language used to present postulates requires the same amount of care and scrutiny we have devoted to experimental narratives.

Galileo did not succeed fully in providing definitions and axioms in a form that would appear as an intuitive and convincing account of how nature operates. Rather, he defined naturally accelerated motion as that motion where the speed increases proportionally to the time, adding a postulate stating: "The degrees of speed acquired by the same moveable over different inclinations of planes are equal whenever the heights of those planes are equal."[16] There is no question that, despite his pretences to the contrary, Galileo's formulation was highly artificial and contrived. Even a cursory survey of the manuscript evidence and correspondence shows that he struggled for decades to present his construction in a "natural" fashion.

On the basis of his definition and postulate Galileo built a mathematical theory that in principle may have remained just that, a mathematical theory devoid of any physical significance. It is curious that despite this pretence, Galileo spent some time trying to defend his axiom by claiming both that it is naturally accepted by the mind, or the "lume naturale" ("good sense" is Drake's translation),[17] and that an experiment or "esperienza" confirms it; pendulums, for example, reach the same height from which they have been released. Galileo is aware that a fall along a rectilinear inclined plane is different from a fall along a circle arc as in an oscillating pendulum. In spite of this and other difficulties, Galileo argues that "l'intelletto resti capace," or "the mind understands" (following Drake) that the bob returns to the same height. "Hence let us take this for the present as a postulate, of which the absolute truth will be later established for us by our seeing that other conclusions, built on this hypothesis, do indeed correspond with and exactly conform to experience."[18] This provisional acceptance of a postulate that is later corroborated by the conclusions underscores the lack of certainty of the postulate and highlights the weakness of Galileo's foundations.

From the definition Galileo proved that the spaces traversed by a body falling with a naturally accelerated motion are proportional to the square of the times. This statement is shown to be equivalent to the odd-number rule, whereby the spaces traversed in successive time intervals are as 1, 3, 5, 7, etc. At this point he introduced the celebrated experiment with the inclined plane showing with a great degree of accuracy that real bodies actually follow this rule. The experiment involves an inclined plane about twelve *braccia* long, arranged so as to have a rather small inclination, raised on one side only one or two *braccia*. Galileo measured time with a water clock. This contrived experiment is presented as having no foundational role at all. Its role is to show that the mathematical construction he has provided actually describes the behavior of real bodies in the world. Galileo's mathematical theory would remain valid even if nature behaved differently.

Galileo perceived the reference to experiments justifying his axioms as a weakness and after publication he continued to reflect on the matter. Eventually, stimulated by his pupil Vincenzo Viviani, he produced a proof of his axiom that was first published in the posthumous second edition of the *Discorsi* in 1656.[19]

Galileo's works were read in strikingly different fashion. Among his disciples, Torricelli was probably the one who was most concerned with the problem of axiomatic foundations. In the 1644 *De motu* he put forward the postulate or principle whereby two combined bodies do not move unless their common center of gravity descends. There is no question that he presented his postulate as a proposition accepted by the mind based on common intuition and general observations. His rather general examples were not contrived experiments, but rather instances of a general statement, such as systems of weights on inclined planes and connected via a pulley, for example. In his reformulation Torricelli was quite successful, since his postulate was both powerful and convincing.

Mersenne, by contrast, had no interest at all in axiomatization and frequently focused instead on the empirical adequacy of Galileo's claims, as with the distance covered by a body in free fall in a given time or with spheres rolling down inclined planes. In both instances he worried about numerical results from specific propositions rather than logical deductive structures.[20]

Next we are going to examine a previous attempt at justifying the law of fall that was later discarded by Galileo. In the debates on falling bodies in the 1640s several scholars were to adopt a similar approach.

3. Falling Bodies and Unit Invariance

An instructive case occurred with the debates on the rule followed by heavy bodies in free fall. In a letter to Benedetto Castelli, Gianbattista Baliani reported that Galileo had defended the odd-number rule by arguing that it was the only one invariant on the choice of the distance.[21] The statement is somewhat cryptic, but I believe it can be interpreted as follows. Units of distance are arbitrary; indeed in Galileo's Italy they varied almost from town to town. Galileo seemed to argue that if nature follows a rule according to which heavy bodies fall, that rule should not depend on arbitrary factors as the units used at Florence or Rome, or indeed any units at all, but must be independent of them. Units are local and conventional, whereas nature's operations should be universal and independent of human conventions. Following the odd-number rule, if a body were to fall a given distance in an arbitrary time, in the second time interval, equal to the first, the body would fall three times that distance, five in the third, and so on. This proposition remains true for any choice of the initial distance fallen. For example, if one were to choose an initial distance four times greater as the initial unit, the correspondent time would be doubled. In the second time interval, equal to the first, the body would again fall three times the initial distance, since 5+7 (=12) is three times 1+3 (=4); the same would happen in successive intervals, since 9+11 (=20) is five times 1+3 (=4). Several other rules lack this invariance property. For example, if a body fell in successive equal time intervals by distances as the natural numbers, 1, 2, 3, 4, etc., then if one were to chose a different unit of distance, the proportion based on the natural numbers-rule would not be preserved. Galileo's choice is not unique, but the problem of determining which rules are invariant under transformation of units is beyond the scope of this paper.

It is not clear what the origin of this invariance rule is, but it is tempting to identify the engineering tradition as a possible source. Bridges or towers stand or fall regardless of whether they are measured in Roman or Florentine units and one can see why a similar reason could be applied to falling bodies. Galileo in the end did not make much of this argument because he deemed it only probable, but a closely related one was used by several scholars of the following generation. Unlike Galileo, who according to Baliani relied on space, those other scholars took time as the independent variable. Despite this change, their reasoning resembled Galileo's in a major respect, because their condition reads like a constraint imposed by our mind on the proportion between space and time of fall. Once again, it is not a contrived experiment that serves as a foundation for the mathematical theory, but rather a condition imposed by our mind on the possible or reasonable form experimental results can take. This strategy shows some similarity to imposing a symmetry principle, but it also appears somewhat less direct and more sophisticated in that it relies on the notion of unit measure.

The seventeenth-century scholars who adopted this form of reasoning were mathematicians Torricelli, Huygens, and Jacques Le Tenneur, as well as the physician Theodore Deschamps. In his correspondence with Mersenne in the late 1640s, for example, Huygens ridiculed alternative rules based on the sequence of natural numbers 1, 2, 3, 4, etc. or on a geometric relation, 1, 2, 4, 8, etc., arguing that they violated the invariance of unit measures. Such alternative rules had actually been proposed by the Bishop Jean Caramuel, the Jesuits Honoré Fabri and Pierre Le Cazre, and the Genua nobleman Gianbattista Baliani.[22] While empirically it may have been difficult to refute those rules, the request that they satisfy unit invariance ruled them out in the eyes of the scholars mentioned above.

Later in the century, interest in this problem shifted from invariance to physical causes and Galileo's odd-number rule was considered as a viable approximation. Probably this is the reason why such debates attracted no further attention.

4. Huygens and Impact

In the Latin and French editions of *Principia philosophiae* of 1644 and 1647, Descartes put forward seven rules of impact for hard bodies. The rules relied on the third law of motion, stating the conservation of quantity of motion, or magnitude of a body times its speed, with no regard to direction. Descartes presented his laws of motion with the help of theological considerations about God's immutability, whereas he claimed that the seven impact rules were self-evident. He did not follow the Archimedean axiomatic approach and did not produce a deductive structure; rather, he argued that his book should be read as a novel. Overall, Descartes favored heuristic approaches and disliked axiomatic presentations, so much so that even his *Géométrie* contains no axioms. Yet it is perfectly possible to seek alternative formulations both of his mathematics and rules of collision.[23]

Huygens was one of the first readers to realize that Descartes's rules were problematic and to tackle the issue in a new fashion. The problem looked in some respects similar to the equilibrium of the balance with equal arms and equal weights, in that one could start from the symmetrical case of equal bodies colliding with equal and opposite speeds. Much like the case of the balance, symmetry considerations require that the two bodies behave in the same way after the impact, but this condition is insufficient to deal with more general cases. The outcome of his efforts was the treatise *De motu corporum ex percussione*, which remained unpublished in his lifetime and saw the light only posthumously in 1703. It consists of five hypotheses and thirteen propositions. It was not entirely uncommon to find the term "hypothesis" meaning "presupposition" or "postulate," as done by Huygens here.

The first hypothesis states:[24] "Any body once moved continues to move, if nothing prevents it, at the same constant speed and along a straight line." This statement corresponds to Descartes's first two laws of nature, but it is devoid of the theological justifications one finds in Descartes. In fact, Huygens provided no justification, suggesting that the matter appeared unproblematic in his eyes. The second hypothesis poses a restriction on the type of bodies investigated, namely hard bodies. For this special case Huygens's hypothesis claimed that when two equal bodies collide with equal speeds, they rebound with the same speeds reversed.

It is the third hypothesis that is of special interest here, because Huygens had recourse to the principle of relativity of motion. Galileo had discussed a similar principle in the *Dialogo*, defending it with a series of observations on the behavior of bodies on a moving ship, but he was far from being the first to do so. In the opening of day four of the *Discorsi* Galileo relied again on relativity of motion in order to argue that horizontal projection does not affect the uniformly accelerated motion of falling bodies. Huygens formulated relativity of motion as a hypothesis or postulate and applied it in a quantitative fashion. In this way he was able to move from a perfectly symmetric case to one where one body at rest is hit by a supervening equal body. With the example of unit invariance for falling bodies fresh in our mind, we notice here a similar approach whereby an invariance condition enables the formulation of propositions about nature. The case of unit measures, however, was entirely devoid of empirical presuppositions: nobody performed experiments in Florence and Rome to test whether the different units of length employed there had any impact on the outcome. Empirical considerations, however, probably entered the principle of relativity of motion in a mediated way. Huygens did not feel the need to justify it empirically, however, as if human intuition by the mid-seventeenth century authorized his move. His account of impacts on a moving barge reads not like a description of an empirical test, but more like the mental operations on weights described by Archimedes and Galileo in their discussions of the equilibrium of the balance:[25]

> The motion of bodies and their equal and unequal speeds are to be understood respectively, in relation to other bodies which are considered as at rest, even though perhaps both the former and the latter are involved in another common motion. And accordingly, when two bod-

> ies collide with one another, even if both together are further subject to another uniform motion, they will move each other with respect to a body that is carried by the same common motion no differently than if this motion extraneous to all were absent.

This abstract statement is instantiated in a more intuitive fashion with the example of the moving barge. Huygens seems to have recourse to a mixture between intuition and experiences that would have been common enough to readers familiar with travels on boat along canals, a common means of transport in the Netherlands and elsewhere:[26]

> Thus, if someone conveyed on a boat that is moving with a uniform motion were to cause equal balls to strike one another at equal speeds with respect to himself and the parts of the boat, we say that both should rebound also at equal speeds with respect to the same passenger, just as would clearly happen if he were to cause the same balls to collide at equal speeds in a boat at rest or while standing on the ground.

The list of hypotheses does not cover all Huygens's assumptions and axioms. Buried in the text of Proposition 8 there is a statement of one of his favorite axioms. The proposition states that in the collision between two bodies with speeds inversely as their magnitudes, the bodies will rebound with the same speeds with which they approached each other. In the proof Huygens converted horizontal to vertical motion, a rather straightforward move if one considers the collision between pendulum bobs. In this context Huygens affirmed:[27] "in mechanics it is a most certain axiom that the common center of gravity of bodies cannot be raised by a motion that arises from their weight." This axiom derives in all probability from a combination of Torricelli's principle and the denial of perpetual motion and, much like its ancestors, is not justified by having recourse to experiments. Huygens probably assumed that his readers would grant it based on common intuition and a wide range of experiences.

In 1673 Edme Mariotte, Huygens's colleague at the Paris *Académie*, published a treatise on the collision of bodies where he put forward a theory closely resembling Huygens's. Unlike the Dutch scholar, however, Mariotte chose as the foundation for his theory what he called "principles of experience," namely propositions based on collision experiments. Whereas Huygens had struggled to build his science almost a priori in Archimedean fashion, Mariotte's treatise is a collection of statements of what happens when bodies with a given magnitude and speed collide.[28]

5. A Newtonian Coda

The case of Isaac Newton is considerably complex and I am therefore going to discuss here briefly only his three axioms or laws of motion in *Principia mathematica*. It is significant that Newton qualified the three laws as "axiomata sive leges": they indeed do serve as axioms to his system. His first law, the law of inertia, subsumes Descartes's first two laws of motion.[29] The second law states that the change in the quantity of motion is proportional to the force impressed, whether applied continuously or in impulses. Historians have extensively discussed this law, despite the fact that neither Newton nor his contemporaries made much of it.[30]

The third law states that the mutual actions of two bodies are always equal and in opposite directions. A corollary to this law states the conservation of quantity of motion in one direction, a result previously established by Huygens. Huygens and Newton presented their equivalent propositions in different ways. In the brief essay on collision published in 1669 in the *Journal des Sçavans*, Huygens gave general rules that could be seen either as presuppositions or results of his investigations. The fourth rule echoes proposition VI of *De motu corporum ex percussione*, stating that quantity of motion (in the Cartesian sense) of two bodies can be diminished or augmented as a result of collision. In rule 5 in the 1669 essay, however, he added that quantity of motion in the same direction is conserved.[31] Newton felt the need to justify his third law in various ways, first by providing examples, then in the scholium by discussing thought experiments, and even reporting the outcome of real experiments, including numerical data.[32] In the scholium he considered both collisions among bodies and attractions. For collisions he sought to test the law by means of pendulum experiments showing that quantity of motion, taking direction into account, is not changed by the impact among bodies, whether hard or soft (see Illustration 5). He concluded the experimental report with the words:[33] "In this manner the third law of motion—insofar as it relates to impacts and reflections—is proved by this theory [of impact], which plainly agrees with experiments."

In the case of attractions, Newton could not experiment with gravity; therefore he justified the third law first by means of an experiment with magnetic bodies placed on floating supports in a bowl of water, showing that action equals reaction because the bodies stay still where they come together, rather than moving in one direction. This case highlights his eagerness to rely on experiments if at all possible. In the case of gravity he had recourse to a thought experiment consisting in cutting the earth in two unequal parts and showing that they do not move as a result of their mutual attractions.[34] In general, despite the case of linear momentum, Newton did not rely on conservation principles and did not make much of

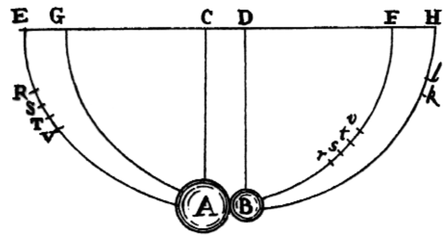

ILLUSTRATION 5: Newton's pendulum experiments.

Two pendulums CA and DB collide and rebound. By carefully subtracting the effects of air resistance (if RV is the effect of air resistance in a full to and fro oscillation, one quarter of it is ST or *st*), Newton showed that action equals reaction for all bodies, whether hard or soft.

them in his system. For example, he still believed that Cartesian quantity of motion (without taking direction into consideration) was a significant measure of motion in the universe and rejected the conservation of living force (mass times the square of the speed) in all its manifestations.

6. Conclusion

Seventeenth-century scholars tackled the problem of presenting and justifying new knowledge about nature in different ways. Some, like Mersenne, Mariotte, and Boyle, attributed a key foundational role to experiment. Despite considerable differences in their approaches, they all believed experiment to be crucial in their investigations as well as in their formal presentation. Others, such as Stevin, Galileo, Torricelli, and Huygens, saw experiment as inherently problematic in this role and sought to find secure foundations elsewhere. I have no doubt that all of them, especially Galileo and Huygens, were remarkable and creative experimentalists. However, they shared a common concern for establishing knowledge about nature in an axiomatic fashion, on the example of mathematics and in a tradition going back to Archimedes. The key idea seems to be that some propositions can appear natural to the mind, yet they entail a number of less natural-looking consequences. These consequences can help to establish a science or portions of it, or at least they can rule out a number of competing alternatives, as in the case of unit invariance for falling bodies. Thus, premises to which the mind naturally consents, often arranged in a contrived fashion, can have surprising empirical implications.

Some axioms look quite convincing, such as the principle of symmetry for the case of the balance with equal arms and equal weights and for equal bodies colliding with equal and opposite speeds, but they do not allow one to go very far. Torricelli's principle provides some flexibility in the formulation of a science of motion. Other principles, such as that later named by Newton as the principle of inertia, became increasingly more acceptable in the second half of the seventeenth century as a result of a conceptual reorientation rather than specific contrived experiments. In this specific case the acceptance of the principle has a historical dimension that becomes especially significant in the course of the seventeenth century. The case of the unit invariance for falling bodies is peculiar in some respects, since it was not formulated as an axiom, but rather as a criterion to rule out competing rules for falling bodies. It does share with other principles the feature of being a requirement of reason limiting the ranges of possible behavior of falling bodies.

The tradition of formulating principles or axioms we expect nature to observe shows us mathematicians in action seeking propositions about nature not, or not only, through experimental activities, but by thinking about a reasonable course nature is expected to follow and by imposing conditions and restrictions on the form of the mathematical relations describing nature's course. This tradition is significant in the formal presentation of a science and is an important component, alongside the much-more frequently celebrated experimental method, in the history of science and philosophy in the seventeenth century.

The contents of my essay resonate with some of the themes of this volume, such as the importance of a priori or non-empirical principles in science, and the temporal and historical dimension of those principles. Of course, I am not advocating a teleological reading of Kantian themes into the past leading all the way from Archimedes, through Galileo, to the first *Critique* and Carnap. Rather, my aim is to provide historians and philosophers of science with material for reflection on the role and significance of a multifarious non-empirical tradition in the history of science. With regard to Kant, I believe it would especially significant to explore what role Leibniz's works, such as "Brevis demonstratio erroris memorabilis Cartesii et aliorum," played in the assimilation and development of the themes explored in this essay.

NOTES

1. I wish to thank all those who offered comments on previous versions of this essay at the University of South Carolina, Columbia, and the University of California, San Diego. Mike Mahoney was kind enough to offer helpful observations on an earlier draft of this essay. A special thanks to the editors for their thoughtful and constructive comments and to Jordi Cat for a fruitful conversation.

2. Dear (1995); Daston (1991); Newman (2004), ch. 5.

3. Another example is Euclid's *Optica* (Euclid, 1985). See Lindberg (1976), 12–13. As Tal Golan pointed out, Euclid's *Elements* too can be seen as a mathematical science dealing with nature, notably physical space.

4. Mahoney (1998), 707–8 and (1985), 4–5 of the online version. Those maxims, however, were not generally accepted. Guidobaldo dal Monte, for example, did not accept no. 3; see Drake and Drabkin (1969), 300, 316, 318.

5. My views do not differ from Mahoney's on this point.

6. Steinle (2008). Funkenstein (1986), ch. 3.

7. Schaffer and Shapin (1985). Shapin (1994). Dear (1995). Some topics relevant to my discussion were introduced in Holton (1973, 1988).

8. Dijksterhuis (1987), 373–77;. Duhem (1991), 247–49.

9. Dijksterhuis (1987), 286–87. I have slightly altered the translation.

10. For a brief survey see Dijksterhuis (1987), 290–98.

11. Stevin (1955), 179 and 401. Stevin was familiar with Archimedes's works in the edition of Federico Commandino.

12. Galileo (1890–1909), 4: 77–78 and 8: 156–57.

13. Galileo (1890–1909), 8:152–54.

14. On the leaning tower experiment, for example, see Camerota (2004), 61–62.

15. Camerota (2004), 91, 138. Galileo (1890–1909), 2:180; 10: 115–16, Galileo to Sarpi, Venice, 16 October 1604; 10: 248–49, Valerio to Galileo, Rome, 18 July 1609. Wisan (1978), especially 5, 44.

16. Galileo (1974), 162. Galileo's alleged claim at 169 that: "sensory experiences . . . are the foundations of all resulting structure" is a serious mistranslation by Drake.

17. Galileo (1974), 162.

18. Galileo (1974), 164.

19. Galileo (1974), 171–75.

20. For Mersenne's changing attitudes to Galileo see Dear (1988).

21. Galileo (1890–1910), 13: 348–49, Castelli to Baliani, 20 November 1627.

22. Bertoloni Meli (2004), 182.

23. Descartes (1983), 59–69.

24. Huygens (1888–1950), 16: 30. Here and in later quotations I rely on the translation by Michael Mahoney at http://www.princeton.edu/~hos/mike/texts/huygens/impact/huyimpct.html

25. Huygens (1888–1950), 16: 32.

26. Huygens (1888–1950), 16: 30. Michael Mahoney drew my attention to Huygens's drawing of a barge on a canal, 16: 122.

27. Huygens (1888–1950), 16: 56. Duhem (1991), 261–64 and 349–56.

28. Mariotte (1673).

29. But see Gabbey (1980), 272–97.

30. Westfall (1971) devoted a whole book to the history of the second law in the seventeenth century. Newton (1999), 416–17.

31. Huygens (1888–1950), 16: 48–49 and 180. Huygens's essay in the *Journal* is reproduced in 16: 179–81. It is worth noticing that Huygens did not make an axiom of the conservation of the sum of the products of the bodies'

magnitudes times the square of their speeds (for hard bodies), but rather he took it to be a result of his investigations in proposition XI. Huygens (1888–1950), 16: 73 and 180, where the same proposition appears as rule 6.

32. In the *Principia* Newton generally relied on a combination of high-power mathematics and empirical data, whether from experiments or astronomical observations. See Smith (2002). For optics see Shapiro (1993), 1–40.

33. Newton (1999), 417, 420–21; quotation from 427.

34. Newton (1999), 427–29. Friedman (2001), 39 n.46.

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