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Mental models in Galileo's early mathematization of nature

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1. Introduction: the question of Galileo's early mathematization of nature

Distinguishing between the mathematical followers of Archimedes, notably Galileo, and the Aristotelians of the late sixteenth century, William R. Shea asserted that '[m]athematicians, under the guidance of Euclid and Archimedes, viewed the world in terms of geometric shapes which obeyed mathematically expressible laws'.[1] In my judgement, Shea's view should be accepted, even though it was not only Euclid and Archimedes who escorted Galileo into new territories such as, for instance, *Two New Sciences*, or the *Discourse on Buoyancy*. A more complex picture has gradually emerged thanks to a number of studies that have examined in detail Galileo's acceptance of the Euclidean theory of proportions (or proportional reasoning) as the language of early mathematized physics.[2]

Yet no research has so far been devoted to the cognitive mechanisms underlying Galileo's mathematization of nature. This paper addresses some questions related to this theme by adopting a cognitive history perspective which relies on the theory of *mental models* (on mental models, see Sect. 2, Pt. I; on cognitive history, see Sect. 2, Pt. II). Moreover, through a discussion of Galileo's alleged use of thought experiments the paper suggests how a cognitive history perspective might complement

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[1] Shea (1972), p. 5.

[2] See Drake (1973, 1974a, 1974b, 1987); Giusti (1986, 1992, 1993); Palladino (1991); Palmieri (2001). For a general treatment of the Euclidean theory of proportions I have relied on: Grattan-Guinness (1996); Sasaki (1985); Saito (1986, 1993). Rose (1975) is the most detailed survey of Renaissance Italian mathematics from a non-technical point of view. See also Sylla (1984), pp. 11–43.

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current historiographical approaches, thus appealing to a broader, cross-disciplinary audience.

The late-Renaissance mathematics that Galileo assimilated was generally cast in natural language. That type of mathematics was in part subject to the same cognitive rules that govern natural languages. Practising mathematics, especially Euclidean and Archimedean mathematics, meant constructing mathematical arguments in the form of verbal proofs. Therefore, to appreciate the import of that practice one must respect its linguistic character. Furthermore, mathematical discourse was concerned with diverse visual constructs, such as the diagrams of Euclid's *Elements* and the representations of balances and weights in the Archimedean tradition. Those constructs interacted with the linguistic construction of proofs in the creative process of the production of new mathematical knowledge. Most Galileo scholars have tended to underestimate the importance of these factors, sometimes because of the anachronistic tendency to re-write (or unconsciously rethink) that form of mathematics in algebraic symbolism.

Over the last thirty years or so a number of studies have addressed various questions concerning Galileo's early mathematization of nature.[3] In particular, Winifred Wisan and Enrico Giusti's contributions are most relevant for the present project. Both scholars have dramatically raised our awareness of the problems concerning the language of Galileo's early mathematical natural philosophy.[4] I now wish to review Wisan and Giusti's results briefly, in order to indicate how this paper will contribute to what I believe are still major unresolved questions.

Wisan's paper is the most detailed analysis to date of the corpus of published and unpublished materials regarding Galileo's theory of naturally accelerated and projectile motion. Wisan took into account numerous ancient, medieval, and Renaissance sources upon which Galileo seems to have relied, but reached the conclusion that Galileo's work on motion was 'novel in its conception and execution'.[5] In my view, the methodological flaw which undermines the strength of Wisan's approach is the translation of Galileo's mathematical language into an algebraic notation that was quite alien to the late Renaissance. For example, in her analysis of Galileo's proof of the law of the balance, Wisan furnishes a reconstruction that has little to do with the Galilean original, simply asserting that Galileo's method is 'in the Euclidean–Archimedean tradition'.[6] However, the translation of verbal mathematical arguments into a symbolic notation inevitably carries with it the risk of missing important aspects that are inextricably linked with the original language. It is difficult to find anything within the Archimedean tradition that bears any resemblance to how Galileo actually remodelled the Archimedean balance (a cornerstone of his early study of simple machines and statics). Indeed, Wisan does not furnish any clue to why we

[3] Drake (1970, 1973, 1974b, 1987); Wisan (1974); Koyré (1978); Galluzzi (1979); Giusti (1986, 1992, 1993, 1994, 2001); De Gandt (1995); Machamer (1998); Remmert (1998), Di Girolamo (1999); Wallace (2000); De Groot (2000).

[4] Wisan (1974) and Giusti (1993).

[5] Wisan (1974), p. 110.

[6] Wisan (1974), p. 158.

should suppose Galileo's proof belongs to that tradition. Her conclusions depend on the historically effacing effect induced by algebraic notation. There is no question that Galileo knew something of the Archimedean tradition, especially of its late Renaissance developments, such as Guido Ubaldo dal Monte's commentary on Archimedes' *On Plane Equilibrium*. But Archimedes' treatment of the balance is completely different from Galileo's, and nothing in Guido Ubaldo's comments suggests any influence on Galileo. We are therefore left with no answer to the question of how Galileo mathematized the Archimedean balance. Yet the importance of this item of Galilean science cannot be denied since it was eventually published by Galileo in *Two New Sciences* as the very foundation of his 'new science' concerning the strength of materials. As we shall see, we need a cognitive approach to the language of proportional reasoning in order to understand how Galileo mathematized the Archimedean balance.

In my view, a major advance in our understanding of Galileo's early physical–mathematical language has been made possible by Giusti's research on the Euclidean theory of proportions in both Galileo and the Galilean school. Firstly, Giusti has contributed to the clarification of Galileo's meaning of *proportion* as a relationship of similarity between two ratios formed by homogeneous quantities. In Galileo's view, no ratios are possible between heterogeneous quantities.[7] Secondly, Giusti has shown that Galileo's notion of proportionality cannot be separated from Euclid's notion of equimultiple proportionality, and that Galileo's use of the technique of the equimultiples is based on Euclid's theory of proportions (more on this in Sect. 3).[8] Finally, Giusti has illuminated the complex development that proportion theory had within the Galilean school and the process through which other Galileans, such as Evangelista Torricelli and Giovanni Alfonso Borelli, came to assimilate Galileo's late rethinking of Euclid's theory. Giusti's research has allowed us to make significant progress towards understanding the technical language of Galileo's mathematization of the natural world. Nevertheless, as we shall see, Galileo proceeded beyond Euclid's theory of proportions. For example, as will be explained in relation to Galileo's early theorems on centres of gravity, he appeals to cognitive resources that are not describable in Euclidean language, but which come to light from a cognitive perspective. A notable disagreement erupted between Galileo and Christoph Clavius on the legitimacy of visual constructs in mathematical proofs that allows us to bring to light the cognitive mechanisms underlying Galileo's extension of proportional reasoning.

[7] Giusti (1993), pp. 57ff. Wallace (2000, pp. 104–105), bases his thesis that Galileo applied the method of *demonstrative regress* on the reconstruction of one of Galileo's *De Motu* proofs in which the author questionably assumes ratios between weights and speeds. For a different view on mathematics *vs*. Paduan Aristotelianism, see Ventrice (1989), pp. 163–195. Lennox (1986, p. 51) argues that Aristotle insisted upon the use of mathematics in optics, mechanics, astronomy, and harmonics and that 'there seems to be nothing in Galilleo's appeal in his new science to mathematical demonstration that Aristotle would not have wholeheartedly endorsed'. This may be true for Aristotle, the Greek philosopher, but does not explain why the sixteenth-century Aristotelians never produced anything like the *Discourse on Buoyancy* (fiercely opposed by the Aristotelians) or *Two New Sciences*.

[8] Giusti (1986, 1992).

In addition, there is the still-unresolved question of Galileo's alleged use of thought experiments, especially in his early work. Concluding his *Études Galiléennes* (1939), Alexandre Koyré put forward his seminal thesis concerning Galileo's Platonism, portraying Galileo's mathematization of nature as the vindication of a Platonic approach to the study of the natural world.[9] As is well known, Koyré's vision of a Platonic Galileo has a correlate; that of Galileo as a 'user and abuser' of thought experiments.[10] Many scholars, as is well known, have followed Koyré's thesis on Galileo's use of thought experiments. I will argue that mental models and cognitive history offer a better framework for understanding Galileo's alleged use of mental experimentation. This suggests that mental models and, more generally, problems related to the study of cognitive mechanisms, should be of interest to a broader audience of scholars in the history and philosophy of science, as well as to cognitive scientists and philosophers of the cognitive sciences.

2. Mental models and cognitive history

This Section is divided into two parts. The first part will present the theory of mental models. The second part will discuss what I see as the most controversial issues concerning both the notion of mental model and its relationship to historical analysis.

2.1. Part I

Perhaps the best way to approach the many issues concerning the theory of mental models is to begin with a question posed by cognitive scientist, Philip Johnson-Laird, the major proponent of mental models: 'how many sorts of mental representation are there?'[11] According to Johnson-Laird, there are three sorts of mental representations: a) propositional representations; b) mental images; c) mental models. Johnson-Laird refers to this hypothesis as the 'triple code' hypothesis.[12] Let us now see how mental models differ from the other two forms of mental representation.

According to the propositional representations view, the mind has 'a unitary system of mental representations based on a language of thought'.[13] Johnson-Laird furnishes a simple example of how propositional representations might work. Suppose that the following description is presented to individuals:

The spoon is to the left of the knife
The plate is to the right of the knife

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[9] Koyré (1978, 1943)
[10] Koyré (1973), pp. 224–271.
[11] Johnson-Laird (1996), p. 90; (1983), pp. 146–166.
[12] Johnson-Laird (1996), p. 92.
[13] Johnson-Laird (1996), p. 93.

then it will be encoded by their minds in propositional representations. Such propositional representations take a predicate-argument form, such as:

(left-of spoon knife)
(right-of plate knife).

Subsequently the individuals might infer that: the spoon is on the left of the plate. Johnson-Laird asserts that the 'propositional theory explains this ability in terms of a mental logic containing formal rules of inference'.[14] It is important to note that the syntax and lexicon of the mental language which is hypothesised to encode propositional representations are unknown. If the mind uses such a system then it will perform deduction tasks corresponding to those of formal logic. But are propositional representations and formal rules of inference sufficient in order to explain how the mind works in all situations? Empirical findings suggest not.

In an experiment, individuals were presented with a determinate description (that is one precisely corresponding to a single state of affairs). After listening to the description, participants had to decide whether the description was true or false of a particular diagram depicting all the relevant objects. The same task was repeated, presenting participants with an indeterminate description (that is, one corresponding to more than one state of affairs). The indeterminate description was consistent with both the first diagram and a second one in which the objects had been laid out slightly differently. After classifying the descriptions participants were unexpectedly asked to rank-order 'four versions of the description in terms of their resemblance to the actual description'.[15] The first two versions were consistent with the layout. The second two were foils. For the determinate descriptions participants reliably rated the first two descriptions higher than the second two. For the indeterminate descriptions they reliably ranked the actual description higher than that consistent with the layout but having a different meaning. According to Johnson-Laird, a plausible interpretation of these surprising findings is as follows: when confronted with determinate descriptions participants attempted to construct an image or more abstract model of the situation. But when confronted with indeterminate descriptions they abandoned the previous strategy and tried to hold on to propositional representations. Why? Because images or models lead to a good memory for the layout but to a poor memory for the verbatim details. The opposite is true for propositional representations.[16]

In summary, according to Johnson-Laird, these results strongly suggest a 'dissociation between two sorts of representation—that is, a preference for models or images for spatially determinate descriptions, and a preference for propositional representations for spatially indeterminate descriptions'.[17]

[14] Johnson-Laird (1996), p. 93.
[15] Johnson-Laird (1996), p. 96.
[16] Van der Henst (1999) explores pragmatic factors that might further illuminate how indeterminacies affect deductive reasoning.
[17] Johnson-Laird (1996), p. 97.

So far a distinction has been drawn between propositional representations and mental images (or more abstract models).[18] But is there a real difference between mental images and mental models? According to mental models theorists, there is.

First of all, a mental image simply represents the ‘perceptible aspects of a situation from an observer’s point of view’.[19] On the other hand, ‘a mental model represents individuals by mental tokens; it represents the properties of individuals by the properties of these tokens, and it represents the relations among individuals by the relations among these tokens’.[20] Therefore, the ‘simplest sort of mental model has an analogical structure that corresponds to the structure of the situation that it represents. . . . [L]ike diagrams, these simple models are isomorphic, or at least homomorphic, to what they represent’.[21] What is interesting for our present purposes is that mental models really seem to constitute a third type of mental representation distinct from mental images. The latter share with the former the analogical structure with states of affairs. Images as well as mental models are isomorphic with the states of affairs they represent. But mental models can incorporate abstract elements that by definition escape ‘imageability’.[22] In summary, it is important to realize that mental models are completely general cognitive constructs, not confined to reasoning tasks, such as syllogistic reasoning. They ‘can be three-dimensional, kinematic and dynamic . . . [models] can embody classes of situations in a parsimonious way. Hence, they can represent any situation, and operations on them can be purely conceptual.’[23]There is now a growing body of research which suggests that many forms of spatial reasoning in humans are to a large extent model-based.[24] Although mental images can be mentally manipulated (for example, rotated), they cannot capture classes of situations.[25]

To sum up, according to the theory of mental models, three types of mental rep-

[18] Bonatti (1994a,b) presents an interesting critique of mental model theory and suggests that there is as yet no convincing evidence to abandon the hypothesis of a mental logic based on rules of inference.

[19] Johnson-Laird (1996), p. 93.

[20] Johnson-Laird (1996), p. 102.

[21] Johnson-Laird (1996), p. 102. According to Johnson-Laird and Byrne (2000), ‘[m]ental models are representations in the mind of real or imaginary situations. Scientists sometimes use the term “mental model” as a synonym for “mental representation”, but it has a narrower referent in the case of the theory of thinking and reasoning’. It is worth pointing out that mental model theory has a strong empirical basis and many ramifications in the cognitive sciences. An extensive bibliographic list collecting very recent work on mental models is in Johnson-Laird and Byrne (2000).

[22] A few findings suggest that *negation*, for example, may be one of these abstract elements. Experiments carried out with the quantifier ‘only’ showed that mental models can represent negation, which is obviously an abstract relation (Johnson-Laird, 1996, pp. 114–120). Other aspects concerning thought mechanisms have been studied within the framework of the theory of mental models. A theory of deduction was put forward by Johnson-Laird and Byrne (1991) a decade ago. (See also Johnson-Laird, 1996, pp. 102–111.) More recently an account of the meaning of naïve causality has been proposed wholly within the conceptual framework of mental models (Goldvarg & Johnson-Laird, 2001).

[23] Johnson-Laird (1996), p. 124.

[24] In particular, see Glasgow and Malton (1999), who develop a formal semantics for spatial reasoning based on Johnson-Laird’s theory of mental models.

[25] Richardson (1999), pp. 41ff.

resentations can be distinguished: 1) propositional representations; 2) mental images; and 3) mental models. All of them seem to be used by humans under different circumstances. Propositional representations function in accord with logical rules of inference. The most interesting characteristic of both mental images and mental models is their isomorphism with the states of affairs they represent. But mental models are able to encode abstract elements that by definition escape imageability. Furthermore, mental models can represent entire classes of situations and be both kinematic and dynamic.

2.2. Part II

In my view, three methodological problems confront the historian who has adopted a cognitive framework. The first concerns the notion of *mental representation* in general. It is a question in the philosophy of the cognitive sciences. The second concerns *connectionism* and the general theory of cognition. The third and more urgent one concerns the *applicabilitiy* of cognitive science to the history of science. In this second Part of the Section, the first two problems will only be discussed insofar as they bear on the third, which can be further articulated as follows. To what extent are the constructs studied by today's cogntive science, such as mental models, metahistorical cognitive mechanisms? In other words, to what extent are those constructs defining charecteristics of cognitively modern humans (assume provisionally that *modern* means the last five or six millennia)? Let us try to answer these questions.

A complex literature has accumulated on what Robert Cummins refers to as the *problem of mental representation*, that is, the problem of the explanatory role assigned to the notion of mental representation by empirical cognitive science.[26] I will therefore have to be extremely selective and enucleate what matters most from a historian's point of view.

As we saw in Part I, what a mental representation ultimately is and how it functions in human cognition remains an empirically open question.[27] However, according to Cummins, the notion of mental representation per se cannot be dissociated from the broader conceptual framework of a theory of cognition. Cummins' view is that if we assume a *computational* theory of cognition, that is, one according to which 'cognition is disciplined by symbol manipulation', then we must commit ourselves to an ahistorical notion of mental representation.[28] Prima facie this seems to run counter to much research in the history and philosophy of science, which has stressed the historicity and cultural-boundedness of scientific development. In the context of the computational theory of cognition, however, 'ahistorical' has a highly technical con-

[26] Cummins (1991), pp. 1–2.

[27] Cummins (1991), p. 1, and, in general, Richardson (1999).

[28] Cummins (1991), p. 13 and pp. 80ff. To prove this point Cummins proposes a mental experiment in which a duplicating machine makes a molecule-by-molecule copy of a human being. He then argues that the physically-equivalent copy preserves the cognitive identity of the original, but of course cannot share its history. Hence the necessity of a commitment to an ahistorical notion of mental representation.

notation. It means that computationally equivalent states are representationally equivalent, regardless of the history of the cognitive system displaying those states. In other words, a cognitive system's ability to compute—that is, its ability to represent and cognize—depends on its current state only, not on the history that led to that particular state.[29]

In my view, historians need not be concerned with this technical notion of ahistoricity, which is related to such basic cognitive mechanisms as mental models.[30] Basic cognitive abilities alone do not explain intellectual history. We need historical analysis to flesh out metahistorical cognitive mechanisms with historically meaningful content. The latter can only come from culturally-bounded frames of knowledge and the rich diversity of historical cultures.[31]

I would argue that cognitive science and the history of science have much to gain from more sustained interdisciplinary collaboration. While empirical cognitive science cannot reproduce historical settings in a laboratory, the models it has to offer can be applied to history, *provided that historians are able to historicize those models*.

Reviel Netz has recently used the expression 'cognitive history' in referring to the possibility of describing those cognitive mechanisms which only exist '*historically*, in specific contexts'.[32] However, on the basis of Jerry Fodor's modular conception of mind, Netz has struck a pessimistic note claiming that cognitive science cannot have access to deep mechanisms, such as the fixation of belief, which would ultimately explain the historical process.[33] I am not a cognitive scientist, but I do not share Netz's pessimism. Cognitive science, like all science, evolves, and I see nothing in its present state to suggest that it will not be able to tackle even such profound and still mysterious processes as belief fixation. Equally optimistically, I believe that the historical study of science has much to contribute to cognitive science, precisely in the form of Netz's cognitive history.

I now wish to confront the challenge coming from the connectionist paradigm in cognitive science. Robert Cummins has furnished a thorough analysis of this ques-

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[29] A familiar example from classical mechanics will help in grasping this technical connotation. Suppose you have a differential equation describing the motion of a particle in a Newtonian universe. At time t and position p the particle's future motion will be determined only by its current state at (p, t); that is, its position and speed, plus the forces acting on it at (p, t). The future motion of the particle does not depend on its history, i.e., on its motion from the initial instant to time t.

[30] Recently, Turner (2001) has exploited the basic cognitive mechanism known as 'blending of conceptual spaces', or conceptual integration, in order to furnish a framework for a new cognitive approach to social science. In Turner's view, social science is concerned with the study of cognitively modern humans, where 'modern' means roughly the last fifty thousand years. (Turner, 2001, p. 4).

[31] Different accounts of mental representation based on evolutionary theory have been suggested that do not have an ahistorical notion of mental representation as a direct consequence. However, in my view, the differences are immaterial because in any case the time scale involved in evolutionary processes renders the cognitive mechanisms in which we are interested, the eventually stabilized cognitive structures, de facto metahistorical. See Millikan (1984, 1993, 2001).

[32] Netz (1999), p. 6.

[33] Fodor (1983); Netz (1999), p. 6.

tion, which I shall follow before making explicit a number of conclusions relevant to this paper's project.[34]

In general, within the framework of the computational theory of cognition, a cognitive capacity, such as that for building mental models, or that for playing chess, is a 'function whose arguments and values are epistemologically related'.[35] Thus, for example, the capacity for playing chess is a cognitive capacity. A chess programme instantiates a cognitive function that mediates between arguments and values. In general, however, cognitive capacities, or functions, are very difficult to specify.[36] Connectionism can in principle overcome the specification problem, because 'it is possible to "train" a network to have a cognitive capacity without having even the beginning of an analysis of it . . .'.[37] This would imply that the definition of a cognitive capacity as an inference function is incompatible with connectionism. For our present purposes, the most interesting form that the connectionist challenge takes is the *incommensurability thesis* (note that this has nothing to do with Kuhnian incommensurability).[38] It argues that 'when a connectionist system satisfies a cognitive function, it computes over representations that have no interpretation in the domain in which the target cognitive capacity is specified'.[39] Cummins suggests the following counter-argument to the incommensurability thesis.

We need not reject the notion of cognitive function providing that we extend its meaning to incorporate non-symbolic representational cognitive functions, that is, functions which take non-symbolic arguments into non-symbolic values. Cummins exemplifies his view precisely with the Galilean scheme of reasoning based on the representation of speed, distance, and time, by means of simple geometrical entities.[40] Indeed, in Cummins' view, the Galilean scheme could be applied to any set of magnitudes that satisfy the geometrical relationships between the geometrical entities associated by Galileo to the physical quantities. Such a scheme, Cummins concludes,

[34] Cummins (1995).

[35] *Epistemologically related* means that arguments and values must be specifiable in terms of an inferential process of symbolic reasoning (Cummins, 1995, p. 106).

[36] Cummins (1995), pp. 106–107.

[37] It is difficult to specify what, for example, the capacity to plan a party with friends consists in (Cummins, 1995, p. 107).

[38] It is called *incommensurability argument* because it is based on the idea that 'connectionist representational schemes are incommensurable with the (typically symbolic) schemes that must be used to specify cognitive functions' (Cummins, 1995, p. 110).

[39] Cummins (1995), p. 108. Let us remember that a *connectionist model* is based on a distributed architecture whose network structure is given by nodes and activation paths intermediate between a level generally called *input* level and a level generally called *output* level. In this case, the cognitive function satisfied by a connectionist network which has learned it has its arguments and values specified in a non-symbolic way, which is crudely equivalent to saying that you are unable to specify what the symbolic representations at the input level are, what those at the output level are, and to explain what deduction has been performed between those representations.

[40] Cummins (1995), pp. 112–113. While Cummins' analysis may be historically inaccurate it nonetheless gives a pretty good idea, as we shall see, of the non-symbolic cognitive functions Galileo used in his early mathematization, namely, mental models.

can obviously be used to reason geometrically.[41] Along these lines, Cummins has proposed a *picture* theory of mental representation, according to which a mental representation is a relation between two isomorphic structures.[42] In Cummins' words, the basic idea is that 'to represent something is to have its structure'.[43]

To conclude, I would argue that we can generalize Cummins' view on cognitive functions as follows. A *mental model* is a *non-symbolic representational cognitive function relating two isomorphic structures*. It takes non-symbolic arguments into non-symbolic values. Briefly:

MENTAL MODEL: (non-symbolic argument) → (non-symbolic value).

3. The visual dimension of Galileo's proportional reasoning

About 1588, the young Galileo circulated a manuscript containing a number of theorems on centres of gravity.[44] These theorems were his chief hope of establishing his reputation as a mathematician and obtaining a university chair. On 8 January 1588, Galileo wrote to Christoph Clavius in Rome asking his opinion about a few difficulties regarding his new theorems. Galileo told Clavius that some people in Florence had already examined his theorems but remained unconvinced that his way of proceeding was sound. More precisely, what Galileo was eager to know was whether the proof of the proposition underpinning all subsequent theorems was able 'to quiet Clavius' intellect entirely'.[45] An exchange of letters ensued that reveals how profound the difference was between Galileo and Clavius' attitudes towards mathematical procedures. But before analysing in detail the discussion between Clavius and Galileo in the next section, we must turn to the fundamental assumption that constitutes the foundation of Galileo's early theorems on centres of gravity.

Galileo posited the following postulate:

> We assume that, of equal weights similarly arranged on different balances, if the centre of gravity of one composite [of weights] divides its balance in a certain

[41] Cummins (1995), p. 112.

[42] Cummins (1996), especially pp. 85–111.

[43] Cummins (1996), p. 93.

[44] The theorems on centres of gravity were eventually published by Galileo in *Two New Sciences* (Galilei, 1974, pp. 261–280; 1890–1909, I, pp. 187ff).

[45] '. . . desidero saper da lei se interamente gli quieta l'intelletto' (Galilei, 1890–1909), X, pp. 22–23). Unfortunately, very few letters from Galileo's correspondence of this period survive. Other mathematicians who took part in the debate were the Marquis Guido Ubaldo del Monte (who later played a fundamental role in securing Galileo's posts both at Pisa and at Padua) and Michael Coignet from Antwerp. None of them commented on Galileo's first proposition (at least in the surviving documents). See Galilei (1890–1909), X, pp. 22ff. At that time, Guido Ubaldo must have been very interested in Galileo's work on centres of gravity since the Marquis was just about to publish his own edition of, and commentary on, Archimedes' *On Plane Equilibrium*. (See Del Monte, 1589.)

ratio, then the centre of gravity of the other composite also divides its balance in the same ratio.[46]

The postulate expresses a proportionality that refers to a mental model; let us call it the *similar balances model*. I now proceed to show how we can reconstruct the mental model.

There are two points raised by Galileo's postulate relevant to our purpose. Firstly, in what sense does Galileo speak of 'similarity' of arrangements of weights on different balances? Secondly, what is the meaning of the phrase 'being in the same ratio' referring to the lengths of the parts of the balances divided by their centres of gravity? The best way of answering these questions is to begin with an example.

Let us refer to Fig. 1 and consider, for example, three balances I, II, III, on which four weights hanging by threads are arranged in 'similar' dispositions (of which, more in a moment). Let A, B, A', B', and A'', B'' be the arms of the three balances and let their centres of gravity be represented by the star-like symbol ★. Let W1, W2, W3, W4 be the four weights, and $D12_{I}$, $D23_{I}$, $D34_{I}$, $D12_{II}$, $D23_{II}$, $D34_{II}$, $D12_{III}$, $D23_{III}$, $D34_{III}$ be the distances of weights W1, W2, W3, W4 from one another, on balances I, II, III. According to Galileo's postulate:

if in balances I, II, III,
as $D12_{I}$ is to $D12_{II}$ so $D23_{I}$ is to $D23_{II}$,
as $D12_{II}$ is to $D12_{III}$ so $D23_{II}$ is to $D23_{III}$,
as $D23_{I}$ is to $D23_{II}$ so $D34_{I}$ is to $D34_{II}$,
etc.,
then as A is to B, so A' is to B' and A'' to B''.

The question now arises as to whether we have correctly interpreted Galileo's notion of similarity of dispositions of weights. In the balances shown in Fig. 1, the distances of the weights from one another are in same ratio. In other words, we have assigned to Galileo's notion of 'similarity' of dispositions of weights a *proportional meaning*, that is, the condition that as $D12_{I}$ is to $D23_{I}$ so $D12_{II}$ is to $D23_{II}$, $D12_{III}$ is to $D23_{III}$, etc. As we shall see in the next section, this interpretation is confirmed by Galileo's own use of the postulate in the demonstration of his first proposition on centres of gravity. *Proportional meaning* is defined by the so-called equimultiple technique expounded by Euclid in the Fifth Book of the *Elements*.[47] It is important

[46] Galilei (1974), p. 261. Di Girolamo (1999) discusses the Archimedean influence on Galileo's work on centres of gravity but does not concern herself either with the question of the notion of proportionality underlying Galileo's approach or with the Galileo–Clavius disagreement.

[47] See, for example, Clavius' rendering of Euclid's definition of sameness of ratios:

'Magnitudes are said to be in the same ratio, the first to second, and the third to the fourth, when the equimultiples of the first and the third, both alike equal, alike exceed, or alike fall short of, the equimultiples of the second and the fourth—whatever this multiplication may be—and those equimultiples are considered that correspond to one other' [In eadem ratione magnitudines dicuntur esse, prima ad secundam, et tertiam ad quartam, cum primae et tertiae aeque multiplicia, a secundae et quartae aeque multiplicibus, qualiscunque sit haec multiplicatio, utrumque ab utroque vel una deficiunt, vel

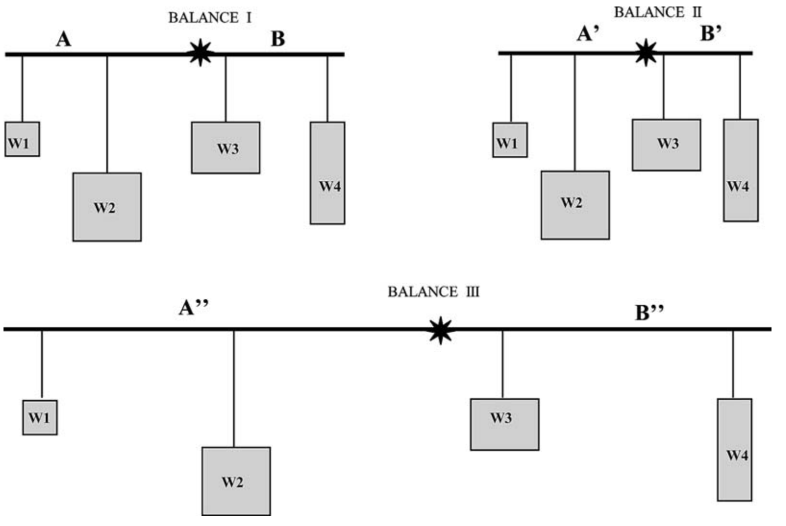

Fig. 1. The similar balances model. Similar dispositions of equal weights W1, W2, W3, W4 on three different balances I, II, II.

to bear in mind that Galileo believed that equimultiple proportionality was an utterly obscure notion. In fact, in 1641, he dictated to Evangelista Torricelli a brief tract in which he proposed to replace equimultiple proportionality with a new definition.[48]

Galileo's extension of the notion of similarity to various dispositions of weights on different balances may have been suggested by Euclid's theory of similarity of

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una equalia sunt, vel una excedunt; si ea sumantur quae inter se respondent] (Clavius, 1999, p. 209, my translation). Clavius (1999) is a facsimile edition of the first volume of Clavius (1611–1612).

In the Euclidean theory of proportions, Galileo depended on the analyses of the Latin commentators of the late Renaissance (and possibly, according to Giusti (1993), on Niccolò Tartaglia and Federico Commandino's Italian editions of Euclid), probably via Clavius' vast commentary on the *Elements*, as I have suggested in Palmieri (2001). De Groot (2000, p. 647), negatively evaluates Giusti's reconstruction of Galileo's proportional reasoning, but he simply criticizes Giusti (1992, a brief paper presented at a Colloquium), ignoring the far more articulated treatment in Giusti (1986, 1993), and Giusti's *Introduction* to Galilei (1990), especially pp. xxixff. Would the Eudoxan elements of proportional reasoning that De Groot finds in the Greek text of the pseudo-Aristotelian *Mechanical Questions* have been accessible to Galileo? Acerbi (2000, p. 20) suggests that a possible way for Galileo of indirectly accessing Greek sources was Jacopo Mazzoni, teacher and later on colleague of his at Pisa. But Mazzoni's technical knowledge of mathematics would hardly have allowed him to detect Eudoxan influences on the Greek text of the *Mechanical Questions* (Purnell, 1972; Schmitt, 1972). The question seems bound to remain open until we uncover further evidence concerning Galileo's ability to read Greek mathematics in its original language. However, De Groot has based his analysis of the *Mechanical Questions* on Apelt (1888), now superseded by Aristotle (1982), thus losing sight of the richer context of the Latin commentaries on the *Mechanical Questions* which Galileo would have read.

[48] Giusti (1993). Palmieri (2001) focuses on the complex relationship between Galileo's and Clavius' analyses of equimultiple proportionality. Clavius' analysis was published in his edition of Euclid (Clavius, 1999, pp. 212ff).

plane figures and by Archimedes' application of this theory to the study of centres of gravity in *On Plane Equilibrium*. Galileo's preference for model-based reasoning led him to reverse the Euclidean–Archimedean theory, as the following considerations should make clear.

The theory of similarity of plane figures is developed by Euclid in the Sixth Book of the *Elements*.[49] In the case, for example, of two triangles, if the angles of the first triangle are orderly equal to the angles of the second and the sides about them are proportional, then the triangles are said to be similar. One of the postulates of Archimedes' *On Plane Equilibrium* states that similar plane figures have their centres of gravity 'similarly placed' (see Fig. 2).[50]

Archimedes does not invoke proportionality in the definition of points that are 'similarly placed', possibly because he is only concerned with plane figures. He defines points 'similarly placed' in similar figures by considering angles.[51] Galileo, therefore, could not directly extend Archimedes' definition of points 'similarly placed' to the case of similar configurations of weights on balances (geometrically represented by straight lines). He had to proceed by generalising Archimedes' notion. The arms of balances with weights similarly arranged could be thought of as being divided into proportional segments by the weights. Let us now elaborate this explanation.

Similar triangles (and, more generally, similar plane figures) show their similarity in a strongly visual sense. The same type of *visual similarity* is evidently discernible

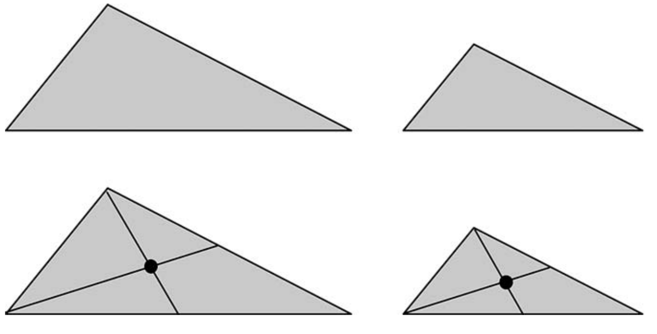

Fig. 2. Similar triangles (above) with their centres of gravity (the black dots) similarly placed (below).

[49] See for example Clavius (1999), pp. 242–304. Cf. the following definition: 'Similar rectilinear figures are those that have angles equal to one another and whose sides about equal angles are proportional' [similes figurae rectilineae sunt, quae et angulos singulos singulis aequales habent, atque etiam latera, quae circum angulos aequales, proportionalia]' (Clavius, 1999, p. 242; see also Heath's slightly different translation in Euclid, 1956, II, p. 188).

[50] Archimedes's postulate in *Archimedis planorum aequeponderantium inventa, vel centra gravitatis planorum* in Archimedes (1544), p. 125. This edition of Archimedes was in Galileo's personal library (Favaro, 1886, p. 264). Galileo's postils to Archimedes' *De Sphaera et Cylindro* have been published in Galilei (1890–1909), I, pp. 233–242.

[51] According to Archimedes' definition, points similarly placed in similar figures are such that straight lines drawn from them at equal angles towards correspondent sides form equal angles [with correspondent sides] (Archimedes, 1544, p. 125. See Fig. 2).

in the similar dispositions of weights on the three different balances of Fig. 1. Could this visual similarity in balances be conceptualised in terms of proportionality? Euclid's Proposition 4, Book VI, proves that the sides of equiangular triangles are indeed proportional and therefore that equiangular triangles are similar. But Euclid's construction of the notion of similar plane figures is rigorously based on the application of *equimultiple* proportionality. Proposition 4, Book VI, depends on the first proposition of Book VI, in which *by means of the equimultiple technique* Euclid shows that triangles (and parallelograms) that have the same height are to one another as their bases (Fig. 3). This immediately leads us to the second question raised above: the meaning of the proportionality attributed to the arms of the balances by Galileo's postulate.

Instead of proceeding by means of the equimultiple technique, Galileo simply conjectured that by placing equal weights at distances that are to one another in the same ratio—that is, by considering what he called 'similar' configurations of weights—the centres of gravity of different balances must divide them into proportional arms. He first saw the *similarity* of various configurations of weights in his mental model of similar balances and subsequently hypothesised that the centres of gravity must accordingly partition the balances into proportional arms. This generalisation of Archimedes' notion of points 'similarly placed' is a *renversement* of Euclid's way of constructing similar figures. For Euclid first proved that equiangular triangles have proportional sides and then deduced their similarity, whereas Galileo first recognised within his mental model the similarity of configurations of weights and then conjectured the proportionality of their arms.

But the question of the meaning of Galileo's *renversement* runs much deeper. Indeed, Galileo's postulate is to all intents and purposes tantamount to a conjecture on the very nature of proportional magnitudes, for a clear reason. Galileo has no proof that equimultiple proportionality can be successfully applied to the case of his balances. Therefore, he is claiming that proportionalities can be generated not only by directly applying the equimultiple technique, but also by means of a totally different procedure, that is, the construction of visually similar configurations of physical magnitudes, such as weights hanging from balances (Fig. 4).

Thus, Galileo's mental model of similar balances succeeds in rendering homogeneous the world of Euclidean proportionality and the world of physical quantities. This is a noteworthy step beyond the reach of proportional reasoning, even though the meaning of the proportionality between the arms of balances remains somewhat ambiguous. This ambiguity can easily be seen if we recall Galileo's postulate and re-write it explicitly indicating the type of proportionality valid in the first set of relationships with $\mathbf{so}_{\mathrm{EQ}}$ (meaning 'equimultiple proportionality'):

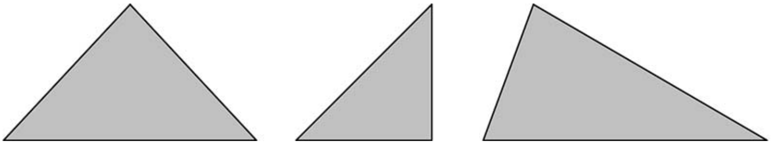

Fig. 3. Triangles that have the same height are to one another as their bases.

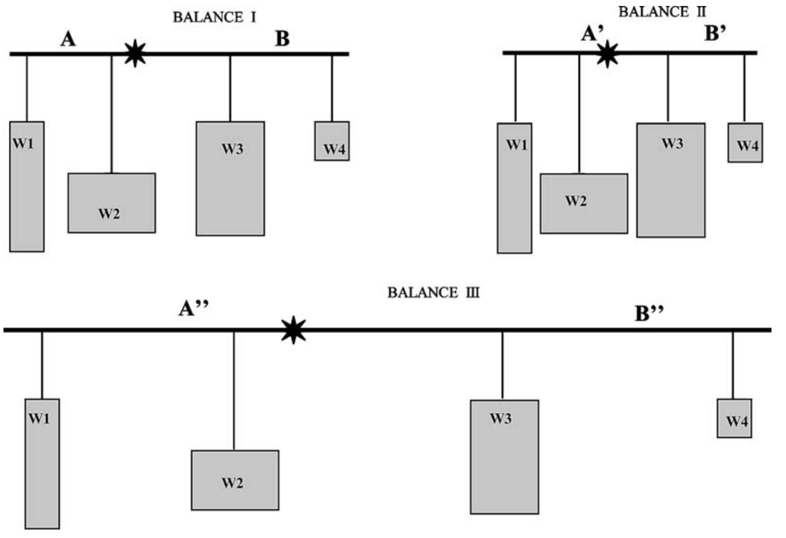

Fig. 4. A set of weights different from that of Fig. 2 generates different positions of the centres of gravity on balances I, II, III, so that, again, as A is to B so A' is to B' and A'' to B''.

> If in balances I, II, III,
> as $D12_{I}$ is to $D12_{II}$ $\mathbf{so}_{EQ}$ $D23_{I}$ is to $D23_{II}$,
> as $D12_{II}$ is to $D12_{III}$ $\mathbf{so}_{EQ}$ $D23_{II}$ is to $D23_{III}$,
> as $D23_{I}$ is to $D23_{II}$ $\mathbf{so}_{EQ}$ $D34_{I}$ is to $D34_{II}$,
> etc.,
> then as A is to B, **so** A' is to B' and A'' to B''.

Whereas the meaning of the proportionality between distances (as $D12_{I}$ is to $D12_{II}$ $\mathbf{so}_{EQ}$ $D23_{I}$ is to $D23_{II}$, etc.) is that of equimultiple proportionality, the meaning of the proportionality between the arms of the balances (as A is to B, **so** A' is to B' and A'' to B'') is left open, in the sense that Galileo's conjecture could turn out to be true or not.[52]

In conclusion, the conjecture implicit in Galileo's postulate replaces the 'obscure' notion of *equimultiple* similarity of distributions of weights with the *visual* similarity embedded in the similar balances mental model (Fig. 5).

4. Picture-proofs: Clavius' constructivist objection to Galileo

James Robert Brown, in his recent book on the philosophy of mathematics, devoted an entire chapter to the question of the validity of mathematical proofs based

[52] It must be noted that there is nothing in Galileo's proposal that prevents one from seeking a proof that Galileo's conjecture is true (in the sense that one could demonstrate it by proving that equimultiple proportionality holds for the arms of the balances). But, equally, there is nothing that guarantees that the conjecture is non-contradictory.

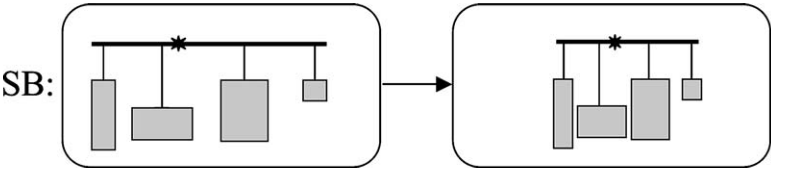

Fig. 5. The similar balances mental model (SB). SB is a representational non-symbolic cognitive function which takes a non-symbolic argument (a balance with a certain arrangement of weights) into a non-symbolic value (a balance with a similar arrangement of weights).

on pictures. According to Brown, '[t]hough not universal, the prevailing attitude is that pictures are no more than heuristic devices; they are psychologically suggestive and pedagogically important—but they *prove* nothing.'[53] Against this 'prevailing' philosophical tendency, Brown asserts that he wants to make a case for the validity of pictures 'as evidence and justification'.[54] A similar concern must have been shared by Galileo and Christoph Clavius, between whom a disagreement erupted on the legitimacy of the role of pictures in proofs.

The essence of the disagreement between Clavius and Galileo in regard to the latter's *Proposition 1* on centres of gravity hinged on the crucial point of the acceptability of visual representation as a substitute for propositional construction. In this sense I refer to Clavius' objection as 'constructivist'. Whereas Galileo implicitly invoked the use of a mental model as a legitimate means of proof, Clavius insisted that proofs must be constructed propositionally.

Galileo's *Proposition 1* on centres of gravity states that:

> If any number of magnitudes equally exceed one another, the excess being equal to the least of them, and they are so arranged on a balance as to hang at equal distances, the centre of gravity of all these divides the balance so that the part on the side of the smaller [magnitudes] is double the other part.[55]

Let us consider, Galileo asks, any number of magnitudes F, G, H, K, N. Let them hang from balance AB at equal distances from one another, then centre of gravity X will divide the balance in such a way that BX is double XA (Fig. 6).

We need not follow the details of Galileo's verbal proof.[56] The intermediate steps of the demonstration are based on a elegant sequence of transformations performed on the mental model (Fig. 7). The first two are as follows: first equivalence: consider all magnitudes called N, then their the centre of gravity remains undisturbed if they are collected so as to hang from D. Second equivalence: consider all magnitudes called O, then their centre of gravity remains undisturbed if they are collected so as

[53] Brown (1999), p. 25.
[54] Ibid.
[55] Galilei (1974), pp. 261–262.
[56] The complete text is in Galilei (1974), 2–263.

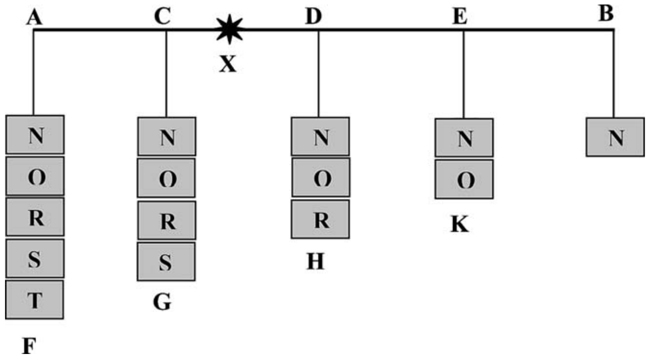

Fig. 6. The balance of proposition 1 (adapted from OGG, I, 188).

to hang from I. In the same way, one can consider all the remaining magnitudes and form corresponding new balances.

The resulting situation is that given in Fig. 8, where both the initial balance and that obtained after all the transformations have been carried out are shown.

According to Galileo, the balances in Fig. 8 are balances on which equal weights are similarly arranged. Their centres of gravity, therefore, divide the balances in the same ratios, i.e., as BX is to XA so AX' is to X'D. Since Galileo has easily proven in a preceding *Lemma* that if line AB is bisected at D, and a point X is chosen so that BX is to XA as AX is to XD, then BX is double XA; he can now conclude that X divides its balance in such a way that BX is double XA.[57] But the crucial question is: are X and X' the same point? According to Galileo, they are the selfsame point since in his mental model the transformed balance is nothing more than the original balance viewed in a different way.

In January 1588, Clavius answered Galileo's request of an opinion by pointing out that in order not to beg the question it was necessary to demonstrate that X and X' coincide.[58] At the end of February, Galileo replied that 'of the same composite, the point of equilibrium is the same', irrespective of the way in which the component parts are being considered.[59] In addition, Galileo sent Clavius a new drawing emphasising that the two balances were to be considered as one and the same balance. In an effort to make his point clearer, Galileo re-drew the original balance placing all the weights contiguous with one another and stressing that the point of equilibrium of the same *composto* does not change when one considers it as being composed of either magnitudes FGHKN or magnitudes NORST (Fig. 9).[60]

A few days later, Clavius reiterated that by assuming that X and X' coincide Galileo was indeed begging that as BX is to XA so AX is to XD. Clavius claimed

[57] See the *Lemma* in Galilei (1974), p. 261.

[58] '. . . il che pare che ricerca d'essere dimostrato, altrimente mi pare *quod petitur principium*' (Galilei, 1890–1909, X, p. 24).

[59] '. . . del medesimo composto uno è il punto dell'equilibrio' (Galilei, 1890–1909, X, p. 28).

[60] Ibid.

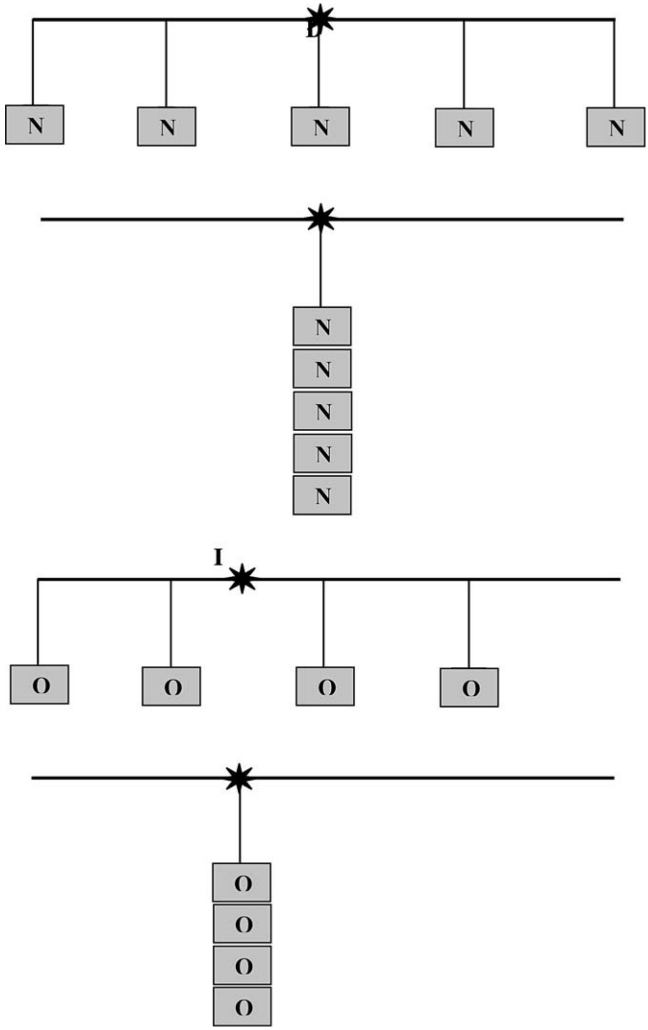

Fig. 7. The first two transformations with N and O magnitudes.

that if one supposes that the centre of gravity of the final balance is, for example, Y (instead of X') then Galileo's reasoning would lead to conclusion that as BX is to XA so AY is to YD. In this case, Galileo's assertion that BX is double XA would not follow.[61] In other words, Clavius objected that the assumption that both points coincide is tantamount to the proportion that has to be proven (BX is to XA as AX is to XD). In essence, for Clavius, the visibility of the coincidence of the two points was not inherent in Galileo's diagram, and therefore Galileo's proof was invalid.

61 Galilei (1890–1909), X, pp. 29–30.

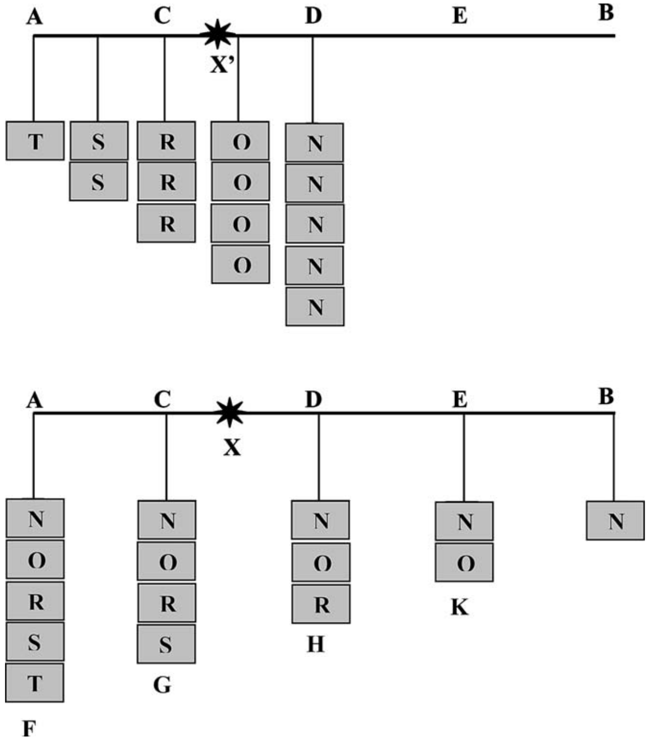

Fig. 8. The final situation: the transformed balance and the original one.

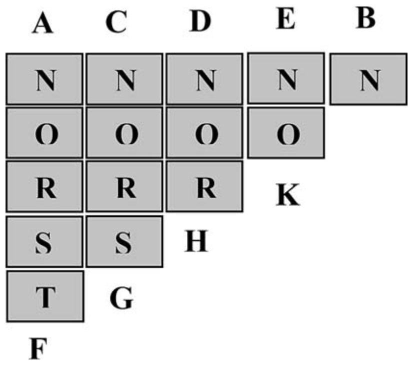

Fig. 9. All magnitudes contiguous to one another (adapted from Galileo's reply to Clavius).

Clavius took exception to Galileo's replacing the propositional construction of the proportionality 'as BX is to XA so AX is to XD' with a leap of faith in his mental model's ability to show that coincidence by successive mental transformations (Fig. 10).

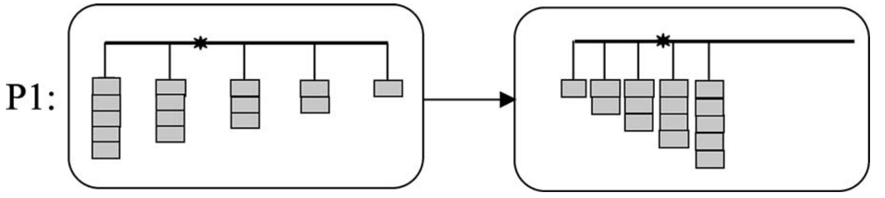

Fig. 10. The similar balances mental model used by Galileo in proposition 1 on centres of gravity, P1. P1 is a representational non-symbolic cognitive function which takes a non-symbolic argument (a balance with a certain arrangement of weights) into a non-symbolic value (a balance with a similar arrangement of weights).

5. Galileo's remodelling of the Archimedean balance

This section is devoted to analysing how Galileo arrived at the proportionality governing the Archimedean balance by means of a mental model. In the final part, it suggests that Pappus' inclined plane theorem may have been the source that inspired Galileo's model-based approach to this problem.[62] The case of the balance in equilibrium is extremely interesting in that it lays bare another mode of functioning of Galileo's mental models. Moreover, Galileo's proof of the Archimedean balance is fundamental because it underpins all the other propositions concerning the simple machines of the *Mecaniche* (*ca*. 1600), such as the steelyard, the lever, the windlass, the capstan, the pulley, the screw, and the Archimedean screw for raising water.[63] Instead of following in Archimedes' steps and proving the proportionality of the Archimedean balance (one whose arms are inversely proportional to the weights and is therefore in equilibrium), Galileo inverts the procedure.[64] Let us see how.

In the both *Mecaniche* and *Two New Sciences*, Galileo observes a cylindrical solid

[62] For the importance of the model of the balance for Galileo's mechanics, see Machamer (1998), Galluzzi (1979), especially pp. 261ff., and Clavelin (1996), pp. 127ff.

[63] In 1960, Stillman Drake (in Galilei, 1960, p. 154, n.) asserted that '[t]he ingenuity (and fallacy) of Galileo's demonstration has been sufficiently recognized elsewhere', referring to Ernst Mach's critique in *The Science of Mechanics*. He subsequently changed his mind, since in Galilei (1974, p. 110, n.), referring to the selfsame proof repeated by Galileo in *Two New Sciences*, he claimed that Galileo's proof is 'much easier to follow than that of Archimedes'. Yet Mach (1989), pp. 17ff., referring to Galileo's proof, also spoke of 'a beautiful presentation'. Mach's critique is too well known to need discussion. See a discussion in Goe (1972), Dijksterhuis (1987), pp. 289ff., and Wilbur Knorr's résumé of the *status quaestionis* in Dijksterhuis (1987), pp. 435ff.

[64] Cf. Archimedes' the law of the balance in Archimedes (1554), p. 127, 'Magnitudines quae fuerint in gravitate commensurabiles, aequeponderabunt, si in distantiis quae secundum gravitatum proportionem fuerint constitutae, permutatim suspendantur.' Archimedes' proof consists of two theorems. It is worth noting that Galileo proves the converse proposition, i.e., if the balance is in equilibrium then the weights are to each other reciprocally as their distances from the fulcrum.

in equilibrium and demonstrates its equivalence with the Archimedean balance (Fig. 11a).[65]

Let us consider Fig. 11b, which represents a cylindrical solid in equilibrium hanging by two threads: CA, BD. Galileo's proof of the Archimedean balance is divided into two parts. The first consists of a series of transformations of the cylindrical solid in equilibrium which lead him to recognize the presence of an Archimedean

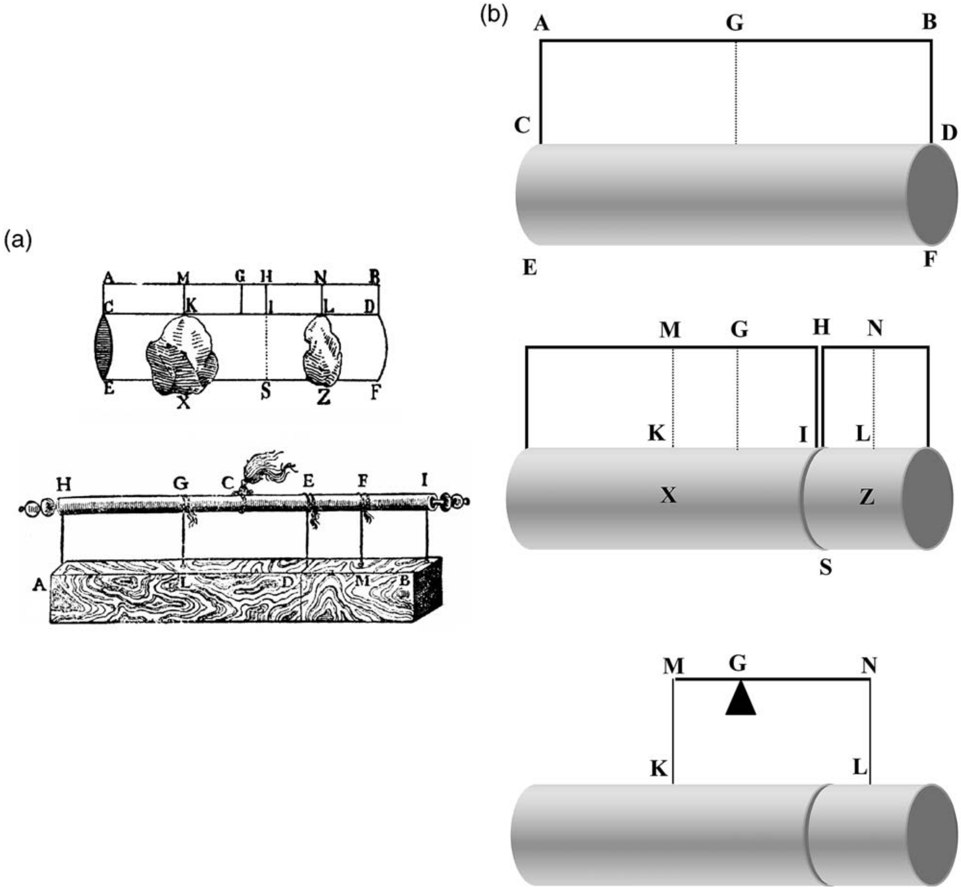

Fig. 11. (a) The original figures from Galileo's *Mecaniche* (OGG, II, 161) and *Two New Sciences* (OGG, VIII, 153) representing a cylindrical solid in equilibrium (above) and a parallelepipedal one in a similar configuration of equilibrium (below). (b) Galileo's mental-model anatomy of a cylindrical solid in equilibrium.

[65] See the original in Galilei (1890–1909, II, pp. 149–191). An English translation is in Galilei (1960, pp. 135–186). As is well known, the *Mecaniche* is a problematic text (Galileo did not publish it). See Favaro's comments on the surviving manuscripts in Galilei (1890–1909, II, pp. 149–154), Drake's *Introduction* in Galilei (1960), and Galluzzi (1979, pp.179ff).

balance within the solid itself. At this level of analysis, Galileo simply manipulates the mental model. The second part is a straightforward application of proportional reasoning to prove the inverse proportionality of arms and weights.

Galileo begins by considering solid CDFE suspended by points C and D and assuming that if one suspends it by the middle point, G, the equilibrium is not affected. He subsequently imagines cutting the solid into two unequal parts CS and SD and adding a string, HI, so that once again the equilibrium is not affected. Finally, he imagines solids CS and SD being suspended by threads MK and NL, where K and L are the middle points of CI and ID (the last cylinder in Fig. 11b). Since he has obtained the final configuration of Fig. 11b without altering the condition of equilibrium, he can now proceed to the second part of the proof and demonstrate that the weights of the cylindrical portions are to each other reciprocally as their distances from the fulcrum (MG, GN).[66] This part of the proof is irrelevant for our purposes.[67] All that is needed is Galileo's analysis of the cylinder according to the steps illustrated in Fig. 11b. By mentally inspecting the three stages of the mental model he is able to see that both the original cylinder, and its final transformation into a balance, correspond to the same object. We now turn to the question of the source that may have inspired Galileo's approach.

In 1598, Galileo lectured at Padua University on the pseudo-Aristotelian *Mechanical Questions*.[68] There is no doubt that Galileo manifested a lifelong interest in the *Mechanical Questions*.[69] Moreover, it has often been suggested by Galileo scholars

[66] The details are in Galilei (1890–1909, II, pp. 162–163; 1960, pp. 153–154).

[67] Basically Galileo shows that as NG is GM so MH is to HN, i.e., AH to HB. He then assumes that the volumes of the cylindrical portions are to each other as AH is to HB and that the weights of the cylindrical portions are as their volumes. The geometrical part of the latter assumption is based on Proposition 13, Book XII, of Euclid's *Elements*, which proves that portions of a cylinder cut by a plane parallel to the cylinder's bases are to each other as their axes. See, for example, Clavius (1999, p. 526). The physical part of the assumption (weights are as volumes) is a hypothesis that Galileo believes to be evident *per se*. In *De Motu* (ca. 1590), he had proven the latter hypothesis by means of the equimultiple technique. See Galilei (1890–1909, I, pp. 348ff). Why he abandoned that proof is not entirely clear. See Palmieri (2001) for the problems concerning Galileo's analysis of weight in terms of the equimultiple technique.

[68] See the university rolls in Galilei (1890–1909) XIX, p. 120.

[69] See Galileo's 1593 discussion of the oar (one of the problems of the *Mechanical Questions*) in his letter to the Venetian, Giacomo Contarini (Galilei, 1890–1909, X, p. 55), and his solution to the paradox of the *rota Aristotelis* in *Two New Sciences*, in Galilei (1974, pp. 29ff). In addition, see Galileo's correspondence with Monsignor Giovanni di Guevara on various aspects of the *Mechanical Questions*, in Galilei (1890–1909, XIII, pp. 369, 377–378, 389–390; XIV, pp. 23, 34–35, 44; XVI, pp. 378–379, 390, 515–516) and Rose & Drake (1971) for the history of that text in the Renaissance. It is unclear how the drawings accompanying the text developed. Guarino (1573, *Dedication*), for example, says '. . . alle figure ch'io ho fatto delle dimostrationi, ho posto i caratteri citati dall'Autore; . . . figure, che sino a questi tempi, si sono desiderate nel testo greco . . .', suggesting that the text from which he translated did not have figures. Nevertheless, his assertion is somewhat baffling, for the following reason. Antonio Guarino was a military engineer in the service of Alfonso II d'Este, Duke of Ferrara. A fifteenth-century Greek manuscript of the *Mechanical Questions* is preserved in the Este Library that contains a few diagrams (Aristotle, XV, 134v.–155v.). Thus, either Guarino did not use the Este manuscript or he refers to other figures. Literature on the significance of diagrams in the mathematics and natural philosophy of the Renaissance is scant. See Rider (1993) and Roche (1993).

that the *Mechanical Questions* influenced his mechanics.[70] According to Antonio Favaro, Galileo had in his library one edition of the *Mechanical Questions*, that edited and commented upon by Niccolò Leonico Tomeo, but he might also have studied, for example, the popular Latin paraphrase by Alessandro Piccolomini.[71]

A recent analysis of Galileo's statics by J. De Groot suggests that one feature influencing Galileo's early treatment of the inclined plane is the pseudo-Aristotle's principle according to which 'for distances covered in the same time by points at different distances along a moving radius, the ratio of the tangential to the centripetal motion is proportional'.[72] Yet such a principle is not present in the Renaissance commentators' understanding of the relevant passages from the *Mechanical Questions*. Both Leonico Tomeo and Piccolomini, for instance, explain the *Question* concerning why what is further from the centre and moved by the same *potentia* moves in a quicker way—which is the basis of De Groot's analysis—by showing that there are two types of motions possessed by that which moves.[73] They note that *praeter naturam* motions (i.e., towards the centre) are greater for that which moves on a smaller circle, whereas *naturales* motions (i.e., circumferential, for Leonico Tomeo, or tangential, for Piccolomini) are greater for that which moves on a greater circle. Thus, they conclude, that which is further from the centre is less hindered by the *praeter naturam* motion.[74] In the proportionality of De Groot's principle, this difference between *naturalis* and *praeter naturam* simply disappears. Indeed, Piccolomini uses that proportionality in order to show that that which is further from the centre must move *naturaliter* more quickly. He claims that since the ratio of the *praeter naturam* motion to the *naturalis* motion must be the same for points belonging to a turning radius, then, according to his definition of 'more quickly', the further the point from the centre the more quickly it moves because it travels a longer distance in the same time.[75] Such a duality of *naturalis* and *praeter naturam* is nowhere to be found in Galileo's early analysis of the inclined plane.

I would argue that some key aspects of Galileo's application of proportional reasoning to mechanical problems, especially the Archimedean balance and the

[70] Rose and Drake (1971), Micheli (1991), and De Groot (2000).

[71] Favaro (1886). Cf. Leonico Tomeo (1530) and Piccolomini (1547), which went through a second edition in Latin and one in Italian.

[72] De Groot (2000), pp. 651, 655.

[73] Leonico Tomeo (1530), pp. 28ff., and Piccolomini (1547), pp. 11v.ff.

[74] Piccolomini (1547), pp. 15v.ff.

[75] Piccolomini (1547), pp. 15v. ff. This is precisely the opposite of what De Groot (2000, p. 651) attributes (perhaps correctly) to the Greek author of the *Mechanical Questions*, i.e., that the latter does not consider the 'motions along the circles' as 'the basis of his comparison of faster and slower'. Yet for Piccolomini, in spite of the points' having the same ratio of *naturales* to *preater naturam* motions, the further the point from the centre the greater its *naturalis* motion is and therefore the quicker it moves. Guarino (1573, comment n. 7) seems to interpret this passage in a different way and recontructs the figure accompaning the proof somewhat differently from Aristotle (XV, 138r.), which has a figure almost identical to that used by Leonico Tomeo and Piccolomini. For the meaning of *natural* in Renaissance commentaries on the *Mechanical Questions*, see Micheli (1991), Altieri Biagi (1965, pp. 1–24).

inclined plane, have had a more direct source in Pappus' treatment of the inclined plane.

Federico Commandino's edition of Pappus' *Collectiones Mathematicae* was published in 1588.[76] Galileo knew Pappus' attempt to solve the problem of the inclined plane very well. In the *Mecaniche*, he criticizes him for having made the erroneous (according to Galileo) assumption that a force is required to move a weight in the horizontal plane and asserts that he wants to tackle the same problem from a different perspective.[77] Nevertheless, we are not so much interested in Galileo's different approach, now well understood,[78] as in Pappus' model-based explanation of the inclined plane. It is this explanation that Galileo might have elaborated and transformed into the more general strategy we have already seen at work in the anatomy of the cylindrical solid in equilibrium. Pappus' problem is this: to find a *potentia* that will draw a weight along an inclined plane given the *potentia* that draws it in the horizontal plane.[79] What is required for the proof to be conclusive (according to Pappus) is the recognition that the *inclined plane* problem is in effect an *Archimedean balance* problem (Fig. 12).[80]

Let weight A be represented by a sphere on the inclined plane. Let the sphere weigh the same as A and let weight B be to weight A as EF is to FG. The substance of Pappus' reasoning reduces to imagining weight B placed at point G,[81] so that an angular balance can be constructed that is in equilibrium around fulcrum L. This is practically an Archimedean balance (Fig. 13). Under these circumstances, Pappus concludes, the sphere cannot roll down the plane, and remains stable exactly as if it were placed on a horizontal plane.[82]

In summary, precisely as Pappus had seen an Archimedean balance within the sphere in equilibrium on the inclined plane (as in Fig. 12), Galileo saw an Archimedean balance within the cylindrical solid in equilibrium. Pappus' procedure may well have furnished a conceptual framework for Galileo's mental remodelling of the Archimedean balance (Fig. 13).

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[76] Pappus (1588). A second edition was published in 1602.

[77] 'È la presente speculazione [concerning the inclined plane] stata tentata ancora da Pappo Alessandrino nell 8º libro delle sue Collezioni Matematiche; ma, per mio avviso, non ha toccato lo scopo, e si è abbagliato nell'assunto che lui fa, dove suppone, il peso dover essere mosso nel piano orizontale da una forza data: il che è falso . . . . Meglio dunque sarà il cercare, data la forza che muove il peso in su a perpendicolo . . ., quale deva essere la forza che lo muove nel piano elevato: il che tenteremo noi di conseguire con aggressione diversa da quella di Pappo' (Galilei, 1890–1909, II, p. 181).

[78] Cf. Galluzzi (1979), pp. 191ff., 215ff.

[79] 'Dato pondere a data potentia ducto in plano horizonti parallelo, et altero plano inclinato, quod ad subjectum planum datum angulum efficiat, invenire potentiam, a qua pondus in plano inclinato ducatur' (Pappus, 1588, p. 313r.).

[80] Pappus (1588), pp. 313r.,v.

[81] ' . . . si pondera AB circa centra EG ponantur . . .' (Pappus, 1588, p. 313r.).

[82] Pappus (1588), p. 313r.

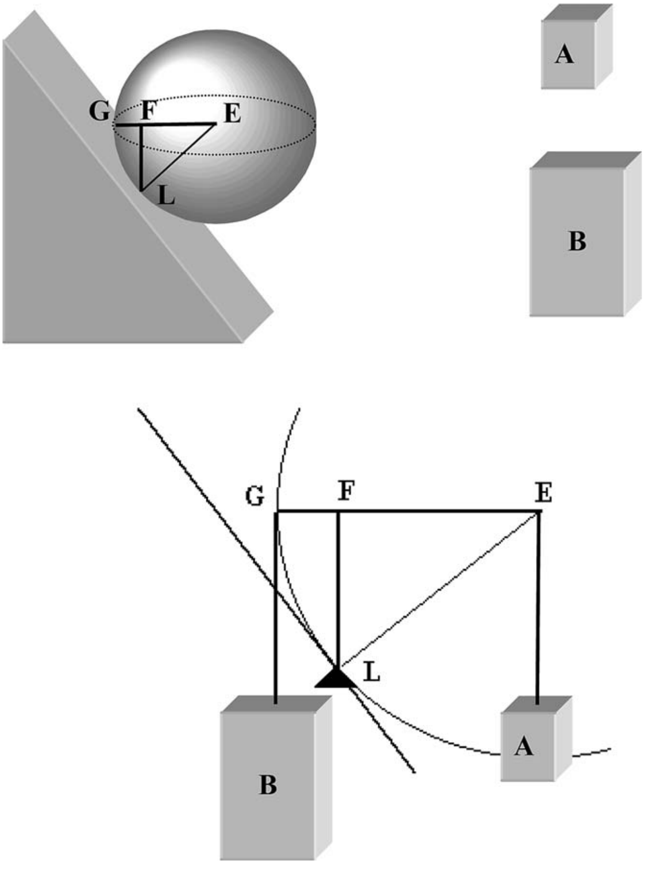

Fig. 12. Above: Pappus' construction for the inclined plane problem. LE is perpendicular to the inclined plane, GE is horizontal, and FL is vertical (adapted from Pappus, 1588). Below: The Archimedean balance of Pappus' theorem (my visualization).

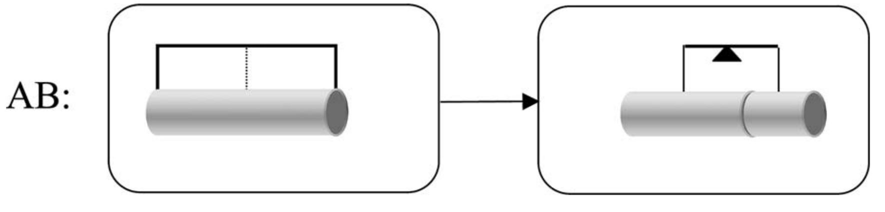

Fig. 13. The Archimedean balance mental model, AB. AB is a representational non-symbolic cognitive function which takes a non-symbolic argument (a cylinder in equilibrium) into a non-symbolic value (a balance in equilibrium).

6. Conclusion: mental models, thought experiments, and the history of science

In this final Section, in the light of the theory of mental models, I wish to develop a new perspective on the vexed historiographical question concerning Galileo's use of thought experiments, and more generally on the significance of a cognitive approach to thought experimentation.

First of all, let us summarise the results obtained in the preceding Sections. After presenting an elementary introduction to mental model research and discussing its applicability to, and potential import on, the history of science, I have suggested that some aspects of Galileo's mathematization were cognitively dependent on non-symbolic mental models. The deduction toolkit of Euclid's theory of proportions simply furnished the symbolic structure of Galileo's mathematization. Indeed, it was the use of mental models that provided the innovation allowing Galileo to break through the barrier of Euclidean and Archimedean traditions.[83] Christoph Clavius recognised this crucial change and strongly objected to Galileo's way of proceeding on centres of gravity. Let us now turn to the question of thought experimentation.

In an influential essay, Alexandre Koyré put forward the thesis that Galileo's early *de motu* research was actually based not on real experiments but on thought experiments.[84] Much scholarly work has been devoted to this question, but, in my view, the issue haunting the debate is the very notion of *thought experiment*.[85] What constitutes a thought experiment? Is a thought experiment just some sort of mental activity mimicking what might happen in a real situation? Koyré did not specify what he intended by *thought experiment*. Recently, six different accounts relevant to our purposes have been furnished of what a thought experiment might consist in.[86] I shall very briefly review them.

1) James Robert Brown has suggested that thought experiments are difficult to define with precision, even though we can easily recognise them. A mental experiment may be destructive, constructive, or both destructive and constructive at the

[83] This raises an interesting question. Are representational non-symbolic cognitive functions present in Euclid and Archimedes as well? Although the case cannot be argued here, I am convinced that they are indeed, and that Galileo, in his innovation, was able to tap the same basic cognitive resources that are at the root of much Greek mathematics. Here I can only furnish the following clue. Netz (1999, pp. 12–88) has discussed the Greek practice of the lettered diagram and the 'pragmatics of letters', suggesting that the diagram was not the representation of something else but an entity in itself. A quick glance at how Galileo lettered his diagrams reveals that there are recurrent patterns of sequential lettering that reflect the way in which the sequences of his verbal proofs are written. This in turn suggests that the diagrams might be seen not only as auxiliary devices but as the material vestiges of non-symbolic, model-based reasoning. Might the Greek practice of the lettered diagram have relied on non-symbolic cognitive functions?

[84] The essay was originally published in 1960. It has been reprinted in Koyré (1973, pp. 224–271).

[85] Budden (1998), Gendler (1998), McAllister (1996), Sorensen (1992), Brown (1991), Prudovsky (1989), Geymonat and Carugo (1981), and Kuhn (1977).

[86] Brown (1991), Norton (1991, 1996), Irvine (1991), and Laymon (1991). Sorensen's model, according to which a thought experiment is simply an experiment which 'purports to achieve its aim without the benefit of execution', is too generic for our present purposes (Sorensen, 1992, p. 205).

same time. The latter is what Brown calls a ‘Platonic thought experiment’. A Platonic thought experiment both destroys an old theory and provides supporting evidence for a new theory.[87] 2) According to John Norton, thought experiments are simply arguments which a) posit hypothetical or counterfactual states of affairs, and b) invoke particulars irrelevant to the generality of the conclusion.[88] 3) In Ronald Laymon’s view, thought experiments may sometimes be understood as being about the ideal limits of real experimentation. A thought experiment of this type would be an imagined situation where a presenter or an audience believe that certain scientific laws or principles allow them to draw a certain conclusion about an idealized description of some state of affairs.[89] 4) For Andrew Irvine, thought experiments are simply ‘arguments concerning particular events or states of affairs of a hypothetical (and often counterfactual) nature which lead to conclusions about the nature of the world around us’.[90] 5) According to Tamar Gendler, ‘[t]o draw a conclusion on the basis of a *thought experiment* is to make a judgment about what would happen if the particular state of affairs described in some imaginary scenario were actually to obtain’.[91] Thus, by ‘focusing on imaginary scenarios and making reference to particulars, thought experiments can provide a fulcrum for the reorganization of conceptual commitments; this explains the way in which they can provide us with novel *information* without empirical input’.[92] 6) In Nenad Miščević’s view, thought experiments are a form of reasoning based on mental models. According to him, ‘mental models are ideal medium for thought experiments’.[93] His analysis partially relies on Philip Johnson-Laird’s concept of mental model.[94]

Finally, without specifying what a thought experiment would consist of, James McAllister has suggested that thought experiments *per se* do not possess any evidential significance, a character that they acquire only under certain assumptions. In Galileo’s case, thought experiments would be evidentially non-inert only under the assumption of what McAllister refers to as the ‘Galilean doctrine of phenomena’, according to which natural occurrences, such as natural falls, can be recognised as occurrences of the same type of the phenomenon ‘free fall’.[95] Under alternative assumptions, such as, for instance, those of the Aristotelians, thought experimentation would be ‘evidentially inert’.[96] McAllister, however, has based his conclusion that

[87] Brown (1991), pp. 122–124.

[88] The particulars irrelevant to the generality of the conclusion are what make ‘thought experiments experiment-like’ (Norton, 1991, p. 130). For example, a particular irrelevant in this sense would the imagined presence of an audience. These particulars must be eliminable, so that any conclusion reached by a good thought experiment will also be reachable by an argument which does not contain these particulars. (Norton, 1991, pp. 129ff).

[89] Laymon (1991), pp. 167ff.

[90] Irvine (1991), p. 150.

[91] Gendler (2000), p. 35. Gendler (2000, pp. 33–63) is a reprint of Gendler (1998).

[92] Gendler (2000), pp. 55–56.

[93] Miščević (1992), p. 221.

[94] Miščević (1992), pp. 220ff., Johnson-Laird (1983).

[95] McAllister (1996), p. 239.

[96] McAllister (1996), p. 233.

the Aristotelians would have rejected Galileo's thought experimentation on a problematic criticism levelled against Galileo by one single Aristotelian philosopher, thus apparently attributing to all Aristotelianism the wholesale rejection of Galileo's modes of argumentation.[97]

I now wish to put forward my interpretation of a well-known example of alleged Galilean thought experiments—the free fall of heavy bodies—while discussing whether it fits the accounts proposed so far. I will subsequently examine the evidence related to McAllister's argument.

This thought experiment was first furnished by Galileo in *De Motu* (*ca*. 1590), and almost fifty years later published in a modified version in *Two New Sciences*. In the first part of his *De Motu* argument, the only one represented in *Two New Sciences*, Galileo proposes the following counter-argument to Aristotle's claim that heavier bodies fall faster than lighter ones. If we imagine a heavy cannon ball and a musket ball being joined to each other then we have a heavier body that should fall faster than either the musket ball or the cannon ball. Yet, at the same time, the combined heavier body should be slower than the cannon ball since it is slowed down by the lighter ball acting as a sort of resistance. Hence, a contradiction destroys the Aristotelian's position. In Miščević's view, Galileo is here relying on mental models. For Miščević, Galileo constructs two separate mental models, one of the slow body and one of the fast body. He then tries to combine the two models, thus discovering that an integrated model is impossible.[98] However, I agree with Norton's view of this part of the thought experiment as being reducible to a *reductio* proof, because Galileo has already had the Aristotelian Simplicio grant that if different bodies are joined to each other then the speed of fall of the combined body must be an intermediate one.[99] It is in the second part of his argument that Galileo poses a question crucial for the relationship thought experiment *vs*. mental model: imagine two identical bodies close to each other falling at the same speed. What would happen if they were joined so as to become one single body double the size of each component (Fig. 14)?

Why, Galileo asks, should the newly formed body's speed double instantly, as Aristotle's theory of motion appears to suggest?[100] In Gendler's terminology, Galileo would here be proposing a 'factive thought' experiment, that is one that should answer the question: 'What would happen?'[101] But in fact Galileo furnishes no answer to that question. Note that by wrapping up his argument with a rhetorical question, Galileo seems to admit (at least implicitly) that there is nothing absurd in

[97] The evidence that McAllister has adduced is based on Shea (1972), p. 11, n. 10, which quotes Giorgio Coresio's claims to have repeated free fall experiments from the leaning tower of Pisa. Cf. Galilei (1890–1909), IV, p. 242. However, far from rejecting Galileo's mode of argumentation, the Aristotelian Coresio put up a magnificent show of counter-argumentation wholly 'commensurable' with Galileo's. See the more detailed discussion below.

[98] Miščević (1992), p. 222.

[99] See Galilei (1974), pp. 66ff., and Galilei (1960), pp. 28ff. Norton's most articulated analysis of this example is in Norton (1996), pp. 340–345.

[100] Galilei (1960), pp. 29–30.

[101] Gendler (2000), p. xii.

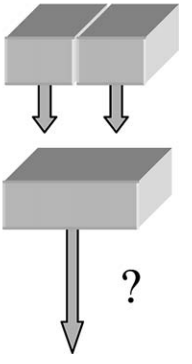

Fig. 14. The free-fall mental model (for the sake of clarity I have anachronistically represented speeds with arrows). Does the speed of the joined body double?

thinking that a speed discontinuity might occur at the time of the two bodies' joining to each other. Let us not forget that throughout *De Motu*, Galileo himself is very much concerned with numerous continuity/discontinuity questions.[102] Thus, I wish to suggest that Galileo is here trying to make sense of a physical situation by means of a mental model, which takes a non-symbolic argument (two identical bodies close to each other falling at the same speed) into a non-symbolic value (the body formed by imagining the two falling bodies finally being welded together). Note that Galileo does not have a symbolic rationale for denying the possibility of a speed discontinuity occurring at the time of the two bodies' joining together. Mental models (in the sense defined in Section 2, Part II) are cognitive functions too basic—and probably too recent in our evolutionary history—to allow us to resolve dynamical problems a priori. Presumably we did not evolve trying to dodge falling bodies in the African savannah.

What does this example suggest, and how does it fit the theoretical accounts reviewed above? There is no denying the inventiveness of Galileo's objection to Aristotle. But is this vague quality enough for the mental model to qualify as thought experiment? I think not. The falling body cannot be a thought experiment: a) in the very broad sense of Brown's Platonic thought experiments, since Galileo's continuity/discontinuity conundrum is neither destructive nor constructive; b) in the sense of Norton and of Irvine since it is by no means an argument; c) in the sense of Laymon's ideal limit since it is very difficult to find what the 'scientific laws and principles' are that an audience would have accepted as a proof of Galileo's claim

[102] *De Motu* is indeed dominated by continuity/discontinuity questions in that much of Galileo's discussion is based on motion within a medium, turning especially on the motion *vs*. rest problem at the interface between different media, such as air and water. In one of the most fascinating examples, in which he attempts to answer a *what-would-happen?* question, Galileo imagines a beam and a chip made of the same wood and floating on water. Then he imagines mentally decreasing the (specific) weight of the medium. What will happen when the (specific) weight of water becomes less than that of wood? (Galilei, 1960, pp. 27ff., and the original in Latin 1890–1909, I, pp. 264ff., 348–349).

concerning falling bodies. Thus Galileo's thought experiment makes use of a mental model, but not in the first part of the argument as Miščević has suggested.

We now turn to what scant, but revealing evidence we have in relation to McAllister's argument concerning the Aristotelian reception of Galileo's mode of argumentation on falling bodies.

First of all, the Aristotelian (indirectly quoted in McAllister's argument) who in 1612 discussed something of Galileo's early critique of Aristotle was Giorgio Coresio, a professor of Greek at Pisa university. Coresio criticized not Galileo's early *De Motu*—then still unpublished—but Jacopo Mazzoni's view, which, Coresio claimed, had been shared by Galileo in the late 1580s.[103] Coresio asserted that Mazzoni, in referring to *De Caelo*, 301 a–b, had argued that experience contradicts Aristotle's contention according to which one could divide a heavy body as the ratio of line CE to line CD, so that if the whole body moved along line CE then the part would have to move along CD in the same time.[104] Coresio objected that Mazzoni–Galileo had failed to understand the thrust of Aristotle's argument, which aimed at proving that no light body could possibly descend. According to Coresio, the correct interpretation of Aristotle's argument is as follows.

First, assume that the heavy moves faster than the non-heavy (Fig. 15). Let A be a weightless body and B a heavy one. Second, suppose that A moves along CD and B along CE, that is, along the faster path (as has been hypothesised). Now let the heavy body, B, be divided as already stipulated. If the whole body moves along CE (the faster path) then the part will have to move along CD, the same line along which weightless body A moves. Thus both the heavy and the weightless will move in the same time, which is impossible according to the initial assumption.[105] But, Coresio insists, to understand correctly Aristotle's argument against the idea that a

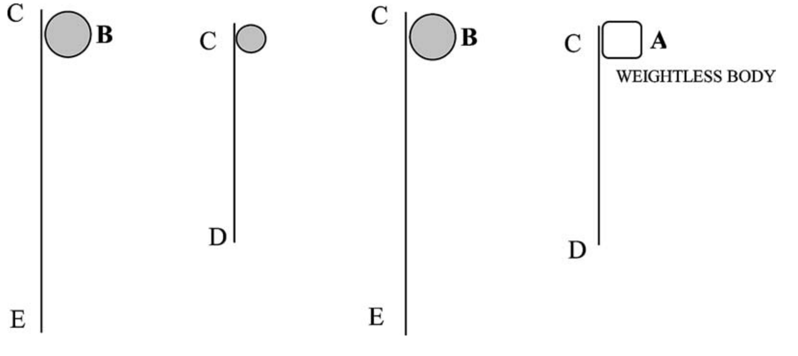

Fig. 15. Coresio's reconstruction of Aristotle's argument. To help the reader I have tentatively added the figures, which are neither in Coresio's text nor in Mazzoni's.

[103] Coresio's argument is in his *Operetta intorno al galleggiare de' corpi solidi*, reprinted in Galilei (1890–1909, IV, pp. 197–244). On Coresio's *Operetta*, see De Ceglia (2000).

[104] Notice that this *experience* has nothing to do with falling bodies and the leaning tower of Pisa, but refers to the behaviour of bodies within a medium such as air or water. (Mazzoni, 1597, pp. 192ff.)

[105] Galilei (1890-1909), IV, pp. 240–241.

light body could possibly descend, we must attribute to Aristotle's notion of 'heavy' in the context of *De Caelo*, 301 a–b, the meaning of an idealized *minimum of heaviness*, that is, of a heaviness which is less than any possible heaviness. If we do not interpret *De Caelo*, 301 a–b, in this way, Coresio argues, then it is really possible to derive a contradiction.[106] Coresio's implication seems to be that Aristotle's original argument (according to which, let us remember, one could divide a heavy body as the ratio of line CE to line CD, so that if the whole body moved along line CE then the part would move in the same time along CD) turns out to be correct if we assume that Aristotle intended a body whose heaviness is less than any possible heaviness. In this case the smallest possible speed corresponds to the smallest possible heaviness, and no light body will ever descend. Coresio seems implicitly willing to accept that there is a sense of 'heavy' that is *both* compatible with Aristotle's proportionality at *De Caelo*, 301 a–b, *and* according to which all heavy bodies might in the limit be said to fall (or rather cease to fall) at the same rate, a proposition which would not violate Aristotle's fundamental dictum that no light body can descend.[107] It is clear that in Coresio's view, Aristotle's line of reasoning at *De Caelo*, 301 a–b, must be understood *under idealized conditions*, if truly absurd consequences such as the descent of light bodies are not to follow.

Although I agree with McAllister's opinion that the theoretical accounts given so far of Galileo's alleged thought experiments fail to prove that they possess evidential significance intrinsically, I see nothing of substance in Coresio's circumstantiated argumentation running counter to the doctrine of natural phenomena attributed by McAllister to Galileo. The moral, I believe, is clear. The question of whether Galileo's model-based reasoning and Coresio's idealization were evidentially inert or not does not seem to depend solely, as McAllister has argued, on the different assumptions attributable to Galileo and the Aristotelian Coresio. Both of them deployed homogeneous strategies of reasoning and were quite willing to engage in modes of argumentation that involved some form of abstraction. It is true that in Coresio's analysis we find no mental models, but a *fixated belief* that the descent of light bodies is impossible. It is equally true that in *De Motu* we find model-based reasoning and possibly an already *fixated belief* that all heavy bodies fall at the same rate.[108] Yet Coresio's logical technique of *reductio ad absurdum* is the same as that adopted by Galileo in the first part of the argument on falling bodies. I would maintain that

[106] Suppose that the chosen part of the heavy body is such that another part can be chosen which is less heavy. Now, since it has been concluded that both the part and the weightless move in the same time, then it follows that the weightless will have to move faster than any portion smaller than the part, and therefore that the non-heavy will move faster than the heavy, which is against the initial stipulation that the heavy moves faster than the non-heavy (Galilei, 1890–1909, IV, p. 241).

[107] Galileo's pupil Benedetto Castelli compiled a list of 'errors' contained in Coresio's *Operetta*, but, significantly, he had nothing to say about the latter's counter-argument to Mazzoni–Galileo's critique of Aristotle (Galilei, 1890–1909, IV, pp. 246–285).

[108] It is extremely difficult to know whether before writing *De Motu* Galileo had already reached the conclusion that all heavy bodies fall at the same rate. It is possible that an in-depth study of *De Motu* will reveal whether in the late 1580s Galileo was already in possession of that fixated belief or whether *De Motu* itself reflects the process by which belief fixation was accomplished.

McAllister's question as to whether thought experiments are evidentially inert might be reformulated in terms of the cognitive processes underlying *belief fixation*. This is a very difficult question concerning not so much the contents of fixated beliefs—such as the impossibility of the descent of light bodies or the equal rate of fall of heavy bodies—but the very processes underlying belief fixation, in the sense explained by Jerry Fodor in his theory of the modularity of mind.[109] How do beliefs fixate? No answer seems to be readily available at present. Moreover, if we agree with Fodor's conclusion that belief fixation is not a surface cognitive phenomenon, but a deep one occurring at the level of so-called *central* mechanisms functioning diffusely across modular sub-systems, and as such, according to him, beyond the grasp of today's cognitive science, then that question might prove intractable.[110] However, mental models, idealization, and more generally non-symbolic schemes of reasoning, especially when they involve visualization processes, seem to run cognitively deeper than verbal logic and rules for symbol manipulation.[111] They might, therefore, play a decisive role in the processes underlying belief fixation. In other words, I am convinced that loosely connected bundles of assumptions must have some relation to belief fixation. The question remains one of further exploring the deep dynamics underlying the bundles of assumptions which framed the attitudes to the natural world of both Galileo and the Renaissance Aristotelians. To put it in a nutshell, the question remains one of further pursuing an interdisciplinary approach to the processes governing belief fixation.

To conclude, cognitive history might have the potential to bring to light a whole spectrum of cases in which cognitive structures and mechanisms have been applied more or less successfully during the scientific development of the past few millennia. Furthermore, it suggests a new context for posing questions about why certain cognitive mechanisms appear to have been more or less successful, both in different individual cases and in different historical traditions. On the one hand, if humans are endowed with a basic cognitive toolkit evolved under certain circumstances, then certain specific tools are likely to be found at work in many different circumstances. On the other hand, processes such as belief fixation may depend heavily on the deep dynamics underlying diffuse systems of assumptions. In Galileo's case, we have seen that specific cognitive tools such as mental models were applied to the mathematization of the natural world. Whether one considers certain mental models, or other forms of idealization, more or less successful, evidentially inert or not, and according to which criteria, may ultimately be irrelevant from a purely historical point of view. However, cognitive history may well contribute to the study of *both* the evolution and stabilization of cognitive mechanisms *and* the fixation dynamics underlying diffuse belief systems, right at the intersection of the areas of interest of historians, philosophers, and cognitive scientists.

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109 Fodor (1983).

110 Fodor (1983), pp. 101ff.

111 Symbol use in humans is qualified by Cummins (1995, p. 120), as 'an astounding bit of cultural technology'. The whole account of mental representation given in Cummins (1996) suggests that the deepest levels of cognition are grounded in non-symbolic schemes of reasoning.

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