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Causality and conservation

Elements of the new metaphysics behind the mathematization of nature in the seventeenth century

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1. Introduction

How do we find order in the ever-changing and fleeting appearances with which we are continually presented? How can we *think* change in a coherent way – i.e. without betraying the stringency of thinking as an act guided by its own norms? This is one of the most basic questions that shaped Western philosophy as a field with its own set of characteristic problems to which any philosopher is forced to return. At the same time it is also a question that any "researcher" actually engaged in the search for order implicitly has to answer through her specific ways of going about in trying to achieve her aim. This is not to claim that she is actually aware of the nature of her engagement, nor that this need to be more than a very tentative approach; but without some norm-bounded practice the research cannot even get off the ground. It is clear that these two perspectives – let's call them the *meta-physical* and the *natural-philosophical* – mutually interact and cannot always be neatly separated. In their most fruitful moments new kinds of natural-philosophical investigations can act as a kind of cognitive experiments in gauging the field of possible meta-physical answers, whereas these answers can in their turn further guide or inspire (or even help stabilize the basic framework of) the researches in the order of the natural world. Taking this interaction seriously implies that we should be careful in separating what we are used to call the history of science from the history of philosophy; but even more importantly, that we can only separate our interest in the most basic philosophical questions from the history of philosophy and science at our own peril. If we want to understand how we can think change in a coherent way, we can do no better than start by reflecting on the nature of the research practices in which we have engaged throughout history, and especially on how we have come to change their internal logic in our attempts to gain a better (or more suitable) grip on the ever-changing and fleeting

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appearances. It is only in the breaking-points, both big and small, with which the history of our thinking is replete that we can see the contours of the dynamics which truly characterizes the most fundamental nature of our human reason.[1]

It is only fitting then to approach the question of the relation between the basic metaphysical concepts of identity and structure along the lines pioneered by Ernst Cassirer's *Substanzbegriff und Funktionsbegriff.* In this seminal work Cassirer argued that the basic orientation of modern mathematical science is expressed by the mathematical concept of a function and that this metaphysical perspective is fundamentally at odds with the Aristotelian one: rather than starting from the concept of substance as the prime bearer of essential identity in the changing world, the mathematical sciences show how to locate this identity in structures, with individual entities now having a derivative status as the elements which fill in the "nodes" of these structures. But while this is an interesting meta-physical distinction in its own right, giving rise to many interesting philosophical puzzles, it is simultaneously – and completely in line with the general orientation of Cassirer's work – also a historical claim about the kind of practices through which natural philosophers have tried to search for order in the natural world.

It is this latter suggestion which I will take up in the present paper. I will try to sketch some elements of a narrative which should allow us to understand how this new meta-physical view was being explored in the work of some natural philosophers. Needless to stress this is also a preliminary exploration on my part, but I do believe it already shows us that some of Cassirer's claims can be historically corroborated; and in this way it also promises to teach us some things about our contemporary meta-physical views – which are in many ways are still hereditary to the inspiration behind these seventeenth century explorations. As I will try to show, the new conception of the causal structure of nature was fostered by a close attention to the technical challenge that was posed by the perennial dream of constructing perpetual motion devices. The new link that was being forged between abstract mathematical conceptual schemes and concrete physical phenomena was closely related to the technological problem of determining the chances of putting lofty promises into material results: reflections on the latter problem allowed natural philosophers like Galileo and Leibniz to come to grips with the kind of metaphysics that underlies a potential successful mathematization of natural phenomena.

[1] In framing the issue in this terms, I am obviously referring to Michael Friedman's *Dynamics of reason* (Friedman 2001), which in its turn is heavily inspired by the work of Ernst Cassirer. And it is indeed Cassirer who pioneered the kind of historical epistemology that I propose in this paper.

2. The causal structure of nature

Rather than starting with the innovations introduced in the seventeenth century, it is useful to begin with identifying what remains unchanged if we compare the metaphysical norms governing an Aristotelian natural-philosophical investigation into nature with these of the new sciences.[2] While to a certain extent prejudging the matter, this has the advantage that it allows us to better formulate the precise nature of these innovations in the short space of this paper.

There is a basic outlook shared by Aristotelian and modern natural philosophy that can be characterized by its essentially *intellectual* character: the rationalistic impulse to find an abstract and coherent framework in which lived experience can be conceptually grasped. Such a program implies that we have to find something permanent with respect to which experienced changes can be coherently determined – which determination is then the function of the concept of causality. What changes in the so-called scientific revolution is the identification of what it is that remains permanent, and correlative, how changes are to be determined. Given the essential function of causality in this respect, this identification and determination will always take place with respect to some regulative ideals about how causes operate in nature. It is at this level that I propose that we locate the fundamental difference between the Aristotelian program and its seventeenth century alternative (which I will designate as "Galilean program" for reasons that will become clear in the remainder of this paper when I will substantiate some of the claims which I will now just introduce without argument as preliminary characterizations of this program).

If changes are to be coherently determined, and if changes are characterized through causal relations, it follows that we must be able to think some kind of equivalence between a cause and its effect (otherwise we would threaten to loose all grasp on what it is that remains identical – and thus conceptually graspable – throughout the changes with which we are continually presented), but this equivalence takes on a fundamental different nature in both programs. Whereas Aristotelian effects are *qualitatively* equivalent to their causes (only a warm thing can cause heat in another thing), Galilean effects are *quantitatively* equivalent to their causes (an effect gives a quantitative measure of the operation of its cause, whatever its qualitative mode of appearance). Whereas the relevant mode of operation in a causal

[2] I will not constantly reiterate excuses for introducing idealizations such as an ideal-typical program of Aristotelian natural philosophy, or the related idea of a scientific revolution. In this paper, I cannot do better than just claim that these kind of constructs (when used circumspectedly) do capture some true oppositions which can be found in the historical actors' texts.

interaction is *generation* according to the Aristotelian (like things beget like things), *conservation* becomes the hallmark according to the Galilean (there is an abstract quantity which does not change throughout the interaction). Accordingly we have a shift of focus from powers and processes towards magnitudes and states (where the latter are primarily characterized through abstract quantities); or to put in Cassirer's terms: from substance to function as the primary bearer of identity throughout change. And the primary cognitive instrument that we thus have at our disposal in thinking change – i.e. the postulated equivalence – now is *geometrical logic* (supplemented by algebraic logic with the advent of the calculus) which displaces *syllogistic logic* from its epistemologically privileged position.
In the following I will try to show to introduce some of the elements that should allow us to understand how this kind of fundamental reversal could come about, and thus what also lies beyond its continuing appeal. My focus will be on "conservation" as a regulative ideal which makes it possible to interpret (proto-)functional relations causally; and which conversely also makes a mathematical science *of nature* possible.

3. Perpetual motion principles

In itself it is not immediately clear how an ideal of quantitative conservation, postulating the equivalence between different states with respect to some abstract quantity, can serve as the logical foundation for *causal* explanation. We only have to bring to mind Bertrand Russell's claim that the functional nature of modern science implies exactly the disappearance of causal relations from nature to see the potential tension here. Nevertheless, it will become clear in § 4 that at least some of the most important historical actors understood their natural-philosophical explanations exactly in this sense. It will also turn out that the clue to this understanding, both historically and philosophically, lies in what we can call the Perpetuum Mobile Principle (PMP), which of course states that such a thing is impossible in nature.

3.1 The different forms of perpetual motion

To correctly gauge the role that PMP played in the development of the new mathematical sciences, it is important to make a few analytical distinctions. Let me start by the following categorization proposed by Alan Gabbey to summarize the kind of distinctions made by seventeenth century natural philosophers (Gabbey 1985, pp. 42-44):

- PM1. "A system or device that maintains indefinitely the motion it already has."

- PM2. “A device that moves perpetually with the same motion or the same repeated sequence of movements, overcoming dissipative influences alone, or in addition doing useful work of some kind. The device receives no power from sources other than the *inertia* (*impetus*, in pre-Newtonian terminology), or the *weight* and inertia, of its constituent bodies.”
- PM3. “As for PM2, except that the power sources, which are still internal to the device, are principally what tat the time were described as *physical* or *natural* agencies (usually magnetism, but also the actions of chemical mixtures, fiery spirits, etc.), rather than inertia or weight, although the latter normally contribute in some way to the performance.”
- PM4. “A device that performs the same kinds of motions or tasks as PM2 or PM3, but avails itself of an endless supply of power from natural or physical sources *external* to itself, such as the sun, moon, or other celestial bodies, winds, rivers and natural fountains, seasonal or daily atmospheric changes.”

In the following I will focus on PM1 and PM2, although it is important to remind you of Gabbey’s conclusion that *none* of the seventeenth natural philosophers “rejected outright the possibility of some version of PM3 or PM4” (ibid., p. 46); but given the limited scope of the present paper I will ignore the many issues surrounding these distinctions and proceed as if a body’s inertia and weight are the only dynamically significant factors (although I will come back to PM4 in the concluding section). This implies that what I called the PMP will turn out to be identified with the denial of the possibility of PM2, which “were held to be impossible, both in theory and in practice, by those who were alive to certain implications of Peripatetic natural philosophy, and by those who were *au fait* with the principles of mechanics” (ibid., p. 45); and again I will simplify the discussion by leaving the Peripatetic natural philosophers out of the picture, as I am primarily interested in the status the principle can have for those philosophers who started from considerations related to the science of mechanics – which we must primarily understand as the science of the simple machines (lever, pulley, etc.).

One of the crucial issues for my story is the relation, or absence thereof, between PM1 and PM2, although in the course of my argument I will also slightly alter the scope of PM1. The present formulation also invites discussions concerning the running down of the universe through repeated collisions in which bodies can be brought to a stop, discussions traced by Gabbey in his article (and of which the Leibniz-Clarke debate contains the most famous episode). I will focus on an issue that is in some sense preliminary to this one, by abstracting from the cosmological context in which these discussions took place, and focussing purely on the understanding of the principles behind the operation of mechanical machines. (One way to phrase the difference is that I will set to the side the question to what extent the world can be understood as a machine, and only treat the question what it is to be a machine.)

To get a feel for what is at issue, let me start by introducing a famous (but often badly misunderstood) example which will then set the agenda for the next section. In 1586 Simon Stevin published his *Beghinselen der Weeghconst* ("Elements of the art of weighing"), which contained his famous "clootcrans"-proof of the law of the inclined plane. In the following I will try to clear up the nature of Stevin's appeal to PMP in the course of the proof (§ 3.3); and draw attention to an often neglected consequence that Stevin draws from his inclined plane law (§ 3.4).

3.3 The ambiguous status of PMP in Stevin

The law of the inclined plane states that the apparent weight ("staltwicht") of a body on an inclined plane is to its absolute weight as the height of the plane is to its length; or that two bodies lying at the two upper sides of a triangular prism hold each other in equilibrium when their respective weights are inversely proportional to the lengths of the sides on which they are placed. The kernel of Stevin's proof consists in the claim that we must have equilibrium when the conditions of the law hold, because otherwise we could construct a closed system consisting of a wreath of spheres lying over the prism which would perform a perpetual motion "out of itself", which according to him is "false" (Stevin 1955, p. 178). In his notes to the modern edition of Stevin's work E.J. Dijksterhuis notes that "the conviction that a perpetual motion is impossible in physical reality is not a sufficient ground for qualifying it as absurd in the ideal sphere of rational mechanics, where friction and resistance of the air are absent" (Stevin 1955, p. 179, n. 1). Thus, according to Dijksterhuis Stevin would make the mistake of denying the possibility of PM1, by basing his proof on a merely empirical generalization which cannot hold the weight of the proof. Alan Gabbey has offered a different criticism by claiming that "curiously, it has not often been noted that Stevin's demonstration is invalid, and that its invalidity derives from the fact that *within the terms he himself would have accepted*, which were those of pre-Newtonian mechanics, such a perpetual motion would not be absurd at all… The consequent of Stevin's counterfactual supposition is an example of constant motion of an idealized closed cycle of bodies under a constant force, which was a straightforward situation in pre-Newtonian physics." (Gabbey 1985, p. 74, n. 26.) What is presumably missing, and which can only be added from within a Newtonian perspective, is a reference to the energy gain that would actually arise in this situation, and which would thus turn Stevin's reference into a genuine appeal to PM2.[3]

It is clear that notwithstanding their quite different analysis of the weaknesses of the proof, both Dijksterhuis and Gabbey put a lot of weight on the fact that Stevin only speaks about a *perpetual* motion, without qualifying the precise nature of this motion, whereas they believe that it should be stated to be accelerated to function validly in the reductio argument. According to Dijksterhuis this perpetual uniform motion would be denied on empirical grounds, while Gabbey thinks it could actually not even be denied consistently by Stevin, as it accords with a basic principle of pre-Newtonian dynamics.

Firstly, let me notice that Dijksterhuis seems to miss the import of Stevin's emphatic stress on the fact that the clootcrans's hypothetical motion is not only perpetual, but also will not need to be started by an external force ("the spheres will *out of themselves* make a perpetual motion"[4]). Thus, even if his denial would be based on nothing more than an empirical belief (or what Mach called "purely instinctive cognition")[5], it is still consistent with the possibility of PM1 – on which Stevin thus need not take a stance. Secondly, it is not obvious that Stevin's hypothetical situation was really so "straightforward" within a pre-Newtonian context as is claimed by Gabbey. The only more or less straightforward cases of *perpetual* motion caused by a constant force were these of motion around the centre of the world (preferably by heavenly bodies, but this restriction is dropped from time to time, and would in all probably also not have been upheld by Stevin, who was a professed Copernican), which is not the case for Stevin's clootcrans – and neither is gravity the moving force in any of these cases. Again, it seems that it is primarily the *self-caused* character of this gravity-driven perpetual motion that is deemed impossible by Stevin. But it is true that it is not clear whether this would constitute a strict denial of PM2 as defined above. This depends on the fact whether "dissipative influences" would be overcome or "additional work" could be done – neither of which is explicitly mentioned by Stevin. And whether this is the case in turn depends on your views concerning the causes of motion: if constant motion requires a constant force, then Stevin's "eeuwich roersel" might well not be a proper case of a PM2 (which is of course what lies behind Gabbey's critique). Now it is not clear whether this

[3] According to Gabbey it is only in a Newtonian framework that the proof can be "restored to apodictic health", because we then can see the clootcrans as a system with a "steadily increasing amount of kinetic energy over a limitless period of time" (which is a consequence of the fact that the motion of the wreath is continually accelerated by an *internal* unchecked force). In this way, his criticism is precisely the opposite of Dijksterhuis's, who had invoked Newtonian principles to criticize Stevin's proof.

[4] "ende de clooten sullen *uyt haar selven* een eeuwich roersel maken" (Stevin 1955, 42; my emphases, altered translation).

[5] Mach 1960, p. 34.

would invalidate the proof "on its own terms" as one might always try to construct a properly Aristotelian (or other kind of pre-Newtonian) argument for the impossibility of this kind of self-caused perpetual motion (or just be content with its empirical falsity). But we might also wonder on what grounds we are justified to conclude that Stevin "would have accepted" the view that a constant force causes a constant motion: Gabbey certainly does not provide any textual evidence for this claim, and neither could I locate one unambiguous statement to that extent in Stevin's works. Maybe the strongest evidence would be exactly the fact that he seems to think of the clootcrans's motion as non-accelerated; but as he nowhere shows interest in analysing the characteristics of motion caused by forces, we might also interpret this as mere silence on these characteristics rather than as a positive stance concerning them. Moreover, given his stress on the inevitability of friction in all motion (which is actually the reason why he believes it is not possible to give an exact science of motion, cf. § 3.4), we might even interpret the possibly uniform character of the clootcrans's motion as due to the fact that the uniform force must continually overcome friction (notice that Stevin nowhere states that the plane is supposed to be frictionless – contrary to what is done in most modern presentations of the argument), in which case he would actually be denying the possibility of PM2.

The least that we can say is that Stevin's own formulation is so open-ended that any straightforward interpretation of its physical import involves quite a bit post-hoc interpretation. At the same time, this might well constitute an important part of its continuing appeal, as it nonetheless is perceived as utterly convincing – whatever the precise dynamical interpretation. For all we know, his proof is consistent both with the denial and acceptance of PM1; it can be read as appealing to the impossibility of PM2 as a genuine mechanical principle; or as based on an empirical belief (maybe because of the known failures to invent purely mechanical perpetual motion machines in the preceding centuries);[6] or as based on implicit Aristotelian cosmological considerations; or even on the "instinctive" insight (Mach) that the kind of situation that is proposed cannot be true. In this sense Stevin's proof exemplary shows the flexibility surrounding PMP, which allowed it to offer apparently common ground to a number of natural philosophers who would actually disagree on its actual status (and also its possible metaphysical consequences or foundations).

[6] Cf. Ord-Hume 1980 and Lorhmann 2006.

3.4 Theory and practice

There is one passage, however, where Stevin does come very close to drawing a conclusion from his law of the inclined plane which could have led him to disambiguate some of these issues. In corollary VI to the law of the inclined plane, while discussing some properties of the weights needed to haul up another weight which lies on an inclined plane, Stevin asks what happens when we consider an inclined plane with zero inclination. In this case, there will be no ratio between height and length, and accordingly (following the law of the inclined plane) neither between the apparent weight of the body on the horizontal and that of the weight of the body needed to move it. Stevin interprets this consequence as follows:

> … by which it is to be understood that a heaviness taking the place of P [the weight "pulling up" the body on the plane], however small it may be, cannot be of equal apparent weight to D [the body on the horizontal plane], but will pull it along (*mathematically speaking*), however heavy it may be. From this it follows that all heavinesses pulled along parallel to the horizon, such as ships in the water, wagons along the level land, et., to be moved need not require the force of a fly beyond that which is caused by the surrounding obstacles, viz. water, air, contact of the axles with the bearings, contact of the wheels with the road and the like. (Stevin 1959, pp. 186-187; slightly altered translation, my emphasis).

It is not farfetched to see a possible starting-point for a line of thought that could result in some kind of inertial principle – and this is indeed exactly how Isaac Beeckman will interpret this passage (cf. § 4.2). But Stevin's own statement is (again) uncommittal: it is not clear whether the smallest weight would have to keep on pulling to keep the body, "mathematically speaking", in motion; or whether it is enough to give it its motion which it would continue in absence of all obstacles (although Stevin's use of "verroersel" rather than "roersel" to describe the effect of the minimal net force, i.e. to *put into* motion rather than motion per se, must be noted). When the obstacles are present it is clear that the "fly" will have to keep on pulling; and it is the latter situation which apparently interests Stevin.

The distinction between what is true "mathematically", and what is true "physically" is an important one in Stevin's work. He introduces his *Weeghdaet* ("The practice of weighing"), the practical pendant of his *Weeghconst*, with an important warning. Theoretical principles can only teach the proportions characterizing equilibrium, i.e. equality of apparent weight, but:

> Note … that this *knowledge of apparent weight is sufficient* for the purpose, for if the same weight lies in either pan of the balance, as we then know (though the balance also

> has its impediment to motion) that little force is required to move the pans, thus it is also in all other cases.
>
> This has been said about the impediment to motion in order that someone, finding in practice the moving force to be perhaps slightly greater than the force moved, may not think this to be a defect of the art, but may understand this to be necessary, since, as has been said above, the moving body, over and above the equality of apparent weight, has to be so much heavier or more powerful than the body to be moved that it overcomes the impediment to motion. Moreover, in order that no one, relying on this appearance of proportionality, shall be deceived, which may very easily happen to *those who hold the false to be true*. (Stevin 1955, p. 299; slightly altered translation, my emphases.)

It is clear why Stevin is not particularly interested in considering what happens when all impediments are removed in the case of the bodies on the horizontal plane: this is a purely mathematical situation, without counterpart in practice. It is only for equilibrium situations that we can bridge the mathematical and the physical, for cases involving motion we have to add practical knowledge about the force needed to overcome impediments that are always present, being careful not to mistake the merely mathematical for the truth.

I believe this throws further light on the ambiguity surrounding Stevin's use of PMP that we diagnosed in § 3.3. Stevin is uncommitted on the possibility of a PM1, just as he is silent about the continuation of the motion on the horizontal in the absence of all impediments. But it is exactly this silence that also excludes a clear position on the question whether every continuous motion needs a continuous force, and which thus left us in doubt about whether Stevin's use of PMP could be seen as a denial of the possibility of PM2. This ambiguity is thus at least partly an outcome of Stevin's careful avoidance of the counterfactual – purely "mathematical" – consideration of the nature of frictionless motion.

4. Towards a new metaphysical foundation

Our analysis of Stevin's use of PMP has shown that its status is closely related to the question of how you think the relation between on the one hand "mathematical", purely ideal, and on the other hand "physical", concrete, situations. In this section we will see how a reorientation of this relation was precisely related to the role that PMP could play as a fundamental physical principle (in its PMP2 guise). It is the clear-cut denial of PM2 that allowed natural philosophers to rethink the relation between the physical and the mathematical as a continuum rather than a separation – and thus enabled a full-scale mathematization of natural phenomena.

But this only highlights the question of what lies behind this use of PMP; i.e., how can this natural-philosophical practice be understood metaphysically? We will see that this was understood to be intimately related to the nature of causality in our natural world (and our means of having a grip on this nature).

4.1 Guidobaldo and Galileo

In 1577 Guidobaldo del Monte published his highly influential *Mechanicourm Liber*, which received an Italian translation (supervised by himself) in 1581, and which remained a classic text throughout the seventeenth century. Both the similarities and dissimilarities with Stevin's mechanical text from 1586 are striking, and deserve a study in their own right. Here, I will only focus on an attitude that both men share in their conception of the relation between a mathematical science and physical phenomena. Exactly as Stevin, Guidobaldo before him also stressed that the proportions characterizing the simple machines are only valid for equilibrium situations. When these machines are put into motion to actually achieve useful work, the inevitable presence of friction makes it impossible to propose valid mathematical proportions characterizing the effects (Del Monte 1581, p. 64r). Guidobaldo's analyses of the multiplication of force that can be achieved with the simple machines are all based on a reduction of their structure to a combination of levers, the operation of which can in turn be explained by considering the relative position of the centre of gravity of the system with respect to a fixed fulcrum.[7] At a few points in his text he also gives as a corollary to the mathematical proportions established thus the following statement: "the space of the power has the same ratio to the space of the weight as that of the weight to the power which sustains the same weight." A statement which he then interprets as follows: "But the power that sustains is less the power that moves; … Therefore the ratio of the space of the power that moves to the space of the weight will be greater than that of the weight to the power." (*ibid.*, pp. 39r-v; translation from Drake & Drabkin 1969, p. 300).

We can of course recognize a precursor to our modern work-principle in Guidobaldo's mathematical consequence. But his stress on its purely mathematical character relegated it to a derivative status, however, as it can not be used to characterize the actual operation of the machines (clearly signalled by his correction to an imprecise proportion in its "moving" form). This changes drastically in Galileo's mechanical treatise, *Le mecaniche*, written somewhere in

[7] For a more detailed consideration of the conceptual structure of Guidobaldo's mechanics, see Van Dyck 2006.

the 1590's, which in all other respects depends heavily on Guidobaldo's treatise.[8] In the longest version of this treatise, Galileo introduces this proportionality between distance (or speed)[9] and weight as the fundamental principle characterizing all simple machines. This also implies that he disregards the clear-cut division that both Guidobaldo and Stevin had drawn between machines in equilibrium and in motion, which in turn implies that he considers the frictionless situation as some kind of limit situation of the actually observable cases of machines in motion. The "work-principle" principle, as an exact mathematical proportion, is no longer relegated to the realm of the purely mathematical, but is apparently considered to have some direct physical validity in itself.

This of course raises the question what lies behind this change of perspective. I believe that a crucial indication is provided by Galileo's introduction to his treatise. In it he rails against mechanicians who make promises to achieve effects that can actually never be attained because they are "impossible by their nature" (Galileo 1960, p. 147). And as his frequent allusions to the "order of nature" in his proofs of the operation of the simple machines makes clear, we have to identify this order with the objective limit imposed by the work-principle. Although Galileo does not specify the kind of false promises which he attacks, it is clear that he is actually targeting presumed constructions of PM2s, quite popular in the *machine theatres* of his days, and also explicitly criticized by his contemporary the engineer Buonaitu Lorini in his immensely popular *Fortificatione* (which in its theoretical aspects is explicitly based on Guidobaldo, including references to the work-principle).[10] In his introduction, he vividly explains why the work-principle is equivalent to a denial of PM2.

> … since it may sometimes happen that, having but a small force, we need to move a great weight all at once without dividing it into pieces, on such an occasion it will be necessary to have recourse to the machine, by means of which the given weight will be transferred through the assigned space by the given force; yet this does not remove the necessity for that same force to travel and measure the same (or an equal) space as many times as it is exceeded by the said weight. So that at the end of the action we will find that *the only profit we have gained from the machine is to have transported the given weight in one piece with the given force tot the given end*; which weight, divided into

[8] A more complete treatment should take some of the complexities involving the precise aims of Galileo's treatise in consideration, but I will enter into these issues here.
[9] In machines the moving force and moved weight always move in the same times, which makes distances and speeds interchangeable.
[10] See Lorini 1609, pp. 205, 238 for the work principle; and p. 237 for the criticism of perpetual motion

pieces, would have been transported without any machine by the same force in the same time through the same distance. (Galileo 1960, pp. 148-149; my emphasis)

We find similar contemporary passages in Lorini (1609, pp. 238-239) and Stevin (1955, pp. 364-365), but none of these is as explicit as Galileo in linking this with the limits imposed on us by nature.[11] What I want to suggest is the following: we can see that the attention of authors like Galileo, Lorini and Stevin is drawn to the work-principle because of a focus on the status of certain technological promises – what can we actually expect to achieve with mechanical instruments? And the intuitive interpretation of the principle in this context (as e.g. given in the above quote from Galileo) brings with it the possibility of highlighting its essentially *physical* significance. All actually operating machines may be confronted with friction, and their actual operation thus cannot be exactly characterized mathematically – but what we do can characterize mathematically about this physical operation are the *limits* of what can be achieved (in a closely related text, Galileo calls this the effect that all machines can attain "formally"; *Opere* VIII, p. 572).

I believe it this insight that lies crucially behind what I called the Galilean program in § 2. I wouldn't want to claim that Galileo was the first or the only one to make this kind of move (we will see in § 4.2 that Beeckman did something very similar), but Galileo certainly was the most explicit and influential one to do so (as we will also back up with further historical evidence in § 4.3). Neither do I want to claim that Galileo's own explicit understanding of his natural philosophical undertakings would have been completely along the lines sketched in § 2 (I think the first one to be completely self-conscious along these lines is Leibniz, cf. again § 4.3), but I do claim that it is at this point that some of the essential elements fall into place.

Let me quickly indicate three consequences we can see in Galileo's own work, and which should justify this claim. Firstly, there is the introduction of the abstract concept of *"momento"* which exactly expresses the objective limits imposed on us by the nature of things. All this obviously need much more careful argumentation, but it is striking that together with the perspective on mechanical phenomena announced in the introduction to his mechanical treatise (and described above), Galileo also offers a new conceptual framework to understand these phenomena by describing them all in terms of the "equalization" of an abstract quantity, momento, on the side of the mover and the moved. Depending on the situation, momento is compounded from weight and distance, or weight and speed – hence the

[11] Stevin in an appendix to the second edition of his *Weeghconst* in the *Wisconstighe Ghedachtenissen* of 1608, adds a short treatise on pulleys in which he also introduces the work-principle as "a common rule" of the art of weighing. (Stevin 1955, pp. 556-557)

equivalence between the work-principle and the positing of a conservation of momento. Due to mathematical limitations of his mathematical proportional framework, Galileo never explicitly defines momento, neither does he often speak explicitly about "conservation" but a close analysis of his proofs unequivocally shows that he sees all machines as characterized by the fact that there is a *quantitative equivalence* between mover and moved, an equivalence which can be expressed by the equality of moment on both sides. Secondly, it is clear that this equivalence expresses something about the *causal structure* of the world for Galileo. As much is unambiguously stated in the controversy on the causes of the floatation of bodies on a liquid; a controversy in which he engaged with a number of Aristotelian philosopher in the years 1612-1615 – and in which he explicitly identified the proper causes in terms of the momento of bodies to achieve certain effects. Thirdly, it is in the same mechanical treatise in which he discusses conservation of momento – and in doing so gives physical significance to the frictionless situations – that he for the first time uses some kind of precursor of our inertial principle in the explanation of mechanical phenomena. In an earlier work (never published during his lifetime), commonly referred to as *De motu* and written not long before his *Mecaniche,* Galileo already gave a proof of the law of the inclined plane; and just as Stevin did, Galileo also went on to draw the consequence that on a horizontal plane a body could be moved by "the smallest of all possible forces" in the absence of friction (Galileo 1960, p. 66); but in his *Mecaniche*, after the introduction of momento, Galileo does go significantly further with it than Stevin did by actually drawing physical consequences from it. In short: as motion along the horizontal does not need force, we should measure the momento of a weight moving on an inclined plane by considering only the vertical distance covered. As far as I know, this is the first unambiguous use of the composition of an "inertia-like" motion with a forced component; a composition which Galileo of course would put to good use in deriving the parabolic form of projectile trajectories.[12] And again, the physical significance of this mathematical composition is secured by the crucial role played by the conservation of momento (as it is only as a result of the possibility of this decomposition that we can see the operation of the inclined plane as a mechanical instrument that is characterized by an equality of momento – as *should* be possible).

[12] Here, and in what follows, I will speak about "inertia-like" and its cognates without apologizing for the potential anachronistic overtones of this expression; see Roux 2007 for a very interesting recent *mise au point* of the often derided search for "precursors" of inertia.

4.2 Stevin and Beeckman

Just as there are a lot of interesting similarities between Guidobaldo and Stevin, we can see a very similar relationship between what Galileo and Isaac Beeckman did with their respective heritages. Beeckman profited greatly from his careful reading of Stevin's work, but in a fragment in his journal dated 1628, after stating that Francis Bacon was not adept in adding mathematics to physical considerations, he adds that Stevin was a bit too much addicted to mathematics, and not enough of a physician (*Journal,* III, p.51). We can imagine that part of his concern must have laid in the strict separation that the latter had proclaimed between theory and practice; and it is indeed the case that Beeckman oversteps this boundary precisely in interpreting some of Stevin's own claims. In one passage from 1618, e.g., he interprets Stevin's corollary VI, discussed in § 3.4, as follows:

> Stevin says in his *Weeghkonst* that on a even plane the least weight, [i.e.] the least power, can start the heaviest, such that … a body … *moves on its own*, as follows from what Stevin prescribes, after it has been moved on an even plane (*Ibid.*, I, p. 212, my translation, my emphasis).

As he appeals to this "principle" in a discussion concerned with a very humdrum practical query (why is it easier to climb stairs while running), it is clear that he would not have considered this to be a purely mathematical consequence;[13] and neither did he hesitate to extrapolate the continuance of the motion once started by the least force. It is often noted that Beeckman was one of the first to explicitly introduce some kind of inertial principle in natural philosophy, as Descartes himself acknowledged his debt in this respect (before their fall-out later in life), but I think it is generally overlooked that Beeckman himself ascribed at least part of this innovation actually to Stevin.[14]

The similarity in the way the inclined plane brings both Galileo and Beeckman to a proto-inertial principle is already striking, but there is more to the story. At different places in his *Journal* Beeckman appeals to the work-principle, but at one place he adds the following specification:

[13] There is even a passage where Beeckman uses the principle to explain why some people's clothes become dirtier than other's in walking on muddy roads (the dirt that is on one's boots will try to continue its motion, thus the more irregular the movement of the feet the more dirt will be catapulted from the boots onto the clothes). *Ibid.*, II, p. 277.

[14] For Descartes' acknowledgement in his *Cogitationes Privatae* around 1619, cf. AT X, p. 219. I obviously don't want to deny that Beeckman sees a much wider scope for his principle of the conservation of motion, as he also detaches it from the horizontal plane to which Stevin's claim was limited, and places it in a cosmological context.

> This would go *in vacuo*, but now it will differ as much as the air hinders the form of the bodies, about which I have earlier written at great length and often. But since we live in the air, and all our axes have a width, etc., in somma that there are impediments all over, so one must moreover investigate through experience how much the impediments hinder. In a fine gold-scale one shall apparently pull up 100 "aeskens" with 101; … The one now who wants to pull up with half the power as much, he must be content that he takes double the time; the one who wants to make it run at double speed, he must do double violence. The one who does everything as well as with a fine gold-scale, what does he want more? There is after all on 100 only 1 [left] to win, unless that someone thinks to have found the *perpetuum mobile.* … Let us thus be content with our instruments already in use, which loose only one in hundred… (*Ibid.,* III, pp. 15-16; my translation)

It is clear that Beeckman understands the physical significance of the principle exactly along the lines sketched above: the frictionless situation is the limit situation which teaches us the boundaries of what is physically possible – as it is an exact expression of the physical impossibility of a perpetuum mobile.[15] Beeckman obviously agrees with Stevin that we must have recourse to experience to determine the effect of friction, but he seems to be much more comfortable with appealing to the limit case as a physically sensible situation – as testified by his free use of the proto-inertial principle. Throughout his *Journal* Beeckman is keenly interested in the practical problem of determining the limits of advantage of a number of different mechanical instruments. And just as Galileo had already used his own proto-inertial principle in ascertaining this in the case of the inclined plane (the horizontal component of motion takes no force), so does Beeckman appeal freely to it in discussing e.g. the use of flywheels.[16]

4.3 Wallis and Leibniz

I now take a step of a few decades, and skip the essential contributions of Torricelli and Huygens to the line of development that I am sketching here. Both did a lot to sharpen the PMP by the use of what is commonly called "Torricelli's principle" – which states that the centre of gravity of a combined system cannot rise out of itself – which especially Huygens

[15] At several place in his *Journal* Beeckman discusses the impossibility of PM2 by appealing to the work-principle; cf. e.g. II, pp. 351-2, 358-9, II, pp. 19-22, 306-7.
[16] Cf. Büttner 2008 for an interesting analysis which places these appeals in a wider context.

put to good use in analysing the phenomenon of collision. But I think we can already come to some tentative conclusions on the basis of what we saw about Galileo and Beeckman.

During his stay in Paris in the 1670's Leibniz read a good deal of works which introduced him into the state of art of both mathematics and mechanics. One of the textbooks on mechanics which he studied with great care was John Wallis's *Mechanica* (of which the first part appeared in 1670). He especially took note of the following proposition that Wallis introduced at the beginning of his treatise, before entering into the by now traditional exposition of the operation of the simple machines.[17]

> PROP VII. Effects are proportional to their adequate causes.
>
> …
>
> SCHOLIUM. I have considered that we have to make a premise of this universal proposition, since it opens the road by which we can go from purely mathematical speculation to physics; or better that it connects the one to the other. (Wallis 1670, pp. 15-16; my translation.)[18]

A decade later we find Leibniz, who has matured in an utterly original thinker in his own right expounding his methodology in natural philosophy in a letter to Pierre Bayle. He starts from a

> Loy de la nature que je tiens la plus universelle et la plus inviolable, scavoir qu'il y tousjours une parfaite Equation entre la cause pleine et l'effect entire. Elle ne dit pas seulement que les Effects sont proportionnels aux causes, mais de plus, que chaque effect entier est equivalent à sa cause. Et quoyque cet Axiome soit tout à fait Metaphysique, il ne laisse pas d'estre des plus utiles qu'on puisse employer en Physique, et il donne moyen de reduire les forces à un calcul de Geometrie. (Leibniz 1887, pp. 45-46)

According to Leibniz we can thus coherently and mathematically determine changes in physical nature because we can start from the metaphysical principle that in any change there is something that remains equal – there is always a perfect "equation" between cause and effect.

It is of course impossible to do full justice to the complexities surrounding Leibniz' use of this axiom, and its position in his wider metaphysical program. I here want to draw attention to its ground in the mechanical tradition stretching from Galileo to Wallis (whom had a manuscript of the preface of Galileo's *Mecaniche*, in which the latter expounds his ideas on the physical

[17] On Leibniz's notes on this passage, see Fichant 1978, p. 229.

[18] "PROP. VII. Effectus sunt, causis suis adaequatis, proportionales. … SCHOLIUM. Universalem hanc Propositionem praemittendam etiam duxi; quoniam viam aperit, que, ex pure Mathematicà speculatione, ad Physicam transeatur; seu potius hanc & illam connectit."

interpretation of the work-principle, in his personal library).[19] To correctly gauge this foundations, let me return one more time to the PMP. We have seen that in the use both Galileo and Beeckman make of this principle, they not only deny the possibility of a PM2, but they also posit the mathematical work-principle as a physically meaningful limit. At one place Galileo offers a very illuminating clarification on this two-sided aspect when he explains that his statement that nature cannot be defrauded by art not only implies that a lesser force cannot strictly speaking overcome a greater resistance, but that moreover a greater force cannot be completely applied in overcoming a lesser resistance![20] It is of course only under this condition that in the absence of friction an applied force will be *completely* used in moving the resistance (in such a way that the moving force will move over a path that is to the path of the moved weight as the weight is to the force); i.e. it is only by appealing to both sides of the PMP that a coherent mathematization of mechanical phenomena becomes possible. And it is this aspect that Leibniz highlights in stressing that an effect must not merely be proportional but also equivalent to its cause. As we already noted with respect to Galileo's concept of momento, this equivalence can not be explicitly expressed in a proportional framework (thus also Wallis's apparent restriction of the principle), but Leibniz's predilection for algebra allows him to give it also a mathematical expression – in another short treatise on this issue, Leibniz's even states that mechanics is a part of algebra because of the *aequationem* between cause and effect.[21]

As we saw, the attention to this double aspect of the PMP was fostered by the practical problem of determining the limits of what can be attained with mechanical instruments; but we now see that Leibniz very explicitly and self-consciously turns it into the metaphysical foundation for mathematical natural philosophy. When we want to find out the nature of the absolute reality underlying the changing appearances, we have to look for what remains invariant throughout. This nature can be abstracted from empirical phenomena by seeing which conceptual characterization allows you to bring the equivalences between causes and effects to light – exactly in the way that Galileo's concept of momento already was introduced as an abstract characterization of the invariant structure underlying the operation of all mechanical instruments. This is of course brought out most famously in Leibniz's criticism of the Cartesian measure for the force of motion, where he shows that this cannot be the true measure for the absolute force of a body, since in that case it would be possible to construct a

[19] On Wallis' owning the preface, cf. the letter to John Collins dated on August 7th 1666, in Wallis 2005, pp. 279-280.
[20] *Opere* VIII, p. 572.
[21] Leibniz 1690, p. 236.

PM2 (on the assumption of the validity of Galileo's empirical law for free fall) – which on Leibniz's new metaphysical foundations implies that there would be no absolute equivalence between cause and effect. At the same time, this combined analysis of the phenomena of collision and free fall allows Leibniz to offer a more adequate measure for the force of motion, by simply inferring that it is that which *is* conserved throughout interactions ("vires motrices, id est *eas quae conservandae sunt*"; Leibniz 1690, his emphasis) – i.e. that which conceptually expresses the absolute invariancies underlying empirical phenomena.

5. Causal structure, effective action – the technological basis of the new metaphysics of mathematizable nature

What can we conclude from this very preliminary story which is meant to help filling in some of the historical background relevant for assessing the change from a metaphysics based on the Aristotelian concept of substance towards one based on the mathematical concept of function? As I tried to indicate in § 2, causal concepts play a crucial role within both perspectives, but the way they do so undergoes a major change. I think we can now be more precise on how this change could have come about, and what some of its implications have been. What is striking about PMP and its central role is the fact that it thinks causality *ex negativo,* as it were. Rather than starting from a consideration of what we can achieve it starts from a an analysis of what we *cannot* achieve. As I showed, natural philosophers like Galileo and Beeckman were exploring some kind of meta-perspective from which it was possible to judge whether certain actions could achieve their promised effects by appealing to the limits of what is achievable. It is this focus on the maximum efficiency of a machine that allows them to understand the causal structure of nature as apt for mathematization. This need not imply that the productive aspect of causality (which is absolutely central from an Aristotelian perspective) is put aside, but it is black-boxed to a certain extent. And as a direct consequence of this black-boxing, an interesting dynamical space for conceptual development is opened: we can now start construing the relevant (causal) concepts as these which allow us to bring the absolute limits expressed in PMP to light (cf. Galileo's *momento* and Leibniz's *vires motrices*), rather than by starting from the requirement that we must have some kind of more or less direct experience of the causal efficacy which is expressed by a putative causal concept. It is moreover this technological perspective – starting from the limitations on what we can achieve – that simultaneously creates the conceptual and epistemological space in which idealization can take on a new sense as an approach towards these limits which can be expressed mathematically.

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