Idealization and Galileo's Proto-Inertial Principle

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Galileo proposed what has been called a proto-inertial principle, according to which a body in horizontal motion will conserve its motion. This statement is only true in counterfactual circumstances where no impediments are present. This article analyzes how Galileo could have been justified in ascribing definite properties to this idealized motion. This analysis is then used to better understand the relation of Galileo's proto-inertial principle to the classical inertial principle.

1. Introduction. Galileo Galilei assumed that a body in horizontal motion will conserve its motion indefinitely. He used this idea to explain the parabolic shape of a body projected from a horizontal table, and it is crucially related to his use of relativity arguments to defuse objections to the possibility of a moving earth. In both roles it functions similarly to an inertial principle, but there are also some crucial factors that put it at a distance (for some earlier discussions and further references, see Coffa [1968], Chalmers [1993], Hooper [1998], and esp. Roux [2006]). Most importantly, because the notion of the "horizontal" is underspecified if not related to a broader spatial framework, Galileo's own writings seem to use the idea in a way in which the relevant motion is both rectilinear and circular (for a recent discussion, see Miller [2014], 110–46). Even if interpreted as rectilinear, the restriction to horizontal motion is highly significant—Galileo never came to terms with the case of a body projected along an oblique direction, such as a typical cannon ball (see Damerow et al. 2004, 216–23, 263–66).

Galileo's way of arguing for this conservation is closely related to a principle that had been enunciated a number of times by earlier writers: that a

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body on the horizontal can be put in motion by a minimal force (see Festa and Roux 2006). Clearly, this minimal force conclusion only holds if we assume an idealized situation in which there are no impediments to motion. Yet, taking this idealizing step is in itself not sufficient to decide the further question whether the motion once initiated will last or come to an end. After all, what kind of empirical information could we conceivably possess about that kind of counterfactual situation? In section 2, I sharpen this question by considering the case of Simon Stevin, one of the authors who had already come to the minimal force conclusion. Stevin is especially interesting because he also enunciated views on the conditions for acceptable idealizations, which implied a sharp distinction between stating the minimal force conclusion (acceptable idealization) and possible extrapolations about the resulting motion (unacceptable idealization). In section 3, I reconstruct how Galileo could have felt entitled to ascribe a definite nature to the resulting motion without necessarily transgressing the spirit of Stevin's norms on acceptable idealizations. In the concluding section 4, I show how this background helps us to better understand the relation of Galileo's "proto-inertial" principle to the classical inertial principle.

In this short article I focus on just one question regarding idealization in the work of early modern practical mathematicians such as Stevin and Galileo: how to legitimately ascribe definite properties to idealized systems. And I only do this with respect to one very specific problem: What can we say about horizontal motion in the absence of impediments? Galileo has more to say on issues regarding idealization than can be taken into account here, but a full analysis must await another occasion (see Koertge [1977] and McMullin [1985] for two influential earlier treatments and Palmerino [2016] for a recent discussion). A similar remark must be made regarding Galileo's proto-inertial principle: I approach this primarily through its role in Galileo's mathematical science of motion, bracketing its role in his astronomical work.

2. Simon Stevin on Idealization. Simon Stevin is in the first place remembered for his ingenious proof of the general condition characterizing the equilibrium of bodies on an inclined plane, published in 1586 (see Van Dyck [2017] for a recent analysis). He introduced the minimal force conclusion as one of the corollaries following from this general condition: in the case of zero inclination there can never be equilibrium between a body on the plane and another body attached to it with a pulley, no matter how small the second body. Hence, "mathematically speaking" the latter body will always pull the first body along the horizon (Stevin 1955, 187). Stevin's stress that the minimal force conclusion is only true when speaking "mathematically" is closely related to his distinction between "speculation" and "practice." The former is characterized by Stevin as "an imaginary operation without natural matter," whereas the latter "is an operation which essentially takes place with natural

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matter" (1961, 619). Strictly speaking, a speculatively established proposition will never be true of material objects, but "the false is admitted in order that the truthful may be learned therefrom" (Stevin 1955, 227).

This brings Stevin very close to recent philosophical discussions on idealization, where idealization is usually understood as the practice of entertaining false propositions in order to learn something about empirical phenomena (see Weisberg [2007] for an influential discussion). But under which conditions can the false teach us something relevant? The following characterization helps us understand Stevin's point of view: "The conclusion of speculative propositions is perfect, but that of practical propositions is imperfect. . . . The property and the end of speculation is that it furnishes a sure foundation for the method of construction in practice, in which by closer and more painstaking care one may get as near to the perfection of the speculation as the purpose of the matter requires for the benefit of man" (1961, 619).[1]

The reason why we idealize is that it allows us to give a "perfect," mathematically structured treatment of the subject matter. In the case of mechanics, this mathematical treatment is made possible by the assumption that each body possesses a unique center of gravity, which grounds the Archimedean proof of the law of the lever. All further mathematical propositions in Stevin's treatise are based on the law of the lever, which implies that the relevance of these idealized statements indirectly depends on the existence of a center of gravity in each body (see Van Dyck 2017, 23–24). It is this presumed existence that provides the bridge between the speculative propositions of Stevin's *Art of Weighing* and the practical operations in his *Practice of Weighing*. In the second proposition of the latter book, Stevin explains how to construct a "most perfect" (1955, 303) material balance that exhibits indifferent equilibrium—which shows that this body indeed possesses a center of gravity (the possibility of indifferent equilibrium follows directly from the definition of center of gravity as given by Stevin).

The idealized propositions are strictly speaking false, but we can construct material objects such as a balance in indifferent equilibrium that exhibit the defining characteristics responsible for the mathematical regularities. This guarantees that by "closer and more painstaking care" we should always be able to find situations for which these propositions are approximately true and that we can also assume that they remain informative for objects in situations in which the approximation no longer holds. We can now suppose that these objects also have a center of gravity and that all differences from the mathematically proven propositions must be ascribed to the presence of material impediments. This will especially happen as soon

1. I have slightly altered the translation.

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as we consider mechanical instruments that are put into motion to achieve practical work:

> Because in several propositions of the Practice of Weighing the motions of bodies will be dealt with, I thought it advisable, before coming to the matter, to explain something of it to the reader. To wit, that the Art of Weighing only teaches us to bring the moving body into equality of apparent weight to the body to be moved. As to the additional weight or the force which the moving body requires in order to set in motion the body to be moved (which weight or force has to overcome the impediments of the body to be moved, which is an inseparable attribute of every body to be moved), the Art of Weighing does not teach us to find that weight or force mathematically; the cause of this is that the one moved body and its impediment are not proportional to the other moved body and its impediment. (Stevin 1955, 297)

"Mathematically speaking" the smallest addition of weight should suffice to put a balance in equilibrium in motion, but practically speaking an extra force will always be needed, the magnitude of which can only be empirically determined.

Notice the parenthetical statement in the quotation: "impediments of the body to be moved" are called "an inseparable attribute of every body to be moved." In an earlier text, Stevin had already defined an inseparable attribute as "that which cannot be taken away from its subject without the demise of the thing" (1585, 21). But this implies the impossibility of an idealized treatment of motion along the lines of the treatment given of equilibrium. We cannot separate the effects of the impediments in an "imaginary operation," as we could do for the effects of material imperfections when proving the law of the lever. The question what would happen with the body after it is put into motion by a minimal force on a frictionless horizontal (and assuming no air resistance) must accordingly be senseless for Stevin, and, indeed, he does remain silent about the issue. But why is this the case?

As Stevin did not rule out the possibility of a vacuum (1585, 142–49), this cannot be due to a purely conceptual fact about the nature of motion (as it arguably would have been for Aristotle). The clue to a better answer is to be found in the contrast with equilibrium. All material instruments will exhibit some friction (which will actually be a factor sustaining equilibrium), but these impediments are not necessary to conceive a body in equilibrium. We can legitimately attribute equilibrium to an idealized body, based solely on the relative position of the body's center of gravity to the body's support. Why "legitimately"? Because we have found out through careful manipulation and conceptual exploration that the concept of center of gravity describes a property responsible for the behavior of all bodies. This property has something of a dual nature: it is empirically grounded and it is a neces-

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sary precondition for the possibility of giving a mathematical treatment of this behavior. It is thus not just empty gesturing to talk about the mathematically well-determined behavior of bodies in the absence of impediments when it comes to their static properties. The empirical relevance of Stevin's speculative treatise is guaranteed by the possibility of empirically exhibiting some of its most basic assumptions.

The same move cannot be made when it comes to motion. Stevin simply knew of no empirically grounded way to separate what we could call the pure phenomenon, present both in the ideal and empirical situations, and the extra factors that mask this phenomenon in most empirical situations. There is no privileged empirical system that can play the role of a balance in indifferent equilibrium. (Stevin further illustrates this in an appendix to his treatises on the *Art of Weighing* and the *Practice of Weighing*, in which he discusses bodies in free fall; see Van Dyck 2017, 33–35.) Stevin articulates a clear norm for mechanical speculation: the idealizing imaginary operation that is at the basis of its mathematical demonstrations must be constrained by the results of specific material operations if it is to be of any epistemic worth.

3. Galileo on Motion on the Horizontal. In what follows, I compare Galileo and Stevin on the question of motion on a horizontal plane, as a way to bring Galileo's own use of idealization into better focus. We will see that in a first phase of his career, Galileo did not feel bound by something like the norm that Stevin upheld for legitimate speculation. In a later phase, he came to conclusions that can be underwritten by this norm, though, and we will see how the probable discovery process also shows Galileo implicitly operating according to it. In the next section, we then analyze Galileo's own explicit way of legitimizing his conclusions. The analysis given here has a rather narrow epistemological focus: How does Galileo's statement about the conservation of motion relate to the empirical evidence at his disposal? Ideally, this analysis should be integrated within a fuller historical and contextualizing description. (It is, e.g., improbable that Galileo would have known Stevin's work, but he was definitely familiar with that of Guidobaldo del Monte, which argues for closely related views, although not linking these to the question of motion on the horizontal [see Van Dyck 2017].)

In his youthful manuscript *De Motu Antiquiora*, written in 1589–92, a few years after Stevin's treatises, Galileo also deduced the general condition characterizing equilibrium on an inclined plane and inferred the minimal force conclusion from it (Galilei 1890, 1:296–302). Again like Stevin, Galileo went on to warn his readers not to expect this conclusion to be borne out when experimenting with material bodies. But unlike Stevin, Galileo was also interested in the nature of the resulting motion. More specifically, he wondered how to characterize it in terms of the conceptual framework

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inherited from Aristotle, in which motions are either natural or forced. Galileo's tentative conclusion was that it is probably best to characterize it as neither natural (since the body is not moving toward its natural place, which is why the motion will not start spontaneously) nor forced (since it is not moving away from its natural place, which is why a minimal force will suffice to move it) but "neutral." In the same treatise, Galileo also considered the similar case of a perfectly homogeneous material sphere rotating around the center of the universe: Would its "neutral" motion last or come to an end (Galilei 1890, 1:306–7)? Significantly, Galileo raised the question but never answered it in his treatise. We can easily understand why Galileo would have to remain undecided. On the one hand, there is no reason why the body should stop once it is put into motion—after all, it has no resistance against this motion. On the other hand, there is no reason why the body should stay in motion—after all, it has no inclination for this motion (and according to the view that Galileo had already defended in his treatise, an external force that puts a body into motion will only remain present in that body for a limited amount of time).

Galileo's silence on the question of conservation of motion on a horizontal plane is of a nature very different from Stevin's. When, for example, treating free fall, Galileo saw no problem in assuming the pure phenomenon to be one in which the bodies fall with uniform speeds measured by (something like) their specific weight, even if there is no empirical operation that can show this phenomenon to be approximately exhibited by material bodies. Accordingly, he saw no problem in separating a pure phenomenon from impediments solely on the basis of theoretical preconceptions (in this case based in an extrapolation from Archimedean hydrostatics). It is accordingly only the gap in his theoretical framework that stopped Galileo from giving a definite statement on the precise nature of the counterfactual motion of a body moving without impediments on a perfectly horizontal plane: there seem to be equally good theoretical reasons to assume that it would remain in motion and that it would stop.

In his *Letters on Sunspots* from 1613, Galileo again considered what happens when a body on a horizontal plane is put into motion (Galilei 1890, 5:134). He now concluded that since the body is "indifferent" to this motion (since it is neither natural nor forced), it will conserve its motion in the absence of all external impediments. (We also know from a letter from his pupil Benedetto Castelli that Galileo already held this view in 1607 [Galilei 1890, 10:170].) While Galileo now presented the conservation as directly following from the body's indifference, the earlier indecision in *De Motu Antiquiora* shows that this extrapolation cannot have been so straightforward.

What did happen in the meantime? I will first sketch a reconstruction that is based on all available evidence, but that will also have to fill in quite a few

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lacunae. This will allow us to see how Galileo's approach to the problem was transformed into one that was in broad agreement with Stevin's norms on acceptable idealizations, although it would never be explicated as such by Galileo himself, for reasons we will consider in section 4.

In 1592, Galileo performed a small experiment, together with his patron Guidobaldo del Monte (see the convincing evidence gathered in Renn, Damerow, and Rieger [2000]). They projected a small inked ball on an inclined surface, such that the ink would leave a trace marking the path followed by the projectile. Their conclusion was that the shape of the trajectory resembled either a parabola or a hyperbola. Renn et al. (2000, 323) suggest that at this point Galileo could have easily inferred the times-squared law for freely falling bodies from the parabolic shape, by assuming the composition of a uniform horizontal motion and an accelerated motion along the vertical. But there is no evidence that Galileo would have taken that step around the time of the experiment, and there a few reasons to assume that he probably could not have. As we just saw, the necessary extrapolation from the minimal force conclusion to the conservation of motion (which would result in a uniform motion) was far from straightforward for Galileo. But even more importantly, as argued conclusively by Renn himself, Galileo never came to terms with the right composition of motions characterizing an obliquely projected body (Damerow et al. 2004, 263–66), which is precisely the case in the experiment with Guidobaldo. It is probably safest to assume that initially Galileo was interested only in the symmetric shape of the trajectory (as also stressed in Renn et al. 2000) and that any link with the law of fall is of a later date.

In 1602, Galileo discussed the relation between motion on inclined planes and properties of the circular motion of a simple pendulum in a letter to Guidobaldo del Monte (Galilei 1890, 10:97–100). Two years later, this time in a letter to Paolo Sarpi, Galileo indicated that he was looking for an evident axiom from which to derive some phenomena observed by him, among which was the times-squared law of fall (Galilei 1890, 10:115). It is plausible to assume a direct link between the content of both letters: in the *Discorsi* from 1638 Galileo tried to demonstrate that the circular path of the pendulum is the brachistochrone (the path of quickest descent), starting from the fact that bodies descending on inclined planes follow the law of fall. After a careful study of the manuscript evidence, Wisan (1974, 175–79) has suggested that this was probably the way in which Galileo would have started thinking about the possibility of a mathematical law characterizing acceleration (thus abandoning the idea that the pure phenomenon of fall is characterized by a uniform speed). Looking for a way to prove a proposition with which a possible demonstration for the brachistochrone could be constructed, Galileo would have realized that this proof could only be completed if he had a mathematically determinate way to express distances traversed in terms of

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times used. At this point it would have made good sense to try find an empirical answer to the question what form this expression should take, by setting up the famous experiment with inclined planes as described in the *Discorsi*.

Having found empirically that bodies descending on an inclined plane approximately follow a times-squared law, the approximately parabolic shape of the trajectory of a projectile would now have taken on extra meaning. Assuming the vertical component to be characterized by the precise law of fall, the mathematics of a parabola immediately implied that the horizontal component should be characterized as a motion with uniform velocity. At this point the question regarding the nature of motion on the horizontal could finally be answered: it has to be conserved. The clue for the answer did not reside in theoretical considerations concerning matter, force, and motion but in an analysis of the conditions under which an empirical phenomenon could be mathematically analyzed. (The puzzle regarding oblique projection is not solved but bypassed. The mathematics demands a uniform horizontal component, and Galileo knows how to give this a physical interpretation for the downward part of the trajectory. This part starts at the moment that the motion is indeed horizontally oriented, as if the body is on a horizontal plane, where it can be characterized as "indifferent" to this motion—which will now be interpreted as implying the conservation of motion that is required by the mathematics of the curve. For the first, upward, part of the trajectory, Galileo would never do better than appeal to vague symmetry considerations (see the discussion in Damerow et al. [2004, 263–66]).

While to some extent speculative, this reconstruction has the value that it brings out the way in which the question concerning the nature of motion on the horizontal was gradually being transformed into one that could possibly be adjudicated on empirical grounds by Galileo's research. Such adjudication could never have been straightforward, though, since it remains a fact that in all physically realizable circumstances moving bodies will finally come to rest. How can we decide whether this is only due to impediments rather than also following from the nature of bodies (and motion)? Merely saying that we can approximate the situation in which the body keeps on moving is of no avail here, since no matter how good the approximation, the fact remains that the body stops—which could always be due to its intrinsic nature rather than to remaining impediments (this was, e.g., the position of Roberval later in the seventeenth century; see Roux 2006, 495). After all, the merely approximately parabolic shape is perfectly consistent with a nonuniform, slightly decreasing horizontal speed.

The crucial extra step that allowed for the adjudication is the insight that only the assumption of conservation of motion makes possible a straightforward mathematical analysis of the empirical phenomena that would otherwise not have been possible. It allowed Galileo to directly relate the approximately parabolic shape of the trajectory to the approximately quadratic

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relation established on the inclined plane through the idealizing move in which he assumed both a precise law of fall and a perfect parabola (and note that for Galileo there would have been no mathematical curve corresponding to a shape closely approximating but not identical with a parabola, so he would not have been able to mathematically link his observation with the law of fall without making this idealization). This new possibility provided Galileo with the good reason to assume the conservation of motion that was lacking before. This reason was based in empirical facts, but these facts got their significance from the explicit goal of giving a mathematical treatment of phenomena of motion. Whether the empirical approximation to the ideal phenomenon was "good enough" thus depended on the fruitfulness of the further mathematical research program that was predicated on this initial idealization.

For Stevin the impediments to motion were inseparable attributes because he did not know any legitimate way to separate a pure phenomenon of motion from these impediments. Galileo's research showed that the parabolic trajectory of a projectile could be taken to exhibit the nature of neutral motion much in the same way that the balance in indifferent equilibrium exhibits the mathematical concept of center of gravity. Imagining the motion of a body on a horizontal plane without impediments had become strongly constrained by simultaneously empirical and mathematical considerations, in a way that it had not been at the beginning of Galileo's attempts.

4. A "Proto-Inertial" Principle? All evidence we have of Galileo's adherence to the idea that horizontal motion is conserved in absence of all impediments dates from after his discovery of the parabolic shape of projectile motion and of the law of fall. Given the crucial role of conservation in linking both phenomena, it is plausible to assume that this role is exactly what brought Galileo to accept it after his initial indecision. When arguing for the validity of his principle, he used a different strategy, though.

In his published writings, Galileo always appealed to the idea that a body on a horizontal plane is indifferent to motion since any resulting motion would be neither natural nor forced, and he assumed that this was enough to claim that this motion would also be conserved in the absence of impediments. Apparently, he felt that he had to present independent reasons to accept both components making up the parabolic trajectory of a projectile (the inclined plane experiment for the vertical component and the indifference argument for the horizontal component), before going on to their composition. Rather than seeing the parabola as the primary empirical mark left by the pure phenomenon of neutral motion, he wanted to present the situation on the horizontal plane as a legitimate exhibition of that phenomenon. But we already saw that this move is not as evident as Galileo made it out to be, since it is not clear how he could have unambiguously decided that indif-

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ference implied conservation without the information provided by the parabolic shape.[2]

It is this same move that explains the main distance separating Galileo's principle from our classical inertial principle. The horizontal plane functions as a device that neutralizes the body's weight so that it becomes indifferent to motion. The absence of a comparable device for the case of oblique projection is the reason why Galileo cannot apply conservation of motion there. It is also the reason why the precise nature of the horizontal (rectilinear or circular) remains underspecified: the directionality of Galileo's conserved motion is not referred to an abstract notion of space that functions as background, but it is defined with respect to the direction of weight, which can be considered as either everywhere parallel with itself or converging in a single point.

The need for a physical device that neutralizes the body's weight shows that for Galileo weight remained what Stevin had called an "inseparable attribute" of all physical bodies, a property that could not be legitimately subtracted from these bodies in an "imaginary operation" (the importance of this fact was already stressed forcefully in Koyré [1966]). There is a further story to be told about this conceptualization of bodies, but it is interesting to point out that Galileo's results contained everything that was needed to justify the further idealization in which essentially weightless bodies are posited to move uniformly and rectilinearly in any arbitrary direction once put into motion. After all, if the uniformity of horizontal motion was already based on the parabolic shape of the trajectory of the projectile, why not directly use the latter to ground this further idealization? This step was implicitly taken by Galileo's pupils Cavalieri and Torricelli, who composed a uniform motion in the direction of projection with an accelerated motion caused by the weight added to the body (see Koyré 1966, 292–304). In this way, the parabola could become the empirical mark not just of the nature of Galileo's neutral motion but of the pure phenomenon of the motion of weightless bodies, that is, something that we can characterize as inertial motion.

Once one has started treating weight as a separable attribute, an external force modifying the pure phenomenon of motion, the logic behind the jus-

2. It is true that after having starting conceptualizing downward motion as naturally accelerated, the step from indifference to conservation has become smaller than it was at the time of *De Motu Antiquiora*, when Galileo still saw downward motion as essentially uniform. Galileo can now claim that a body gains speed if it moves down, and it loses speed if it moves up. If it moves neither up nor down, there is hence no reason why it should gain or lose speed, which would imply conservation. This symmetry consideration holds some intuitive appeal, but it prejudges the question whether weight (as measured by resistance against upward motion) is the only reason why a body would lose a speed imparted to it. Again, it is not clear how we could ever decide this on purely empirical or theoretical grounds.

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tification for this idealization has started shifting, though. There can no longer be the presumption that we could ever encounter a close approximation of this pure phenomenon in the empirical world, as was still suggested by Galileo's device of the horizontal plane. It has rather become a necessary condition for the successful mathematization of empirical phenomena such as the shape of projectile motions.

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