

Mechanics Lost: Husserl's Galileo and Ihde's Telescope

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Abstract Don Ihde has recently launched a sweeping attack against Husserl's late philosophy of science. Ihde takes particular exception to Husserl's portrayal of Galileo and to the results Husserl draws from his understanding of Galilean science. Ihde's main point is that Husserl paints an overly intellectualistic picture of the "father of modern science", neglecting Galileo's engagement with scientific instruments such as, most notably, the telescope. According to Ihde, this omission is not merely a historiographical shortcoming. On Ihde's view, it is only on the basis of a distorted picture of Galileo that Husserl can "create" (Ihde 24:69–82 2011) the division between Lifeworld and the "world of science", a division that is indeed fundamental for Husserl's overall position. Hence, if successful, Ihde's argument effectively undermines Husserl's late philosophy of science. The aim of this paper is to show that Ihde's criticism does not stand up to closer historical or philosophical scrutiny.

1 Introduction

The Crisis of European Sciences and Transcendental Phenomenology deserves to be mentioned among the classical texts of twentieth-century philosophy of science. However, interpretational problems arise already on the cover page. For, contrary to what the title might suggest, the crisis diagnosed by Husserl does not concern the sciences themselves but rather *our* philosophical understanding of science and thus the *meaning* science has for *us* as members of modern society (Husserl 1970, p. 12). Despite all the sophistication and effectiveness of the special sciences, *modern scientific culture* is in a critical state because—and this is one of the central

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messages of the *Crisis*—we lack a proper understanding of the intricate relations between the “world of science” and what Husserl calls the *Lifeworld*.

What, precisely, is the Lifeworld? As commentators have pointed out (Carr 1970; Moran 2012, chapter 6), there is no straightforward answer to this question. The term carries a host of meanings and it is one of the more unfortunate aspects of the *Crisis* that Husserl is not always careful in the way he uses it. In the context of this paper, however, the following aspects are particularly important: On the one hand, the Lifeworld is the immediately given world of pre-scientific experience. It is the world of chairs, trees, mercury columns and all the other stuff that is in principle intuitable through acts of immediate perception. On the other hand, the Lifeworld is the communal world of persons, of fellow citizens, strangers and loved ones. Hence, the term “Lifeworld” also denotes the world as experienced by a community of subjects in relation to their values, emotions, practical interests and cultural predispositions.

On the basis of these preliminary remarks, it seems clear that the Lifeworld is fundamentally different from the “world of science”: For one thing, it is the very point of our methodologically regimented scientific endeavours to approach reality in a way that leaves no room for subjective factors, cultural implications, emotions or (non-epistemic) values. Hence, the resulting “scientific image” is characterized by a non-relativity, objectivity and universality that is quite foreign to the Lifeworld. On the other hand, the “world of science” is composed of entities that, for the most part, do not show up in acts of immediate experience. Instead of chairs, trees and mercury columns, the “world of science” is populated by genes, fields and photons.

Husserl considers the question of how the Lifeworld and the “world of science” are related to be one of the most pressing issues in modern philosophy. The urgency of this question derives from the fact that the relation between the Lifeworld and the “scientific image” seems to be one of potential conflict. Let me give just one example to illustrate the kind of conflict I have in mind: Our lifeworldly interactions with physical reality rest on a number of (usually tacit) assumptions. To begin with, we take physical things to be clearly localizable. For instance, a windowpane does not exist everywhere in space and time. It is rather confined to a definitive place and to a definitive time, such as “my office” and “now”. We also take it to be impossible for a physical thing to be in mutually incompatible states at the same time: at any point t , a windowpane is either shattered or not shattered, but never both. Finally, we take any change of state to be connected by a clearly identifiable sequence of occurrences: If the windowpane was not shattered at t_1 , but is shattered at t_2 , then there must be one, and only one, chain of events leading from the window’s state at t_1 to the window’s state at t_2 .

Let us now switch to the “world of science”. Consider a simple physical system, consisting of a photon gun (a device capable of emitting single photons), two photon detectors (A and B) and a beam splitter that directs one half of the photons towards A and the other half towards B. The setup of the system is simple indeed: Whenever we press a button, the photon gun fires and a photon is registered by one of the two detectors. In quantum mechanics (QM) such a physical system is represented by the vector ψ (the wave function) in a Hilbert space (a generalization of Euclidean space

with any definite or indefinite number of dimensions). The wave function contains all information about the system, and its evolution over time is governed by the Schrödinger equation. Furthermore, it is one of the main theorems of QM that measurements always result in eigenvalues that correspond to one of the eigenvectors of the space. In our setup, only two measurement outcomes are possible, each with the probability of 0.5. Before measurement, however, the mathematics of QM represents the system in a *superposition* of states, with one state representing the photon as a wave rushing towards detector A and one state representing the photon as a wave rushing towards detector B. It is only after measurement that the superposition of the two states collapses into one or the other.

It is worth emphasizing that QM is arguably the most successful theory ever devised. According to some, this lends support to the view that the mathematical formalism of QM is not merely a useful calculation device, but rather a literal description of reality. Although we never observe quantum phenomena such as superposition states, the success of QM is said to warrant the belief that the structure of the formalism corresponds to how the world is structured: Before a photon is registered by a detector (thus acting as a particle), the photon *is* a wave that is smeared out over a large region of space—just as the theory says. And before a detector beeps, the photon indeed rushes both towards A *and* B—until superposition collapses due to an act of measurement.

It should be apparent that if one understands the mathematics of QM in a straightforward way as a literal description of reality, then the resulting image is indeed hard to reconcile with the aforementioned pre-scientific intuitions about the empirical world. Before measurement the photon does not allow for unequivocal localization due to its wave-like character and due to the lack of an unambiguously identifiable trajectory. Problems also arise for the second intuition: If “rushing towards detector A” and “rushing towards detector B” are mutually incompatible states, then the second intuition too is violated before the collapse of the wavefunction. Finally, according to QM, it is also not the case that there is one unique chain of events leading from the emission of the photon to its detection. Before measurement, the system exists as a superposition of eigenvectors, each with a probability of getting the corresponding eigenvalue as a result of measurement. It is only after the photon has been detected that these objective probabilities collapse into one unique value.

In light of this example, the portrayal of the Lifeworld/science-relationship as a relation of potential conflict seems warranted. Since, from a Lifeworld perspective, “the scientific image presents itself as a *rival* image” (Sellars 1991, p. 22), we are challenged to come up with strategies of how this rivalry can be settled. One such strategy—and the one that is more prevalent today—is to argue for a “primacy of the scientific image” (Sellars 1991, p. 32). In its strongest form, this amounts to the claim “that the common sense world of physical objects in Space and Time is *unreal*” (Sellars 1991, p. 173; my emphasis) and hence has the status of an illusion (Albert 2013). Of course, even on this view, the Lifeworld of pre-scientific experience exists. But it exists only in a derivative sense: Certain physical processes between the world and our physiological apparatus result in the *appearance* of a macroscopic reality that is more or less adequately described by our lifeworldly

intuitions. But since these intuitions only concern an illusion, and not the real world, they are no more relevant in epistemic matters than, say, pre-theoretical intuitions we might have about the Ames Room. This, then, is the gist of the view labelled “objectivism” by Husserl: When our interest is in truth about the real world and when science and our lifeworldly intuitions square off, the smart money is on science.

Husserl considers the objectivist mindset to be the principal reason for the critical state of modern scientific culture. His aim is thus to expose the shortcomings of objectivism and to propose an alternative construal of the Lifeworld/science-relationship. But what is it that makes objectivism unacceptable in the first place? What is wrong with the idea that our best theories tell us the one true story about what the world is like? Husserl’s main argument may be summarized as follows: Objectivism takes the scientific perspective towards the world as its starting point and then goes on to claim that this particular perspective and no other is capable of providing us with the one true image of the world. What makes this absolutism of the scientific worldview dubious, however, is its naivete, i.e. its lack of awareness of the various preconditions that are taken for granted whenever we put our scientific machinery to work. On Husserl’s view, the single most important precondition is that—as phenomenological analyses are supposed to reveal—the Lifeworld is always already presupposed as a “meaning-fundament” (Husserl 1970, p. 48) when we approach the world from a scientific point of view. If this is correct—if the worldview that threatens to eliminate our lifeworldly intuitions is necessarily grounded in and thus presupposes the Lifeworld—objectivism indeed appears to be flawed: To substitute the scientific image for the Lifeworld of pre-scientific experience would then be like sawing off the branch on which science is sitting.

The claim that all theoretical activities presuppose the Lifeworld as a necessary ground follows naturally from the basic principles of Husserlian epistemology. According to these principles, all inferential practices and all assertions about the empirical world refer back to the direct, intuitive givenness of the things, processes and states of affairs about which something is asserted (Wiltsche 2012, 2016). However, it is characteristic for the *Crisis* that Husserl chooses a different, less abstract path towards this conclusion. The late Husserl takes a *historical* approach to show that objectivism emerged as an unintended by-product during the early days of modern science. Husserl’s historical narrative centers around the “‘trail-blazer’ of physics” (Husserl 1970, p. 24), Galileo Galilei. Galileo is in the limelight of Husserl’s attention for two closely related reasons: On the one hand, Galileo revolutionized physics with his rigorous application of geometry to the physical world. On the other hand, however, Galileo obfuscated the true nature of his own revolution because he failed to inquire into the “how” of geometry’s applicability to reality. It is due to his unreflective usage of geometry that Galileo bequeathed two legacies to his successors: a new physics and an objectivist interpretation of what this new physics allegedly is. While Husserl approves of the former, he seeks to show that the latter is a flawed and ultimately disastrous metaphysical hypostatization of the methods employed in Galilean physics. Let us now, with these remarks as a backdrop, take a closer look at §9 of the *Crisis*, where Husserl’s take on Galilean science and metaphysics is to be found.

2 Husserl's Galileo

Husserl begins the lengthy §9 by cautioning the reader that Galileo “was not yet a physicist in the full present-day sense [since] his thinking did not, like that of our mathematicians and mathematical physicists, move in the sphere of symbolism, far removed from intuition” (Husserl 1970, p. 24). Indeed, as recent scholarship has confirmed (Machamer 1998; Palmieri 2003), mathematics, for Galileo, meant proportional geometry. Unlike Newton, who later replaced geometry with algebra and thus introduced a purely symbolic notation to physics, Galileo sought to describe the empirical world in geometrical terms, often supplemented by “‘sensible’ models” (Husserl 1970, p. 24) such as diagrams or visual representations of levers, planes, weights and balances. At first glance, this commitment to geometry could be seen as a minor historical detail. Following Husserl’s analysis, however, Galileo’s focus on geometry as the primary tool of physical research turns out to be crucial. Let us, in order to see why, take a brief look at Husserl’s views concerning the nature and origin of geometry (Tieszen 2005, chapter 3).

Geometrical objects such as lines, points, planes, triangles or spheres are nowhere to be found in the Lifeworld of pre-scientific experience. Rather, what we experience here are physical bodies with vague, inexact shapes, with “more or less perfect surfaces”, with “more or less rough or fairly ‘even’ [edges]” (Husserl 1970, p. 376). Of course, tools such as files or chisels give us the “technical capacity of perfecting, e.g., the capacity to make the straight straighter and the flat flatter” (Husserl 1970, p. 25). But, by and large, “the things of the intuitively given surrounding world fluctuate, in general and in all their properties, in the sphere of the merely typical” (Husserl 1970, p. 25).

On Husserl’s view, it is the practices of perfecting and measuring the outcomes of our perfectings that carry the germ of geometry. Imagine a situation in which we compare two surfaces, concluding that surface B is smoother than surface A. In order to even make this comparison, we must implicitly presuppose an ideal limiting pole against which varying degrees of perfection are measured. Hence, whenever we push the limits of perfection, we implicitly presuppose “*limit-shapes* [...] toward which the particular series of perfectings tend, as toward invariant and never attainable poles” (Husserl 1970, p. 26). Husserl claims that geometry came into being when people stopped bothering with questions of technological realizability and instead shifted their focus to a systematic inquiry of the ideal realm of pure limit-shapes.

There are two aspects of Husserl’s views about geometry that are particularly relevant here. The first is that the relation between geometry and the Lifeworld is a peculiar one: Both are intimately related because the former originates in the latter. Since geometry is *founded* on basic experiences of physical bodies and their inexact shapes, its rootedness in the Lifeworld is, according to Husserl, essential to the very *meaning* of geometry. At the same time, however, the realm of pure geometrical objects and the Lifeworld are also categorically distinct. Geometrical objects are, as I have pointed out, nowhere to be found in the Lifeworld of pre-scientific experience—they must be “created” out of the latter through acts of “‘pure’

thinking” (Husserl 1970, p. 377), “through a peculiar sort of mental accomplishment: *idealization*” (Husserl 1970, p. 348; my emphasis). Idealization, as Husserl understands the term (cf. Husserl 1983, §§73–74; Drummond 1984), is the process through which the vague, imprecise and morphological concepts with which we describe real bodies are replaced by exact, pure and mathematical concepts. The objects that are so constructed are ideal and hence cannot become present in acts of sensuous experience. But if the process of idealization is successful, pure geometrical objects may serve as ideal limits against which all of their inexact variations can be projected. This is precisely what happens in virtually all branches of modern physics: Frictionless planes, for instance, do not belong to the inventory of the empirical world. They only come into existence through a mental operation whose point it is to construct a limiting case against which actual instances of real planes can be projected. But in order for this mental operation to be carried out in a self-conscious manner, there must be a basic acquaintance with real surfaces and with technical procedures to make real surfaces flatter. Without this acquaintance, the idea of a perfectly flat plane would not even be meaningful to us. It is in this way that the Lifeworld of pre-scientific experience is always already presupposed as a “meaning-fundament” when we do physics with idealities such as frictionless planes, point masses or perfectly rigid rods.

The second issue that must be addressed is that of *technization*. As I have just pointed out, it is part of the original meaning of geometry that ideal, geometrical objects are founded on simple experiences of real bodies. However, Husserl is perfectly clear that one does not need to be aware of this original meaning in order to put geometry or geometrical objects to work. Once established, geometry—like so many other mathematical methods—easily becomes a technique that can be applied mechanically. This is true in particular if geometry is arithmetized and thus “becomes free [...] from all intuited actuality” (Husserl 1970, p. 44). In the realm of formalized thought we can manipulate strings of symbols without ever reflecting on what these symbols really stand for or on how they were bestowed with meaning in the first place. But technization is by no means restricted to the realm of formalized thought, as the earlier example demonstrates: Although the Lifeworld is essential to the original meaning of ideal geometrical objects, generations of physicists have successfully utilized frictionless planes or point masses without reflecting on their rootedness in the sphere of pre-scientific experience.

On Husserl’s view, Galileo laid the foundation for objectivism when he inherited geometry as an already established technique and made it the centerpiece of his new scientific vision. Of course, Galileo’s use of geometry was not unprecedented in the history of the physical sciences. But what distinguished Galileo from the tradition before him is that he did not just make occasional use of geometrical models in order to “save the appearances” in this or that segment of reality. Husserl argues that Galileo was after something much more radical, namely the complete “*mathematization of nature* [through which] *nature itself* is idealized under the guidance of the new mathematics [and] becomes [...] a mathematical manifold” (Husserl 1970, p. 23). So, on Husserl’s reading, the radicalism of the Galilean project lends itself to the thesis “that everything which manifests itself as real [...]

must have its *mathematical index*” (Husserl 1970, p. 37) and must therefore be translatable into the language of geometry.

According to Husserl, Galileo “is at once a discovering and a concealing genius” (Husserl 1970, p. 52). This formulation amply illustrates that Husserl’s views on “the father of modern science” are best described as ambiguous. On the one hand, Husserl holds Galileo’s scientific achievements in high esteem and acknowledges that they would have been impossible without Galileo’s rigorous application of geometry. On the other hand, however, Husserl critically notes that Galileo did not reflect seriously enough on the “how” of geometry’s applicability to empirical nature. Galileo ignored questions concerning the meaning and origin of geometry and thus remained unaware of its constructive nature and its groundedness in the Lifeworld of pre-scientific experience. It is due to this “fateful omission” (Husserl 1970, p. 49) that Galileo did not conceive of geometrical objects as what they are—constructed limiting-poles against which real physical objects can only ever be projected. Galileo’s naivete misled him to believe that geometry is the key to revealing the essential structure of the universe, which, on Galileo’s view, is mathematical in nature.

It cannot be emphasized enough that what Husserl criticizes is not that Galileo used geometrical models, but that his use of geometry was naïve and thus led to a false metaphysics. Ignoring the constructive nature of geometry as well as the ideal nature of geometrical objects, Galileo took translatability into the language of mathematics as the criterion for what counts as real. On Husserl’s view, this gives rise to a number of problems, the most serious of which concerns the “plena” or “specific sensible qualities” (cf. Husserl 1970, §9c). The problem, in a nutshell, is this: Galileo’s project of a complete mathematization of reality is based on the assumptions that all phenomena of nature can in principle be submitted to mathematics and that their behaviour can be explained and predicted by a relatively small set of quantitative laws. Now, certain properties of physical things (Galileo mentions shape, size, position, motion or rest, contiguity and number) meet the demand for quantification and mathematization quite naturally. In the case of other properties, however, the project of an all-encompassing mathematization seems to be fraught with difficulties. For instance, we always experience real objects as having a certain color, taste, warmth or odor. But these secondary qualities defy any effort to be straightforwardly measured and directly translated into the language of pure geometrical shapes. How is this recalcitrance to be interpreted? Is it an indication that the project of a complete mathematization of nature is unfeasible after all? Or is there a way to reconcile secondary qualities with the view that the universe is mathematically structured?

Following his understanding of geometry as the measure of what counts as real, Galileo showed no reluctance to advocate the second option and to take the untranslatability of secondary qualities as a sign for their non-existence. On his view, “tastes, odors, colors, etc., [...] are nothing but empty names [that] inhere only in the sensitive body” (Galilei 2008, p. 185). Consequently, “if one takes away ears, tongues, and noses, there [...] remain the shapes, numbers, and motions, but not the odors, tastes, or sounds” (Galilei 2008, p. 187). Of course, at first sight, this proposal does not solve the initial problem because we are still left with an

incomplete picture of reality, which, after all, manifests itself through primary *and* secondary qualities. However, since its very inception by Galileo, objectivism is tied to the promise that secondary qualities can be accounted for by way of reduction to primary qualities: color is *indirectly* mathematizable (and thus reducible to wavelengths) through the theory of electromagnetic radiation; warmth is *indirectly* mathematizable (and thus reducible to motions of electrons, atoms and molecules) through the theory of thermodynamics. It is in this way that objectivism promotes the “surreptitious substitution of the mathematically substructured world of idealities for the only real world, the one that is actually given through perception, that is ever experienced and experienceable—our everyday life-world” (Husserl 1970, pp. 48-49).¹

On Husserl’s view, Galileo’s distinction between primary and secondary qualities drives a wedge between the Lifeworld and the “world of science”. What we are left with is the paradoxical situation that the Lifeworld—the unsurpassable “meaning-fundament” of all of our scientific and extra-scientific practices—is degraded to the status of a mere illusion and that, consequently, the “real world”—the world of which science speaks through its mathematical models—is forever put beyond our experiential grasp. However, as §9 is supposed to show, this paradox could have been prevented had Galileo paid attention to the presuppositions underlying his methodology: It is only because Galileo did not acknowledge its constructive nature that he could take geometry as the measure for what counts as real. And it is only because Galileo ignored geometry’s groundedness in simple acts of pre-scientific perception that he could promote an objectivist construal of the Lifeworld/science-relationship—a construal that, on Husserl’s view, pulls the rug from under the feet of science.

3 Ihde’s Telescope

Let me recap: Galileo plays such a prominent role in the *Crisis* because Husserl sees in him the godfather of modern objectivism. It was Galileo’s distinction between primary and secondary qualities that drove a wedge between the Lifeworld and the “world of science”. And it was Galileo who based his project on the assumption that the real world is not the world that we constantly experience, but rather a curious netherworld that remains largely inaccessible through simple perceptual acts and that is only indirectly given through mathematical models. So, on Husserl’s view, Galileo initiates a process that obfuscates our understanding of science to this very day: the process of a constant estrangement between the “world of science” and its unsurpassable foundation, “the world which gives itself [...] in actual experience, the ‘world of sensibility’” (Husserl 1970, p. 347). However, it is precisely this

¹ Husserl’s view that the doctrine of primary and secondary qualities originates with Galileo and plays a crucial role in Galilean science is widespread, especially among philosophers. However, historians of science have emphasized that “the doctrine hardly figures after *Il Saggiatore*” (Gaukroger 1978, p. 208) and is virtually absent in Galileo’s later works. Indeed, as we shall see in Sect. 4, a different distinction plays a much more prominent role in Galileo’s actual scientific practice. But, as we shall also see, Husserl’s critique applies to both distinctions.

portrayal of Galilean objectivism to which Don Ihde objects. According to him, “Husserl got it wrong” (Ihde 2011, p. 75). Or, more specifically: Husserl “got science itself wrong and that through his own reductionistic version of Galileo” (Ihde 2011, p. 75). In what follows I will try to unpack the reasons for this harsh verdict.

As we have seen in the previous section, Husserl pays almost exclusive attention to Galileo’s achievements in theoretical physics and to his use of geometry. But, according to Ihde, this focus results in a profoundly misleading image of the “father of modern science”. Galileo was, as Ihde points out, not only a theoretical physicist. He was also a “lens-grinder”, a “user of telescopes”, a “fiddler with inclined planes” and a “dropper of weights from the Pisa Tower” (Ihde 2011, 78). So, while Husserl’s Galileo is a pure theoretician, Ihde stresses that the real Galileo was much more than that: he was also an experimenter, an engineer, and a user of instruments.

For Ihde, the fact that Galileo’s practical engagements are virtually absent from Husserl’s narrative comes as no surprise. This is because Husserl’s construal of science is “highly conservative” (Ihde 2011, p. 71): First, Husserl takes mathematical physics as the model of successful empirical science. Hence, in focusing on physics as the exemplar of all empirical sciences, Husserl’s reflections are guided by a discipline that is among the most abstract and most ahistorical areas of empirical research (cf. Ihde 2011, p. 70).² However—and this is what Ihde considers to be the second conservative moment, Husserl makes physics even more abstract by reducing it to its theoretical core. According to Ihde, Husserl did not conceive of physics as a practice that manifests itself in research communities, technologies, and experimental setups. Rather, Husserl conceived of physics (and of science in general) as a “formalistic”, “abstract”, “highly generalized and virtually non-empirical” (Ihde 2011, p. 71) endeavour. Given this conservatism, Ihde finds it not surprising “that Husserl’s Galileo is a pre-selected and reduced Galileo” (Ihde 2011, p. 78), an over-intellectualized, disembodied Galileo who is at home in the armchair and not in the laboratory, the workshop or the observatory (cf. also Ihde 1990, p. 38).

From a historical perspective, there is certainly some truth in Ihde’s criticism. As recent studies have confirmed (cf., e.g., Biagioli 2006; Valleriani 2010), Galileo was indeed a hands-on personality who devised and performed experiments (more on this in Sect. 5), who invented measurement devices and who popularized instruments for different scientific and non-scientific purposes. What is more, Ihde is also right that these practical aspects are largely absent in the *Crisis*. So, no matter what one may think of Husserl’s overall position, it is true that his Galileo is an abstraction.³ However, the point of Ihde’s criticism is not merely to expose certain historical inaccuracies in Husserl’s treatment of Galileo. Rather, Ihde advances the

² I find Ihde’s assertion that physics is among “the most ahistorical [sciences]” (Ihde 2011, p. 70) dubious, to say the least. But since this claim plays no role in the following discussion, I will not pursue it any further.

³ It must be noted, however, that Husserl never makes a secret of the fact that he is “simplifying and idealizing the [historical] matter” (Husserl 1970, p. 57). Husserl’s Galileo is—to put it in Derrida’s words—“the exemplary index of an attitude and a moment, rather than a proper name” (Derrida 1989, p. 35).

much stronger claim that Husserl *uses* a distorted picture of Galileo to “creat[e] the division [between the Lifeworld and the world of science]” (Ihde 2011, p. 69; my emphasis). This claim is a very strong one indeed: The point of Ihde’s argument is that the main problem in the *Crisis*—the problem of an increasing estrangement between Lifeworld and science—is in reality a *self-fabricated pseudo-problem* that arises as a result of Husserl’s cooked up interpretation of Galileo. If we replace Husserl’s questionable reading of Galileo with a historically more accurate counter-narrative, then, according to Ihde, we come to realize that the gap between science and Lifeworld that allegedly originated with Galileo is nothing but a philosophical chimera.

The counter-narrative offered by Ihde centers around the telescope, an optical device whose history is inextricably connected to the Galilean revolution in astronomy. The historical facts are fairly straightforward (cf., e.g., Swerdlow 1998; Van Helden 2010): Galileo heard rumors of a recently invented “spyglass” in 1608. He made first attempts to produce his own telescope soon thereafter but was dissatisfied with the magnification of only 3×. Galileo thus learned to polish and grind lenses and improved his instruments to 8× and, by the beginning of 1609, even to 20×. Only one year later, in 1610, he published an overview of his telescopic observations in his *Sidereus Nuncius*. Galileo reports to have observed hundreds of previously unknown stars, moons orbiting Jupiter and mountains and canyons on the earth’s moon.

There are two reasons why Galileo’s work with the telescope is important for Ihde’s argument. First, it is undeniably true that the evidence published in the *Sidereus Nuncius* was revolutionary enough to shake the then-reigning worldview to its very foundations. Aristotelian cosmology was based on the assumptions that a) all heavenly bodies revolve around the stationary earth and that b) bodies in the supralunar sphere show no signs of irregularity due to their ethereal composition. While the moons of Jupiter undercut the first assumption, mountains and canyons on the earth’s moon put pressure on the second. So, there can be no doubt that the telescopic evidence gathered by Galileo caused a major break with many firmly held beliefs about humanity’s place in the universe.

Far more important, however, is the following, second aspect: According to Ihde, the telescope is not simply a device that made it possible for Galileo to gather data that would have been unattainable otherwise. Following his general maxim that “science is [...] essentially [...] embodied technologically in its instrumentation” (Ihde 1990, 1991, p. 103, 1993, p. 59, 2011, p. 77), Ihde argues that instruments such as the telescope are “‘extensions’ to human perceptual and bodily activity” (Ihde 1991, p. 75, 1979, p. 15). What Ihde has in mind here can be illustrated by considering Merleau-Ponty’s famous example of a blind man’s stick (Merleau-Ponty 2002, p. 165): For a blind person who has learned how to use her stick, the stick is no longer a normal external thing among others. Rather, the stick has been incorporated in her body schema and now acts as a “bodily auxiliary, an extension of the bodily synthesis” (Merleau-Ponty 2002, p. 176). This means, among other things, that the stick has become a source of direct, non-inferential information about the world. Since the stick is a literal extension of the body, perception mediated through it exhibits the same immediacy and non-inferentiality that

characterizes all other forms of direct, lifeworldly perception. The point of Ihde's proposal is that we must think of scientific instruments in exactly the same way (Ihde 1991, p. 30, p. 75). The telescope is for Galileo what the stick is for a blind person: an extension of the body that modifies existing bodily modalities, expands the sphere of what can be immediately perceived, and, consequently, enlarges the Lifeworld of directly experienceable things.

We are now in a position to appreciate the full force of Ihde's criticism. As we have seen, Husserl blames Galileo for not having paid attention to the meaning and origin of geometry. It is due to his disregard for geometry's groundedness in acts of simple, pre-geometrical perception that Galileo could advocate the substitution of an idealized "formula-world" (Husserl 1970, p. 48) for the world that we constantly experience. On Ihde's view, however, the situation is quite the reverse: The problem is not Galileo's disregard for geometry's groundedness in the Lifeworld. The problem is rather that Husserl ignored the instruments that prevented science and Lifeworld from drifting apart in the first place. According to Ihde, Galileo did not just posit a new "scientific world" that is located somewhere beyond our experiential grasp. Galileo forestalled any danger of a radical break between the Lifeworld and the "world of science" by also providing the "mediating technologies" (Ihde 2011, p. 78) that made his new "scientific world" directly perceivable. Galileo's use of the telescope illustrates the point well: Galileo challenged the scientific establishment with an entirely new world that included, among other things, mountains on the moon. But this new world was, as Ihde argues, neither a matter of postulation or inferential reasoning, nor was it out of Galileo's experiential reach. Quite the opposite: Since the telescope enhanced Galileo's bodily modalities in a way that made the moon perceptually available to him (Ihde 2011, p. 80), mountains on the moon were no less part of Galileo's Lifeworld than the Alps or the Tower of Pisa. This, then, is the gist of Ihde's view: The rift between science and Lifeworld that allegedly originated with Galileo and that plays such a prominent role in the *Crisis* never existed. It never existed because instruments such as the "telescope mediated science and the lifeworld [...] thus leaving Galileo in a lifeworld to begin with" (Ihde 2011, p. 81).

It is important to acknowledge that Ihde's criticism is not only a criticism of Husserl's reading of Galileo. Rather, according to Ihde, Husserl's misinterpretation of Galileo is symptomatic of the general inability of classical phenomenology to come to grips with modern science. "Orthodox" Husserlians base their philosophy of science on the view that the objects populating the Lifeworld and the entities populating the "world of science" are differently constituted, the main difference being that the latter are "lacking sensory dimensions, and [that] they are always derived from the plenary, sensory world of bodily existence" (Ihde 1991, p. 102). However, on Ihde's view, this sharp dichotomy—together with the philosophy of science that builds on it—breaks down if we acknowledge that scientific entities—from distant planets to subatomic particles—"are often, if not typically, *instrumentally* constituted" (Ihde 1991, p. 102).

Here then is a critique of classical phenomenology from the perspective of a more contemporary Euro-American phenomenology, focused specifically on a new

appreciation of instrumentation as the embodied meditation needed to make the unification of the lifeworld and scientific objects possible. (Ihde 1991, p. 103).

Intellectual patricide has been a distinctive trait of the phenomenological movement in the twentieth- and twenty-first century. Although Husserl is commonly recognized as the founding father of phenomenology, many later phenomenologists have accused him of various philosophical vices such as foundationalism, Cartesianism, essentialism, solipsism, intellectualism, subjectivism or theoreticism. Ihde's attacks on Husserl's philosophy of science fit squarely into this scheme. According to Ihde, it is Husserl's "lofty perspective" (Ihde 1991, p. 18), his fixation with theory and his neglect of the social, practical and bodily dimensions of science that result in a view which is similar to those held by the Logical Positivists: Science is reduced to a "logical-mathematical theory machine" (Ihde 2011, p. 75) which is "driven by the ascendancy of abstraction, formalism, and idealization" (Ihde 2011, p. 72). Following Ihde's analysis, one of the main problems with this view is that it creates a problem where none in fact exists: If Husserl had realized that there is more to science than theory, formalization, and idealization, the pseudo-problem of an increasing estrangement between Lifeworld and science would never have arisen. Since Ihde considers Husserl's take on Galilean science to be a particularly telling example of his theoreticism, Ihde's counter-narrative is supposed to illustrate what a more fruitful phenomenological perspective towards science might look like.

4 Mechanics Lost? Example One

Let me recap: According to Ihde, it is Husserl's neglect of the practical, social and technological dimensions of science that leads, first, to a distorted image of Galileo and, secondly, to the pseudo-problem of a radical break between the Lifeworld and the "world of science". The goal of Ihde's counter-narrative is to correct this mistaken view. In order to do so, Ihde calls for "a sensitive historical reinterpretation of not what Galileo says about his theory but what must occur at the level of praxis" (Ihde 1990, p. 52). The crucial point of this reinterpretation is that a radical break between Lifeworld and science never occurred because Galileo's scientific practice was embodied through his instruments.

I must confess, however, that I find Ihde's "sensitive historical reinterpretation" neither sensible nor compelling. Most importantly, I cannot help but be struck by the absence of mechanics and kinematics from Ihde's rendition of Galilean science. Focussing exclusively on astronomy, Ihde claims that Husserl's Galileo is nowhere to be found in the historical record. But what if Husserl's Galileo is impossible to find because Ihde is looking in the wrong place? As I will argue in the following, this is indeed the case. Two examples from Galilean mechanics will help to show that, while the analysis in the *Crisis* may rightly be considered selective, Husserl nevertheless succeeds in identifying a methodological trait in Galileo's approach that had a significant impact on the subsequent development of modern science. Before coming to my first example, however, some stage-setting is necessary (cf., for the following, Koertge 1977; McAllister 1996).

Within the Aristotelian tradition that preceded Galileo, mechanics was concerned with *natural occurrences*,⁴ i.e. with physical processes exactly as they take place under natural circumstances. For instance, the Aristotelian law of falling bodies (according to which the speed of a falling body is proportional to its absolute weight divided by the resistance of the medium) was supposed to account for (and was taken to be supported by) a wide variety of real falling objects observable under normal, lifeworldly conditions. In contrast, Galileo conceives of mechanics as the study of *phenomena*, i.e. the invariant forms that allegedly underlie natural occurrences. A natural occurrence, for Galileo, is always the result of one or more phenomena and a great number of accidents. Although Galileo acknowledges that the accidents are responsible for the huge variety of observable natural occurrences, he claims that they must be ignored in physics. Since they are “infinite in number” and since “it is not possible to give any exact description [of them]” (Galilei 1954, pp. 252–253), Galileo thinks of causal accidents as *impediments* that must be systematically excluded if we are to arrive at a true account of reality. One of the means through which we may avoid the mess of accidents and gain access to the real world of pure (i.e. accident-free) phenomena is the technique of *geometrical idealization*. This is what Galileo has in mind when he famously declares that the “book that is constantly open before our eyes, that is the universe, [...] is written in mathematical language” (Galilei 2008, p. 183) and “that trying to deal with physical problems without geometry is attempting the impossible” (Galilei 1967, p. 203).

Let us now take a look at the first example. After having dealt with uniform and naturally accelerated motion in the third chapter of the *Discorsi*, Galileo turns to the issue of projectile motion in the fourth chapter. Quite generally, the novelty of Galileo’s account lies in the idea of treating this kind of motion as a compound of uniform horizontal motion and naturally accelerated vertical motion. It is on this basis that Galileo goes on to prove a number of theorems, the first of which reads: “A projectile which is carried by a uniform horizontal motion compounded with a naturally accelerated vertical motion describes a path which is semi-parabola” (Galilei 1954, p. 245). A slightly modified version of Galileo’s proof goes as follows (cf. Fig. 1; Galilei 1954, pp. 248–250; Clavelin 1978, pp. 409–411, 449–450; Gaukroger 1978, pp. 198–201): Imagine a frictionless elevated horizontal plane *ab* on which a perfectly spherical body moves with uniform speed from *a* to *b*. Let the line *be* represent the flow of objective time and divide it into segments *bc*, *cd* and *de*, each representing equal timespans. Now, in order to determine the trajectory of the body after it has lost contact with *ab*, remember that Galileo thinks of projectile motion as a compound of uniform horizontal motion and naturally accelerated vertical motion. Taken together, these two motions result in a curved path, terminating in point *i* after one interval of time (*bc*). But where will the object be after two intervals of time (*bd*)? Well, since the horizontal motion is uniform, the body will have covered twice the horizontal distance after two time intervals (the

⁴ The terminology of “natural occurrences” and “phenomena” is due to McAllister. “Phenomenon”, in this context, is an umbrella term that refers to all relatively general and stable features of the world that—although unobservable in many cases—are considered interesting from a scientific point of view.

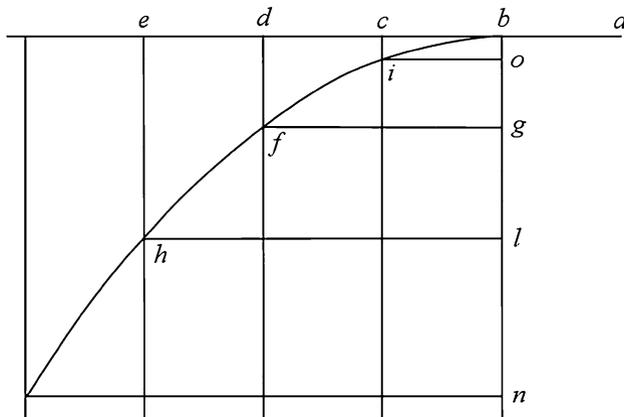


Fig. 1 Galileo's proof of theorem I

horizontal distance is represented by the line gf). The vertical motion, on the other hand, is accelerated. As Galileo has already sought to establish (Galilei 1954, p. 174), it is thus governed by the law according to which the distance fallen is proportional to the square of the elapsed time. Hence, we can conclude on the basis of this law that after two time intervals the body will not have covered twice, but four times the vertical distance that it had covered after one interval of time (the vertical distance travelled is represented by the line df). By the same logic we can conclude that the body will have covered three times the horizontal distance (lh) and nine times the vertical distance (eh) after three intervals of time, and four times the horizontal distance and sixteen times the vertical distance after four intervals of time. Thus, as can readily be seen in Fig. 1, the body indeed describes a path which is semi-parabola.

Only those lacking a soul will deny that Galileo's proof is a thing of beauty. But there is a natural question to ask from a phenomenological point of view: What exactly is the problem for which Galileo's proof is the solution? At first blush, the answer seems obvious: Galileo seeks to solve a problem in mechanics. And mechanics is the area of physics that investigates the behaviour of *physical* bodies in *physical* environments when subjected to *physical* forces. But is the matter really so simple? We may move closer to an answer by looking at Galileo's proof in a bit more detail.

Although projectiles such as arrows, spears and bird shots have always been observed to follow some sort of curved path, pre-Galilean mechanics lacked the resources to determine their precise trajectory in a satisfactory manner. Galileo thus sets himself the task of providing a theoretical framework within which the problems surrounding projectile motion can be settled once and for all. But, of course, it wouldn't be Galileo if he did not do so in a truly revolutionary way. In line with the general methodological assumptions underlying his new science, Galileo does not tackle the issue at the level of natural occurrences, i.e. at the level of actual arrows, spears and bird shots, as they can be observed under normal, lifeworldly conditions. Galileo's first step is rather to impose a geometrical grid on the

Lifeworld in order to transform a physical fact (“Actual projectiles follow some sort of curved path.”) into a geometrical problem (“How does a perfectly spherical object that moves on a frictionless plane with uniform motion behave in void space after it has lost contact with the plane?”) by setting up an abstract model (Fig. 1). After having replaced the complexity and messiness of the Lifeworld with the tidiness of a geometrical model, Galileo’s second step is to offer a mathematical solution that successfully accounts for projectile motion as it is represented by the model. The third and final step is to claim that the theory that has been obtained in the course of the analysis is nothing less than a truthful description of the phenomenon of projectile motion, i.e. of projectile motion as it would appear if all causal impediments and accidents were put aside. It is at this level of the analysis that the theory finally becomes prescriptive for experience. Actual observable instances of flying arrows, spears and bird shots are now conceived of as mere approximations to the ideal case which is represented by the model.

It is important at this point to not underestimate the true significance of Galileo’s account. For instance, one could argue on Galileo’s behalf that the aim of science cannot be to give a complete description of the whole multitude of facts that make up a concrete physical situation. To do so is not only impossible for practical reasons; it is usually not even desirable, since many facts (say, the color of a projectile) have no causal efficacy at all in influencing the phenomenon we are interested in (say, the trajectory of projectiles). Following this line of argument, one could claim that Galileo’s primary achievement is to offer a powerful tool for *reducing the complexity* of concrete physical systems. On this view, what we are dealing with when looking at a model such as Fig. 1 is simply a “tidied-up” rendering of the Lifeworld. What makes this particular representation of reality so successful is that it reduces the complexity of its target system in just the right way: It is through the systematic exclusion of causally irrelevant factors that Galileo’s model singles out those features that do have a causal bearing on the behaviour of projectiles.

Although I do not deny that reduction of complexity is an important accomplishment of scientific models (I shall say more on this in the next section), this interpretation still fails to capture the full significance of Galileo’s modelling practice. That Galilean models do not represent their physical target systems in a simplified but otherwise realistic manner becomes apparent if one acknowledges the idealizing assumptions that are built into the model with which Galileo works and to which his proof applies. Firstly, it is assumed that the projectile is perfectly spherical and that no energy is lost due to frictional effects. Secondly, Galileo assumes that it is possible to ignore the tendency of physical bodies to fall towards the center of the earth. Thirdly, the model involves the assumption that the surface of the earth can be treated as an ideal geometrical plane.⁵ And fourthly, Galileo assumes it to be possible to ignore the perturbation effects that are caused by the medium through which every physical body falls. All of these assumptions are necessary in order to set up the model and in order for Galileo’s proof to go through.

⁵ Without this assumption, the object could not move along *ab* with uniform speed because it would constantly go uphill, thus being retarded by its tendency to strive towards the center of the earth.

At the same time, however, all of them are known to be false of the world in which we live. Hence, what we are dealing with when looking at Fig. 1 cannot possibly be a simplified but otherwise realistic representation of a particular segment of the Lifeworld. What we are dealing with is rather a representation of an *ideal limiting case* that is found nowhere in the domain of concrete, intuitable things.

Acknowledging these idealizing assumptions, it becomes finally possible to capture the full breadth of Galileo's approach. Instead of merely simplifying the Lifeworld, the point of Galileo's modelling practice is to generate *ideal quasi-worlds* that are represented by geometrical models. The decisive step in Galileo's methodology is to substitute these quasi-worlds for the Lifeworld in order to set the stage for his proofs and demonstrations. Getting clear about this is important because it shows that—at least as far as mechanics is concerned—the target of Galilean science is not the Lifeworld of concrete, intuitable things. The target is rather an ideal quasi-world—a “formula-world” (Husserl 1970, p. 48)—with which the Lifeworld is replaced right at the outset of Galileo's analysis. Hence, a closer look at Galileo's work in mechanics confirms a claim made by Nancy Cartwright about the physical sciences in general: The “fundamental [solutions, laws and] equations do not govern objects in [empirical] reality; they only govern objects in models” (Cartwright 1983, p. 129).

Interestingly enough, Galileo himself is perfectly aware of this. For instance, while discussing the tenability of the idealizing assumptions underlying his analysis of projectile motion, he admits that the “conclusions proved in the abstract will be different when applied in the concrete and will be fallacious to this extent, that neither will the horizontal motion be uniform, nor the path of the projectile a parabola” (Galilei 1954, p. 251). Yet, while for many a divergence between abstract theory and concrete reality sounds like bad news for the former, Galileo does not appear to be particularly worried. His relaxed attitude stems from the metaphysical background of his new science: Galileo holds that rigorous mathematical proofs yield insight into the necessary structure of reality and thus allow us to participate in the perfection of God's knowledge (cf., e.g., Galilei 1967, p. 103; McTighe 1967, pp. 375–378; Redondi 1998). Consequently, when—as in the case of his treatment of projectile motion—a rigorous proof is found, he feels warranted to regard the finalized model as a truthful representation of the “real world” of pure, accident-free phenomena. Granted, things in the Lifeworld may not work out as neatly as they do in the model. But since the Lifeworld is nothing but a veil of accidents behind which the “real world” is hidden, this is not even to be expected.⁶

Let me summarize: If my interpretation is correct, then the decisive step in Galileo's approach is to mathematize nature by substituting geometrical models for

⁶ A historical episode illustrates the point nicely: Four years after Galileo's death the gunner Giovanni Ranieri attempted to apply Galileo's theory of projectile motion to his craft. However, as Ranieri reports in a letter to Evangelista Torricelli—Galileo's successor at the University of Pisa—the experimental results did not even come close to matching the theoretical predictions. Replicating the situation represented in Fig. 1, Ranieri used an elevated gun to perform a number of point-blank shots. While the theory predicted a range of approximately 96 paces, Ranieri achieved ranges of 400 paces and more (cf. Segre 1991, pp. 94–97). Particularly interesting is how Torricelli reacts to Ranieri's complaint: Torricelli explains the empirical inadequacy of Galileo's theory by pointing out “that Galileo [speaks] the language of geometry and [is] not bound by any empirical result” (Segre 1991, p. 44).

the Lifeworld of pre-scientific experience. There are two reasons why this step is crucial for Galilean science: First, Galileo is convinced that physics can only become a real science if its problems are formulated and solved mathematically. But since mathematics cannot be applied to complex physical systems in a simple and straightforward manner, Galileo must provide a method that makes the Lifeworld amenable to quantitative analysis. The method that Galileo offers is to transform physical problems into mathematical questions through geometrical idealization and model construction. Even more important, however, is the second reason: While the use of geometrical idealization and model construction could also be justified pragmatically (I shall say more on this in the next section), Galileo holds the much stronger view that these methods are actually truth-producing. Hence, instead of merely allowing us to deal with reality in a predictively successful way, models are said to be representative of the “real world” of pure, accident-free phenomena. Following Husserl’s lead, it is precisely this objectivist interpretation that brings science and the Lifeworld on a collision course with each other. Since idealized models such as Fig. 1 do not merely represent simplified renderings of the Lifeworld, but physically unrealizable quasi-worlds, the “scientific image” that is promoted by Galilean physics presents itself as a rival to the world that becomes manifest in acts of pre-scientific experience. And this, according to Husserl, leaves us in a quandary: On the one hand, objectivism implies that the Lifeworld is nothing but a veil that needs to be removed in order to catch a glimpse of the “real world” of pure, accident-free phenomena. At the same time, however, the methods through which this veil ought to be removed presuppose the Lifeworld as their necessary “meaning-fundament”. If this is true, then objectivism leaves us in a paradoxical situation indeed: To advocate objectivism is, as I have said earlier, to saw off the branch on which science is sitting.

We can now return to Ihde’s criticism. Ihde maintains that a radical break between Lifeworld and science never occurred because Galileo’s scientific practice was embodied through his instruments. This claim, however, is clearly at odds with the case study I have discussed in this section. On the interpretation offered here, it is due to his objectivism that Galileo’s modelling practice causes a rift between the Lifeworld of pre-scientific experience and the “world” of which science speaks through its models. Yet, since these models represent *ideal quasi-worlds*, it is unclear how scientific instruments should prevent this rift from occurring. It may be true that telescopes modify our bodily capacities in a way that makes mountains on the moon sensuously accessible to us. But it is hard to see how telescopes might be of similar relevance when we are dealing with idealities such as frictionless planes or perfectly spherical projectiles.

5 Embodied Mechanics? Example Two

The aim of the previous section has been to show that Ihde’s criticism is mistaken. Contrary to what he claims, Husserl is justified in blaming Galileo’s objectivism for creating a gap between science and Lifeworld. And it is not through instruments that this gap can be closed, but through a careful phenomenological analysis of the

presuppositions that underlie Galileo's modelling practice. So far, so good. But is this enough to settle the case in favour of Husserl? Presumably not. In what follows, I will anticipate a possible counter-argument that, although only hinted at by Ihde, could be seen to cast doubt on the analysis presented thus far.

Kuhn has famously claimed that "a concept of science drawn from [classics and textbooks] is no more likely to fit the enterprise that produced them than an image of a natural culture drawn from a tourist brochure" (Kuhn 1996, p. 1). Kuhn's point is that classics and textbooks are poor guides to understanding science because they represent just one side of the coin: Not only is their aim pedagogic and persuasive. They also restrict attention to the finished achievements, thereby concealing the rocky road leading to this end. Since a textbook-driven or classics-driven image of science only portrays the final outcome but not the process of knowledge-production, those who buy into this image typically tend to overemphasize *theory* at the cost of *practice*. Science is viewed as a purely intellectual endeavour and not as a practical undertaking that is situated in research communities, institutions, experimental setups, and a wider social context. Let us call this the *failure of textbook-driven historiography*.

The development of modern Galileo scholarship exemplifies this failure well. When browsing through Galileo's published works (especially in mechanics), it is sometimes hard to believe that one is dealing with the founding texts of modern *experimental science*. What dominates in these works are demonstrations and proofs that look like geometry more than anything else. At several points, Galileo is openly dismissive of experimental methods in physics (cf., e.g., Galilei 1954, p. 276). And those experiments that do appear are so polished that it is almost inconceivable that they were ever actually performed (Cushing 1998, pp. 81–84). Given all this, it is no surprise that some historians flat out denied that experiments played any role at all in the establishment of Galilean science (Koyré 1968, p. 94). It was only after extensive research on Galileo's unpublished manuscripts that a different picture emerged: It became clear that Galileo was a much more prolific experimenter than his published works would lead us to suspect (cf., e.g., Settle 1961; Naylor 1976, 1980; Hahn 2002). Today it is commonly accepted that, while experiments may not have been essential for the way in which Galileo presents himself in print—perhaps because he did not want to bore his lay readers with a "parade of data" (Drake 1973, p. 305)—experimentation was an essential component of his actual scientific practice.

Why is any of this relevant for an assessment of Ihde's criticism of Husserl? Well, in the face of the argument presented in the previous section, Ihde could concede that—at least as far as mechanics is concerned—scientific instruments do little to prevent Lifeworld and science from drifting apart. But he could counter by arguing that this does not necessarily undermine his main point. An argument to this effect could proceed as follows: The interpretation offered in the previous section lays stress on the modelling practice that is essential to Galilean physics. The Galileo we have encountered so far is a theoretician who employs a special mental operation in order to replace the vagueness of lifeworldly objects with the exactitude of idealities such as frictionless planes or perfectly spherical projectiles. Now, while Ihde could happily admit that model-building is an integral part of

Galilean science, he could accuse me of committing the failure of textbook-driven historiography by reducing Galilean science to this single trait. Galileo was not only an armchair speculator who sought to replace the Lifeworld with abstract models. A closer look at his unpublished manuscripts shows that the Galileo of the *Discorsi* was working in an “empirical episteme” (Naylor 1989) and that, consequently, his experimental program was no less decisive for his overall project than the method of geometrical idealization. If this much is admitted, the last step of the argument suggests itself: Even if Galilean mechanics was not embodied through instruments, it was embodied *technologically* nonetheless, namely through *experiments*. Or, to put the same point another way: It may be true that Galileo’s modelling practice confronts us with a “scientific world” that is fairly different from the world that we experience under normal, lifeworldly conditions. But Galileo forestalled any danger of a radical break between Lifeworld and science by also providing the technological means through which his “scientific world” remains connected with the Lifeworld. On this view, then, it was through *experimental arrangements* that Galileo prevented Lifeworld and science from drifting apart.⁷ In what follows, I will take a closer look at Galileo’s experimental practice in order to show that this argument fares no better than the previous one.

Midway through the first chapter of the *Discorsi* Galileo critically engages with the Aristotelian law of falling bodies according to which the speed of a falling body is directly proportional to its weight divided by the resistance of the medium. The first step in Galileo’s attempt to refute this law is not an actual, but an ingeniously designed thought experiment: Galileo asks us to imagine that a heavy cannon ball is attached to a lighter musket ball and that the combined system is thrown from a tower. What will happen according to the Aristotelian theory? First, since the lighter musket ball will retard the heavier cannon ball, the combined system will fall slower than the cannon ball alone. But, on the other hand, since the combined system is heavier than the cannon ball, the combined system will also fall faster. This result is, of course, unacceptable: The combined system cannot fall *both slower and faster*. As a consequence, the Aristotelian theory must be rejected. Galileo replaces it with his own law according to which “large and small bodies move with the same speed” (Galilei 1954, p. 64).

There can be no doubt that Galileo’s thought experiment is a sparkling example of scientific reasoning. But, as it stands, it is not without its problems. First, even if the thought experiment is strong enough to refute the Aristotelian theory, this does not automatically establish the truth of Galileo’s law. This is because the negation of the Aristotelian theory is obviously not equivalent to the assertion that all bodies fall with the same speed. Second, Galileo’s law stands in apparent conflict with the behaviour of countless actual bodies such as rapidly falling cannon balls and slowly descending feathers. Hence, Galileo needs to offer evidence in support of the view that (a) the observable differences between falling bodies are caused by mere accidents and that (b) heavy and light bodies would fall alike if all causal

⁷ As mentioned, this argument is not explicitly formulated by Ihde. However, he alludes to it in several places, for instance when he notes that Galileo not only “needed a telescope, but [...] also an inclined plane and a swaying chandelier in the Pisa Cathedral” (Ihde 2011, p. 78).

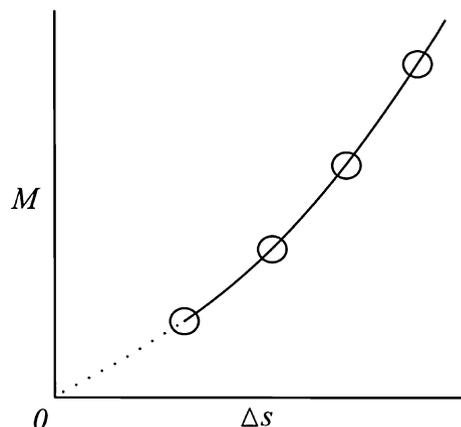
impediments and accidents were put aside. This, however, is not an easy task, for Galileo lacks the technological means to provide the conditions under which the truth of his law could be observationally confirmed. Galileo makes up for this deficiency with a series of tightly connected actual experiments to which we will now turn (cf. Galilei 1954, pp. 62–83; Clavelin 1978, pp. 328–333; Gaukroger 1978, pp. 210–218; Garrison 1986).

The general idea behind these experiments is to investigate the behaviour of “bodies of different weight [when] placed in media of different resistances” (Galilei 1954, p. 68). Consider, first, a ball of gold and a ball of lead, both released in a very dense medium such as quicksilver. As Galileo observes, the difference in speed is dramatic: “[G]old not merely sinks to the bottom more rapidly than lead but it is the only substance that will descend at all; all other metals and stones rise to the surface and float.” (Galilei 1954, pp. 71–72) Consider, second, the case in which the same bodies are placed in a less resistant medium such as water. Since now both bodies descend, the speed difference is much less significant than in the first case. Consider, finally, what happens in air, the least resistant medium available under normal circumstances: According to Galileo, “the variation of speed [...] is so slight that in a fall of 100 cubits a ball of gold would surely not outstrip one of copper [or lead] by as much as four fingers” (Galilei 1954, p. 72). It is on the basis of these results that Galileo draws the following conclusion:

[I]f we find as a fact that the variation of speed among bodies of different specific gravities is less and less according as the medium becomes more and more yielding, and if finally in a medium of extreme tenuity, though not a perfect vacuum, we find that [...] the difference in speed is very small and almost inappreciable, then we are justified in believing [...] that in a vacuum all bodies would fall with the same speed. (Galilei 1954, p. 72)

Galileo’s demonstration can be further explicated by means of a diagram (cf. Fig. 2 in which M represents the resistance of the medium and Δs the difference in speed). What Galileo argues for is that all bodies would fall with the same rate of

Fig. 2 Galileo’s analysis of free fall



speed if the only force acting on these bodies was gravitational pull. In order to establish this, Galileo devises a series of experiments in which one factor (the weight difference between two bodies) is held constant, while another (the density of the medium) is gradually varied. The first stage of the experiment is to place two bodies of different specific weights in a very resistant medium such as quicksilver. In Fig. 2, the result is depicted by the point on the farthest right. The next step is to vary the experimental conditions by releasing the same two bodies in less resistant media such as water with decreasing levels of salinity. In Fig. 2, the results are depicted by the subsequent two points. At the final stage, the experimental conditions are varied even further by placing the two bodies in the least resistant medium available to Galileo. The point on the farthest left represents the result of this iteration.

The reconstruction up to this point nicely illustrates that—and to which extent—Galileo’s scientific practice is indeed technologically embodied by experiments. As the solid line in Fig. 2 indicates, Galileo interpolates the available data so as to establish a functional relationship between speed differences on the one hand and the density of media on the other. However, as valuable as this insight is, it is still not sufficient for Galileo’s purposes. This is because the facts described by Galileo’s law would only obtain in the absence of any factor other than gravitational pull. Hence, since such conditions cannot be experimentally realized, Galileo is forced to *extrapolate* beyond all possible experience in order to advance an *ideal limiting case* in which his theory would be literally—and not just approximately—true. Of course, the direction of Galileo’s extrapolation (which is depicted by the dotted line) is prefigured by experimentally obtained experiences. But this does not change the fact that Galileo’s reasoning involves a leap from the observable behaviour of actual falling bodies to an ideal situation which is found nowhere in the domain of intuitable things.

The case study I have discussed in this section shows two things. First, there is no point in denying that parts of Galilean science are indeed embodied technologically through experiments. It is by virtue of experimentally constructed situations that Galileo is able to gather evidence that would be hard to obtain otherwise. As we have seen, this evidence is crucial because it suggests that speed differences decrease with decreasing density of the medium. However, and this is the second thing that needs to be stressed, Galileo does not stop here. A closer look at his scientific practice reveals that the experimentally obtained data merely function as a springboard for the stipulation of an ideal limiting case in which his law of falling bodies would actually hold. Like so many other limiting cases in physics, the ideal circumstances envisioned by Galileo are physically unrealizable. Since Galileo stipulates ideal vacuum conditions,⁸ even modern vacuum chambers do no more than to approximate this limit.

⁸ Galileo explicitly refers to a “space entirely void of air—and of every other body, however thin and yielding”. Hence, from a contemporary perspective, Galileo not only stipulates a “classical vacuum”, i.e. a portion of space bereft of air. Rather, Galileo’s vacuum is a portion of space in which not even an electromagnetic field (which is built up of photons) is present (Braam 1991). I am relying on Ernan McMullin’s translation (1985, p. 267) here because it captures the meaning of the original better than Crew’s and de Salvio’s translation. The original passage reads as follows: “[...] e perché solo uno spazio

As I have emphasized in Sect. 2, what Husserl criticizes in the *Crisis* is not Galileo's use of abstract models and ideal limiting cases, but how he interprets them. We are now in a position to make this claim more concrete. Galilean science is driven by the ideal that all physical occurrences can be explained and predicted with a relatively small set of fundamental laws. However, as Galileo is well aware, the main obstacle to the realization of this ideal is that even the most mundane physical occurrences are too complex to be subsumed under fundamental laws in a straightforward manner. Hence, the first step of Galileo's analysis is to replace the messiness of the Lifeworld with the tidiness of idealized models and ideal limiting cases. It is these models and limiting cases that are the main targets of Galileo's theorizing.

It is important to note that, as far as this description goes, there is nothing objectionable about Galileo's approach. In essence, it is the way in which physics is still done today: Suppose we want to predict the speed of a skier who is about to race down a slope. According to classical mechanics, the final speed is determined by all forces to which the skier is subjected during her ride. However, as true as this may be, it is not very helpful from a practical point of view. Since the skier is subjected to an extremely large number of forces—from the gravitational pull of any matter within her past light cone to the slightest bump she is experiencing during her ride—the attempt to apply the theory directly is a hopeless endeavour. A much more sensible approach is to get a first provisional grip on the problem by replacing the actual target system with an idealized model. For instance, we idealize the slope to become a perfectly flat and frictionless plane, and we ignore all other factors except the strength of the gravitational field and the initial height of the plane. Of course, since we are now working with an ideal limiting case (and not just with a simplified representation of the target system), we cannot expect our calculations to coincide with the net force to which the skier is actually subjected. But we know that we can improve our predictions by reintroducing coefficients with mathematically convenient forms for neglected forces such as friction or air drag.

The point of the previous paragraph is that the use of abstract models and ideal limiting cases can be justified on purely pragmatic grounds. This, however, is not how Galileo thinks about the matter. As we have seen earlier, the “father of modern science” argues for the metaphysically much more demanding position according to which the method of idealization is actually truth-producing. Hence, on this view, ideal limiting cases not only serve the pragmatic purpose of giving a first handle on a computationally complex problem. In some cases, and under certain circumstances, ideal limiting cases allow us to catch a glimpse on the “real world” of pure, accident-free phenomena. Hence, when Galileo extrapolates beyond all experience to advance the ideal limiting case in which we could observe the pure and unadulterated effects of gravity, he not only takes this counterfactual scenario to be an approachable limit of reality. He also takes this ideal limiting case to be a truthful

Footnote 8 continued

del tutto voto d'aria e di ogni altro corpo, ancor che tenue e cedente, sarebbe atto a sensatamente mostrarci quello che ricerchiamo [...].” (*Opere* 8, p. 117) I would like to thank Simone De Angelis for helping me with the translation of the original text.

description of the phenomenon of free fall, i.e. of free fall as it would really look if all accidents and causal impediments were put aside. It is at this point of Galileo's analysis that the theory becomes prescriptive for experience: Actual instances of falling bodies are now seen as mere approximations to the ideal case which is found nowhere in nature. And this has severe consequences for our understanding of the science/Lifeworld-relation: On Galileo's view, the real world is not the world that we constantly experience, but a curious netherworld that remains hidden behind the veil of accidents. It is this objectivist construal of Galileo's methods—and not the methods themselves—that Husserl attacks so vehemently in the *Crisis*.

Let us now return to Ihde. A modified version of Ihde's argument says that a radical break between science and Lifeworld never occurred because Galileo's scientific practice was embodied technologically through his experiments. If correct, this argument shows that the main problem in the *Crisis*—the increasing estrangement between Lifeworld and science that, according to Husserl, originated with Galileo—is in reality a self-fabricated pseudo-problem. However, as I have tried to show, Ihde's modified argument fares no better than the original one. The reason is that, although it is correct that much of Galilean science is embodied through experiments, the argument overlooks that the decisive move in Galileo's reasoning is the leap from experimentally obtained data to an ideal limiting case which is found nowhere in the domain of intuitable things. Taken by itself, there is nothing objectionable about this. Looking at how physics is done, it is a practical necessity to introduce idealized models in order to make computationally complex problems more tractable. Scientific idealization only becomes a problem if abstract models and ideal limiting cases are taken to be our best shot at getting in touch with the "real world" that allegedly exists "behind" the Lifeworld of directly experienceable things. If we understand idealization in this way, then, on Husserl's view, we are making the mistake to "take for *true being* what is actually a *method*—a method which is designed for the purpose of progressively improving, *in infinitum*, through 'scientific' predictions, those rough predictions which are the only ones originally possible within the sphere of what is actually experienced and experienceable in the lifeworld" (Husserl 1970, pp. 51–52).

6 Concluding Remarks

Even though the primary objective of this paper was to draw attention to several shortcomings of Ihde's criticism of Husserl, I also hope to have shed some new light on the crucial §9 of the *Crisis*. On my view, Husserl is successful in identifying a turning point in the history of the physical sciences. It is, as Husserl points out, with Galileo that models and ideal limiting cases become key components of the methodological toolbox in physics. And while it is beyond question that this innovation proved to be immensely effective, Husserl is among the first to realize that idealization and model building pose a serious challenge to our philosophical understanding of the Lifeworld/science-relation. The question, in a nutshell, is this: Should we interpret scientific models realistically, i.e. as truthful descriptions of a presumed reality that is inaccessible through acts of direct, immediate perception?

Or should we restrict our realistic commitments to the empirical substructures of models and remain agnostic about the rest, thus favouring a pragmatic interpretation of our modelling practices? The lesson to be learned from Husserl's foray into science history is that only the second line of argument prevents the problem of objectivism from arising. And although our modelling techniques have become increasingly sophisticated in the past four centuries, this lesson is, I believe, no less valid today.⁹

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References

- Albert, D. (2013). Wave function realism. In A. Ney & D. Albert (Eds.), *The wave function. Essays on the metaphysics of quantum mechanics* (pp. 52–57). Oxford: Oxford University Press.
- Biagioli, M. (2006). *Galileo's instruments of credit. Telescopes, images, secrecy*. Chicago: University of Chicago Press.
- Braam, P. (1991). How empty is the vacuum? In S. Saunders & H. Brown (Eds.), *The philosophy of vacuum* (pp. 279–285). Oxford: Clarendon Press.
- Carr, D. (1970). Husserl's problematic concept of the life-world. *American Philosophical Quarterly*, 7(4), 331–339.
- Cartwright, N. (1983). *How the laws of physics lie*. Oxford: Clarendon Press.
- Clavelin, M. (1978). *The natural philosophy of Galileo*. Cambridge: MIT Press.
- Cushing, J. (1998). *Philosophical concepts in physics. The historical relation between philosophy and scientific theories*. Cambridge: Cambridge University Press.
- Derrida, J. (1989). *Edmund Husserl's Origin of Geometry: An introduction* J. Leavey (Trans.). Lincoln: University of Nebraska Press.
- Drake, S. (1973). Galileo's experimental confirmation of horizontal inertia: Unpublished Manuscripts. *Isis*, 64/3, 290–305.
- Drummond, J. (1984). The perceptual role of geometric idealizations. *The Review of Metaphysics*, 37(4), 785–810.
- Galilei, G. (1890–1909). In A. Favaro *Le Opere di Galileo Galilei. Edizione Nazionale* (Vol. 20). Florence: Barbera.
- Galilei, G. (1954 [1638]). *Dialogues concerning two new sciences* H. Crew & d A. de Salvio (Trans.). New York: Dover.
- Galilei, G. (1967 [1632]): *Dialogue concerning the two chief world systems* S. Drake (Trans.). Berkeley and Los Angeles: University of California Press.
- Galilei, G. (2008). *The essential Galileo* M. Finocchiaro (Ed., Trans.). Indianapolis: Hackett.
- Garrison, J. (1986). Husserl, Galileo, and the processes of idealization. *Synthese*, 66(2), 329–338.
- Gaukroger, S. (1978). *Explanatory structures. A study of concepts of explanation in early physics and philosophy*. Hassocks: Harvester Press.
- Hahn, A. (2002). The pendulum swings again: A mathematical reassessment of Galileo's experiments with inclined planes. *Archive for the History of Exact Sciences*, 56, 339–361.

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- Husserl, E. (1970 [1936]). *The crisis of the european sciences and transcendental phenomenology. An introduction to phenomenological philosophy* D. Carr (Trans.). Evanston: Northwestern University Press.
- Husserl, E. (1983 [1913]). *Ideas pertaining to a pure phenomenology and to a phenomenological philosophy. First book* F. Kersten (Trans.). The Hague: Martinus Nijhoff.
- Ihde, D. (1979). *Technics and praxis*. Dordrecht: Reidel.
- Ihde, D. (1990). *Technology and the lifeworld. From garden to earth*. Bloomington: Indiana University Press.
- Ihde, D. (1991). *Instrumental realism. The interface between philosophy of science and philosophy of technology*. Bloomington: Indiana University Press.
- Ihde, D. (1993). *Postphenomenology: Essays in the postmodern context*. Evanston: Northwestern University Press.
- Ihde, D. (2011). Husserl's Galileo needed a telescope! *Philosophy and Technology*, 24, 69–82.
- Koertge, N. (1977). Galileo and the problem of accidents. *Journal in the History of Ideas*, 28(3), 389–408.
- Koyré, A. (1968). *Metaphysics and measurement. Essays in the scientific revolution*. Cambridge: Harvard University Press.
- Kuhn, T. (1996 [1962]). *The structure of scientific revolutions*. Chicago: University of Chicago Press.
- Machamer, P. (1998). Galileo's machines, his mathematics, and his experiments. In P. Machamer (Ed.), *The cambridge companion to Galileo* (pp. 53–79). Cambridge: Cambridge University Press.
- McAllister, J. (1996). The evidential significance of thought experiments in science. *Studies in History and Philosophy of Science*, 27(2), 233–250.
- McMullin, E. (1985). Galilean idealization. *Studies in the History and Philosophy of Science*, 16(3), 247–273.
- McTighe, T. (1967). Galileo's "Platonism": A reconsideration. In E. McMullin (Ed.), *Galileo. Man of science* (pp. 365–386). New York and London: Basic Books.
- Merleau-Ponty, M. (2002). *Phenomenology of perception* C. Smith (Trans.). London: Routledge.
- Moran, D. (2012). *Husserl's Crisis of the European Sciences and Transcendental Phenomenology: An introduction*. Cambridge: Cambridge University Press.
- Naylor, R. (1976). Real experiment and didactic demonstration. *Isis*, 67(3), 398–419.
- Naylor, R. (1980). Galileo's theory of projectile motion. *Isis*, 71(4), 550–570.
- Naylor, R. (1989). Galileo's experimental discourse. In D. Gooding, T. Pinch, & S. Schaffer (Eds.), *The uses of experiments* (pp. 117–134). Cambridge: Cambridge University Press.
- Palmieri, P. (2003). Mental models in Galileo's early mathematization of nature. *Studies in the History and Philosophy of Science*, 34, 229–264.
- Redondi, P. (1998). From Galileo to Augustine. In P. Machamer (Ed.), *The cambridge companion to Galileo* (pp. 175–210). Cambridge: Cambridge University Press.
- Segre, M. (1991). *In the wake of Galileo*. New Brunswick: Rutgers University Press.
- Sellars, W. (1991 [1963]). Philosophy and the scientific image of man. In *Science, perception and reality* (pp. 1–37). Atascadero: Ridgeview.
- Settle, T. (1961). An experiment in the history of science. *Science. New Series*, 133(3445), 19–23.
- Swerdlow, N. (1998). Galileo's discoveries with the telescope and their evidence for the Copernican theory. In P. Machamer (Ed.), *The cambridge companion to Galileo* (pp. 244–270). Cambridge: Cambridge University Press.
- Tieszen, R. (2005). *Phenomenology, logic, and the philosophy of mathematics*. Cambridge: Cambridge University Press.
- Valleriani, M. (2010). *Galileo engineer*. Dordrecht: Springer.
- Van Helden, A. (2010). Galileo and the telescope. In A. Van Helden, S. Dupré, R. van Gent, & H. Zuidervaat (Eds.), *The origins of the telescope* (pp. 183–202). Amsterdam: KNAW.
- Wiltsche, H. A. (2012). What is wrong with Husserl's scientific anti-realism? *Inquiry*, 55(2), 105–130.
- Wiltsche, H. A. (2016). Science, realism and correlationism. A phenomenological critique of Meillassoux' argument from ancestry. *European Journal for Philosophy*. doi:10.1111/ejop.12159.