Reenacting Galileo's Experiments: Rediscovering the Techniques of Seventeenth-Century Science by Paolo Palmieri

Lewiston/Queenston/Lampeter: The Edwin Mellen Press, 2008. Pp. xiv + 288. ISBN 978–0–7734–5018–9. Cloth $119.95, £74.95

Reviewed by
Steffen Ducheyne
Ghent University
Steffen.Ducheyne@UGent.be

Before I start with my review proper, it is worth mentioning a particular feature of the book under review. Paolo Palmieri's *Reenacting Galileo's Experiments* is more than a monograph on Galileo's science (*scienza*) of motion: in addition to the text of this book, readers are invited to consult the corresponding website of the Experimental History and Philosophy of Science Research Unit at the University of Pittsburgh (www.exphps.org). This website contains a series of videos illustrating some recently performed reconstructions of Galileo's experiments and a 68 page-long report of the team's reenactment of them.[1] These moving images have the potential to be worth more than a thousand printed illustrations.

The structure of the book is straightforward. After a short introduction (3 pages) and the three main chapters—to wit:

Chapter 1. Galileo and Experiment (16 pages),
2. The Puzzle-Box (78 pages), and
3. New Science (92 pages)

—a general conclusion (4 pages) follows. There are three important appendices to the book:

[1] In his review of this book in *Isis*, Joseph C. Pitt [2009] does not mention this unique feature of Palmieri's monograph. Nor does he mention the characteristic 'robustness' [see below] involved in Palmieri's reenactments. The report, which contains the links to some 30 videos, can be downloaded from www.exphps.org/pdfs/projects/Galileo's%20pendulum%20experiments.pdf.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 7 (2010) 59–68

Appendix 1. A discussion of the computer models that Palmieri used in his investigations (25 pages),
2. The reconstructions of Galileo's experiments (35 pages), and
3. Palmieri's translations of some crucial Galilean fragments on pendulums which are based on the original texts in Antonio Favaro's *Edizione Nazionale* (13 pages).

Chapters 2 and 3 contain virtual Galilean dialogues which are based on the writings of Galileo and his contemporaries.[2] The first dialogue, occurring in chapter 2, is between Galileo and his collaborator Benedetto Castelli; the second and third, in chapter 3, between Galileo and his pupil Vincenzo Viviani and between Viviani and Evangelista Torricelli, respectively.

The aim of this monograph, which becomes clearer as one works through it, is to study 'Galileo's innovative methodology', that is, his 'experimental philosophy' [1]. Additionally, Palmieri wishes to show that 'there is much to learn from reenacting the experimental practices of scientists (typically of a past period)' [3: cf. 193–194] and that

> [w]hile obviously fundamental, textual hermeneutics need not . . . be exclusive, especially when experimentation is invoked in scientific texts of the past. [3]

In what follows, I shall not survey Palmieri's monograph *a capite ad calcem*; rather I shall highlight what I consider to be the merits and possible shortcomings of the book under review.

Chapter 1 serves as a general stage-setting for the problem of Galilean experimentation. Central to *Reenacting Galileo's Experiments* is the so-called 'matching problem', that is, whether Galileo's published accounts are in agreement with his actual experiments. For instance, Galileo's claim about the isochronism of the pendulum has puzzled modern interpreters. Hitherto, Palmieri writes, solutions to the matching problem 'rest on arbitrary, anachronistic assumptions about what constitutes good or bad empirical evidence for a theoretical claim'; and, furthermore, they are understood solely from Galileo's published accounts [8]. In order to remedy this situation,

[2] A genre which Stillman Drake has tried before [1981].

Palmieri has reenacted Galileo's experiments using computer simulations, which are more robust with respect to the repeatability and consistency of outcome over a wider range of parameters that control the experiments. 'Since Galileo does not tell us much about the setup of his experiments', Palmieri notes,

> we face formidable indeterminacies, which may affect our interpretation of the texts to a point that we risk failing to see what Galileo might have seen, and vice versa. To resolve the indeterminacies we need to make the experiments robust over as wide a range of parameters as possible. [9: cf. 195–196, 238]

This I conceive as a major advance in comparison to previous attempts at reenacting Galileo's experiments.

From 1602 and onwards, Galileo claimed—erroneously, as we now know—that the motions of a simple pendulum were isochronous, although he admitted that he had no mechanical proof in support of it.[3] While Ronald Naylor has speculated that Galileo must have relied on 'a wider range of evidence than he indicated in the *Discorsi*' [1974, 23], others have claimed that Galileo 'published some things [i.e., the isochronisms of the circular pendulum] which he knew to be false' [Hill 1994, 513] and that Galileo's claim about the isochronism of the pendulum was 'based more on mathematical deduction than on experimental observation' [MacLachlan 1976, 173]. In appendix 2, Palmieri shows that light pendulums set to swing from modest angles can indeed be isochronous; however, by using heavier pendulums or greater angles, the isochronism of the simple pendulum breaks down—a phenomenon, Palmieri says, Galileo could not have failed to notice himself [37ff]. Galileo's epistemic rule that experience does not teach the causes of things neutralized the problem of discrepancy from isochrony [244].

Palmieri distinguishes between three important stages in the development of Galileo's experimental philosophy, which are fleshed out

[3] See Galileo's letter to Guidobaldo del Monte (Guido Ubaldo dal Monte) on 29 November 1602 [Favaro 1890–1909, 10.97–100; translated on pages 258–260]. See pages 101–122 for Palmieri's discussion of corresponding material from the *Discorsi*.

and contextualized in the next two chapters. According to Palmieri [10–17], these stages are:

Stage 1. Galileo stuck to the epistemic rule that the causes of phenomena are not taught by experience and that they can only be established *via* some form of reasoning.

2. Here Galileo emphasized that the causes of phenomena may be investigated on the basis of patterns of phenomena generated by the variation of an artifact's control parameters—at this stage, causal inference is still guided by reason.
3. Finally, Galileo bracketed, but did not fundamentally reject, the search for causes. Correspondingly, he came to distinguish between causality and inference so that the 'inferentially engageability' of mathematics was separated from causal knowledge.[4]

In chapter 2, Palmieri provides adequate contextualization of the conceptual difficulties that Galileo had to overcome, by surveying the work of Girolamo Borri and Giacomo Zabarella and, more particularly, their attempts at reconciling internal and external causes of motion [24–43]. Thereafter, it is shown convincingly that Galileo's early work on (the causes of) local motion resulted from a generalization of Archimedes' study of floating bodies, that is, by conceiving all local motions as acting along the lines of a balance [43–62]. At the same time, Galileo conceived of mechanical causes as acting in accordance with, rather than in opposition to, nature. Although his early work on local motion was not without tensions [see 52, 62, 79], mathematical deduction and causal inference fitted hand in glove in Galileo's early conception of the study of local motion.

Chapter 3 deals with Galileo's *Discorsi* project, in which he 'bracketed' the search for causes, specifically, the cause of acceleration [125, 140]. Correspondingly, Palmieri shows that the 'Second Day' in Galileo's *Discorsi*, which addresses the resistance to fracture, is based on an empirical rather than a causal principle, that is, the principle of the equilibrium of the balance of different arms [139–150]. Its principles are, therefore, on a par with the non-causal principles introduced in 'Day Three' and 'Day Four', which address local motion and the motions of projectiles respectively.

---

[4] Palmieri warns that these stages are not distinguishable chronologically with precision [11].

Surely, the most intriguing and exciting material surveyed in this monograph is Palmieri's reenactment of several of Galileo's experiments. I recommend that readers study this material in conjunction with the material provided on the website of Pittsburgh's Experimental History and Philosophy of Science Research Unit. In the remainder of this review I shall, however, point to some possible worries for Palmieri's assessment of Galileo's experimental philosophy. More precisely, in what follows, I seek to evaluate his claims about Galilean causation. The idea that Galileo increasingly played down the significance of causal explanation in his later work has been suggested before by Stillman Drake [1981, xxviii] and Pietro Redondi [1998, 185], for example. I shall divide my discussion between two topics: Palmieri's assessment of the role of causation in Galileo's *scienza* and his claims regarding demonstrative *regressus*. In doing so, I allow myself the freedom to refer to some of my own work.

I am sympathetic to Palmieri's approach to Galilean causation. He starts from the premise that instead of focusing on the past traditions from which Galileo's terminology seems to be derived, we should pay more attention to the notion of causation as embedded in Galileo's scientific practice.[5] In this context, Palmieri points to the significance of using artifacts that allow him to vary parameters in a more controlled way [10, 87]. This seems to be related to what I have labeled Galileo's interventionist notion of causation [Ducheyne 2006, 443–444, 452, 458], which first emerged explicitly in his *Discourse on Floating Bodies* (1612). The defining characteristic of causal interventionism is that in order to establish whether A is a cause of B, we need to establish whether deliberate hands-on variations in A result in changes in B. Unfortunately, Palmieri does not go into the details of Galileo's causal interventionism in the *Discourse on Floating Bodies*, which nevertheless contains vital clues on the matter [see Drake 1981, xxvii, 26, 74; Favaro 1890–1909, 4.27, 4.64, 4.89]. Although it is certainly correct that in some parts of the *Discorsi* (1638) Galileo set aside the search for a causal explanation of acceleration, this does not imply that he dispensed with causal explanations entirely. In 'Day Three' of the *Discorsi*, Galileo's spokesman, Salviati, states that

[5] Compare with my own view on the matter in Ducheyne 2006, 448.

> at present it is the purpose of our Author merely to investigate and to demonstrate some of the properties of accelerated motion (whatever the cause of this acceleration may be). [Crew and de Salvo 1954, 167; Favaro 1890–1909, 8.202: cf. Drake 2001, 272; Favaro 1890–1909, 7.260–261]

However, even in the *Discorsi*, his most a-causal work, Galileo introduced and speculated on the causes of certain phenomena. For instance, in 'Day One' of the *Discorsi*, Salviati notes:

> I know for a certainty, that it [i.e., the cause of the cohesion of water] is not owing to any internal tenacity acting between the particles of water; whence it must follow that the cause of this effect is external [*onde resta necessario che la cagione di cotal effetto risegga fuori*]. [Crew and de Salvo 1954, 70; Favaro 1890–1909, 8.115]

Similarly, Salviati says that

> the variation of speed observed in bodies of different specific gravities is not caused by the difference of specific gravity but depends upon external circumstances [*non ne sia altramente causa la diversi gravità, ma che ciò dependa de accidenti esteriori*] and, in particular, upon the resistance of the medium, so that if this is removed all bodies would fall with the same velocity; and this result I deduce mainly from the fact you have just admitted and which is very true, namely, that, in the case of bodies which differ widely in weight, their velocities differ more and more as the spaces traversed increase, something which would not occur if the effect depended upon differences of specific gravity. [Crew and de Salvo 1954, 73; Favaro 1890–1909, 8.118]

Moreover, in 'Day Four' of Galileo's *Dialogo* (1632) causal explanations play a pivotal role [see Ducheyne 2006, 453–459]. The tides were to Galileo's mind a physical proof that the Earth moved. Salviati stresses that in dealing with questions like these, 'a knowledge of the effects is what leads to an investigation and discovery of the causes' [Drake 2001, 484]. Such an investigation may lead to the true, primary, and universal causes of the effects we observe [Drake 2001, 485, 533; Favaro 1890–1909, 7.444, 7.485]. Galileo constructed a mechanical model—alas, the details have been lost—on the basis of which he sought to demonstrate that the tides are caused by a combination of the Earth's annual motion from west to east and its diurnal motion from west to east. The resulting mixed motion is 'the most fundamental and effective cause of the tides, without which they would not take place' [Drake 2001, 497; Favaro 1890–1909, 7.454]. In renouncing competing explanations of the tides, Salviati formulates a positive criterion for a true cause (*vera causa*) of the tides, namely, artificial reproduction:

> But I believe that you have not any stronger indication that the true cause of the tides is one of those incomprehensibles than the mere fact that among all things so far adduced as *verae causae* there is not one which we can duplicate for ourselves by means of appropriate artificial device. For neither by the light of the moon or sun, nor by temperate heat, nor by differences of depth can we ever make water contained in a motionless vessel run to and fro, or rise and fall in but a single place. But if, by simply setting the vessel in motion, I can represent for you without any artifice at all precisely those changes which are perceived in the waters of the sea, why should you reject this cause and take refuge in miracles? [Drake 2001, 489; Favaro 1890–1909, 7.447]

Galileo later adds that

> if it is true that one effect can have only one basic cause, and if between the cause and the effect there is a fixed and constant connection, then whenever a fixed and constant alteration is seen in the effect, there must be a fixed and constant variation in the cause.
>
> *che se è vero che di un effetto una sola sia la cagion primaria, e che tra la causa e l'effetto sia una ferma e costante connessione, necessaria cosa è che qualunque volta si vegga alterazione ferma e costante nell'effetto, ferma e costante alterazioni sia nella causa.* [Drake 2001, 517; Favaro 1890–1909, 7.471]

The material briefly surveyed in this passage seems to suggest, *pace* Palmieri, that, in his later period, Galileo did not exclusively endorse a-causal principles. What this reveals, according to my own judgment, is that the late Galileo relied on causal as well as a-causal principles, depending on the specifics of the context at hand.

When discussing the young Galileo's decision to use the *methodus resolutiva* in order to establish the true cause of acceleration, Palmieri notes:

> The resolutive method has nothing to do with the real process of discovery of the cause of acceleration. So why does Galileo says that he is going to use the method, here and now in the notes, in order to investigate the true cause, not the *unknown* cause? Because this is the first time he has gotten round to putting ideas in writing. This is especially true because the resolutive method starts from the 'given', the objective of inquiry. The objective of inquiry is assumed to be 'given', to be known. We assume that we can grasp it. Take the example of Greek mathematics. If Greek mathematicians eventually publish an analysis, it is because the resolutive method creates suspense in the reader, the illusion that discovery unfolds before the reader's eyes. [72; italics in original]

This quotation is worth giving in full because it brings some of Palmieri's assumptions to the fore. Palmieri assumes that the natural/philosophical analysis or resolution starts from the given, in this case, a cause, just as the mathematical analysis or resolution does. Indeed, the mathematical analysis proceeds from *what is sought*—as if it has been achieved—and by working backwards one arrives at what is *proved or known previously*. However, the natural-philosophical analysis consists in reasoning from *what is known*, an effect, to *what is sought*, its cause. In other words, there is an important asymmetry between mathematical and natural-philosophical analysis [cf. Ducheyne 2005, 219]. As a consequence, Palmieri's criticism is directed at the mathematical analysis, but not at the natural/ philosophical analysis, his true object of criticism. In the accompanying footnote 93 on the same page, Palmieri adds:

> I am at variance with the myth of a logic of discovery, a resolutive method, or analysis, or *regressus*, either in philosophy or mathematics or natural science. I think that some recent historiography (cf. Wallace 1992) has labored under the delusion that such a method, in whatever form, existed, and that it was applied by early modern natural philosophers. None of the scholars embracing this historiography has ever produced a reconstruction of such a method based on the documented

> praxis of early modern natural philosophers. This historiography starts from the prejudice that accounts of methods to be found in the logical literature of the time reflect the praxis of early modern philosophers, and then coerces the historical data into the straightjacket of those accounts. . . .

While I am in agreement with Palmieri that some of the claims on demonstrative *regressus* have been blown out of proportion in the past, I do not think that it follows from this observation that demonstrative *regressus* was of no importance at all to understanding Galileo's scientific work. It can be argued that demonstrative *regressus*, although it will not tell us much about the characteristic innovative aspects of Galileo's *scienza* or about the specific inferences as provided in his scientific practice, was nevertheless important to understanding some general features of Galileo's scientific thinking. That is, it can be argued that, although Galileo surely innovated with respect to the specific procedures by which causes are inferred from their effects, demonstrative *regressus* is still relevant to understanding Galileo's science in so far as he thought that the science proceeds from effects to causes and in so far as he used its terminology.[6] In other words, while the semantics of Galileo's causal talk was definitively innovative, the syntax remained traditional.[7]

I conclude this review with some general remarks. It would have been useful if the material covered in the appendices were incorporated into the main text. Parts of Palmieri's work would have been more precise if more secondary literature had been taken into account, especially when Galileo's intellectual trajectory is concerned. In this way, the reader could have gotten a better sense of the specifics of how Palmieri's account differs from and improves upon previous work. Earlier, I pointed out that the aim of the book becomes clearer whilst working through it. During that process, I came to realize that it is not a monograph on Galileo's experimental philosophy in general—consider the fact that little or nothing is said about the role of abstractions and idealizations in Galileo's experimental *scienza*—but a more specific study of the Galilean matching problem. Despite the reservations listed in the preceding paragraphs, I think that, in the

---

6 This is what, according to my understanding, has been accomplished in Wallace 1992.

7 I have made this case for Newton in Ducheyne 2005.

end, Palmieri has written a fascinating work, which no one seriously interested in Galileo's *scienza* should overlook. This is an exciting book, which, in combination with the corresponding website, offers insight into some of Galileo's experiments and on that account it is to be valued.

BIBLIOGRAPHY

Crew, H. and de Salvo, A. 1954 [1914]. *Dialogues Concerning Two New Sciences.* New York.

Drake, S. 1981. *Cause, Experiment and Science, A Galilean Dialogue Incorporating a New English Translation of Galileo's 'Bodies That Stay atop Water, or Move in It'.* Chicago.

________ 2001 [1953]. *Dialogue Concerning the Two Chief World Systems.* New York.

Ducheyne S. 2005. 'Newton's Training in the Aristotelian Textbook Tradition: From Effects to Causes and Back'. *History of Science* 43:217–237.

________ 2006. 'Galileo's Interventionist Notion of "Cause"'. *Journal of the History of Ideas* 67:443–464.

Favaro, A. 1890–1909. ed. *Le opere di Galileo Galilei, Edizione Nationale.* 20 vols. Florence.

Hill, D. 1994. 'Pendulums and Planes: What Galileo Didn't Publish'. *Nuncius* 9:499–515.

MacLachlan, J. 1976. 'Galileo's Experiments with Pendulums: Real and Imaginary'. *Annals of Science* 33:173–185.

Naylor, R. 1974. 'Galileo's Simple Pendulum'. *Physis* 16:23–46.

Pitt, J. C. 2009. 'Review of Reenacting Galileo's Experiment, Rediscovering the Techniques of Seventeenth-Century Science'. *Isis* 100:661–662.

Redondi, P. 1998. 'From Galileo to Augustine'. Pp. 175–210 in P. K. Machamer ed. *The Cambridge Companion to Galileo.* Cambridge.

Wallace, W. A. 1992. *Galileo's Logic of Discovery and Proof, The Background, Content, and Use of His Appropriated Treatises on Aristotle's* Posterior Analytics. Dordrecht.