



HAL
open science

Was There a Cartesian Experimentalism in Seventeenth Century France?

Sophie Roux

► **To cite this version:**

Sophie Roux. Was There a Cartesian Experimentalism in Seventeenth Century France?. Dobre, Mihnea; Nyden, Tammy. Cartesian Empiricism, Kluwer Academic Publishers, pp.47-88, 2014, 978-9402405804. hal-01508779

HAL Id: hal-01508779

<https://hal.science/hal-01508779>

Submitted on 1 Dec 2022

HAL is a multi-disciplinary open access archive for the deposit and dissemination of scientific research documents, whether they are published or not. The documents may come from teaching and research institutions in France or abroad, or from public or private research centers.

L'archive ouverte pluridisciplinaire **HAL**, est destinée au dépôt et à la diffusion de documents scientifiques de niveau recherche, publiés ou non, émanant des établissements d'enseignement et de recherche français ou étrangers, des laboratoires publics ou privés.

3. Was there a Cartesian Experimentalism in 1660s France?

Sophie Roux

École Normale Supérieure, Paris

Abstract In order to determine if there existed an experimentalist Cartesianism in France in the 1660s, I concentrate on Jacques Rohault, and address in turn the three following questions. 1. Is there a difference in the way Descartes and Rohault deal with experiments? I state that there is no doctrinal difference between them: the experiments they carry out are of the same order; they attribute the same epistemological functions to them; they share the same ontology. The main difference between them is that, unlike Descartes, Rohault made experiments a means of popularization of the Cartesian philosophy. 2. How does Rohault treat experiments in his *Mercredis*? Studying quite closely the evolution that led to the greater priority attributed to experiments in the scientific circles that prefigure the *Académie des sciences*, I show that, in 1660s France, the treatment Rohault give to experiments in his *Mercredis* is exceeded by the radical experimentalism of the other French learned societies. 3. Did this radical experimentalism bring out a transformation of Cartesianism? I establish that, while the first criticism to Descartes concerns his dogmatic pretensions, there emerges in the last thirty years of the seventeenth century what has since become a historiographic cliché, the idea that Cartesians neglected experiments in favor of hypotheses and speculation.

“A true philosophy topic for an essay in the baccalaureate... must deal with a problem and not a doctrine”

“Empiricism is not *the* theory of experience, but *one* possible theory of experience....”

Georges Canguilhem, *Œuvres complètes. Tome 1*, p. 275 and p. 498.

3.1 Introduction*

The title of this volume and the title of this chapter seem to constitute a philosophical paradox and a historiographic provocation. If Descartes was a rationalist, and all those called “Cartesians” were faithful to him, then the existence of Empiricist Cartesians would be a philosophical paradox. If the most remarkable characteristic of modern science has been its commitment to experimentation, or more precisely, the commitment to experimentation that was proper to the Royal Society, then it would be historiographic provocation to speak of an experimentalist Cartesianism.¹ But before going any further, we should indicate what we mean by “experimentalism” and “Cartesianism”: a discussion that failed to set out the scope of these categories would wade through the mire of uncertainty.

The subject of this chapter is not empiricism as such, but what I would call experimentalism. Both empiricism and experimentalism are philosophical categories that apply to doctrines rather than to practices. It's not enough to have experiences or to carry out experiments to be an empiricist or an experimentalist: there must be an explicit doctrine derived from them. But these two categories must in turn be distinguished from each other: empiricism relates to doctrines on the origin of our knowledge, while experimentalism describes doctrines derived from the constitution of the natural sciences. Doctrines are called empiricist when they defend the thesis that holds that all our knowledge absolutely comes from experience, and they are most often opposed to doctrines described as rationalist. Nonetheless, the category of empiricism includes a wide range of various

* Unless otherwise indicated, translations are mine. I thank Mihnea Dobre and Tammy Nyden for their challenging questions and careful editing.

¹ See Chap. 1 by Dobre and Nyden on the history of this issue.

doctrines, in particular depending on what one means by “knowledge” (an idea, a proposition, know-how, etc.) and by “experience” (a dream, a quotidian observation, a laboratory measurement, etc.). Experimentalist doctrines are those that support the thesis that experiments, or a certain kind of experiment, have a certain function in the development of the natural sciences. Here, too, this is a vast category, with associated doctrines varying depending on the experiences taken into account and the function attributed to them. It is common, for example, to oppose immediate sensory observations, experiences mediated by instruments, and data produced by sophisticated devices such as computers. It is also common to distinguish the function held by experiments that allow for the development of hypotheses and the function held by experiments that test hypotheses.

Empiricism and experimentalism are distinct categories. It is possible to support a doctrine arising from one of these categories without supporting any doctrine arising from the other category. Clearly one can be an experimentalist without being an empiricist: it’s enough to recognize that certain experiments in the natural sciences have a function without having to support the notion that all our knowledge absolutely arises from experience. This is the case, for example, if one defends the idea that, alongside experiments, mathematic reasoning, whether relying on innate ideas or arising from the construction of concepts in pure intuition, also has a role in the natural sciences. But it does seem that any empiricist, inasmuch as she defends a theory of all knowledge, is an experimentalist: a doctrine of knowledge in general applies in the particular case of knowledge of natural sciences. Nonetheless, we can avoid this conclusion by playing—it’s true, not very fairly—on the many meanings of the words “experiment” or “experience.” For example, we can consider that the senses, commensurate with things, are the source of all knowledge that we have of things, and even go so far as to say that they allow us to know things perfectly, and yet not support the version of experimentalism by which the only experiences worthy of interest are those provided by observation and measuring devices. On the contrary, it is precisely because we consider that our senses can measure things that we can judge that observation and measuring devices are superfluous, or even deceptive.

Although empiricism and experimentalism are categories that can include a wide range of doctrines, they are still fairly easy to define. But what about Cartesianism? We could adopt a strong definition of Cartesianism, according to which to be a Cartesian, you must support the same set of theses as Descartes. But if so, either this set of theses coincides exactly with the set of theses found in the Cartesian corpus, in which case the only Cartesians are those like Pierre Ménard, who rewrote *Quixote* word for word, or else this set of theses is a subset of theses one more or less explicitly favors. In these conditions it is not surprising that certain historians fall back on a weaker definition of Cartesianism by which any author influenced by Descartes is a Cartesian. Thus, almost every author from the second half of the seventeenth century becomes one way or another a Cartesian, for they all had read Descartes. The situation quickly becomes aporetic, and it must be said, irremediably, for they all did not read the same works by Descartes, nor did they understand them in the same way. We consistently fail to achieve a view from nowhere of Cartesianism, and even more to provide an essential definition. To escape this aporia, I have proposed in other articles what I call a “polemic conception of Cartesianism.”²

To summarize, and to simplify, the historian who has an essentialist conception of Cartesianism is seeking an essential definition of this category. The historian who has a polemic conception of Cartesianism, while admitting from the outset that this definition does not really exist, attempts to tease out the polemics and controversies in which a Cartesian configuration can at some point be seen. While the essentialist historian studies works to evaluate their faithfulness to the works of Descartes and rate them according to their distance from them, the polemic historian pays particular attention to the controversies in which Cartesians and anti-Cartesians explicitly opposed each other, as well as to the internal quarrels in which Cartesians tried to define what constituted their identity, for even those who claimed to be Cartesians were not in agreement as to what this claim implied. The essentialist wants to isolate the intrinsic meanings of philosophemes, while the polemicist takes into account their historical variations.³

A certain historical relativism goes with the polemic conception of Cartesianism: even if one limits oneself to France, 1670s Cartesianism is not the same as 1750s Cartesianism, and that of the 1840s is not that of the 1930s.⁴ This historical relativism does not however lead to an absolute nominalism in which each text would embody its own conception of Cartesianism. Rather, there are particularly significant moments in the history of philosophy in which a particular conception of Cartesianism succeeded in becoming stable beyond the particular moment of its inception. As I have shown in one of my papers illustrating the polemical conception of Cartesianism, such a

² Roux 2012, 2013.

³ For a study of a polemic among Cartesians, see Moreau 1999. For a study of a controversy between proponents and opponents of Cartesianism, see Roux 2012. In the present chapter, I’ll return to a point briefly touched on in this article, that is the way in which the Cartesians appear opposed to experimentalism in this controversy, see Roux 2012, 84–87. The present chapter uses some of the ideas present here and there in Roux 1998.

⁴ Azouvi 2002.

case emerges from the controversy between the “new philosophers” and the “old philosophers” that occurs in France in the years 1670–1690. It is essential for the polemical historian to note this type of moment.

If a category like that of Cartesianism is relative to the historical status of the perpetual battlefield that is philosophy, one can wonder if the same is not true for the category of experimentalism. In a word, it seems to me that despite the formal similarity of these “-isms,” there is less historical variability in the case of the category of experimentalism than in the case of Cartesianism, but that it still exists, at least in the manner we implement in the history of philosophy. Historical variability is less strong in the case of experimentalism than in the case of Cartesianism: while experimentalism can be defined outside of history, as I did in the beginning, Cartesianism cannot be defined without arbitrary choices, as I have just recalled. Yet historical variations exist: in order for Jeanne, a historian of philosophy, to affirm that philosopher Suzanne was an experimentalist, Suzanne’s doctrine must correspond to the general definition that Jeanne gave to what it means to be an experimentalist, but in addition, Jeanne must feel it relevant to characterize Suzanne as an experimentalist. But it seems to me that this relevance is relative: it is relevant to say that Suzanne is an experimentalist rather than a feminist, it is relevant to say the Suzanne is an experimentalist rather than saying that Mathilde is an experimentalist. In other terms, while one can provide an absolute definition of experimentalism, it will be more or less absolute depending on to what this experimentalism is compared.

By recapitulating what I’ve stated so far, I can provide details on what I will deal with in this chapter. On one hand, I have distinguished experimentalism and empiricism: in this chapter, I’ll speak only of experimentalism, and not of empiricism.⁵ On the other hand, Cartesianism, and to a certain extent experimentalism, are relative categories: I will have to pay particularly attention to the point of view I adopt to judge the experimentalism of this or that philosopher. These premises stated, the problem is to determine if, as the title of this chapter suggests, there exists an experimentalist Cartesianism in France in the 1660s. I hope to resolve this problem by discussing a particular case, that of Jacques Rohault (1618–1672), the French Cartesian the most often cited for his experimental commitment and his experimentalist doctrine.⁶ If I succeed in showing that this Cartesian given as an example of an experimentalist was not truly one, or in any case, not in an exceptional fashion, then it seems likely that in 1660s France there is no experimentalist Cartesianism, or at the very least, that describing Cartesianism as experimentalist is not the most relevant description. As one can see, inasmuch as relevance is relative, my discussion will not deal with Rohault alone, but with Rohault as compared with other philosophers. More precisely, I’ll deal in turn with the three following questions:

1. Is there a difference in the way, according to their texts, Descartes and Rohault treat experiments? I will state that there is no doctrinal difference between them: the experiments they carry out are qualitatively and quantitatively of the same order; they attribute the same epistemological functions to experiments; they share the same ontology. The main difference between them is that, Rohault, unlike Descartes, seems to have made experiments a means of communication (popularization or propaganda) of the Cartesian philosophy.
2. How can we consider the way in which Rohault treats experiments in his *Mercredis*? It is no longer a matter of comparing Rohault to Descartes, but rather of situating him in the field of natural philosophy of his day. My thesis is that, in 1660s France, the treatment Rohault give to experiments in his *Mercredis* is exceeded by the practices of the other learned societies. To establish this thesis, I will study quite closely the evolution that led to the greater priority attributed to experiments in the scientific circles that prefigure the *Académie des sciences*.
3. Did this break bring out a transformation of the category of Cartesianism? My response to this question is affirmative. While the criticism of Samuel Sorbière (1615–1670) and Jean Chapelain (1595–1674) with respect to Descartes was based on his dogmatic pretensions, there emerges in the last thirty years of the seventeenth century, among all sorts of adversaries of Cartesianism, what has since become a historiographic

⁵ For some comments on the refusal of empiricism among most Cartesians, see Clarke 1989, 43–70. There were however some exceptions, for example Dom Robert Desgabets, Henricus Regius or Pierre-Sylvain Régis. On Desgabets and Régis, see Schmaltz 2002; on Regius, see Chap. 7 by Bellis; on Desgabets, see Chap. 8 by Easton.

⁶ Already at the end of the seventeenth century, Rohault was the Cartesian who could be saved as an experimenter and experimentalist; in this regard see Leibniz to Nicaise published in *Journal des savants* cited below n131. See also Savérien 1783, xxviii–xxx, lv–lvi; Mouy 1934; Blay, “Introduction,” in Rohault 2009, xxix; Chap. 9 by Dobre in this volume. Clarke 1989, 202–211, proposes a more nuanced, and in my opinion more exact, discussion, if only because coming from a systematic comparison of Malebranche and Rohault, he gives a relative appreciation of Rohault’s experimentalism. The different articles by Trevor McClaughlin devoted to Rohault (in particular McClaughlin 1977, 1996, 2000) must be read, but aside from the fact that they repeat themselves, they do not in my opinion go into enough details of the texts.

cliché, the idea that Descartes and the Cartesians neglected experiments in favor of hypotheses and speculation.

3.2 Descartes and Rohault

Historians who want to characterize the Scientific Revolution in terms of scientific experiments must respond to a massive objection: there were plenty of experiments before this period. To overcome this objection, they must define what is special about experiments during the Scientific Revolution. They have held that these experiments were special because they allowed for testing a hypothesis in an interventionist fashion, were founded on the measurement of quantities, were proper experiments in the Baconian sciences, were singular experiments attested to by detailed reports by trustworthy witnesses, or supposed the creation of some form of scientific community.⁷

Whatever type of experiments that one may claim to be proper to the Scientific Revolution, we can find them in the Cartesian corpus. In his first writings, Descartes attacks “philosophers who neglect experiments and believe that truth must come from their own brain, like Minerva from the head of Jupiter,” and pays homage to Francis Bacon, a fairly strong one, given his reticence to recognize any interest there might be in reading an author other than himself.⁸ The reader of the *Discourse on Method* is invited to go see with his own eyes the dissection of a heart, and the experiments by which Descartes, largely inspired by Harvey, intends to show the circulation of blood are manifestly implementing a hypothetico-deductive scheme.⁹ The explanation of the origin of the rainbow found in the *Meteors* supposes the use of a device that is, if not complex, at least carefully designed (a prism, a glass flask) and that requires delicate measurement of angles.¹⁰ His meticulous and detailed observation of sleet and snowflakes in Amsterdam in the evening of February 4, 1635 compares well to the texts of Robert Boyle, which have been described as perfect examples of a style proper to the Royal Society.¹¹ Lastly, certain passages show that Descartes was well aware of the material conditions that must be met for a scientific community to be able to work effectively. I’m thinking neither of the project for an Academy written for the Queen of Sweden, which in the end only assigns turns to speak, nor to the plan to establish within the Royal College rooms, professors, and celebrations aimed specifically to the various types of trades.¹² I’m thinking instead of the care given by Descartes at times to the way in which a community of observers must be set up, for example, when the goal is to compare barometric measurements in various places and times, and that to do so, he sent Mersenne a duplicate of the graduated scale he used himself.¹³ To be honest, the only experiments we don’t find in Descartes are those carried out by a community of scientists assigned to establish facts.¹⁴ And it still remains to be proven that this practice actually existed other than as an ideal, and that it was indeed important in the Scientific Revolution, whatever one means by that.

If Descartes is not considered to leave room for experiments, it is not because of his experimental practice described in his most scientific writings. Rather, it is because of the doctrine of experience he supports in his more philosophical writings. This doctrine is opposed to empiricism, and is only moderately experimentalist. On one hand, with regard to empiricism, the *Meditations* create a distance from sensory experiences, not because they are strictly speaking false, but because reason must decipher them to gather objective physical meaning.¹⁵ On the other hand, and more important with regard to experimentalism, if the question of knowing the place of experimentation in Cartesian science has always been a topic of discussion, we can nonetheless state that

⁷ See for example Koyré 1953; Kuhn 1976; Shapin and Schaffer 1985; Dear 1995.

⁸ *Regulae ad directionem ingenii*, Regula V, in AT X 380. References to Bacon are to be found in Descartes to Mersenne, January 1630, 23 December 1630, 10 May 1632, in AT I 109, 195–196, 251.

⁹ *Discours de la méthode*, Cinquième Partie, in AT VI 46–55. For comments, see Des Chene 2001, 19–25.

¹⁰ *Météores*, Discours Huitième, in AT VI 325–344. For comments, see Garber 2001a, 94–104; Zittel 2009, 202–206, *passim*.

¹¹ *Météores*, Discours Sixième, in AT VI 298–308. For comments, see Zittel 2009, 219–225. For the idea that such reports are typical for the Royal Society, see Shapin and Schaffer 1985, 60–65; Dear 1995, *passim*.

¹² These two projects, published in AT XI 659–660 and 663–665, were known to Baillet 1691, II, 433–434 and 663–665.

¹³ Descartes to Mersenne, 13 December 1647, in AT V 99.

¹⁴ On this point, see Garber 2001b, who holds that this is true for all natural philosophers prior to the Royal Society.

¹⁵ I developed this point in Roux 2011, 178–180.

Descartes attributes to experience and experiments a function that is not negligible, but one that is nonetheless secondary.¹⁶

As indicated by texts cited countless times, experiments have an important epistemological function for Descartes: they allow us to identify the way God chose to produce a given phenomenon from among all the manners possible; and similarly to choose, among all the possible manners we have to conceive a way to explain the phenomenon, the explanation that corresponds to the actual created world.¹⁷ But this epistemological function is secondary, in two ways. First, Descartes feels that the true work of physics is not to carry out experiments to establish facts, but rather to explain the facts according to a few general principles. When mathematical physics intervenes, as for the rainbow, at least part of this explanation can take the deductive form in the sense that we understand it today. But in all other cases, and there are many of them in Cartesian physics, the explanation relies on exhibiting a hypothetical causal chain of motions of corpuscles. This is of course the case of the rainbow itself, when the issue is explaining the nature of colors. In these conditions, it's not surprising that, in a secondary position, there are certain fundamental propositions that experience and experiments cannot refute, that is, propositions that establish the ontological paradigm according to which all phenomena can and must be explained in terms of motions of corpuscles. This is the case for the proposition that the essence of matter is its extension, or for the three laws of motion that determine all phenomena of nature and determine them as phenomena of nature. It is only after this paradigm was established, according to Descartes once and for all, and in a totally demonstrative manner, that experiments are used to allow for a choice among several possible causal chains.

Are things any different for Rohault? This has been the position often defended, as if though, failing to totally save Descartes, it was important to at least preserve one of his disciples. In this light, while Descartes may not have been totally modern, from Descartes to Rohault a great step would be taken to a more substantial experimentalism and to a more clearly accepted modernity. Contrary to Descartes, Rohault would have been able, in his *System of Natural Philosophy*, to recognize the necessary alliance between experience and reasoning, distinguishing three forms of experience, and recognizing the importance of hypothetico-deductive reasoning in physics. Indeed, Rohault does note that there exist two symmetrical errors that are among the causes of the lack of progress in physics, the first being inattention to experiments, the second being exclusive promotion of experiments, to the detriment of reasoning:

For they who fall into the first of these Errors, hinder themselves of the best Means of finding out new Discoveries, and of confirming their own Arguments [*raisonnements*] likewise; And they who fall into the second, by depriving themselves of the Liberty of drawing Conclusions, hinder the Knowledge of a large Train of Truths, which may many Times be deduced from one single Experiment. Wherefore he can not but be very advantageous to mix Experiments and Arguments [*raisonnement*] together.¹⁸

Indeed again, Rohault notes, as would any good commentator systematizing the remarks of his master, that, alongside the sensory observations we all make without particular intent, and the specific knowledge that men of the field acquire by experimental practice, there is a third type of experiment,

those which are made in Consequence of some Reasoning [*celles que le raisonnement previent*] in order to justify whether it was just or not. As when after having considered the ordinary Effects of any particular subject, and formed a true Idea of the Nature of it, that is of That in it which makes it capable of producing those Effects; we come to know by our reasoning, that if what we believe concerning the Nature of it be true, it must necessarily be, that, by disposing it after a certain Manner, a new effect will be produced, which we did not before think of; and in Order to see if this Reasoning holds good, we dispose the Subject in such a manner as we believe it ought to be disposed in Order to produce such an Effect.¹⁹

But just saying that one needs experiments and reasoning does not really make for a substantial thesis in epistemology: as Rohault himself recalls a page later, this was also the position of Aristotle. And the application made by Rohault of the hypothetico-deductive scheme is very special. If one judges from the ordinary effects of a "subject," "the true Idea of the Nature," nature cannot be a hypothesis that could possibly be refuted by a later experiment. The aim in fact is merely to reformulate rough sensory experience in terms of the Cartesian

¹⁶ The bibliography is large but useless, because it is very repetitive. The discussions that are the most reliable, because they are more nuanced, although not exactly in the same way, seem to me those of Clarke 1982 and Garber 2001a. Homage must also be paid to the studies "Descartes expérimentateur" and "Descartes et Bacon" published in Milhaud 1921.

¹⁷ *Discours de la méthode*, Discours Sixième, AT VI 64–65; *Principia philosophiae*, II 204, AT VIII 327.

¹⁸ Rohault 1987, The Author Preface, I, unpaginated.

¹⁹ Rohault 1987, The Author Preface, I, unpaginated; 1681, Préface, unpaginated. The verb "prévenir" used transitively did not have the same meaning in the seventeenth century as it does today: "prévenir," according to Furetière's *Dictionnaire*, is "to be the first to do the same thing, to win in races; *celui qui prévient* arrives the first at the goal, wins the prize."

ontology. To see this, one can look, for example, at chapter 12 of the first part of the *System of Natural Philosophy*, devoted to explaining the effects that Aristotelians would attribute to *horror vacui* by the weight of air and the presence of a subtle matter.²⁰

This is certainly a chapter rich in experiments, experiments with syringes, siphons, glass tubes, an experiment with a carp bladder, the experiment known as the vacuum in the vacuum, the Puy-de-Dôme experiment, this time carried out in the clock towers of Notre-Dame.²¹ It is not important that Rohault was not the first to imagine or carry out these experiments; the real problem, in fact, is in the explanations he uses.²² For him, explaining means to expose which of the general principles of Cartesian physics are compatible with these experiments (e.g., a body can only be placed in motion by a body that touches it; all motion is in circles, that is to say a closed curve; vacuums are impossible; there is a subtle matter; the existence of pores allows for its circulation, etc.). Rohault presents these physical principles as “a Foundation which cannot be contested.”²³ These uncontested foundations are the very same principles of general physics of Descartes. It is even, so to say, their epistemological characterization that they are uncontested: in particular, they cannot be refuted by an experiment.

Consider, for example, the analysis given of the operation of a syringe. It is obvious, writes Rohault, that because one end is open, the piston cannot be drawn back without a circular motion of air. This is obvious because of the general principle that in a full world, all motion is circular. But what if the end is closed? Either the syringe has pores, and the motion will take place, or the syringe does not have pores and there will be no motion. And since motion does take place, there must therefore be pores in the glass of the syringe.²⁴ The hypothetico-deductive process corresponding to the third type of experiment in the Preface is present, but under the general supposition that the world is full. Thus the epistemological framework used by Rohault is in fact as follows: given, on one hand, the general principles of Cartesian philosophy, and on the other, the fact that the piston of a syringe can be drawn, it means that there are pores in the glass. The experiment intervenes not to allow for a choice between the Cartesian ontology and another. Instead, the general principles of this ontology being given, it is simply about allowing a choice between two of their possible instances. This is precisely the epistemological situation described by Descartes when he writes that experiments allow us to choose an explanation among the various explanations possible for a phenomenon.²⁵

One could object that this kind of situation is common in physics: far from the basic ontological choices being questioned by experiments, these choices must be considered as given in order to formulate theories, among which the experiment will allow a choice to be made. This is a legitimate objection. But even a theory that has in the background predetermined ontological principles must offer something new with respect to the experimental situation that it is supposed to explain, for example, by allowing for quantitative predictions for what will be the case in analogous experimental situations. What happens in Rohault’s physics is simply a translation or a change of language: to the description of experiment in terms of objects perceived by the senses is added the description of a matter that would by its properties be able to produce the experiment that is perceived by the senses. The problem of the translation proposed by Rohault is that the second description offers nothing more than the first. If we continue to read this chapter, we will reach the presentation of the weight of air: it is not an experimental demonstration, as it was for Torricelli or Pascal, by the prediction of the height reached by mercury in a tube, or the heights it would reach at different altitudes. Rather, it is deduced from the principle that a body can only be moved by a body touching it: as air is the only body touching the piston, “we must think that it is the Air that causes this surprizing Motion; for, considering that the Air always contains in it a great Quantity of the Particles of Water, and other terrestrial Bodies;...we shall...assert; that the grosser Air is heavy, and consequently that by

²⁰ -See for example McClaughlin 2000, 336, n52. In the following paragraph, I detail the comments presented in Roux 2011, 128–134.

²¹ Rohault 1987, I, Chap. XII, 56–78.

²² McClaughlin 1977, 227–228 and McClaughlin 1996, 471–475, 480–481 identify the various sources of Rohault’s experiments. We sometimes read that Rohault helped Florin Périer edit Pascal 1663, but I don’t see an argument for this. Nonetheless, the two remarks that make up the *Avertissement*, unpaginated, of this edition show that the editor knew the work of Rohault; likewise the presentation entitled *Nouvelles expériences faites en Angleterre, expliquées par les principes établis dans les deux Traitez precedens de l’Equilibre des Liqueurs, & de la Pesanteur de la masse de l’Air*, shows that he knew the work of Boyle.

²³ Rohault 1987, I, Chap. XII, Sect. 5, 57: 1681, I, 80 for the French.

²⁴ Rohault 1987, I, Chap. XII, Sect. 6–9, 57–58. This very chapter is analysed in this volume by Mihnea Dobre as well, albeit with quite different conclusions. See Chap. 9 by Dobre.

²⁵ See the texts whose references are given above, n16.

its weight, the Sucker is forced into the Syringe.”²⁶ There is absolutely no quantitative prediction that could potentially invalidate a hypothesis, but rather the consequences of the principle that a body can be moved only by a body touching it. Thus, contrary to what the Preface affirms, and also to what Rohault’s mentor Claude Clerselier (1614–1684) will claim, reasoning cannot “prévenir,” that is to say anticipate, experience and experiments. It does not allow us to anticipate what it will be.²⁷ As we will see towards the end of the second part of this chapter, when we examine Huygens’ judgment of Rohault, it happened that Rohault would refuse to take into account experiments that contradicted what he considered to be established. When we realize that most of the experiments presented in this chapter come from the writings of Pascal, who, in addition to his very firm position on the importance of experiments in physics, had practiced quantitative physics and was opposed to formulating hypotheses on subtle matter, we can see that Rohault presents, with regard to experimentalism, no progress toward a stronger and more substantive doctrine, but rather a regression toward a weaker and less substantial doctrine.

Rohault’s doctrine of experiments does not come from an epistemological broadening that might have left more room for experiments than had Descartes. As we have just shown, Rohault’s epistemology is not only, as has written Trevor McClaughlin, “compatible” with that of Descartes, it is fundamentally identical to it.²⁸ This epistemological identity does not however mean that there are no differences between the *System of Natural Philosophy* and the *Principles of Philosophy*, or between Rohault and Descartes.

First, the *System of Natural Philosophy*, without excluding considerations that arise from metaphysics or general physics, does not always assign them the same function as do the *Principles of Philosophy*. Thus, while Descartes states the idea that God conserves the motion he created, according to Rohault, the conservation of motion is not based on the immutability of the action of God.²⁹ While Rohault, like Descartes, argues that everything we can think of can be done by God (in order to establish the infinite divisibility of matter), he adds that we can see smaller and smaller animals depending on the power of our microscopes, or the division of gold into finer and finer leaves.³⁰ Rohault also often insists that he wants to reason “as a physicist.” Thus he declares on several occasions that, reasoning as a physicist, he does not include the omnipotence of God, by which, of course, the created things could have been different than they are.³¹ Although it is a matter of relatively undecidable questions, I think that this distancing of metaphysics can be read not only in intellectual terms, but in institutional terms as well. In the context of the great battle of Cartesianism, Rohault had adopted a classical strategy to retain a bit of autonomy for physics by sharing tasks between physicists and metaphysicians: by claiming not to enter the domain of the metaphysician, he can expect that the metaphysician will return the favor.

Secondly, Rohault has indubitably set up, with his *Mercredis*, a social scheme with no equivalent in Descartes. Descartes never considered that public experiments would constitute a serious weapon in his great battle against Aristotelians. While he had opened the possibility of a new audience for philosophy, for example when he declared that he wrote the *Discourse on Method* in French so that “even women can understand something,”³² as we see in his correspondence, except for a few princesses, it was in fact primarily written for and commented by professional philosophers, theologians, and professors. Rohault, on the contrary, gave his lectures, according to Clerselier, before “people of all stations and conditions, prelates, abbots, courtesans, doctors, physicians, philosophers, surveyors, regents, schoolboys, provincials, foreigners, artisans, in a word, people of all ages, sex and profession.”³³ Other sources attest to the fact that a certain number of persons of quality, young students (for example Pierre-Sylvain Régis) and women (for example Madame de Bonneveaux or Madame de Guerderville) attended these lectures. It also seems fair to note, as has McClaughlin, that Rohault’s family relations gave him an exceptional familiarity with artisans.³⁴

²⁶ Rohault 1987, I, Chap. XII, Sect. 10, 59. Rohault does mention what happens in the case of mercury in Sect. 23, 64, but absolutely not as a crucial experiment.

²⁷ On the use by Clerselier of the affirmation that the reasonings of Rohault anticipate (“préviennent”) experiments, and the way that Rochon mocks this affirmation, see below in the third part of this chapter.

²⁸ Pace McClaughlin 1996, 478.

²⁹ Rohault 1681, I, Chap. XI, Sect. 5, 71.

³⁰ Rohault 1681, I, Chap. IX, Sect. 9–12, 56–60 and Chap. XXI, Sect. 2–3, 160–161. On the addition of these empirical facts, see Roux 2006, 127, where I note that in the preface, Rohault considers this question as too metaphysical.

³¹ Rohault 1687, I, Chap. v, Sect. 12–13, 34–35; Chap. VI, Sect. 9, 41; Chap. VIII, Sect. 2, 45–46; Chap. IX, Sect. 2, 51 and Sect. 12, 60.

³² Descartes to Vatier, 22 February 1638, in AT I 560.

³³ Clerselier 1682, unpaginated.

³⁴ McClaughlin 1996, 475–476.

Given this, if Rohault is different from Descartes, it is not because he supported a different epistemology of physics. Rather, it is because, in a different institutional situation, he systematized social practice tied to the communication of philosophy that had at best been dimly seen by Descartes. Hence the idea that, to seize any novelty of Rohault, it is appropriate to not only read his *System of Natural Philosophy*, but to take seriously the very practice of his lectures. To do so, one must compare them to comparable enterprises from the same period. As I will now show, Rohault flourishes at the moment when something that can be called a radical experimentalism began to develop in France; that is to say, a doctrine by which the veritable work of physicists was mainly to make observations and to carry out experiments in closed social spaces like the Royal Society or the Académie des sciences. Some have held that Rohault participated in the establishment of this radical experimentalism, or that he was influenced by it.³⁵ Instead, it seems to me that compared with this radical experimentalism, Rohault's Cartesian experimentalism switched to become "arrière-garde," something old fashioned.

3.3 *The Académie Montmor, the Compagnie des sciences et des arts, Rohault's Mercredis*

The history of learned societies in seventeenth-century France has often been written in a retrospective mode. The aim has been to seek out in these societies the first hints of an institutionally decisive event, the founding of the Académie des sciences. Thus, the first histories of the Académie des sciences sought in these earlier learned societies the proof of seniority for the French compared with the English and their Royal Society.³⁶ Later histories have tended to project institutional phenomena that would only apply to the Académie des sciences of the eighteenth century back onto the relatively informal societies of the seventeenth century, which were merely a certain number of vectors of scientific exchanges among many others. (In particular, it seems to me that scientific work was carried out and sanctioned much more in correspondence than in these societies, which is why the following discussion bears no judgment as to the importance of these societies for the progress of science.) Whether in the earliest histories or in those written by our contemporaries, this retrospective style has imposed what I would call a continuist genealogy, according to which an uninterrupted line connects one society to the next, and all of them to the Académie des sciences.³⁷ The meetings held by Mersenne would thus have begat the Académie Le Pailleur (Pascal's *Academia parisiensis*), which begat the Académie Montmor, which begat the Compagnie des sciences et des arts (the name of a project from 1664, also known as the Académie Thévenot), which in turn begat the Académie des sciences. In this series of begettings, Théophraste Renaudot's Conférences and the Académie Bourdelot are often left aside, as are other specialized circles like that of the mathematician Claude Mylon (1615–1662), a choice that creates its own problems.

Yet the main problem with this continuist genealogy is that it produces between these societies a difference that is both too great and too small. Too great, because this genealogy leads us to think that each of these societies had a well-defined identity and individuality. But not only is it true that some scientists, for example Gilles Personne de Roberval (1602–1675), Ismaël Boulliau (1605–1694), or Pierre Petit (1598–1677), worked throughout the century in various societies, but that at any given time, the same individuals were simultaneously members of multiple societies, even when they embodied opposing parties: Adrien Auzout (1620–1689), who was anti-Cartesian, attended Rohault's *Mercredis*; Pierre Michon Bourdelot (1610–1685), at the time physician attached to the Condé family and moderator of the eponymous Académie, gave talks at the Académie Montmor; Géraud de Cordemoy, a Cartesian attorney, joined some times at Melchisédech Thévenot when he brought together the anti-Cartesian experimentalists.³⁸ But the difference produced by the continuist genealogy is also too great in that it neglects the differences of type of institution or orientation among some of these societies. By difference of type of institution, I mean, for example, the difference between informal meetings held occasionally and societies with strict rules, whether for the days they met, the terms of admission of participants, or the way sessions were held, with for example, the designation of a moderator and a secretary. As for the discontinuity of orientation, this refers to the general goals set out by these societies and the intellectual means

³⁵ McCloughlin 1996, 478; McCloughlin 2000, 341–342.

³⁶ Cassini 1693, 26; Duhamel 1698, 7–9; Fontenelle 1733, 4–5. See on the contrary, and correctly, Brown 1934, 91–105.

³⁷ Brown 1934 is the pioneering work, on which all others rely. See also Mesnard 1963; Taton 1966; Hahn 1971; Hirschfield 1981. For a pertinent critique of the manner in which relations between learned societies and the Académie des sciences were conceived, see Mazaauric 2007.

³⁸ Huygens 1888–1950, XXII, 535, 540, 543–544, 554; Borch 1983, III, 423, 435; IV, 173. On Petit and Auzout, see below, n75.

they afforded themselves to achieve them. Discontinuity of type of institution and discontinuity of orientation are not always linked. I will thus be able to show that there is a discontinuity of orientation between the *Académie Montmor* in its early years and in its later years, with the development of a radical experimentalism that began in the *Académie Montmor* and continued in the *Compagnie des sciences et des arts*.

On the whole these questions have not been given much consideration in the secondary literature since the seminal work of Harcourt Brown, with the exception perhaps of works on the *Académie Bourdelot*, which I will set aside for the very reason that its functioning, at least as a means of aristocratic patronage, is fairly well known.³⁹ To evaluate Rohault, not as the author of the *System of Natural Philosophy*, but rather as the organizer of his *Mercredis*, I must get at the root and explore primary sources. I'll begin by presenting the early days of the *Académie Montmor*. I'll then show that it experienced the development of a radical experimentalism as of 1661. This will allow me to then evaluate Rohault's *Mercredis*.

The *Académie Montmor* operated, as far as we can tell, and with some long interruptions, from the end of 1657 to June 1664.⁴⁰ Its name comes from the man who was both its patron and its moderator, Henri-Louis Habert de Montmor (1600–1679), Conseiller du Roi and Maître des Requêtes, a member of *Académie française* from almost the earliest days of that institution. As a patron, Montmor protected both Gassendi and Descartes. On one hand, he offered Descartes a house in the country and, as we know from Adrien Baillet, attended Descartes' funeral in 1667. On the other, he hosted Pierre Gassendi in the last two years of his life, served as the executor of his estate, and helped to publish his *Œuvres complètes* (Lyon, 1658). Despite this, Montmor's *Académie* was considered to be Cartesian. Sorbière indeed wrote that Montmor endeavored to make Descartes' physics a Latin poem; Chapelain wrote that he created his *Académie* solely to establish the doctrine of Descartes and to allow for its beautiful dreaming; Dom Robert Desgabets (1610–1678) noted, in a letter he sent to Jean-Baptiste Denis (1643–1704) on July 28, 1667, that ten years earlier, "his esteem for the philosophy of Monsieur Descartes led him to be very faithful to the assemblies held by Monsieur Montmor."⁴¹ It is appropriate in these conditions not to speculate on its exact doctrinal orientations, but rather to consider two documents of what one might call an institutional nature, since they were written by the *Académie's* secretary, Samuel Sorbière, as well as the testimony of travel journals and correspondence.

The first of these documents, the *Académie's Règlement* (1657), shows that the list of participants and the conduct of the sessions were strictly regulated. The circle of Montmorians, "made up of people curious about nature, medicine, mathematics, liberal arts, and mechanics" was limited to its founding members and those that two thirds of those present allowed to join, even if it was planned that they would "carry out correspondence with scientists from France and abroad."⁴² The high point of each meeting was two speeches prepared in advance on a subject chosen by the President. Once these speeches were read, each member present could give his comments "each in turn, and in a few words."⁴³ The fact that this academy was specialized in the study of nature was indicated by the description of the people admitted, but also by the introductory affirmation: "the goal of these conferences will not be the vain exercise of the mind and useless subtleties, but rather always the clearest knowledge of the works of God and the advancement of the commodities of life, in the arts and sciences that serve to best establish them."⁴⁴ We will note, however, that while the 1657 *Règlement* gives a few instructions as to the conduct of exchanges—in sum, that one must not speak to say nothing, nor speak without having the floor—it says absolutely nothing about carrying out experiments in the study of natural things. The point of the conferences was quite literally to meet to confer, or more accurately, to listen to speeches written in advance.

³⁹ Béguin 1999, 362–379.

⁴⁰ Brown 1934, 68–74, discusses the informal meetings that took place at Montmor's house before 1657. The meetings were interrupted by Roberval's insult to Montmor, then by political affairs between December 1658 and August 1659 (Boulliau to Huygens, 6 December 1658 and Chapelain to Huygens, 20 August 1659, in Huygens 1888–1950, II, 287, 468; Oldenburg to Saporta, 11 July 1659, in Oldenburg 1965–1973, I, 294–295), then from May to October 1661 because of the illness of Madame de Montmor (Chapelain to Huet, 26 September 1661 and Chapelain to Huygens, 16 October 1661, in Chapelain 1880–1883, II, 153, 159). On the end of Montmor Académie see below, n69.

⁴¹ Baillet 1691, II, 442, 462; Bougerel 1737, 372–373, 434–436; Sorbière to Montmor, 22 August 1657, in Sorbière 1660, 371; Chapelain to Heinsius, 22 September 1667, to Bernier, 16 February and 26 April 1669, in Chapelain 1880–1883, II, 530, 622, 640; Chapelain 1662, 52; Denis 1668, 2–3.

⁴² Sorbière and Du Prat 1657, 634, Art. VII–IX.

⁴³ Sorbière and Du Prat 1657, 633, Art. II–VI. In his letter to Hobbes dated 1 February 1658, in Sorbière 1660, 632, Sorbière indicates that this scheme drew the opposition from those who did not want to have to write speeches.

⁴⁴ Sorbière and Du Prat 1657, 633, Art. I.

It is therefore not surprising that in the list of topics proposed for 1659 sent by Oldenburg to Boyle—“the source of the truth of opinions now in fashion. The explanation of the principles of Descartes, the insufficiency of motion and figure to explain the phenomena of nature (an enterprise to be proven by an Aristotelian). After, on the brain, on nutrition, on the use of the liver and spleen, on memory, on fire, on the influence of the stars, if the fixed stars are suns, if the Earth is alive, on the generation of gold, if all our knowledge depends on the senses”⁴⁵—nothing seems to be able to or have to rely on experiments that would have taken place during the meetings. This remark is even more true for the speeches Sorbière himself made in 1658–1659, on fevers and the cold, on motion, on rarefaction and condensation, on the idea that our limited knowledge of the natural sciences should not discourage us from studying them, on the truth of our knowledge of nature, on the source of the diversity of our opinions on a given subject.⁴⁶ Although Sorbière insists that the aim is to avoid “metaphysical thoughts,” or insists on the need to “mix into our discussions all that we know that is useful and curious in the Arts of the Sciences and that applied immediately to the commodities of life,”⁴⁷ when we read these speeches, we would readily describe them as metaphysical, in the sense that this term has now taken to mean those theories that no specific experiment could confirm or contradict. And there was also metaphysics in the contemporary meaning, as in the session where they examined the manner in which secondary and primary causes work together to produce the phenomena of our world.⁴⁸ In addition, of course, the sessions were devoted to reading scientific letters, but even when these referred to experiments, they did not imply carrying out any: the intent was to report on experiments done elsewhere. It was, for example, to the *Académie Montmor* that Huygens communicated in 1658, by means of a letter to Chapelain, his “System of Saturn,” that is, the fact that Saturn is surrounded by a thin flat ring; it was before the Montmorians that Clerselier read a letter in which Descartes is purported to have responded to the attacks of Roberval, a letter that Clerselier would admit a dozen years later having written himself to refute him “more gallantly and with more authority”; it is at least in part before the *Académie Montmor* that were read and commented the letters Fermat sent to Marin Cureau de la Chambre (1594–1669), Clerselier, and Rohault to contest the explanation of refraction proposed by Descartes.⁴⁹ On these lines, it was to the *Académie Montmor* that Jean-Baptiste Denis presented for the first time his theories on the transfusion of blood, but we have no evidence that he did so other than in the form of a speech.⁵⁰ The least we can conclude is that experimental practices were not at the heart of the first *Académie Montmor*.

Given this conclusion, the second document, the speech given by Sorbière in 1663 to the Académie, and which he then sent to Colbert, may appear surprising. Sorbière insists on the place experiments had held in the *Académie Montmor*, contrary to what its *Règlement* and a look at the subjects dealt with have led us to conclude: “we left each the freedom to bring his experiments, we exhorted the most industrious to experiment, we preferred them to any other discussion.”⁵¹ Later, Sorbière details these experiments:

We have even seen with pleasure Monsieur Rohault come here with his set of magnets, and Monsieur Pecquet put on his ceremonial garb to carry out according to his method his dissections. Monsieur Petit played his artillery with gunpowder and fulminating gold. Monsieur Thévenot showed his tubes designed to examine the ascension of water, which rises on its own to its own level. Monsieur de Monconys brought an enchanted horse that the Devil carried, according to its grooms, and that he had bought to show in our presence the falseness of this opinion.⁵²

This description certainly corresponds to the interests of the various parties, with Petit already in the days of Mersenne carrying out experiments to see if a cannon ball would fall, whatever the height from which it was dropped. Montconys often tried to refute all sorts of beliefs. It is attested elsewhere that Jean Pecquet (1622–1674), author of the *Experimenta nova anatomica* (1661), did dissections, that Rohault demonstrated magnetic phenomena, that Monconys gave a talk on the ascension of water, and that Thévenot showed a spirit level.⁵³

⁴⁵ Oldenburg to Saporta, 11 July 1659, in Oldenburg 1965–1973, I, 294–295. The question of knowing if the Earth is alive was dealt with by Chapelain, see BNF, Ms. 12847, mentioned in Collas 1912, 331.

⁴⁶ Sorbière 1660, 60–64, 181–189, 190–193, 194–202, 694–700, 701–704, 712–714.

⁴⁷ Sorbière 1660, 695, 100.

⁴⁸ Richard Jones to Boyle, 20 March 1660, in Boyle 2001, I, 405–406.

⁴⁹ Chapelain to Huygens, 10 May 1658, in Huygens 1888–1950, II, 173–176. Clerselier 1667, unpaginated. Clerselier to Fermat, 13 May 1662, in Descartes 1667, 284–286, *passim*.

⁵⁰ Denis 1668, 2–3.

⁵¹ Denis 1668, 161.

⁵² Denis 1668, 216–217.

⁵³ One of the experiments of Pecquet is reported in Oldenburg’s letter to Saporta, 27 August 1659, in Oldenburg 1965–1973, I, 308: “Only Monsr Pecquet brought an experience of his of the winds engendered in the body of man wch was odde, vid. yt he had known a man, who, wherewoever he touched him on his body, gave from him much wind by his mouth, even when he touched him on his tigh or his feeth,” but about Pecquet’s dissections, see especially Sorbière 1660, 22–59. We find the *Discours sur l’ascension de l’eau sur un niveau, en un tuyau*

Beyond the doubts one may have as to the intellectual gains from seeing Petit's artillery or Monconys's horse, what is remarkable is the way in which Sorbière insists on these experiments, which were not mentioned at all in the 1657 *Règlement*.

To understand Sorbière's move, we must place this 1663 speech in its historical context. At a time when the *Académie des sciences* was in gestation, his intent was to prepare the future by drawing lessons from the past, as well as to show the *Académie Montmor* in the most favorable light, as if as the mother of all the learned societies specialized in the study of natural things, it had full legitimacy to determine what the *Académie des sciences* should be.⁵⁴ But even in this context, Sorbière disqualifies the radical experimentalists, meaning by that those who gave the highest, or even the exclusive, priority to experiments. He criticized them in particular for having made the most of the troubles of the *Académie Montmor*, and of then signing its death warrant. In addition to the people who fomented discord to be able in the end to position themselves as the arbiters among scientists,

There is another type of person, who in the general collapse of this assembly wanted to rally and take control, using a very obvious pretext, but with a plan whose execution was impossible for us. They preached only for experiments and demanded that we meet only to do them, or that we speak only spontaneously. They said that we need to take care only to act properly, and that there is no need to reason on a topic before doing some experiment, which would supply enough material for discussion without any other meditation.⁵⁵

The 1663 speech is a strategic rewriting of the past. Faced with the radical experimentalism that, as I will show, developed as of 1661, the aim is to affirm that the *Académie Montmor* carried out experiments as much as any private learned society could reasonably do, and called on the public authorities to do more. For "to endeavor to create an academy of physics" only can be done by "kings, rich sovereigns, or a few wise and wealthy republics."⁵⁶

If there was any doubt that Sorbière is rewriting history and that the *Académie Montmor* was a society where speeches and not experiments took first place, we can conclude by looking at some third-party testimony. Whether he's writing to Michaelis, Hartlib, or Boyle, Oldenburg gives the same verdict on the state of natural philosophy at the *Académie Montmor*: "in Paris there are many men who promise a great deal but few who give"; "I wish only, these discourses may not rather tend to speculation and shew of wit, yn usellnes to the life of man, wch latter I much doubt off, considering the nature of most of ye French, and indeed of most of men, yt lover rather to praise yn to worke"; "French natural philosophers are more discursive yn active or experimental"—which, according to Oldenburg, confirms the Italian proverb, "Le parole sono femine, le fatti maschii."⁵⁷

One could say that Oldenburg, who was not yet secretary of the Royal Society, but was already well established in England and tied to Boyle by a relationship of patronage (it was in his capacity as tutor to Richard Jones, Boyle's nephew, that he was traveling in France), was merely projecting well-established prejudices on the people he met in France. But the judgment of French astronomer Ismaël Boulliau in a letter to Huygens from July 1661 was scarcely any different:

If one could persuade our gentlemen of Paris, who are rich, curious about beautiful things, and desirous of immortal glory, to make some expense to acquire knowledge by experiments, one might hope for something. But they want to acquire what is the best and most beautiful with no pain, other than that given to making beautiful speeches and philosophizing in the clouds, with no application, and with no expense. You have seen it by your own experience, and I will say no more in writing.⁵⁸

In that last sentence, Boulliau alludes to Huygen's experience during his second stay in Paris. The brief notes Huygens took in his travel journal about the sessions of the *Académie Montmor*, which he attended very faithfully from November 1660 to February 1661, constitute another conclusive document with regard to the content of these sessions: "Des Argues made a speech on whether the mathematical point truly exists," "Monsieur de Neuré read his paper on the causes of thunder," "Rohault read the experiments of water rising in small tubes," "Rohault explained the little tubes," "a dispute between Rohault and Auzout," "Bourdelot spoke of gout...Pequet against Bourdelot," "Monsieur de la Potterie spoke of the elementary fire under the sky of the Moon," "Bourdelot again spoke of gout and did so very well," "Monsieur Pecquet spoke of the generation of the

étroit, récité par Mr. de Montconys, chez Mr. de Montmor in Montconys 1665–1666, III. Thévenot's spirit level is mentioned in Thévenot 1681, 10–12 and in Thévenot to Huygens, in Huygens 1888–1950, IV, 18–19.

⁵⁴ Sorbière 1663, 160.

⁵⁵ Sorbière 1663, 162.

⁵⁶ Sorbière 1663, 160, 216.

⁵⁷ Oldenburg to Michaelis, 26 April 1659, to Hartlib, 30 July 1659, and to Boyle, 23 July 1659, in Oldenburg 1965–1973, I, resp. 240, 260, 287.

⁵⁸ Boulliau to Huygens, 11 July 1661, in Huygens 1888–1950, III, 293.

chicken in the egg and was booed.”⁵⁹ From these notes and from the testimony of Oldenburg and Boulliau, we see both that the sessions of the *Académie Montmor* were acts of speech (talking, reading, explaining, making speeches), and that despite the worthy resolutions of the 1657 *Règlement*, the atmosphere was not totally polite, and the speeches almost always ended in arguments. Ten years later, Desgabets still recalls the “the slight mocking [*petites railleries*]” that accompanied his reading of his *Discours de la communication ou transfusion du sang*, which led him to understand that “some held this thought to be ridiculous.”⁶⁰

The contrast is great with the radical experimentalism that developed from 1661 and until the foundation of the *Académie des sciences* in 1666. In a general fashion, by radical experimentalism I mean the doctrine according to which the true work of those who study things of nature is nearly exclusively to carry out experiments in a socially closed space. This is a very general definition, whose principal virtue is to offer a contrast with the first years of the *Académie Montmor*. To provide historical consideration to the radical experimentalism that developed in the period 1661–1666, in the following, I’ll first examine its causes; I will then present the transitional period of 1662–1665; I’ll conclude by showing that radical experimentalism is behind the project of the *Compagnie des sciences et des arts*, which prefigures the *Académie des sciences*.⁶¹

In continuity with what has been said above, we shouldn’t be surprised that one of the causes of the development of a radical experimentalism was the feeling that the speeches inevitably ended in disputes. In his 1663 speech, Sorbière tends to reconstruct history in this regard by affirming that the *Académie* at first experienced a period of harmony: “there were said in this place excellent things, and there were even carried out several beautiful experiments. And when a topic had been thoroughly considered in two or three sessions, it seemed as if we had exhausted all of its human subtlety.”⁶² He however soon recognized that dissonance had arrived in the assembly due to “certain spirits who felt themselves above the laws we had established”; because of this, “all others...were as if smothered under torrents of words.”⁶³ In reality, it seems that verbal excess was the normal status of the *Académie Montmor*, and that everyone soon grew tired of it. Huygens evoked the excessive statements of the Aristotelian Antoine de la Poterie, Gassendi’s former secretary.⁶⁴ Chapelain reports to Nicolas Heinsius (1620–1681) “this bickering among the philosophers and mathematicians of the Montmorian Academy” and attributes it to the system of Descartes.⁶⁵ Boulliau, who belonged to neither of these two societies, contrasted the urbanity of the literary circle of the Venetian ambassador to the vehemence of the Montmorian:

From certain persons I have learned that the Venetians are more agreeable, more polite, more urbane, and use complimentary words in discussion. The Montmorian are sharper, and dispute with vehemence, since they quarrel about the pursuit of truth; sometimes they are eager to rail at each other, and jealously deny a truth, since each one, although professing to inquire and investigate, would like to be the sole author of the truth when discovered.⁶⁶

If disputes were an endemic illness of the *Académie Montmor*, it remains to be explained, since so many institutions affected by endemic illnesses endure, why this one became so intolerable that participants came to desire something different. This is because French scientists learned of a different mode of operation. The letters of Oldenburg, first secretary of the Royal Society, and of Huygens, who after his stay in London from March to May 1661 remained a correspondent of Robert Moray, made French scientists aware of the experimental

⁵⁹ Huygens 1888–1950, XXII, 535, 537, 539, 540, 543, 544, 546, 553, 554, 560.

⁶⁰ Denis 1668, 2–3.

⁶¹ McLaughlin 1975. It cannot be contested that this project prefigures more the *Académie des sciences* than do the notes that, probably at the request of Colbert, were written in 1666 by Jean Chapelain and Charles Perrault, both members of the “*Petite Académie*,” a small council in charge of proposing initiatives to glorify the King. The main goal of Chapelain’s note, published in Chapelain 1666, 513 (Collas 1912, 384–388, establishes that Chapelain was the author) was to distinguish “scientists by profession,” who are busy only with cabals in the court, and “good faith scientists,” who of course were the true scientists. The note from Charles Perrault, who proposed an “*Académie Royale générale*” divided into four sections (Belles-Lettres, History, Philosophy, in the sense of natural philosophy, Mathematics), is very short and the project it promotes was soon abandoned because it faced resistance from already established institutions as the Sorbonne and the *Académie française* (Duhamel 1698, 7–9; Fontenelle 1733, 5–7).

⁶² Sorbière 1663, 160–161.

⁶³ Sorbière 1663, 161.

⁶⁴ Huygens, *Journal*, 9 November 1660, in Huygens 1888–1950, XXII, 535; Christiaan Huygens to Lodewijk Huygens, 26 April 1662, in Huygens 1888–1950, IV, 117.

⁶⁵ Chapelain to Heinsius, 6 February 1659, in Chapelain 1880–1883, II, 17.

⁶⁶ Boulliau to Heinsius, February 1658, quoted and translated in Brown 1934, 78–79.

commitment of the Royal Society, which rapidly became a model for them.⁶⁷ The correspondence between Chapelain and Huygens is clear on this point.

Promise that you will teach us about what you will learn of English science. We are told here that the learned people of London have great plans for the advancement of the knowledge of nature. It seems that our Academy is warmed by the emulation they have given it, and that we want to apply ourselves to experiments in favor of all other exercises where only the mind takes part.⁶⁸

Huygens highlighted that the aim of these gentlemen was more to do experiments rather than engage in reasoning, to which Chapelain responds immediately that this “will serve as a strong prompt to the members of the Academy to apply themselves to experiments on which natural sciences are founded, rather than on speculations and conjectures.”⁶⁹

We know that the meetings of the *Académie Montmor* officially ended in June 1664, and that Melchisédech Thévenot “received in his home the debris of this assembly.”⁷⁰ Thévenot (1620–1692), a diplomat in Italy, a traveler, a cartographer and bibliophile, is known for having made a spirit level, written the first treatise in French on swimming, being the King’s Librarian as of 1684, and a member of the *Académie des sciences* beginning in 1685.⁷¹ According to the brief autobiography that he wrote shortly before his death, he supported a “a company of persons known to be very clever,” taking advantage of the insight of Frenicle and Steno, who lodged with him at the time, and primarily carried out anatomical and astronomical observations.⁷² In his *Discours de la navigation*, he presents this company as if it were the *Académie Montmor* itself: “the assembly formed at the home of Monsieur de Montmor worked at my home the last two years it met.”⁷³ Correspondence from this period confirms this description, and shows in particular that there was no real discontinuity between a part of the *Académie Montmor* in its later years and the *Compagnie des sciences et des arts*. In fact, the scientists who grew tired of the disputes and who by the emulation of the English converted to experimentalism had begun to go their own way with Thévenot even before the dissolution of the *Académie Montmor* in 1664, without the creation of a true academy in an institutional sense.

In 1663, certain sessions of the *Académie Montmor* remained the opportunity to read Cartesian writings, the *Discours du mouvement local* by Géraud de Cordemoy and the *Discours des fièvres* by Rohault, which were published one year later, after Descartes’s *World*.⁷⁴ The general impression from this correspondence and these journals is that nonetheless from 1662 through 1666, radical experimentalism was present everywhere and in all sorts of manners, while at the same time, if we can say, things became less formalized in the expectation of a new institutional framework. Petit, Auzout and “a bit,” Thévenot, felt themselves to be alone as partisans of experiments among the Montmorians, or more generally among French scientists, carried out in the spring of 1662 various astronomical observations.⁷⁵ In late 1662 and early 1663, some of the Montmorians began to come

⁶⁷ On this point, begin with Brown 1934, 119–122.

⁶⁸ Chapelain to Huygens, 30 May 1661, in Huygens 1888–1950, III, 273.

⁶⁹ Huygens to Chapelain, 14 July and Chapelain to Huygens, 20 July 1661, in Huygens 1888–1950, III, 295 and 299.

⁷⁰ Huygens to Moray, 12 June 1664, Huygens 1888–1950, V, 70. Chapelain to Bernier, 16 February 1669, in Chapelain 1880–1883, II, 622.

⁷¹ The most detailed study of the various activities of Thévenot is currently that of Dew 2009, 81–130.

⁷² Thévenot 1694, Avertissement, unpaginated.

⁷³ Thévenot 1681, 8.

⁷⁴ It is in this edition that it is said that these two speeches were made at the *Académie Montmor* before being published, without their authors being named.

⁷⁵ Petit to Huygens, 8 March and 5 May 1662, in Huygens 1888–1950, IV, 73, 127. From the beginning of his correspondence with Huygens, Petit complained of the way in which, in France, people of quality neglected mechanics, see Petit to Huygens, in Huygens 1888–1950, II, 257. Petit, Auzout and Thévenot are mentioned meeting on Tuesdays in the letter from Petit to Huygens, 17 October 1664, in Huygens 1888–1950, V, 124. The same three would meet Christopher Wren when he came to Paris a few years later (Oldenburg to Boyle, 24 August 1665, in Oldenburg 1965–1973, II, 480). Pierre Petit (1598–1682), born in Montluçon, resided in Paris from 1633 on, wrote objections against the metaphysics of the *Discourse on Method* and against the explanation of refractions in the *Dioptrique*, and communicated Torricelli’s experiment to Pascal. *Intendant général des fortifications* from 1649, he was part of the various scientific circles and regretted not being a member of the *Académie des sciences* (see the lettre from Boulliau quoted by Brown 1934, 138). The explanation can perhaps be found in his character; see the cruel portrait made of him in Sorbière to Hobbes, early 1663, in Hobbes 1994, 551–554; and Christiaan Huygens to Lodewijk Huygens, 28 Sept. and 9 Nov. 1662, in Huygens 1888–1950, IV, 241, 256, *passim*. Adrien Auzout (1620–1691), born in Rouen, contributed to Pascal’s experiments on the vacuum, worked as an astronomer with Jean Picard at the *Académie des sciences*, of which he was briefly a member (1666–1668) before retiring to Italy and

together at the home of the Marquis de Sourdis, “but they go there in smaller numbers, and it is clear that in a short while there will be nobody at all wanting to go there,” explained Sorbière, who thought that Sourdis had nothing to give “except his wretched, crude, vulgar hospitality in an unattractive part of his unattractive house,” “as if a group of grasping rustic schoolmasters were entertained by an Irish professor [*quemdam professorem*].”⁷⁶ In the spring of 1663, Huygens was in Paris, and the issue of the day was to establish “new laws and ordinances” for the *Académie Montmor*; a “general assembly for telescopes” was organized at the same time at the home of Auzout: the power of various telescopes was tested by Auzout, Huygens, Petit, Monconys, and Étienne d’Espagnet, who had the lenses of some of these telescopes made using a new process.⁷⁷ In November 1663, after nearly two years of unsuccessful attempts, an air pump was finally built for Montmor on the model of Boyle’s pump, and was used for experiments, in particular on the phenomena of abnormal suspension shown by Huygens. At nearly the same time, astronomical observations took place in Issy, at the home of Thévenot. A month later, dissections were carried out at the home of Montmor.⁷⁸ Shortly before the official dissolution of the *Académie Montmor*, the vacuum machine was paraded to different salons, always seeking to “create a more solid and better regulated establishment for this academy.”⁷⁹ As for the meetings held at the home of Thévenot, what the Danish scientist and traveler Ole Borch (1626–1690) said seems to indicate that they were devoted to carrying out chemistry experiments on liquors in June–July 1664, then, at least from November 1664 to March 1665, to engaging in anatomical observations under the direction of Nicolas Steno (1638–1686) then Jan Swammerdam (1637–1680).⁸⁰ It seems likely that these last observations correspond to the ones found in the three fascicles by Swammerdam that close the *Recueil de divers voyages* by Thévenot, that is to say *Histoire naturelle de l’Ephémère*, *Histoire naturelle du Cancellus ou Bernard L’Hermitte*, and *Le cabinet de Mr Svvammerdam* [sic], *docteur en médecine*. In early 1666, the *Académie Royale des sciences* was founded, and Thévenot retired to Issy, officially to “philosophize and speculate with more liberty in the country,” but in fact, we may also conclude, out of spite for not having been among the first members of the *Académie des sciences*.⁸¹

By the continuist genealogy that I noted above, the *Compagnie des sciences et des arts* is presented as if it were a society along the lines of the *Académie Montmor*. From an institutional point of view, this is incorrect. The rare indications I’ve been able to collect give instead the impression that the group of scientists advocating radical experimentalism (Thévenot himself, Petit, Auzout, d’Espagnet, and, when they were in Paris, Huygens, Steno and Swammerdam) would gather, at the home of Thévenot and other places, and without a set date.⁸²

England, apparently for having criticized Charles Perrault’s translation of Vitruvius; see Brown 1934, 138, 138–141.

⁷⁶ Sorbière to Hobbes, 23 December 1662, in Hobbes 1994, 542. Petit to Huygens, 8 November 1662, in Huygens 1888–1950, IV, 262, mentions also that the *Académie Montmor* was on the verge of moving to the home of Sourdis.

⁷⁷ Christiaan Huygens to Lodewijk Huygens, 6 April 1663, in Huygens 1888–1950, IV, 324–325. Christiaan Huygens to [Constantyn Huygens], 20 April and 4 May 1663, in Huygens 1888–1950, IV, 333, 338. Contrary to what the editors of Huygens’s *Œuvres complètes* affirm, the d’Espagnet who appears in Huygens correspondence may not be the chemist Jean d’Espagnet (1564–1637?), first Président of the Parlement of Bordeaux: it is more likely his son, Étienne d’Espagnet, counselor at the Parlement of Bordeaux.

⁷⁸ Huygens to Moray, 18 November and 19 December 1663, in Huygens 1888–1950, IV, 433, 474. Huygens to Constantyn Huygens, 30 November 1663, in Huygens 1888–1950, IV, 452. Auzout to Christiaan Huygens, December 1663, in Huygens 1888–1950, IV, 481–482. The unreliability of the Montmor pump was noted by Shapin and Schaffer 1985, 265–269.

⁷⁹ Huygens to Moray, 12 March 1664, in Huygens 1888–1950, V, 41.

⁸⁰ Borch 1983, III, 464; IV, 6–7, 164, 173, 180–181, 186, 274, 283–284. Borch attributes the anatomical observations to Swammerdam, it is Chapelain to Huet, 31 July 1665, in Chapelain 1880–1883, II, 406, who mentions Steno. This anatomical fashion was in no way proper to the meetings of Thévenot: Borch mentions the anatomical preparations that took place at the home of Montmor in February 1665 (when the *Académie* was no longer meeting there) and the ones done by Steno at the home of Bourdelot in May 1665.

⁸¹ Chapelain to Steno, 15 March 1666 and 27 May 1667, in Chapelain 1880–1883, II, 447, 514.

⁸² Borch also mentions, among those regularly meeting at the home of Thévenot, Vossius (the scholar Isaac Vossius, 1618–1689), Borelli (the chemist and builder of instruments Jacques Borelli (?–1689), later a member of the *Académie des sciences*), Ville Bressé, Bressié or Bressieu (the chemist and engineer Étienne de Villebressieu, who travelled with Descartes at the beginning of the 1630s, and who was the most important source for his first biographer, Pierre Borel, for this period), Frenicle (the mathematician Bernard Frenicle de Bessy (?–1674), who was already living in the home of Thévenot), and Martell (Thomas de Martel (1618–1619–1679–1685?), a bourgeois of Montauban, who was already part of the scientific circles of Paris at the beginning of the 1640s, was a correspondent of Hobbes, then of Oldenburg; the best biography to date is that of Noel Malcolm in his edition of Hobbes’s *Correspondence*).

There are no official rules and regulations stating how these meetings were to take place, and I would almost say that such rules couldn't exist. The meetings begin in 1664, that is to say at a time when Louis XIV had already awarded gratifications to a few humanists, including some scientists like Marin Cureau de la Chambre, Johannes Hevelius (1611–1687), and Huygens, and everyone knew that an *Académie des sciences* would be founded. At issue was not about establishing the rules for a private society, but rather the rules of what would become the *Académie des sciences*. From the point of view of the orientation of these meetings, as correspondence and travel journals attest, the scientists participating in them continued and systematized the activities of observation or experimentation that would take center stage from 1662 on.

A final document can show what was the orientation of the *Compagnie des sciences et des arts*, the project which we know thanks to a copy sent to Huygens.⁸³ The publishers of the *Œuvres complètes* of Huygens hypothesized that this project corresponded to the new rules being sought by the *Académie Montmor* in 1663. The manuscripts reported on by Trevor McClaughlin show that it was more a program from scientists who met at the home of Thévenot⁸⁴. If, as I suppose, the *Compagnie des sciences et des arts* is the product of a scission within the *Académie Montmor* between the more experimentalist of its members and the others, there is no strong contradiction between either position. The very name of the “*Compagnie des sciences et des arts*” is important in this light: bringing together in a company the arts and sciences mean attempting to reform the sciences by putting them through, so to speak, the sieve of the arts. As Thévenot wrote,

If we have so often call to complain about those who have applied themselves so fruitlessly to these sciences or these studies, the same is not true for those who have cultivated the arts. For they have continually made great progress, and brought them to a very high degree of perfection above that which they were at their beginnings...

Most sciences, as we have them now, and their systems, are just a pure mind game of man, who naturally flees the difficulty of clear reasoning, of finding true precepts, and of drawing the consequences, always ready to admire his work and supporting with a great deal of stubbornness what he advances without foundation.

In the arts, on the contrary, when the worker has poorly reasoned and puts into practice a false reasoning, he is immediately convinced by the lack of success of his work and is corrected by the damages suffered.⁸⁵

The idea that the arts are superior to the sciences because, when confronted with the test of reality, they immediately sanction mistakes, so that, instead of being locked into a dead end, they allow us to see the ways to increase knowledge, is a recurring theme in Bacon.⁸⁶ But we also find it in Descartes. Responding to Fromondus who criticized him for creating such a gross (*crassa*) philosophy as mechanics, Descartes defended mechanics with the argument that “as it concerns use and practice, all those who lack the smallest thing are used to being punished with loss of all their expenses.”⁸⁷ And Thévenot uses Descartes to criticize those who would content themselves with the work of the commentator:

Those who spoke in good faith of physics or medicine recognized this necessity to carry out experiments and observations to know something about them. Descartes said so everywhere he had the opportunity to talk about it, everyone is now convinced, and that is what the large number of men of letters who today follow his philosophy should primarily occupy themselves, otherwise it would not be of more use for us to have many commentators of Descartes and Gassendi than it has thus far served us to have employed so many centuries commenting the systems of Epicurians, Plato, and Aristotle.⁸⁸

In fact, the project for a *Compagnie des sciences et des arts* speaks only of experiments and techniques, whether for making astronomic observations, learning to understand the human body thanks to chemistry, anatomy, or medicine, inventing new machines and new secrets “both for the manufacture of arts as for curiosity,” or facilitating navigation “to increase commerce and to have opportunities to discover the wonders that can be encountered in unknown countries.”⁸⁹ More important, alongside the traditional functions of the president, “to ensure proper behavior and silence,” and the secretary “to record anything remarkable said or done in the assemblies and to maintain exchanges with the other academies and scientists,” which are both found in all

⁸³ Huygens 1888–1950, IV, 325–329.

⁸⁴ McClaughlin 1975, 236; see also Dew 2009, 96. The autobiography of Thévenot found in the Avertissement of Thévenot 1694 affirms that this project was presented to Colbert.

⁸⁵ Thévenot 1681, 3–6.

⁸⁶ See for example Bacon, *Novum organum*, I, 73–74, in Bacon 1996–, XI, 116–119.

⁸⁷ Descartes to Plempius for Fromondus, 3 October 1637, in AT I 421.

⁸⁸ Thévenot 1681, 7.

⁸⁹ This point is already highlighted in McClaughlin 1975, 238–242, who notes the common points between the project for the *Compagnie des sciences et des arts* and the practices of the *Académie des sciences*, which is true, but who also suggests that this commonality of doctrine arises from a Gassendist reference, which seems doubtful to me.

academies, they include curators “to keep ready and to execute everything needed for experiments and the machines to design the machines for workers to operate.”⁹⁰

Once again we think of Bacon. The allusion is clear when the subject is “making a history of nature as universal as possible, on whose solid foundation one can work to build a physics, and do the same for the history of the arts, and the inventions of men that are in use.”⁹¹ If the demand for usefulness is omnipresent (usefulness for individuals and for the general public, a search for commodities for humanity in general and for France in particular) in this project, it does not exclude the fact that one is curious, in particular to learn to distinguish true from false, between what works and what doesn’t:

One will attempt to learn all the well-tested secrets....

One will also test all the important secrets about which one has the description, when there is some likelihood that they could succeed, to approve them and use them if they are good, or to undeceive those who believe in them on their faith in others, without testing them, if they are not true. Lastly one will work to undeceive the world of all the vulgar errors that have passed for so long as truths due to the lack of the necessary experiments to discover their falsehood...

One will also work to learn of all the tricks of tradesmen and merchants and their sophistries with the means to uncover them, which one will publish to prevent the public from being tricked, and to oblige workers to work more faithfully....

If the *Compagnie* is consulted about any new inventions, about machines, or about major public or private projects, it will deputize those from among it that will be the most conversant in these matters... so that for want of such an examination neither the prince nor the private person will commit themselves to useless expenses and in less than honorable enterprises, as we too often see.⁹²

In its proliferation, the project for the *Compagnie des sciences et des arts* constitutes the clearest possible testimony of what was radical experimentalism. More generally, it’s time to summarize what has been shown so far about learned societies. The *Académie Montmor* was initially a society where one conferred and made speeches. As of 1662, the degradation of the speeches into arguments and the model of the Royal Society favored, for some of the Montmorians, the development of a radical experimentalism. The *Compagnie des sciences et des arts*, without being a formally instituted academy, brought together in 1664–1665 those who beginning in 1662–1663 distinguished themselves as the members of the *Académie Montmor* partisans of experiments.

How to situate Rohault’s *Mercredis* with respect to this evolution, which led to a radical experimentalism? There is no doubt that the *Mercredis* were organized on Wednesdays from 1658–1659 and until the death of Rohault in 1671, during which participants could view all sorts of experiments.⁹³ That means that they began two years after the start of the *Académie Montmor*, and that, with the exception of the two first years, were concomitant with the appearance of a radical experimentalism, later institutionalized in the *Académie des sciences*. The dates here are important. The beginning of the *Mercredis* has at times been pushed back to the middle of the 1650s, but I don’t think that is supported by the evidence. They are however incontrovertibly attested to in 1659 by the testimony of Clerselier in the preface of volume II of Descartes’s *Lettres*.⁹⁴ The only document that leads Pierre Clair to “conjecture” that the *Mercredis* began in 1655 is the *Éloge de Pierre-Sylvain Régis* written by Fontenelle.⁹⁵ In it, Fontenelle evokes both the disgust theology caused to Régis and the latter’s discovery of Cartesian philosophy thanks to Rohault’s *Mercredis*: these events are not properly dated, and the only temporal indications given would lead us instead to think that they took place at the beginning of the 1660s. Fontenelle notes that Pierre-Sylvain Régis “had only four or five months left in his stay in Paris” when he discovered Cartesian philosophy, which we take to mean before his departure for Toulouse. And we know from other sources that Régis’s lessons in Toulouse began in 1665.⁹⁶ If we add to that the fact that Louis Moreri, in his *Grand dictionnaire*, says of Rohault that he “taught ten or twelve years in Paris,” but also that the prefaces of two works published in 1657, the preface to volume II of Descartes’s *Lettres* and the preface of Savinien Cyrano

⁹⁰ Huygens 1888–1950, IV, 329.

⁹¹ Huygens 1888–1950, IV, 327. The first reception of Bacon in France was explored in Le Dœuff 1984; on baconianism in mathematics, see Goldstein 2008. The question of the reference to Bacon in late seventeenth century France remains however to be explored, but it may be noted that it was Huygens who in December 1660 lent Thévenot Bacon’s *Opuscula varia posthuma, philosophica, civilia, et theologica*, published two years prior, and that one finds in his later projects for the assembly of physics in an injunction to “work on natural history more or less following the plan of Verulamius” (Huygens 1888–1950, XXII, 540; VI, 95–96 and XIX, 268).

⁹² Huygens 1888–1950, IV, 325–327.

⁹³ The most exhaustive presentation of Rohault’s *Mercredis* remains that of Clair 1978, 42–56.

⁹⁴ Clerselier 1659, unpaginated.

⁹⁵ Clair 1978, 43.

⁹⁶ Fontenelle 1994, 143.

de Bergerac's *Histoire comique* do not mention the *Mercredis*, it seems to me that we must affirm that Rohault's *Mercredis* began at the very end of the 1650s.⁹⁷

We can also wonder, in a preliminary fashion, about the sources that allow us to reconstitute what these *Mercredis* must have been like, at least insofar as we suppose them to differ from the *System of Natural Philosophy*. The testimony most frequently cited is that of Clerselier in the preface to volume II of Descartes's *Lettres* (1659), and even more in the long preface to Rohault's *Œuvres posthumes* (1682). But such testimony cannot be neutral: if Rohault was the "head of the Cartesian school,"⁹⁸ Clerselier was the general or the patron. Thus, when he wrote in the 1659 preface that the same people who came to the *Mercredis* to refute the Cartesian doctrine, "after having been convinced by the force of his demonstrations, and fully persuaded by the correctness and the appropriateness that the experiments had with his reasoning, were finally constrained to give each other their hand, and from the open enemies they had been of this doctrine, to declare themselves the partisans and the defenders,"⁹⁹ he was not providing for future historians an objective testimony as to what Rohault's *Mercredis* were like. Rather, he was defending Cartesian philosophy against the attacks of his contemporaries. This is all the more true in 1682, in the preface he wrote in the heat of the battle of Cartesianism, when no Cartesian had been named as a member of the *Académie des sciences* and when Cartesianism had been condemned by the religious and political authorities.¹⁰⁰ This preface is a panegyric, like the elogia of academicians that Fontenelle would begin writing a few years later. Clerselier shows that Rohault, dead only some ten years earlier, was both socially respectable (he was the preceptor of the sons of the Prince de Conti, and should have been the preceptor of the Dauphin; people of quality participated in his *Mercredis*, there were even "thousands" of them), totally orthodox from the point of view of the Catholic religion (those who called him a heretic were envious and malicious scandalmongers), and of course, he was very capable in the sciences, in particular due to the experiments that his "totally mechanical mind" and his "skilled and artful hands" allowed him to carry out, as well as to his ability to stay within the limits of "a peaceful and honest dispute."¹⁰¹

We thus should not adopt a superficial reading of testimony like that of Clerselier. We can have doubts, for example, that "thousands of people" truly attended the *Mercredis*. The disputes there were not always "peaceful and honest": the anecdotes reported here and there show that Rohault was regularly in opposition to Adrien Auzout; that he practically came to blows with the gassendist Gilles De Launay (fl. 1656–1677); that "he didn't respond well to difficulties, he got angry."¹⁰² Better, one can reread this testimony in light of the few documents we can find elsewhere. Rohault took the decision to publish his *System of Natural Philosophy* because his writings were in the hands of an infinite number of persons," and thus that "they had become unrecognizable" and that he was "not sufficiently explained and understood."¹⁰³ We also have traces of the physics of Rohault prior to the *System of Natural Philosophy*: his *Fragment de physique* (1662) about which specialists debate whether its authorship should be attributed to Rohault or to Cyrano de Bergerac, his *Physique* (1667), recently edited by Sylvain Matton, and the notes taken by a lawyer, "Monsieur F.," who attended the *Mercredis* from 1660 to 1669. The latter document is certainly the most interesting, precisely because it consists of notes taken, if not on the spot, surely at the latest a few days after, and without any intention to be published.¹⁰⁴ From the testimony of Clerselier and the notes of Monsieur F., we can draw the following conclusions.

From an institutional point of view, contrary to the meetings of the *Académie Montmor*, the *Mercredis* did not rely on a closed group whose members took turns as speakers and listeners: at the *Mercredis*, Rohault was almost always in front of his listeners or spectators, who, at least officially, were subject to no selection process whatsoever. "At least officially," for a writer reports that on days of lectures, Rohault's first wife, "stood...at the doorstep of her house, and refused entry to any who did not look to be people of quality...; she wanted velvet and let nothing stop her."¹⁰⁵ "Almost always" because it happened that others were in charge of the experiments;

⁹⁷ Moreri 1759, 310. Clerselier 1657, unpaginated. Le Bret 1657 is all the more telling that, regarding Cyrano's illness, he explicitly mentions Rohault, and does not mention the *Mercredis*: "I would do ill by Monsieur Rohault if I didn't add his name to such a glorious list, since this illustrious mathematician who carried out so many beautiful physical proofs...had so great a friendship for Monsieur de Bergerac...that he was the first to discover the true cause of his illness...."

⁹⁸ Baillet 1691, II, 442.

⁹⁹ Clerselier 1659, unpaginated.

¹⁰⁰ About this battle, see Roux 2012. See also Chap. 2 by Ariew.

¹⁰¹ Clerselier 1682, unpaginated.

¹⁰² Clair 1978, 46–49.

¹⁰³ Clerselier 1682, unpaginated.

¹⁰⁴ Manuscript 2225 of the Bibliothèque Sainte-Geneviève. McCloughlin 1977, n18, attributes this manuscript to René Fédé. Clair 1978, 50–56, provides a few extracts.

¹⁰⁵ Quoted in Clair 1978, 46.

Huygens notes, in December 1660: “at Rohault’s home, a Spaniard separated silver ore.”¹⁰⁶ The *Mercredis* thus differed formally from the encounters that scientific institutions constituted at the time. Whether networks of correspondents or learned societies, these were closed groups; Hobbes reproached, for example, the Royal Society for its “private” character.¹⁰⁷ To use language of our day, they were more like courses given year after year by a professor than seminars bringing together different scientists. And if they did last for more than ten years, it seems difficult for Rohault to have been able to do anything else than to repeat himself from one year to the next, at least for the experiments he was able to do. In fact, Huygens saw experiments on magnetic phenomena in November 1660, which would be viewed by Ole Borch in May 1664.¹⁰⁸

From the point of view of the orientation of the *Mercredis*, the place given to experiments was as limited as in the beginnings of the *Académie Montmor*. In a conference, by definition one confers, that is to say, one meets to speak together. When in 1682, Clerselier describes the *Mercredis*, he notes that their goal was to “*explain* one after another all the questions of physics,” and that to do so, after an improvised speech about an hour long, Rohault would respond at length to the objections made.¹⁰⁹ The aim was, as with Montmor, to have a primarily discursive mode of action, although here without the support of a previously written text. It is not surprising in these conditions that it was as much Rohault’s qualities as an experimenter as an orator that were praised by Clerselier himself, as well as by other contemporaries.¹¹⁰ It was only as final confirmation that the experiments took place:

...to hear him speak about this [the most difficult and curious questions of physics], you would think that he was in concert with nature and that nature took pleasure at revealing her secrets to him....For he directly communicated everything he said about these matters. And so that there would remain no doubt, he added as proof many beautiful experiments that he carried out in front of everyone, and most often he would alert everyone to the effects of the experiments, according to the principles he had previously established, before even getting to the experiment itself.¹¹¹

In the *Mercredis*, the burden of proof was thus not on the experiments: these only took place after everything else, as a sort of complement. Although Clerselier does not hide that Rohault reasoned according to “previously established... principles”, he took from the preface to the *System of Natural Philosophy* the affirmation that everyone could anticipate (*prévenir*) the effects of the experiments.

As for the subjects dealt with in the *Mercredis*, all testimony agrees to say that there were great experimental moments with Rohault: there were experiments on phenomena of capillarity (the famous “little tubes” in which water seemed to rise on its own), magnetism (Rohault had a box in which he kept all the objects required for experiments, including a magnet so powerful that it earned the admiration of all), on colors (on certain days an artificial rain would make appear a rainbow against a white cloth).¹¹² The confrontation of Huygen’s travel journal with the notes of Monsieur F. is nonetheless very surprising: while they concern the same period, they do not report the same thing. Huygens reports solely on experiments, while Monsieur F. mentions none, unless in reference to illustrations.

On 13 and 17 November 1660, Huygens saw “experiments with quicksilver” that showed the weight of air and its resistance, then “experiments with magnets.” On 20 and 21 December, after being invited by a note from Clerselier to go to the home of Rohault, he saw a Spaniard carry out “the separation of silver ore,” and then “experiments with tubes and small pipes.” When instead we look at the notes of Monsieur F., we get the

¹⁰⁶ Huygens 1888–1950, XXII, 541.

¹⁰⁷ On the criticism that Hobbes addressed to the Royal Society as a closed private space, see Shapin and Schaffer, 1985, 113–114.

¹⁰⁸ Huygens 1888–1950, XXII, 536; Borch 1983, III, 423.

¹⁰⁹ Clerselier 1682, unpaginated. I highlight.

¹¹⁰ See for example **F. 1660–1661**, 1r: “...the conferences were written in a tumult, and at odd hours, he [the person collecting the conferences] was not as able as he would have liked to imitate the correctness and the incomparable precision of the terms of he who had the task of representing the feelings.” Foucher 1675, 64–65: “You know that he was intent on reasoning with consequence, and as he perfectly possessed all the subjects he dealt with, he explained them with a great deal of order, and with a certain clarity, accompanied by a natural eloquence that one recognized more in its effect than in the disposition of the terms he would use”. Malebranche, *Recherche de la vérité*, Preface to Volume II, in Malebranche 1958–1967, II, 564: “...everyone know with what accuracy and what force this learned man resisted the blows that others wanted to bear to him, and that with two or three words pronounced without heat and without movement, he struck down the imagination of those full of themselves who thought to cover him in embarrassment”. Clerselier 1682, unpaginated. : “... he summarized so well and in such good order everything objected to him, and responded with such clarity and enlightenment...”

¹¹¹ Clerselier 1682, unpaginated. On the meaning of the verb “*prévenir*,” see above, n18. Three pages later, he refers again to the magnet, where the experiments had been anticipated (“*prévenues*”) by the speeches.

¹¹² Clerselier 1682, unpaginated; Huygens 1888–1950, XXII, 539, 540.

impression that the *Mercredis* were a general course on Cartesian philosophy, where they studied knowledge in general in November, arguments in favor for the existence of bodies (it was at this occasion that there was a bit of metaphysics) and the definition of matter as extension and the experiments of Torricelli and Pascal on the vacuum in December, the conservation of momentum in January, then from February to May, the difference, sense by sense between qualities that we feel and the qualities as they are in things. Monsieur F. sometimes mentions questions from the public and the Rohault's responses. For example:

Someone asked if the matter of the heavens is the same as that of bodies. The answer was yes, since the extension is always extension wherever it is located. Someone asked if a vacuum can exist, and the response was no, since that would mean extension without matter, which is not possible; four walls would be distant from each other by this supposition, there would be no matter, hence no distance, hence they would touch.¹¹³

Monsieur F. does not explicitly mention experiments, but curiously, he refers to illustrations that are not found in his notes, and which he explicitly says in the case of comets, can be found in the book of Descartes—must we conclude that Rohault distributed the illustrations of the *Principles of Philosophy*? The three last entries in Monsieur F.'s notes are disparate by their dates and topics: on tides (November 1669), on the loadstone (17 November 1660, which agrees with the affirmations of Huygens' travel journal), on comets (not dated, but an allusion to one of the comets of 1664–1665). One is tempted to say that when a visitor of distinction like Huygens was expected, Rohault prepared to show his most notable experiments, whatever his lesson plan.

In summary, contrary to the first meetings of the *Académie Montmor*, Rohault's *Mercredis* had a vocation for teaching rather than for research, and like them, they at times included moments of experimentation, perhaps less significant than it has been said, both in terms of quality and quantity. What was important was to explain Cartesian philosophy. This form of lectures, which at first was undoubtedly a novelty and an attraction, must soon have been considered passé by scientists, given the radical experimentalism that began in the early 1660s, first in the *Académie Montmor*, in the context of the *Compagnie des sciences et des arts*. This discredit could only have grown after the foundation of the *Académie des sciences*. Rohault was no longer a scientist, he was merely a Cartesian.

We can find a trace of this evolution, I feel, in the attitude of Huygens toward Rohault. During his stay in Paris from 1660 to 1661, Huygens, who had met Rohault at the home of Montmor, came three times to Rohault to see experiments, that is to say much less regularly than he went to the *Académie Montmor*, which Huygens attended nearly every week from November 1660 to February 1661, and one time less than to the *senatulus* of Cartesian ladies who met at the home of Madame de Bonneveaux.¹¹⁴ Until 1665, Rohault, although he was not in direct correspondence with Huygens, is mentioned from time to time in letters to and from Huygens, without any mention of new experiments being done. I however find no trace of him after 1666, including at the publication of the *System of Natural Philosophy* in 1671, or a year later, at the death of Rohault.¹¹⁵ A factor explaining the progressive disinterest of Huygens with respect to Rohault arises perhaps from Rohault's stubborn insistence that capillary phenomena (the rising of water in thin tubes and the circular shape of the surface of the water) were explained by the weight of air, whereas Huygens had shown that this phenomenon also took place in a vacuum.¹¹⁶ When Huygens, who had had great difficulties to make his own pump work properly, offered to Rohault to help him build one, he did so only with the explicit condition that Rohault ask for help, for, as he wrote to his brother, "there is always a bit of pedantry in what he does, as you may have noticed."¹¹⁷ It seems to me that we can see in the lack of interest from Huygens for Rohault the sign that Rohault had fallen away from the experimental community.

¹¹³ F. 1660–1661, 12r.

¹¹⁴ Huygens 1888–1950, XXII, 536, 541; III, 210. One can note in passing that although at times one speaks of the "emancipatory" character of Cartesianism for women, the attitude of Huygens and his correspondents to this *senatulus* should lead one to a more nuanced judgment as to the type of knowledge women could access in this period; and it is significant that there is no sign of the presence of women in the most visible learned societies, whether the *Académie Montmor*, the *Académie Bourdelot*, or the *Compagnie des sciences et des arts*.

¹¹⁵ Huygens 1888–1950, III, 397, 414, 432; IV, 6, 7, 11, 69, 367, 459; V, 29, 41, 101, 105. Aside from correspondence, Rohault's *Traité de physique* is mentioned in the preface of the *Discours de la cause de la pesanteur*; it also appears in certain critical notes on the Cartesian explanation of magnetism (Huygens 1888–1950, XIX, 572).

¹¹⁶ On Rohault's description and explanation of these phenomena, see Rohault 1681, I, Chap. xxii, Sect. 67–84, 204–214. On Huygens's lack of confidence in Rohault's explanation, see his letters to Moray from 9 December 1663, 20 February and 12 March 1664, resp. in Huygens 1888–1950, IV, 459; V, 29, 41.

¹¹⁷ Christiaan Huygens to Lodewijk Huygens, 18 January 1662, in Huygens 1888–1950, IV, 11. See as well the letter to the same from 4 January, in Huygens 1888–1950, IV, 4.

As I'll now show, the period 1665–1690 corresponds to the time when the historiographic cliché developed according to which there was an essential contradiction between Cartesianism and experimentalism. This cliché was not totally unfounded at the time it developed, since Cartesians of that period had as their first goal to make known their master's doctrine, and they were as we have shown in the exemplary case of Rohault, much less experimenters and experimentalist than some of their contemporaries. It would however be an error to think that there was a historical necessity here or an ineluctable destiny set from the very first books of Descartes. Cartesianism was never an essence. It's a category that developed historically, though debates, polemics, and controversies, as well as distortions, misunderstandings, and changes in perspective.

3.4 Descartes and the Cartesians: the constitution of a cliché

Descartes's philosophy in general, and his natural philosophy in particular, were often attacked. The intensity and the intent of these attacks varied, however. Initially at least, they concerned neither Descartes' experimental abilities or his disposition toward experimentalism. Professors who, like Vopiscus Fortunatus Plempius (1601–1671), Libertus Fromondus (1587–1653), or Jean-Baptiste Morin (1583–1656) analyzed what the *Discourse on Method* said about Cartesian physics were not preoccupied with whether this physics used experiments. For them, what was important was to know if, given its principles, it could correctly explain natural things. Astonishingly for us, the Aristotelians thought that Descartes was mistaken, but at least at first, they did not think that his preoccupations were entirely new. Thus Daniel Garber, on considering the reactions of Fromondus and Morin, was able to say about the publication of the *Discourse on Method* and the *Essays* that accompanied it, that this was a revolution that did not happen.¹¹⁸

In the following paragraphs, my goal is to sketch out a cartography of the criticisms of Cartesians physics beginning at the last third of the seventeenth century. It can be shown that, alongside the moral critique from faithful Gassendists like Sorbière and Chapelain on the way Descartes communicates the truth, there appears an epistemological critique concerning, if not the absence of experiments in Cartesian physics, at least the secondary status they held. I will begin by presenting the moral critique of the Gassendists in the 1660s, and show that it can be found in Huygens and Leibniz in the 1690s. I will then present the epistemological critique as it appears in Mariotte, a physicist of the *Académie des sciences*. I will finally show something much more surprising, that this epistemological critique is also present in the Jesuit Antoine Rochon.

Let's begin with the criticism from the Gassendists Sorbière and Chapelain, who let it be noted, did not care much for one another.¹¹⁹ Although they did not really contribute to the advancement of scientific knowledge, they are often considered responsible for the exclusion of the Cartesians from the *Académie des sciences*, as well as important in its experimentalist commitment.¹²⁰ At first, that is to say, at the end of the 1630s and the beginning of the 1640s, Descartes was for them an author to be promoted, both for the renewal of philosophy to which he was able to contribute, and for the beauty of his style—which is not without ambivalence, for a beautiful style is also what allows him to pass off uncertain hypotheses as truths.¹²¹ But progressively, and in particular after the metaphysical quarrel between Descartes and Gassendi (1644), the criticism overtook the praise. Although they could have found in Gassendi's arguments against Cartesian physics in terms of experimental practice, whether for the observation of the trajectories of planets, the formation of salts, or the

¹¹⁸ Garber 1988.

¹¹⁹ In his letter to Francheville, 16 March 1665, in Chapelain 1880–1883, II, 390, Chapelain accuses Sorbière of having copied Gassendi without understanding him.

¹²⁰ On Chapelain's Gassendism, see Collas 1912, 60–64, 151–154, 331–336, 383–388. On Sorbière's Gassendism, see Pintard 1983, 334–348, nuanced however 418–420, 425, 429. On the responsibility of the Gassendists on the fact that the first members of the *Académie des sciences* were not Cartesians, see Taton 1966, 36; on the fact that they would have been *ipso facto* Gassendists and experimentalists, see McClaughlin 1975, 239–240.

¹²¹ See in particular Chapelain to Balzac, 31 May and 29 December 1637, in Chapelain 1880–1883, I, 153, 189; Sorbière to Petit, 10 February 1657, in Sorbière 1660, 691. On the fact that the illustrations and comparisons used by Descartes would calm the crowd, see Sorbière to Mersenne, 23 December 1647, in Mersenne 1932–1988, XV, 585–587. On Gassendi's "too great literature," see Sorbière 1694, 124–126.

behavior of a weight dropped from the top of a mast, the criticism of Chapelain and Sorbière were primarily on Descartes's dogmatism.¹²²

For them, dogmatism designates not only a doctrinal content opposed to skepticism; it also refers to, and mainly to, a moral or political attitude revealed by Descartes' practices in terms of the communication of the truth. In the parallels he establishes between Gassendi and Descartes, Sorbière notes that, while the former does not hesitate to present his thoughts in a familiar fashion with those with whom he conversed, the latter refused all dialog and referred those he spoke with to his writings.¹²³ According to Sorbière, there was something revealing in the way Descartes behaved when compared with other men: he never treats them as equals, but as their master, the "head of a sect," to whom all intellects will submit, obliging everyone he met to learn his doctrines:

He did not disapprove in those he wished to instruct what Aristotle asked of a good student, docility and patience to mull over a doctrine in the mind until it was strongly imprinted on the memory.... There is no wonder that after four or five years of assiduous meditation and continuous repetition of certain terms, one does not want to lose the fruit of the pains taken to learn them, and if one convinces oneself in the end, that by the ease that one has acquired in repeating them, they mean more than they are. I am not astonished that Monsieur Descartes, requiring this from his sectators, those who having obeyed him having so shaped their mind to his philosophy, that it seems to me that they hold it closer to heart than he did himself. Even Lullists and Paracelsists end up persuading themselves that the gibberish they have stubbornly learned is founded on good reasons.¹²⁴

The terms "sectators" and "sectarian" recur in the letters of Chapelain in the 1660s to designate the Cartesians, and there is no doubt that, although the first term can simply designate the partisans of a philosophical school, it is used by Chapelain in a negative fashion. Thus, he notes that Descartes was "happy to have sectators who swore on his dogma and believed that which he did not believe himself."¹²⁵

It is not that they were totally uninterested in Cartesian physics. But beyond the fact that they only did so late in the game, their criticism has nothing to do with the lack of experiments in the physics of Descartes and the Cartesians. Thus what poses a problem for Sorbière is the possibility, in general physics, of obtaining the three Cartesian elements from the "large indefinite body" that is the extended matter. If it is possible to break it, then nothing can "stop my little bodies of a certain size and shape: rather than allowing indefinite division and imagining all of nature as a large, fluid, permeable body, whose motions can divide it at any point of these dimensions."¹²⁶ Likewise, Chapelain believes that the affirmation that a vacuum is impossible leads to contradictions, but does not engage at all in the question of whether the explanations proposed by Descartes are or are not plausible with respect to experiments:

The quarrel he [Descartes] had with Monsieur Gassendi, my intimate friend, made me want to look again with more attention at his system. I found great brilliance, great novelty, and a happy use of the ancient doctrine of Democritus for the multiplication of worlds and the modern experiments with magnets for the constitution of his machine, as well as beautiful applications of the nature of motion that could only arise from a greatly inventive mind clever at using everything to his ends. But I was stopped first by his *postulata* as to the creation of matter, which is less that of a physicist than a theologian, and of motion, which he made without admitting the least vacuum, which according to his own positions I found impossible, and I consequently saw that everything he deduced for the formation of worldly bodies and for particular generations fell apart, and in falling ruined all his claims. I also judged that what made him exclude the vacuum from his universe was only to have the means of explaining light and the tides, and that without this ambition, by admitting the smallest vacuum in the great plan ["plan" says the French; perhaps one should read "plein", i.e., "fullness"], he could have moved his illusion forward and made it if not true, at least beautiful and plausible enough for it to not be easily shown to be false.¹²⁷

In the last third of the seventeenth century, what I call the "moral" criticism of Descartes and the Cartesians becomes more acute, for example with Huygens and Leibniz, and there is clearly a backlash from the worldly success of Cartesianism and the propaganda of a now well-defined Cartesian party. Huygens, when he reads

¹²² On the experimental practice of Gassendi, see Rochot 1964; as for his judgment on Cartesian physics, he responds to Rivet who asked him for a critique (*censura*) of the *Principia philosophiae* that it was superfluous, for such a work should die before its author, see Gassendi 1964, VI, 217.

¹²³ Sorbière to Petit, 10 November and 10 February 1657, in Sorbière 1660, 679 and 691. Conversely, Clerselier 1667 notes that Roberval refused to put in writing his objections to Descartes.

¹²⁴ Sorbière to Petit, 10 November 1657, in Sorbière 1660, 679–680. For other passages where Descartes is described as a "head of a sect," see Sorbière to Saumaise, 10 March 1650 and to Petit, 10 February 1657, in Sorbière 1660, 535, 691. In the correspondence to Mersenne, Descartes is compared to Fludd, see Sorbière to Mersenne, 15 April and 23 December 1647, in Mersenne 1934–1988, XV, 201, 585–587.

¹²⁵ Chapelain to Carrel de Sainte-Garde, 27 May 1662, in Chapelain 1880–1883, II, 235–236. The word "sectators" describes Cartesians in Chapelain to Heinsius, 6 February 1659, to Carrel de Sainte-Garde, 15 December 1663 and to Bernier, 16 February and 26 April 1669, in Chapelain 1880–1883, 17, 341, 622, 640.

¹²⁶ Sorbière to Saumaise, 10 March 1650, in Sorbière 1660, 536.

¹²⁷ Chapelain to Carrel de Sainte-Garde, 16 February 1662, in Chapelain 1880–1883, II, 203–204. Carrel de Sainte Garde published in 1663 his *Lettres contre la philosophie de Descartes*.

Baillet's *Vie de Monsieur Descartes*, notes that unlike Galileo, Descartes wanted to establish himself as the "head of a sect" and the "author of a new philosophy." Descartes is at fault not for being mistaken, for all errors can be corrected, but for having presented what he proposed as if it were truths established once and for all, so much so that "those who believe him and have become his sectators imagine that they possess the knowledge of the causes of everything, as much as it is possible to know them. Thus they often waste time supporting their master's doctrine, and do not study how to penetrate the true reasons for this great number of natural phenomena about which Descartes came out only with illusions."¹²⁸ A year later, in his letters to Gerhard Meier (1646–1703), it is those who claim to be Cartesians that Huygens particularly attacks, for they are mistaken when they think it possible to protect all the theories of this man of great genius (*omnia viri ingeniosissimi dogmata*).¹²⁹

Leibniz's verdict is quite similar. In a letter to Huygens, which also followed the reading of the work of Baillet, Leibniz, who had also noted that "Monsieur Descartes had a strange ambition of becoming the head of a sect," notes that "...the Cartesians are too pre-convinced of their hypotheses. I prefer a Leeuwenhoek who tells me what he sees than a Cartesian who tells me what he thinks," adding, "It is nonetheless necessary to join reasoning to observations."¹³⁰ When he addresses himself to Paul Péllisson (1624–1693) or Claude Nicaise (1623–1701) in the *Journal des savants*, Leibniz insists that the Cartesians are unfaithful to the genius of Descartes in becoming his sectators and his "paraphrasists":

I have infinite esteem for Monsieur Descartes, and I recognize his value perhaps better than some who declare themselves Cartesians.... But I find that nothing brings more harm to the sciences than the spirit of a sect and than servitude. And indeed, the Cartesians find almost nothing new, and barely progress.¹³¹
The best response that the Cartesians could make would...to get rid themselves of the spirit of a sect, always contrary to the advancement of science.... of taking on experiments and demonstrations instead of general reasonings that serve only to maintain idleness and cover ignorance. They should try to take a few steps forward and not content themselves with being simple paraphrasists of their master. They should not neglect or despise anatomy, languages, criticism, for want of knowing their importance and value.... I would add that I do not know and by what star, whose influence is the enemy of all sorts of secrets, the Cartesians have done almost nothing new, and that almost all the discoveries have been made by people who are not Cartesians. I know only the little pipes of Monsieur Rohault that deserve the name of a discovery by a Cartesian. It seems to me that those who are attached to a single master thus reduce themselves by this sort of slavery, and conceive of almost nothing after him.¹³²

But the moral critique of the Cartesian's herd mentality is joined by an epistemological critique on the role of experiments in Cartesian physics. This was in particular the case for the member of the Académie des sciences Edme Mariotte (1620–1684), whose radical experimentalism I have studied elsewhere.¹³³ Three of the reasons given by him in his *Essai de logique* published anonymously in 1678 for the lack of progress made by physics came from the negligence of experiments by Cartesians. First, notes Mariotte, "several philosophers" stubbornly search for the causes of the principles of experience, when instead it would be better to use these principles as principles, in other words, to use them to deduce several "beautiful consequences." The examples he later gives of cases where this error has been committed leave no room for doubt as to the fact that he's thinking of Cartesians: he mentions the Cartesian type of explanation of elasticity and magnetism.¹³⁴ A second reason for the lack of progress in physics can also be attributed to Cartesians: "most philosophers," affirms Mariotte, are infatuated with a few hypotheses that are insufficiently established from an experimental perspective, yet want to use them to explain everything.¹³⁵ Although he gives no examples of this fault, from his physics essays we see that one of these poorly established hypotheses was the Cartesians' subtle matter.¹³⁶ The case is even clearer for a third cause of the lack of progress in physics, the way in which some claim to explain a natural effect by a single cause, when several causes contribute to producing it. Mariotte follows this diagnostic by a summary of some of the propositions of his *Traité de la percussion* that establish that one cannot explain the communication

¹²⁸ Huygens 1888–1950, X, 404–405.

¹²⁹ Huygens to Meier, June 1691, in Huygens 1888–1950, X, 104. See also to Meier, 26 March 1691, in Huygens 1888–1950, X, 54.

¹³⁰ Leibniz to Huygens, 20 February 1691, in Huygens 1888–1950, X, 52. The notes of Leibniz on Baillet can be found in Leibniz 1961, IV, 315–324.

¹³¹ Leibniz to Pelisson, 18 March 1692, in Leibniz 1923–, I–7, 292.

¹³² *Journal des savants*, 13 April 1693, 163–164. On the fact that Cartesians did not discover anything, see Leibniz to Gallois, [1677] and to Malebranche, 22 June 1679 in Leibniz 1923–, II–1, 569, 717; to Swelingius, in Leibniz 1961, IV, 329–330.

¹³³ Roux 2011.

¹³⁴ Mariotte 1992, 97, 103.

¹³⁵ Mariotte 1992, 98.

¹³⁶ Mariotte 1717, I, 170–171; II, 341.

of motion between bodies solely by the conservation of motion, one of the Cartesian laws of nature, or even the fundamental Cartesian law of nature.¹³⁷

It is known that in their teaching at least, the Jesuits made way for new observations and experiments that were performed in the first half of the century;¹³⁸ in these conditions one should not be surprised if, in the polemical texts they wrote against Descartes after the condemnation of his writings, some of them insisted on his inability to take seriously the experimental character of the new science. The *Lettre d'un philosophe à un cartésien de ses amis*, published anonymously in 1672, is here the most telling. It seems to have been published by Antoine Rochon (1637-??), a professor of philosophy in Bordeaux and *prédicateur* in Toulouse who finally quit the Jesuits for the Benedictines in 1685. It is said that another Jesuit scientist, Ignace-Gaston Pardies (1636–1674) polished up Rochon's book.

It's quite interesting that, far from being content to deal solely with matters of faith, Rochon defends a certain way of proceeding in physics. Thus he opposes Cartesians first with the recent experimental discoveries that go against Descartes' physics. Steno and the members of the *Académie des sciences* have shown, "unless one has more deference for the idea of Monsieur Descartes than for the testimony of ones own eyes," that no nerve reaches the pineal gland, that no valve explains the motion of our limbs, and that Mariotte has shown that the optical nerve does not end on the retina.¹³⁹ Descartes rested his entire physics on top the laws of motion, but the "author of the *Discours du mouvement local* [Ignace-Gaston Pardies] claims to show that of the seven rules of motion Monsieur Descartes wished to establish, only one is true."¹⁴⁰

Rochon then considers the argument of the new philosophers that Aristotelian qualities, virtues and forms do not bring any new knowledge.¹⁴¹ Against this argument, Rochon insists, in the specific instance of the growing of plants, that Cartesian explanations add nothing to what everybody knows. If the explanation of a given phenomenon is to say that certain corpuscles animated by certain motions produce this phenomenon, it has no informational content.

Everything you say gives no knowledge of the particular and of what is in fact in a plant. You content yourselves by saying that the pores are arranged *in a certain manner*, that they are *of a certain figure*, that the parts that *conform to certain openings* pass through, and that others are stopped. You try to get away with "*a certain*." But if I ask you what this certain figure is, and what the certain manner and what is this certain juice and these certain parts, you have nothing to say other than that you know no more.¹⁴²

Pointing out that Descartes had challenged the Jesuits to find an issue on which his philosophy would not be more satisfactory than the philosophy of the School, Rochon challenges a whole assembly of Cartesians to explain the simplest thing, the formation of a pumpkin in a single night. His prognosis is, as one might guess, that they will be unable to do better than to parade once again with their "certain figures," "certain shaped pores," "certain motions" and "certain ways".¹⁴³

Rochon notes at this point that the Cartesians claim to anticipate the outcome of experiments (*prévenir l'expérience*):

It is true that your gentlemen do wonders when they can latch on to an experiment they have done a hundred times to be sure of it. In that case they are happy to show the beauty of their doctrine by anticipating, they say, the experiment [*en prevenant, disent-ils, l'expérience*], and by showing that it must follow their principles. This is called devining everything one sees and precisely predicting the past. I never hear of this advantage they give themselves of anticipating the effects of nature without remembering what happened to Cardano.¹⁴⁴

If Cartesians are to be compared to Cardano, it's because Cardano would defend his rules in astrology by drawing up the horoscopes of the dead. When he tried however to extend his computations to future events concerning the living, what actually happened obliged him to resume his computations retroactively to adapt them to the actual events. Thus, if it is true that "in the experiments they have already done," "everything is in marvelous agreement with nature," to be truly convincing, Cartesians would need to be able to say "what would happen if one carried out a certain experiment that I suggest to them, and that they probably had never done."¹⁴⁵

¹³⁷ Mariotte 1992, 98.

¹³⁸ Brockliss 1995a, 454–456; 1995b, 190–194, 199, 209–216.

¹³⁹ Rochon 1673, Sect. 47, 120–122.

¹⁴⁰ Rochon 1673, Sect. 48, 122–124.

¹⁴¹ Rochon 1673, Sect. 50, 128–129.

¹⁴² Rochon 1673, Sects. 59–60, 140–144, and 142 for the quotation.

¹⁴³ Rochon 1673, Sect. 84, 194–196.

¹⁴⁴ Rochon 1673, Sect. 85, 197–198. "Prévenir l'expérience" is what Rohault claimed to do with his third sort of experiment, see above.

¹⁴⁵ Rochon 1673, Sect. 85, 202.

Thus it is in the period 1670–1690 that was constituted the affirmation that Cartesianism is essentially in contradiction with experimentalism, an affirmation destined to become a cliché in the eighteenth century.¹⁴⁶ Where there's smoke there's fire applies to the Cartesians when faced with the first scientific institutions focused on experimentation and observation, but this certainly does not apply to Descartes himself, unless insofar as he had become in the hands of the Cartesians the object of a paraphrase and a Scholastic teaching. This explains in part the contradictory verdicts given to the function of experiments in the Cartesian sciences.

The fate of an expression can illustrate this. We know that Descartes presented the *World* as a fable, and that in the letter-preface that he wrote in 1647 for the *Principles of Philosophy* he recommended to his readers that they read the book “first in whole as if a novel,” that is to say, as he says himself, all at once, not interrupting ones reading, and suspending the question of truth.¹⁴⁷ Readers critical of Descartes quickly adopted this expression to ridicule him. Thus we know the witticism of Pascal as reported by: “the late Monsieur Pascal called Cartesian philosophy the novel of nature, more or less the same as the story of Don Quixote.” In the years that concern us, we can find this idea everywhere, in Chapelain, Sorbière, Huygens, Leibniz, or Charles Perrault.¹⁴⁸

3.5 Conclusions

In the great silence of their libraries and classrooms, today's historians no longer see the conflicts that marked the history of science, or at least, they cannot perceive what was so clear and distinct about them. Even when they develop an irenic vision of this history, they can be tempted to project on certain moments of the past lines of conflict that did not yet exist, or that on the contrary, had in the meantime disappeared.

The doctrines of empiricism and experimentalism, even if they could be defined in absolute terms, as I did at the beginning of this chapter, must be the object of a historical modulation when they are used to characterize a philosopher with respect to the philosophers of his time. Indeed, this kind of characterization is necessarily relative, for it refers to positions that existed at that time. The main idea of this chapter is in particular that the kind of experimentalist commitment there was at the time of Descartes was no longer the case in the 1660s: the very existence of the *Académie des sciences* and the scientific movement that had prepared its foundation introduced significant modifications in the field of natural philosophy. Rohault and Descartes had the same epistemological positions, but they are modulated differently when compared to the field formed by the philosophical positions of the 1630s or that of the 1660s.

It would nonetheless be a mistake to draw from this chapter ammunition to strengthen the historiographic prejudice according to which France was blinded by a rationalism so blinkered that it could claim to be universally applicable, while England benefited from the lights of full-fledged experimentalism. I believe on the contrary, as I indicated in the second part of this chapter, that the epistemological styles of France of the first *Académie des sciences* and of England of the first Royal Society are identical, at least with regard to the use of experiments. Many things have masked this similarity: the philosophical confusion between empiricism and experimentalism, or the confusion between carrying out experiments and having a doctrine of experimentation, the difficulty of distinguishing three hundred years later between the social and intellectual issues of the 1630s and the 1660s, and finally, the existence of founding national myths, with their pantheons of great men. It was not however my aim in this chapter to explore all these issues.

References

- Alcover, Madeleine. 1990. *Cyrano revu et corrigé (Lettres, Etats du Soleil, Fragment de physique)*. Genève: Droz.
Azouvi, François. 2002. *Descartes et la France. Histoire d'une passion nationale*. Paris: Fayard.
Bacon, Francis. 1996—. *The Oxford Francis Bacon*, eds. G. Rees and L. Jardine, 15 vols. Oxford: Clarendon Press.

¹⁴⁶ Voltaire to Maupertuis, 1 October 1738, in Voltaire 1968–1977, V, 307–308: “if one had to get into this other and no less frivolous question, which one nonetheless agitates, of knowing who was the greater physicist, Descartes or Newton, it would be enough to consider that Descartes almost never carried out experiments.... If one wanted to discuss the physics of Descartes, what could one perceive there other than hypotheses?”

¹⁴⁷ AT XI 31, 48.

¹⁴⁸ See the references given in Roux 2006, n40.

- Baillet, Adrien. 1691. *La vie de M. Descartes*, 2 vols. Paris: Daniel Horthemels.
- Béguin, Katia. 1999. *Les Princes de Condé. Rebelles, courtisans et mécènes dans la France du Grand Siècle*. Seyssel: Champ Vallon.
- Borch, Ole. 1983. *The Journal of the Danish polyhistor Ole Borch, ed. with introduction and indices by H.D. Schepelern*, 4 vols. Copenhagen, London: Reitzels Forlag, Brill.
- Bougerel, Joseph. 1737. *Vie de Pierre Gassendi*. Paris: Jacques Vincent.
- Boyle, Robert. 2001. *The Correspondence of Robert Boyle*, eds. Michael Hunter, Antonio Clericuzio, Lawrence M. Principe. London: Pickering & Chatto.
- Brockliss, Laurence W. B. 1995a. Descartes, Gassendi, and the Reception of the Mechanical Philosophy at the French Collèges de Plein Exercice, 1640–1730. *Perspectives on Science*, vol. 3 (4):450–479.
- Brockliss, Laurence W. B. 1995b. Pierre Gautruche et l'enseignement de la philosophie de la nature dans les collèges jésuites français vers 1650. In *Les Jésuites à la Renaissance*, éd. Luce Giard, 187–219. Paris: Presses universitaires de France.
- Brown, Harcourt. 1934. *Scientific Organizations in Seventeenth Century France*. New York: Russel and Russel.
- Chapelain, Jean. 1662. Liste de quelques gens de lettres vivans en 1662. Composée par ordre de M. Colbert, par M. Chapelain. In *Memoires de littérature et d'histoire. Tome second*, ed. P. Desmolets. Paris: Noyons, 1749.
- Chapelain, Jean. 1666. Lettre de M. Chapelain à M. Colbert dans lesquelles il expose son sentiment sur les divers projets de création et d'encouragement que méditait le ministre en faveur des arts et des sciences. In *Lettres, instructions et mémoires de Colbert*, ed. Pierre Clément, 8 vols. Paris: Perrin, 1861–1882, Vol. V, 513–514.
- Chapelain, Jean. 1880–1883. *Lettres de Jean Chapelain*, ed. Philippe Tamizey de Larroque, 2 vols. Paris: Imprimerie Nationale.
- Chapelain, Jean. 2005. *Les lettres authentiques à Nicolas Heinsius (1649–1672). Une amitié érudite entre France et Hollande*, ed. Bernard Bray. Paris: Honoré Champion.
- Clair, Pierre. 1978. *Jacques Rohault (1618–1672). Bio-bibliographie. Avec l'édition critique des Entretiens sur la philosophie*. Paris: CNRS.
- Clarke, Desmond M. 1982. *Descartes' Philosophy of Science*. Manchester: Manchester University Press.
- Clarke, Desmond M. 1989. *Occult Powers and Hypotheses. Cartesian Natural Philosophy under Louis XIV*. Oxford: Oxford University Press.
- Clerselier, Claude. 1659. Préface. In *Lettres de Monsieur Descartes. Tome second*, ed. Claude Clerseilier. Paris: Charles Angot.
- Clerselier, Claude. 1667. Préface. In *Lettres de Monsieur Descartes. Tome troisième et dernier*, ed. Claude Clerseilier. Paris: Charles Angot.
- Clerselier, Claude. 1682. Préface. In *Œuvres posthumes de Rohault*, ed. Claude Clerseilier. Paris: Guillaume Desprez.
- Collas, Georges. 1912. *Jean Chapelain. 1595–1574. Étude historique et littéraire d'après des documents inédits*. Paris: Perrin.
- Dear, Peter. 1995. *Discipline and Experience. The Mathematical Way in the Scientific Revolution*. Chicago: Chicago University Press.
- Denis, Jean-Baptiste. 1668. *Lettre écrite à Monsieur Sorbière... par Jean Denis, touchant l'origine de la transfusion du sang et la manière de la pratiquer sur les hommes*. Paris: Jean Cusson.
- Des Chene, Dennis. 2001. *Spirits and Clocks. Machine and Organism in Descartes*. Ithaca, NY: Cornell University Press.
- Descartes, René. 1667. *Lettres de Monsieur Descartes. Tome troisième et dernier*, ed. Claude Clerseilier. Paris: Charles Angot.
- Dew, Nicholas. 2009. *Orientalism in Louis XIV's France*. Oxford: Oxford University Press.
- Duhamel, Jean-Baptiste. 1698. *Regiae scientiarum Academiae historia*. Paris: Jean-Baptiste Delespine.
- F. 1660–1661. *Conférences, recueillies par M. F. avocat qui y a, dit-il, ajouté du sien*. Mss 2225, Bibliothèque Sainte-Geneviève.
- Fontenelle, Bernard Le Bovier de. 1733. *Histoire de l'Académie royale des Sciences. Tome I. Depuis son établissement jusqu'à 1686*. Paris: Gabriel Martin, Jean-Baptiste Coignard et Hyppolite-Louis Guérin.
- Fontenelle, Bernard Le Bovier de. 1994. *Œuvres complètes. Tome VI*. Paris: Fayard.
- Foucher, Simon. 1675. *Critique de la recherche de la vérité. Où l'on examine en même temps une partie des principes de M^r Descartes. Lettre par un académicien*. Paris: Martin Coustelier.
- Garber, Daniel. 1988. Descartes, the Aristotelians, and the Revolution That Did not Happen in 1637. *The Monist*, 71 (4):471–486.
- Garber, Daniel. 2001. *Descartes Embodied. Reading Cartesian Philosophy through Cartesian Science*. Cambridge: Cambridge University Press.
- Garber, Daniel. 2001a. Descartes and Experiment in the *Discourse and Essays*. In *Descartes Embodied. Reading Cartesian Philosophy through Cartesian Science*, 85–110. Cambridge: Cambridge University Press.
- Garber, Daniel. 2001b. Experiment, Community, and the Constitution of Nature in the Seventeenth Century. In *Descartes Embodied. Reading Cartesian Philosophy through Cartesian Science*, 296–328. Cambridge: Cambridge University Press.
- Gassendi, Pierre. 1658. *Opera Omnia*, Lyon, 6 vols. Repr. Stuttgart: Friedrich Fromman Verlag, 1964.
- Goldstein, Catherine. 2008. Écrire l'expérience mathématique au XVII^e siècle ; la méthode selon Bernard Frenicle de Bessy. In *Réduire en art. La technologie de la Renaissance aux Lumières*, eds. Pascal Dubourg–Glattigny et Hélène Vérin, 213–224. Paris: Éditions de la Maison des Sciences de l'Homme.
- Hahn, Roger. 1971. *The Anatomy of a Scientific Institution. The Paris Academy of Sciences, 1666–1803*. Berkeley, Los Angeles, London: University of California Press.

- Hirschfield, John Milton. 1981. *The Académie royale des sciences. 1666–1683*, New York: Arno Press.
- Hobbes, Thomas. 1994. *Thomas Hobbes. The Correspondence: 1660–1679*. In *The Clarendon Edition of the Works of Thomas Hobbes*, vol. 2. Oxford: Oxford University Press.
- Huygens, Christiaan. 1888–1950. *Œuvres complètes de Christian Huygens*, ed. Société Hollandaise des Sciences, 22 vols. La Haye: Martinus Nijhoff.
- Koyré, Alexandre. 1953. An Experiment in Measurement. *Proceedings of the American Philosophical Society* 97 (2):222–237.
- Kuhn, Thomas. 1976. Tradition mathématique et tradition expérimentale dans l'évolution des sciences physiques. *Journal of Interdisciplinary History* 7:1–31.
- Le Bret, Henri. 1657. Préface. In *Histoire comique par Monsieur de Cyrano Bergerac. Contenant les Ests & Empires de la Lune*, ed. Henri Le Bret. Paris: Charles de Sercey.
- Le Dœuff, Michèle. 1984. Bacon chez les Grands au siècle de Louis XIII. In *Francis Bacon. Terminologia e fortuna nel XVII secolo*, ed. Marta Fattori, 155–178. Roma: Ateneo.
- Le Gallois, Pierre. 1672. *Conversations tirées de l'Académie de M. l'Abbé Bourdelot contenant diverses recherches et observations physiques*. Paris: Thomas Moette.
- Leibniz, Gottfried Wilhelm. 1961. *Die philosophischen Schriften von G. W. Leibniz (1671–1702)*, ed. C. I. Gerhardt, 7 vols. Repr. Hildesheim-New York, G. Olms.
- Malebranche, Nicolas. 1958–1967. *Œuvres de Malebranche*, ed. André Robinet, 20 vols. Paris: Vrin.
- Mariotte, Edme. 1717. *Œuvres de Mr Mariotte, de l'Académie Royale des Sciences, divisées en deux tomes, Comprenant tous les Traitez de cet Auteur, tant ceux qui avoient déjà paru séparément, que ceux qui n'avoient pas encore été publiez*. Leiden: P. van der Aa.
- Mariotte, Edme. 1992. *Essai de logique*, eds. Alan Gabbey and Guy Picolet. Paris: Fayard.
- Mazauric, Simone. 2007. *Fontenelle et l'invention de l'histoire des sciences à l'aube des Lumières*. Paris: Fayard.
- McLaughlin, Trevor. 1974. Une lettre de Melchisédec Thévenot. *Revue d'histoire des sciences* 27 (2):123–126.
- McLaughlin, Trevor. 1975. Sur les rapports entre la Compagnie de Thévenot et l'Académie royale des Sciences. *Revue d'histoire des sciences* 28 (3):235–242.
- McLaughlin, Trevor. 1977. Le concept de science chez Jacques Rohault. *Revue d'histoire des sciences* 30 (3):225–240.
- McLaughlin, Trevor. 1996. Was there an Empirical Movement in Mid-seventeenth Century France? Experiments in Jacques Rohault's *Traité de physique*. *Revue d'histoire des sciences* 49 (4):459–481.
- McLaughlin, Trevor. 2000. Descartes, Experiments, and a First Generation Cartesian, Jacques Rohault. In *Descartes' Natural Philosophy*, eds. Stephan Gaukroger, John Schuster and John Sutton, 330–345. London, New York: Routledge.
- Mersenne, Marin. 1932–1988. *Correspondance du P. Marin Mersenne, religieux minime*, commencée par Paul Tannery, publiée et annotée par Cornélis de Waard et Armand Beaulieu, 17 vols. Paris: Éditions du CNRS.
- Mesnard, Jean. 1963. Pascal à l'Académie Le Pailleur. *Revue d'histoire des sciences* 16 (1):1–10.
- Milhaud, Gaston. 1921. *Descartes savant*. Paris: Alcan.
- Montconys, Balthazar de. 1665–1666. *Journal des voyages de Monsieur de Monconys*, 3 vols. Lyon: Horace Boissat & George Remeus.
- Moreau, Denis. 1999. *Deux cartésiens. La polémique entre Antoine Arnauld et Nicolas Malebranche*. Paris: Vrin.
- Moreri, Louis. 1759. *Le Grand dictionnaire historique ou Mélange curieux de l'histoire sacrée et profane. Tome Neuvième*. Paris: Les Libraires associés.
- Oldenburg, Henry. 1965–1973. *The Correspondence of Henry Oldenburg*, eds. and trans. Alfred Rupert Hall et Marie Boas Hall. 9 vols. Madison, Wisconsin-London: University of Wisconsin Press.
- Pascal, Blaise. 1663. *Traitez de l'équilibre des liqueurs et de la pesanteur de la masse de l'air*. Paris: Guillaume Desprez.
- Perrault, Charles. 1666. Note de Charles Perrault à Colbert pour l'établissement d'une académie générale. In *Lettres, instructions et mémoires de Colbert*, ed. Pierre Clément, 8 vols. Paris: Perrin, 1861–1882, Vol. V, 512–513.
- Pintard, René. 1951. Autour de Pascal: l'Académie Bourdelot et la querelle du vide. In *Mélanges d'histoires littéraires offerts à Daniel Mornet*. Paris: Nizet.
- Rochon, Antoine. 1673. *Lettre d'un philosophe à un cartésien de ses amis*. Paris: Estienne Michallet.
- Rochot, Bernard. 1964. Gassendi et l'expérience. In *Mélanges Alexandre Koyré*. Vol. II, 411–422. Paris: Hermann.
- Rohault, Jacques. 1681. *Traité de physique*. 2 vols. Lyon: Claude Galbit.
- Rohault, Jacques. 2009. *Physique nouvelle (1667)*, ed. Sylvain Matton. Paris, Milan: Séha, Archè.
- Rohault, Jacques. 1987. *System of Natural Philosophy, Illustrated with Dr. Samuel Clarke's Notes, taken mostly out of Sir Isaac Newton's philosophy*. New York and London: Garland Publishing.
- Roux, Sophie. 1998. Le scepticisme et les hypothèses de la physique. *Revue de synthèse*, 119 (2–3):211–255.
- Roux, Sophie. 2006. La philosophie naturelle à l'époque de Le Nôtre. Remarques sur la philosophie mécanique et sur le cartésianisme. In *Fragments d'un paysage culturel, André Le Nôtre, Institutions, arts, sciences et techniques*, ed. Georges Fohrat, 98–111. Sceaux: Musée de l'Île-de-France.
- Roux, Sophie. 2011. *L'Essai de logique de Mariotte. Archéologie des idées d'un savant ordinaire*. Paris: Classiques Garnier.
- Roux, Sophie. 2012. A French Partition of the Empire of Natural Philosophy (1670–1690). In *The Mechanisation of Natural*

- Philosophy*, eds. Daniel Garber and Sophie Roux, 55–98. New York, Dordrecht, Boston, London: Kluwer Academic Publishers.
- Roux, Sophie. 2013. Pour une conception polémique du cartésianisme. Ignace-Gaston Pardies et Antoine Dilly dans la querelle de l'âme des bêtes. In *Qu'est-ce qu'être cartésien?*, ed. Delphine Kolesnik, 315–337. Lyon: Presses de l'ENS de Lyon.
- Savérien, Alexandre. 1783. *Histoire des philosophes modernes. Tome VI. Histoire des physiciens*. Paris: Bleut and Guillaume.
- Schmaltz, Tad. 2002. *Radical Cartesianism: The French Reception of Descartes*. Cambridge: Cambridge University Press.
- Shapin, Steven and Simon Schaffer. 1985. *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*. Princeton: Princeton University Press.
- Sorbière, Samuel and Du Prat, Abraham. Règlement de l'assemblée de physiciens, qui se fit à Paris, chez Monsieur de Montmor l'an 1657. 1657. In Sorbière à Hobbes, 1^{er} fév. 1658, in Sorbière 1660: 633–634. To be found also in Huygens 1888–1950, IV, 514–515.
- Sorbière, Samuel. 1660. *Lettres et Discours sur diverses matières curieuses*. Paris: François Clousier.
- Sorbière, Samuel. 1663. Discours prononcé le 3 d'avril 1663. À l'ouverture de l'Académie de physiciens qui s'assemblent tous les mardis chez Monsieur de Montmor. In Guillaume Bigourdan, Les premières réunions savantes de Paris au XVII^e siècle, *Comptes rendus hebdomadaires des séances de l'Académie des sciences*, 164, séances du 22 et du 29 janvier 1917:159–162 and 216–218.
- Sorbière, Samuel. 1694. *Sorberiana sive Excerpta ex ore Samëlis Sorbiere*. Toulouse: Colomeis.
- Taton, René. 1966. *Les origines de l'Académie royale des sciences, Conférence du Palais de la Découverte, 15 mai 1965*. Paris: Palais de la Découverte.
- Thévenot, Melchisédech. 1681. Discours sur l'art de la navigation. In *Recueil de voyages de Mr Thévenot dédiés au Roi*. Paris: Estienne Michallet.
- Thévenot, Melchisédech. 1694. *Bibliotheca Thevotiana sive Catalogus impressorum et manuscriptorum Liborum Bibliothecae viri Clarissimi D. Melchisedecis Thevenot*. Paris: Florentin et Pierre Delaulne.
- Voltaire, François-Marie Arouet. 1968–1977. *Les Œuvres complètes de Voltaire*. Geneva, Banbury & Oxford: Institut et Musée Voltaire & Voltaire Foundation.
- Zittel, Claus. 2009. *Theatrum philosophicum. Descartes und die Rolle ästhetischer Formen in der Wissenschaft*. Berlin: Akademie Verlag.