# The Dynamics of Experimentation and its Role within a Philosophy of Scientific Practices

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## 1. Introduction

It's already been twenty-five years since Ian Hacking stated, provocatively, that philosophers should *start* to think about the adventure that began back in the 17th century. He meant modern science, of course —what was then termed "experimental philosophy." Hacking was intimating that the whole tradition in philosophy of science (including all 20th century proposals up until Lakatos, Laudan, and the semantic and structuralist conceptions, at least) is profoundly inadequate to analyse the scientific phenomenon. And the main reason would be that philosophers have not elaborated the tools for an adequate understanding of experimentation —nor therefore of its role in theory formation.

During these 25 years, a small trend of "experimentalist" authors has grown, among them names such as Hacking, Franklin, Cartwright, and also historians like Heilbron, Galison, Buchwald, Steinle —not to forget a good number of sociologists such as Collins, Schaffer, Pickering, etc. In my opinion, the emergence of *new experimentalism* has been one of the most exciting recent development in the theory of science, if not *the* most fascinating. The reaction among the community of specialists and teachers of philosophy of science still seems disappointing to me, being so scarce as it is. But perhaps we can be confident that the situation is changing.

Science's old name, "experimental philosophy," suggests already that modern science can be regarded as a hybrid of philosophy (logic, theory, argument) and experiment (intervention, technics, observation). Of course presenting it this way involves a simplification, but I believe it constitutes a useful idealisation, if our purpose is to provide a rudimentary model of how the cognitive activities of scientists are structured. (Above all, such a model hides the role of interactions among members of the scientific community, and between them and other social actors. However, the model can integrate social factors, especially if we understand that knowledge —data and models and theories— is a social product almost by definition.)[1]

Following that suggestion, let me propose a *triadic model* of scientific activity based on considering *three 'phases' in its cognitive processes*, or three broad *categories of scientific practices* (which no doubt would have to be subdivided into finer types): *theoretical* activities, *experimental* activities —subsuming here the particular case of observations— and also *communicational* activities. This in itself cannot sound new, but the key idea is to emphasize that the experimental 'phase' is not reduced or subordinated to the theoretical one, and that it calls for a deeper and novel analysis. Furthermore, both types of activities are in interaction, and the complexity of those interactions still defies philosophers of science.

Such a scheme is very different from the views offered by traditional authors, which by following the linguistic turn and emphasizing logic were led to something like a model where there is one principal 'phase' of theory formation, merely punctuated by the injection of basic statements (corresponding to what are usually called observational data). What is characteristic of experimental and observational activities remained outside the philosopher's analysis, be it because it was considered transparent (as with empiricism) or regarded as exasperatingly swampy (as Popper liked to say). In joint work with Javier Ordóñez, we have criticized such models for their *theoreticism*, and we have also traced the origins of this tendency back to philosophically inclined theoretical physicists such as Boltzmann.[2]

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[1] Which does not mean (beware the *non sequitur*) that the analysis of knowledge can be merely sociological.

[2] Cf. Ferreirós, J. and Ordóñez, J., "Hacia una Filosofía de la experimentación," *Crítica*, v. 34, n. 102, (2002), pp. 47–86.

No doubt, betting for a philosophy of science that is able to analyse the experimental phase complicates matters for the aspiring philosopher, because it will force her to augment her panoply of tools. A fine analysis of the factors that enter into experimental activity should include questions belonging to the cognitive sciences; it cannot be reduced to a logical scheme, nor can it be treated in the style of the linguistic turn, and it is also insufficient to speak of "paradigms" or "values." Considered from this angle, the misery of theoreticism stems from the way it reduced the richness and complexity of scientific procedures to an affair merely of conceptual and theoretical elaboration.

A philosophy of science that closes its eyes to the epistemic specificity of experimental life will thereby renounce the goal of understanding what is most characteristic of scientific knowledge. Properly considered, this already offers an explanation for the peculiar situation we saw towards the end of the 20th century, when the rationalism and faith in progress of philosophers was confronted head-on by strong sociological approaches. The views originating in theoreticist and "logicistic" approaches to the philosophy of science were, *malgré lui*, feeding the sociologism of the 1980s and 90s. That is because of the way they promoted losing sight of the processes by which data are obtained (produced?) in science. They promoted excessive simplification of our models of scientific practice, and also rigidly formalistic conceptions of human rationality.[3]

In my opinion, the "third way" that can take us out of that bog consists in a reflection upon scientific practices, understood not as an attractive yet void formula, but rather as the decision to fully consider the epistemic and

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[3] The importance of formalistic rationalism as a stimulus for sociological conceptions is clear in the work of a central author like Harry M. Collins. See in particular his classic COLLINS, H. M., *Changing Order. Replication and Induction in Scientific Practice*, The University of Chicago Press, Chicago, 1992; original edition 1985). It is also abundantly known how simplified versions of the theses promoted by Popper, Lakatos, or Quine have been appropriated by specialists in STS or sociology of science.

cognitive specificity of scientific activities, and in particular experimental activities. The plural form is of the essence: *there is not scientific practice in the singular, but a plurality of coexisting practices, and the crux of the analysis has to do with their heterogeneous cognitive roots and their complex interactions.* It is for this reason that, as a first step and to counter their traditional oblivion, we must consider the roots and the dynamics of experimentation.[4] The way opened by studies of the philosophy of experiment opens a promising course for navigating the waters between the Scilla of theoreticism and the Caribdis of sociological reductionism.

## 2. Experiment and its Interpretation: The Basic Structure

In traditional speech about data and experimental results, these are presented as ready-made elements emerging from some black box. The possible structural and dynamical complexities in the *modus operandi* of that black box, what I shall call the "processes of data formation," are not a focus of attention. Indeed, the traditional idea of empiricists is that we are actually talking of a "white box:" a transparent process of reception of impressions, which ends up in an automatic and infallible disposition to formulate basic statements. On a completely different line, we find Popper's peculiar idea that basic statements are like pillars introduced from above (theories rule) into the "swamp" of observational and experimental work, pillars ultimately justified "by convention."[5] Although Popper never extracted the radical conclusions that this position is calling for, others (e. g., Lakatos) did.

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[4] Note that this formulation, properly understood, involves the theoretical phase, since both 'phases' are in almost constant interaction. But it emphasizes that which is still the least known and understood.

[5] Cf. POPPER, K. R., *Logik der Forschung*, Tübingen, Mohr, 1935, chapter 5. (Translated into English by the author with the assistance of Julius Freed y Lan Freed: *The Logic of Scientific Discovery*, Hutchinson, London, 1959.)

It will be worthwhile to pause for a note on terminology. Here, as in previous work, I follow the scientist's usual way of talking when it comes to experimental and observational data. An alternative terminology has been proposed by Bogen and Woodward, who contrast "data" and "phenomena" with connotations that are fundamentally different from mine, as their "data" mean the fluctuating outcomes of particular experimental trials, while "phenomena" are the stable constructs which are theories are meant to predict and explain.[6] So the reader should beware: *data* in my sense are the "phenomena" of Bogen, Woodward and others, which is why I emphasize the need to speak about *processes of data formation*.

In detailed considerations, it is customary to think that the production of an experimental result involves at least three elements —a material procedure, an instrumental model, and a phenomenic model:[7]

a) The *material procedure* is a complex of objects and actions, or interventions, performed practically in the material world: arranging the apparatus and the specimens, and making them function properly (i.e., setting them to work in the proper sequence *and controlling* their performance).

b) The *instrumental model* expresses a certain conceptual understanding on the side of the experimenter about how the apparatus works; this is central to the design, realization, and interpretation of the experiment. Such models can be of a highly theoretical and mathematical nature, but sometimes they depend on a modest amount of low-level theory.

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[6] Cf. BOGEN, J. and WOODWARD, J., "Saving the Phenomena," *The Philosophical Review*, v. 97, (1988), pp. 303–352. A good number of other philosophers (for instance, Mauricio Suárez) have adopted this peculiar terminology.

[7] See, e.g., PICKERING, A., "Living in the Material World," in GOODING, D., PINCH, T. J. and SCHAFFER, S. (eds.), *The Uses of Experiment*, Cambridge University Press, Cambridge, 1989, pp. 276–277.

c) The *phenomenic model* codifies basic elements of the way in which the experimenter understands conceptually aspects of the phenomenal world that are under study; without it, the results would lack sense and meaning and could not be interpreted. And again, phenomenic models do not always depend on high theory.[8]

To present these ingredients concretely, giving a clear and simple example that we shall continue using in the sequel —Newton's famous experiments on the decomposition of sunlight—, the three elements are as follows:

a') The material procedure includes the prisms (made of some or another kind of glass, sometimes filled with water), screens, procedures to modify the incident light (from simple holes on a window shutter, to lenses employed to colimate the light), etc.

b') The instrumental model is built upon an interpretation of the material procedure in terms of an antecedently established theory, geometrical optics, so that in this case it depends on high theory. (The model did not consider details about possible differences between different kinds of glass, and this was historically important.)

c') The phenomenic model is again formulated by means of geometrical optics, concretely by using the concepts of a ray of light and ideas about its behaviour upon reflection or refraction. The model assumed idealisations that are typical of geometrical optics, like ignoring the fact that shadows have fuzzy edges.

On this last point I should add a clarification. You know of course that Newton was a corpuscularist, believing light to consist in tiny corpuscles travelling at great speed, and that he opposed the wave theories that had been formulated at the time. However, in his optical writings he made an effort to establish key theses —in particular the principle that simple light

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[8] Remember, e. g., the "taxonomies" of Kuhn's late work.

rays are associated with colours and have a characteristic refrangibility—on a basis that was *neutral* with respect to the physical theories in dispute. This is why his phenomenic model does not presuppose corpuscularism and is based on geometrical optics, by then a classic theory, well established at least among "mathematicians."

Traditional images of experimentation would suggest that, at the stage of justification, the material procedure and the instrumental model remain fixed and unaltered. Their features would be relatively natural and uncontroversial, both for the particular scientist who first proposed them, and for the scientific community that must replicate the experiments and judge the results. Meanwhile, the phenomenic model would be more flexible or "plastic," since of course one allows for the possibility of competing theories defended by different scientists. Moreover, in what was traditionally (since the 19th century) presented as the prototype experiment, the main goal would be to measure in great accuracy some data to be contrasted with theoretical predictions, or perhaps some parameter fixed by theory (e. g., a physical constant, as a result of which the phenomenic model would be refined and specified to a greater level of precision).

But sociologists of science have challenged those assumptions, studying in detail cases where one finds the scientist showing almost no flexibility as regards the phenomenic model, but treating the other two components as very flexible indeed. Famous in this regard is Pickering's work on what he called "the hunting of the quark," some experiments performed by the Italian physicist Morpurgo during a period of 15 years. The studies of Harry Collins on the search for gravitational waves are also well known and have been celebrated.[9] Pickering concludes that the three

[9] For discussion and questions about the details of the case studies offered by Collins and Pickering, see FRANKLIN, A., "Experiment in Physics," in ZALTA, E. N., (ed.), *The Stanford*

structural elements a), b) and c) are equally *plastic resources* that, far from being fixed and determined, can be modified at will until a result of coherence is attained. He believes that experimental work begins in such a way that there *no definite* relation between the structural ingredients: "incoherence and uncertainty are the distinctive seals of experiment," as shown abundantly by studies of laboratory life. But at the end of the day, some form of non-trivial coherence is obtained, a *stabilization* such that "material procedures, (...) when interpreted through an instrumental model, produce facts within the framework of a phenomenic model."[10]

The analysis of such processes of interactive stabilization between the three structural elements constitutes what, in the sociologist's perspective, would correspond to our *dynamics of experimentation*. Any conclusion we may finally extract about experimental activity, be it about its epistemic relevance, or say its dependence upon contextual factors, will obviously hang on the characteristics attributed to the structural ingredients, and to their interrelations.

If Pickering's position is somehow typical, the main point in dispute today would no longer be the "social construction" of experimental results, a conception of sociological reductionism that has been superseded by many promoters of social studies of science. But there remain the hot problems of the epistemic reliability of experimental data, the extent to which they provide information on natural processes, as opposed to the possibility of vicious circles,[11] or a mere coherentist stabilization such as

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*Encyclopedia of Philosophy (Summer 2003 Edition)*, http://plato.stanford.edu/archives/sum2003/entries/physics-experiment/ (access on November 2007) See also the abridged Spanish version: FRANKLIN, A., "Física y experimentación," *Theoria*, v. 17, n. 44, (2002), pp. 221-242. An interesting exchange between Franklin and Collins took place in *Studies in History and Philosophy of Science*, v. 25, n. 3, (1994), pp. **.

[10] PICKERING, A., "Living in the Material World," in GOODING, D., PINCH, T. J. and SCHAFFER, S. (eds), *The Uses of Experiment*, pp. 277-278.

[11] Cf. COLLINS, H. M., *Changing Order. Replication and Induction in Scientific Practice*, passim.

described by Pickering.[12] All of this depends on whether the structural elements are "equally plastic" or not.

The coherentist thesis has been formulated again by Hacking, who speaks of a "self-vindication" of laboratory sciences, and presents the idea as a kind of expanded Duhem thesis.[13] If correct, the thesis of Pickering and Hacking would have noteworthy consequences. It would be definitive confirmation of the "theory-ladenness" of results, certainly in the company of their correlative "technics-ladenness" and "social-ladenness," but forcing us to abandon as elusive or noumenical —to abuse of Kant's terminology— any possible "nature-ladenness." Maybe the business of science would have its continuity and production of technological effects guaranteed (albeit one could not quite understand why), but from an epistemic point of view it would lack any special justification.

Points like those are thus crucial to any conclusion with respect to the epistemic reliability of the whole scientific enterprise, hence to the project of a philosophy of science. After all Einstein, even during his period of greatest enthusiasm for the theoretical and mathematical components of science (and although he was willing to grant that "the creative principle resides in mathematics"), emphasized that "experience remains, of course, the sole criterion of the physical utility of a mathematical construction." And some years later, Feynman would begin his lectures saying: "The principle of science, the definition, almost, is the following: *The test of all knowledge is experiment*. Experiment is the *sole judge* of scientific 'truth'."[14] So, is that "metaphysical" idea, the concept of Nature, totally foreign to this game?

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[12] Cf. PICKERING, A., *The Mangle of Practice*, The University of Chicago Press, Chicago, 1995.
[13] Cf. HACKING, I., "The Self-Vindication of the Laboratory Sciences," in PICKERING, A. (ed.), *Science as Practice and Culture*, The University of Chicago Press, Chicago, 1992, pp. 29-64.
[14] Quoted from the famous FRANKLIN, A., *Feynman Lectures on Physics*, Addison-Wesley, Reading, 1963, in FRANKLIN, A., "Experiment in Physics," in E. N. ZALTA (ed.), *The Stanford Encyclopedia of Philosophy*, note 1.

### 3. The "Experimenter's Regress"

Let us come back to Newton's famous experiments. Contrary to common lore, historians have established that his work on the composition of light, and in particular his *experimentum crucis*, were by no means an immediate success. Indeed, Schaffer has turned this case into another argument for the decisive influence of sociological factors in science's decision making. The *experimentum crucis* was contested during some fifty years, mainly —but not only— due to the difficulty of replicating its quantitative results.[15] A quick reading of the controversies suggests that Newton was arguing as follows: "simple" rays of light behaved according to his statements, but this could only be detected using "good" prisms, and "good" prisms were those which produced "simple" rays.

Thus the case is presented as a clear illustration of what Collins has termed the "experimenter's regress," that menaces the epistemic reliability of experimental results. The experimenter's regress of Collins consists in a vicious circle that stems from severe problems with the replication of experiments and the calibration of scientific instruments. The main problem is that correct results are only obtained using apparatus that functions properly, while the apparatus is functioning properly only if it provides correct results.[16] So in the last analysis the outcomes of a scientific controversy do not depend so much on what "Nature" has to "say," or on any special use of methods with some epistemic virtues, but on who is the experimenter in a social position of dominance, that enables her or him to determine what is correct and what functions properly. Collins offers as prototypical the case of J. Weber's experiments on gravitational waves,[17]

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[15] See SCHAFFER, S., "Glass Works: Newton's Prisms and the Uses of Experiment," in GOODING, D., PINCH, T. J. and SCHAFFER, S. (eds.), *The Uses of Experiment*, pp. **.

[16] Cf. COLLINS, H. M., *Changing Order*, chapters. 4 and 5.

[17] See COLLINS, H. M., *Changing Order*, ** and FRANKLIN, A., "Experiment in Physics," passim. Collins makes a lot of the statement that "there are no formal criteria" that could be

but the example of Newton's experiments, in Schaffer's interpretation, is by no means worse. In this case, Schaffer argues that it was well into the 18th century (around 1715), when Newton enjoyed a position of extraordinary influence as President of the Royal Society, that he displayed diplomatic activities ending up in the promotion of his scientific views in France.

I believe that this interpretation is incorrect, which incidentally shows that good history of science (such as Schaffer's) is still not sufficient for an in-depth philosophical analysis. I shall now offer my own revision of the case, where the third kind of practices mentioned above (practices of communication) plays an important role.

In good measure, the polemics generated by Newton's work and his *experimentum crucis* were caused by himself, by what we might call a youthful error in his strategy of argument. Retrospectively one can locate the main error, not in anything Newton did while *investigating* the matter, but in the way he *wrote* his first published paper on natural philosophy.[18] The 'error' was motivated by Newton's great experience with mathematical texts, and his lack of experience in physical controversies. The young Newton believed that he could solve the question in great brevity and full precision by writing *more geometrico*: two carefully planned experimental "demonstrations," together with a series of definitions and propositions, would suffice to convince his readers. (The reader should notice that modern protocols for doing and reporting experimental research only consolidated during the 19th century, while the millenary Euclidean style

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applied to decide whether the instruments are functioning properly. On this topic, see footnote 4 above.

[18] Cf. NEWTON, I. "A Letter of Mr. *Isaac Newton*, Professor of the Mathematicks in the University of Cambridge; Containing his New Theory about *Light* and *Colors*," *Philosophical Transactions of the Royal Society*, v. 80, (1672), pp. 3075-3087. Available on the web, see http://www.newtonproject.sussex.ac.uk/prism.php?id=47 (access on November 2007). Reprinted (among others) as "Letter to Mr. Oldenbourg on Light and Colours," in HORSLEY, S. (ed.), *Opera Quae Extant Omnia*, **, London, 1779–1785, vol. IV; reprinted by F. Frommann, Stuttgart, 1964.

of writing mathematics remained paradigmatic for scientists throughout the 17th and 18th centuries.)

Newton based all of his argument on two experiments, of which the first was extremely rudimentary, even though he offered some variations of the theme (one prism projecting an elongated image onto a screen), and the second was quite sophisticated, being offered in a single purportedly definitive version. This "*experimentum crucis*" employed two prisms and two screens by means of which a monochrome ray of light was isolated, with results that were meant to stamp the key proposition that sunlight consists in a mixture of *coloured rays with different specific refrangibilities*. The famous "crucial experiment" was thus made to support all of the weight of proof, single-handedly.

Both experiments turned out to be difficult to replicate, again in large measure because of their "mathematical" character, i.e., and perhaps surprisingly to the reader, precisely because the results were quantitative. The elongated image that Newton obtained in Cambridge was a palette of colours (an artificial rainbow) enlarged by a factor of 5, but the Jesuit Antoine Lucas working in Liège only obtained an elongation by a factor of 3. Was this perhaps because sunlight is different in both locations? When Lucas published his discrepancy in the *Philosophical Transactions*, Newton took it very seriously as an offence to his honesty as a gentleman and his reliability as a reporter of observed physical phenomena. The discrepancy was relevant in the context of discarding alternative explanations of the result, by an argument relying on Snell's law of refraction. Therefore Newton felt an imperious need to attack his opponent and annihilate him. Quite unfortunately, it never occurred to him that they could be confronting a real problem caused by differences in the nature of the prism's glass. As the crystalline composition of glasses produced in different places and factories differed greatly, it was plainly naïve to expect standardized quantitative results as an outcome!

Another source of difficulties was made manifest by a highly reputed French experimenter, Edme Mariotte, founding member of the *Académie des Sciences*. Around 1680, Mariotte set out to reproduce the supposedly "crucial" *experimentum crucis*, finding results that he interpreted to contradict and even refute Newton. Having isolated a "simple" violet beam of light, he obtained after the second refraction tones of red and yellow colouring both ends of the violet image.[19] What the English was inclined to consider an understandable imperfection of the experimental setting, was interpreted by his much more empiricistic colleague as a very clear contrary result, a vindication of the old theory of the modification of light by the prism (that Newton was intent on refuting). It was "evident" that in this experiment a ray of light of the kind that Newton called "simple" had been modified or shown to be complex. Given Mariotte's deserved reputation as an experimenter, this episode brought as a result a very long delay —almost 40 years— for the acceptance of Newton's theory in France and other places.

This time the discrepancy between both actors can be located in their instrumental and phenomenic models, or more precisely in what we may term —following Hempel— the "bridge principles" necessary for Newton's interpretation of the results. Their discrepancy measures the conceptual distance between the simple ray promoted in geometrical optics, hence in Newton's models, and the concrete beam of light that the experimenter was able to isolate. In a sense, the epistemic character of modern science was at stake: whether it was to be crudely empirical, based directly upon the observed in the style of Mariotte, or inextricably linked with mathematical idealisations, as Newton advocated. In the latter's opinion, the study of Nature had to be mathematical, and the narrowest beam obtained by an experimenter was, self-evidently, very far from the "simple" ray in the model. The corresponding adjustments were more than enough to explain away Mariotte's observations.

Such incidents show the enormous difficulties encountered by scientific research in its infancy, and make us wonder how it was possible to obtain any clear advances given all the material and technical difficulties: inexistence of standardized instruments, lack of experimental protocols, unreliability of the practical and intellectual training on the side of the *savants*. Little wonder that, if you wish to look for rhetorical elements in writings and letters from the time, you will find plenty of material that can be used for the conclusion that Collins' regress was fully in action, that the dispute was impossible to close except by an appeal to politico-diplomatic operations. A clear example of social construction and negotiation, it seems.

## 4. Complications in the Dynamics of Experiment

Is that really so? Were there elements that made it possible to break the vicious circle of Collins and Schaffer? I believe the answer is yes. In the present case, those elements were elaborated by Newton himself in the initial researches during the period 1666–1670, and were presented to the public mainly in the *Opticks* of 1704. Let me argue the case.

The two polemics mentioned above indicate two important aspects of the complex dynamics of experimentation. First, experiments *are dependent on technics*,[20] so that it has often happened like in Newton's

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[19] See GUERLAC, H., *Newton on the Continent*, Cornell University Press, Ithaca, 1981, pp. 98–99. Mariotte's work appeared as a book: MARIOTTE, E., *De la nature des couleurs*, **Paris, 1681; which can also be found in his *Oeuvres*, vol. 1, ** Leiden, 1717.

[20] I employ this uncommon term in order to try to capture the Spanish distinction between technology (a sophisticated form of technical development, dependent upon science) and

case: it is impossible to strengthen the experimental results without a simultaneous advancement of technics, and this complicates experimental work enormously. Knowing the composition of light made it necessary to learn about glass, its composition, and the techniques for its production. Without this process of refinement of glass production techniques, the later development of spectrography would have been impossible.[21] And of course, the process could not be completed quickly, but required many years. We have seen how this created considerable difficulties for the early attempts at quantification. And yet, all that is not sufficient to produce a vicious circle.

Second, we have encountered complications linked with the models employed (models of the experimental design and of the phenomena), and very especially difficulties linked with the "bridge principles," relating to the kind of theoretical development that was sought. Such problems could not be avoided, even with Newton's special effort to employ instrumental and phenomenic models that remained neutral between the theories in confrontation. Newton's idea was to force his results upon all parts, and so it happened with those who were favourable to mathematisation, to an alignment between physical optics and the other mathematical sciences. But these were not "all" parties.

Looking at the long duration, modern science has sided with Newton, with the option to go "beyond the appearances;" it has made a bet for mathematised theories. By contrast, Mariotte's case is reminiscent of the later criticisms of Goethe against the theory of colours: An option for the empirical and the visible, sometimes based on (geometrically) very poor arguments, but sometimes offering an intelligent critique of insistence on

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the more primitive and basic "technics" (in my language, respectively, *Tecnología* and *Técnica*).

the idea that *Natura* has an interior and an exterior, an apparent shell hiding a real content. Such insistence, however, triumphed due to the predictive efficiency of the models that were based on it, their high level of empirical adequacy, the considerable explanatory abilities shown, and not least their very important technological applications.[22]

But there are more aspects to be considered. Third, and already suggested, is the idea that the *qualitative aspects* of a series of experiments can be crucial. Experimental complexities have the effect that sometimes the attempt to quantify may be premature, as happened with Newton's experiments around 1670. This idea runs against the image of experimentation created in the 19th century, which depicts it mainly as quantification. The truth is that experiments always have an important qualitative component, which can be decisive not only in favouring some theory against some other, but also in supporting a certain interpretation of the experimental results themselves.[23] Qualitative aspects of experimentation are a crucial theme for some recent authors like F. Steinle.[24]

And fourth, a central aspect of experimental research, which was severely misrepresented by the inherited conceptions (here theoreticism was quite efficient in biasing and distorting): It is absolutely essential to take into account that data are not obtained automatically, instantly, or transparently. I believe one must speak about *processes of data formation*;

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[21] On this topic, see e.g., McGucken, W., *Nineteenth Century Spectroscopy*, Johns Hopkins University Press, Baltimore, MD, 1969, and Sánchez Ron, J. M. *Historia de la Física Cuántica*, Crítica, Barcelona, 2001.

[22] Nevertheless, sometimes one may be inclined to think that the search for the simple ultimate element (that holy grail of physicists) could be illusory, the product of an erroneous approach.

[23] This question was raised already in Kuhn, Th. S., "The Function of Measurement in Modern Physics," *Isis*, v. 52, n. 2, (1961), pp. 161-193. Reprinted in Kuhn, Th. S., *The Essential Tension*, The University of Chicago Press, Chicago, 1977, ch. VIII, pp. 178-224. A very interesting paper that unfortunately founds no continuation in his work.

one has to emphasize that experimental research must be analysed in terms of *series of experiments*. Examples can be multiplied at will, and although I shall continue focusing my discussion on the case of Newton, let me mention that of Pieter Zeeman and the celebrated effect he discovered in 1896 (the influence of magnetic fields on spectral lines, splitting them). A first successful experiment was far from convincing him, and he proceeded to make some others with the aim to control some variables (density, temperature, distribution of the substance emitting the radiation) that could conceivably affect the outcome. (Conceivably, that is, according to what theory suggested, or sometimes according to what analogies with other experiments suggested.) Zeeman wrote the following sentence, which clearly suggests the topic of *series* of experiments as the source of data:

"The different experiments ... make it more and more probable that the absorption —and hence also the emission— lines of an incandescent vapour are widened by the action of magnetism."[25]

Incidentally, Zeeman's early experiments were rather exploratory, guided by vague considerations about the possibility of an interrelation, but later his research was guided by Lorentz's theory that spectral lines are caused by the vibration of atoms, and at the same time his experiments brought important modifications and refinements into this theory.[26]

To come back to Newton, the point I want to make is that *his practice of argumentation differed from the practice of his experimental researches.*

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[24] See, e. g., STEINLE, F., "Challenging Established Concepts: Ampère and Exploratory Experimentation," *Theoria,* v. 17, n. 44, (2002), pp. 291-316.

[25] Pieter Zeeman in October 1896, quoted by ARABATZIS, T., *Representing Electrons: A Biographical Approach to Theoretical Entities*, The University of Chicago Press, Chicago, 2006, p. 176. On this topic see also BUCHWALD, J. Z. and WARWICK, A. (eds.), *Histories of the Electron: The Birth of Microphysics*, The MIT Press, Cambridge, MA, 2001.

[26] The intriguing predictions of Lorentz's theory of "ions" turned out to be correct, but the measurements of the ratio *e/m* showed that the intervening particles were much smaller than those in electrolysis, they had to be sub-atomical —and thus the "ions" became "electrons." See ARABATZIS, T., *Representing Electrons: A Biographical Approach to Theoretical Entities*, ch. 4.

As we saw above, his publication of 1672 was a clear bet for "decisive" experiments, carefully selected to support central elements of the theory; in this case, the *experimentum crucis*. In the midst of polemics, in 1776, he wrote: "For it is not the number of Exp[erimen]ts, but their weight that has to be considered; and when one may serve, what need is there of many?"[27]

With such rhetoric he was trying to stop his opponents from trying new designs of their own invention, and restrict their attention exclusively to the *experimentum crucis* with two prisms. But his own practice in the 1660s had tended to multiply trials, exploring different designs and possible influences, trying to control alternatives and variables.[28] It had been a long series of experiments, widely varied, which in my view reinforces the idea that *it is never the single isolated experiment, but a whole experimental series, in its complexity, what counts when it comes to establishing experimental results.*

Newton himself seems to have learnt the lesson well through the polemics of the 1670s, which he experienced as such an unpleasant thing. This may well be why the *Opticks* of 1704 is actually more similar to the university lectures of 1670–72 than to the famous paper, as far as the number and variety of experiments goes. While in the paper (Newton's letter to Oldenbourg, 1672) there was an attempt to base the key proposition —that sunlight consists in a mixture of rays of different refrangibilities— upon just one experiment, in the *Opticks* this is presented as a conclusion after 10 different experiments.[29] It was not only Newton in

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[27] Cited in GUERLAC, H., *Newton on the Continent*, p. 94.

[28] The available information is broad and of high quality, because both Newton's notebooks and his university lectures of 1670–1672 are available. A brief and precise summary can be found in the excellent work of WESTFALL, R., *Never at Rest: A biography of Isaac Newton*, Cambridge University Press, Cambridge, 1980, pp. 156–175, 211–222, and 237–252.

[29] Compare the exposition in NEWTON, I., "A Letter of Mr. *Isaac Newton*, ... Containing his New Theory about *Light* and *Colors*," with that found in his *Opticks*, S. Smith and B. Walford, London, 1704, pp. ** (32-62 of the Spanish edn.).

the 1660s, but the whole scientific community of his time, that needed a wide variety of experiments before the “data” concerning refrangibilities of the different rays could be accepted. This is no exception, but rather the rule, and that is why I have been talking about series of experiments and *processes of data formation*.

Newton’s liking for simple and “crucial” experiments (which was truly mathematical and very little Baconian) had its repercussion in later times, a long history. There emerged a tradition that counted some inheritors well into the 19th century, for instance, A. M. Ampère and W. Weber. But the tradition subsequently vanished. Here it is relevant that the 19th century was the time when experimental protocols were standardized and refined.

## 5. From the “Circle” to the Helix of Experimental Research

To the question whether there existed elements that could break the vicious circle of Collins and Schaffer, I have answered yes. Let me make it even more explicit.

The circle is broken, and turned into a helix, mainly in two ways. First, strengthening the reliability of the experimental results according to criteria that are properly experimental, i.e., characteristic of experimental activity (and not of the theoretical ‘phase’). And second, by Newton’s insistence on the idea that one had to be careful with the notion of a “simple” ray. Let us consider both aspects in some detail.

Here is a concrete example of the characteristic criteria of experimental practice at work. In order to show that the prisms did not modify visible light, but merely decomposed or analysed it, Newton performed a diversity of experiments: One employing two prisms juxtaposed in opposite senses; another with three prisms that projected their spectra against the same screen, in such a combination as to recover white light; a third with a prism followed by a lens that made the rays converge.

In the *series* formed by all these experiments performed by Newton around 1670 or earlier, we find at work two of the key elements that Hacking and Franklin have isolated as *properly experimental criteria.* (For this question of properly experimental criteria, one should see the pioneering work of Hacking and also Franklin's discussion of experimental strategies.)[30] Above all, there is a convergence of results obtained in three different ways, by distinct material procedures. And there is also a good measure of control over the interventions, which is obtained through planning based on the phenomenic and instrumental models that are employed. Such interventions can be guided by theories as in this case (guided ultimately by the clever use and application of principles of geometrical optics) but in other cases they can be much more exploratory in character, or they can even be suggested by mere analogy.

Notice also that the results of those three experiments, being qualitative and not quantitative, can be reproduced without the problems created by the different dispersive powers of the prisms (due to the kind of glass employed), and so many of the difficulties derived from lack of technical knowledge disappear. Newton employed also prisms filled with water in an attempt to reduce the doubts caused by difficulties in the precise replication of his quantitative results.

The second element that Newton employed in order to break the circle was his insistence on the idea that one had to be careful with the notion of a "simple" ray. He repeated it time and again, but the point was not (as Schaffer wants to picture it)[31] that only Cambridge prisms produced simple

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[30] Cf. HACKING, I., *Representing and Intervening*, Cambridge University Press, Cambridge, 1983, and FRANKLIN, A., "Experiment in Physics," passim. In my view, Franklin does not insist sufficiently on distinguishing what belongs to verbal argument from what constitutes cognitive factors that are characteristic of experimental research. See the comments in FERREIRÓS, J. and ORDÓÑEZ, J., "Hacia una Filosofía de la experimentación," pp. 47–86.

[31] Cf. SCHAFFER, S., "Glass Works: Newton's Prisms and the Uses of Experiment," pp. **.

rays —it was that neither those nor the ones in Paris or Liège produced them. The point is merely to insist upon the difference between the simple ray of the mathematical model, a geometric line, and the experiment's beam, something that can only be more or less coarse. If Mariotte did not understand or want to concede the point, it was because he rejected the use of mathematical models of the phenomena, in favour of crude empiricism. And there is, of course, evidence that other scientists accepted Newton's proposal of geometrical models, if only tentatively in view of their predictive success, and that the scientific community at large ended up favouring such models wholeheartedly.[32]

In that way, the circle did not come back onto itself, but rather —to exploit the geometric metaphor— it "regressed" on a slightly higher plane, forming a helix. A helicoid can seem a circle to us, when we look upon it from a certain biased angle, as our sociologists often do. But while circles are fundamentally retrograde, bringing us back to the same point time and again, helices progress by ascending from plane to plane. (Concerning the progressive connotations of this "helical" metaphor of scientific research, let me just say that my discussion concerns only local behaviour, and does not predetermine what may happen more globally in the development of a scientific discipline.)

## 6. Concluding Remarks

Despite all that has been said in the past, experimentation enjoys relative autonomy with respect to theory formation. I have been emphasizing that both are interacting 'phases' within the cognitive activity of scientists, and no one reigns above the other. Experiment brings into play epistemic or cognitive factors that are distinctive and characteristic. These are features that cannot be reduced to formal criteria, except of

[32] None of my remarks is meant to deny that the process was long, winded, and far from

course in the way in which the flight of a bird can be captured by a mathematical model. (Notice that "reduced" is not the adequate word, just like a "logicistic" insistence on basing everything upon explicit formal criteria can only impoverish the account and make it unable to deal with the richness of scientific activity.) One has to underscore that the "measures" of experimental validity or reliability are (in part) *intrinsic* to this kind of practice, and in this sense autonomous. The dynamics of the experimental phase is (partially) determined by its own peculiar restrictions.

I hasten to add that the dynamics of the theoretical phase is, in my opinion, also (in part) determined by its own peculiar restrictions. Here and at this crude level of analysis, there is no asymmetry between them. But if that analysis is correct, one has to conclude that experimental and theoretical practices complement and enrich each other. As emphasized at the start of this paper, there is not *scientific practice* in the singular, but a *plurality* of coexisting practices, and the crux of the analysis has to do with their heterogeneous cognitive roots and their complex interactions.[33]

The aforementioned complementarity is likely to be the main source of the epistemic strength showed by scientific knowledge, and a clear reason why science is different from philosophy or religion. One has to conclude, furthermore, that a conception of the philosophy of science which self-imposes limitations on its methods, such that it can only analyse correctly scientific theories (such was the effect, e.g., of the linguistic turn), makes it *ipso facto* unable to account for the epistemic richness of scientific knowledge. Here lies the source of that solidarity between "logicism" and sociologism which was mentioned at the start (footnote 4).

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straightforward.

[33] Some interesting proposals concerning the role of models as "mediators" between theories and data can be found in the compilation MORGAN, M. S. and MORRISON, M. (eds.),

From what has been said above, it is easy to extract examples of intrinsic characteristics of the experimental phase: such are the above-mentioned criterion of convergence between results or representations with diverse procedural and instrumental origins, or the heavy dependence of experimentation upon instruments and technical practices (from Gilbert and Galilei until today, there is scarcely one experimental or observational datum of interest that does not depend on instrumentation). A third example, not mentioned before, is what might be called in somewhat naïve language "objective features" of some data or results, like regularities in observed movements (such as in Galilei's observations of Jupiter's moons), or evidence for entities with constant properties (as in the work of Thomson and Zeeman on the electron).

At the beginning I stated that calling for a philosophy of science that is able to analyse the experimental phase complicates matters for the philosopher, since it forces her to broaden the panoply of tools. Fine analysis of the factors that enter into experimental activity should include questions belonging to the cognitive sciences, with a strong basis on biology and physics, and it should also include the analysis of instrumental or technical practices. Experimentation is not a simple matter of observation, for it sets into play many diverse processes of manipulation and perception. (Notice that perception, a high-level cognitive process, must be neatly distinguished from the mere sensorial stimulation that was so dear to Quine.) It involves mechanisms of motor control, attention, perception, memory, language, etcetera, in short: the whole gamut of cognitive processes studied by psychology and neuroscience, and more.

This viewpoint does not seem to be accepted by many partisans of social studies, such as Pickering, who regards the three main structural

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*Models as Mediators. Perspectives on Natural and Social Science*, Cambridge University Press, Cambridge, 1999.

ingredients of experiment —material procedure, instrumental model, phenomenic model; see sect. 2 above— as comparable elements, which can be treated as if they had very similar levels of material and cognitive complexity.[34] Pickering defends that, starting from a situation of disparity and disunion, the scientist modifies those ingredients in search of compatibility and stability, *and that* such a process can be adequately understood from the assumption that all of them are eminently plastic resources. Among the factors that condition the process of interactive stabilisation of these resources, there are according to Pickering "material resistances," but also all kinds of limitations to which theoretical work can be subject, many of them arbitrary, and also, quite naturally, all kinds of sociological factors.

Once again we find the old temptation to which philosophy has yielded so often, the wish to solve everything too quickly, simplifying too much, relying on an impoverishing analysis, as if that should not put the main goal in jeopardy. Pickering's resulting scheme is too "philosophical," to use this adjective in its negative connotations. He esteems too highly our creative, modifying abilities, the margin of freedom that is open to our elaboration of artificial universes. In my opinion, the point is simply that the three ingredients mentioned above are not homogeneous —not by far. The material procedures and their operations are of far greater complexity than the models, and their complexity keeps defying our analytical abilities.

It is quite easy to say "material procedure," but think of some particular case, such as the prism experiments that we have mentioned repeatedly, or even more the experiments with tubes of cathode rays, electromagnetic plates, and substances like sodium or lithium, performed

[34] Cf. PICKERING, A. "Living in the Material World," pp. ** and PICKERING, A., *The Mangle of Practice*, pp. **.

by Zeeman and Thomson. To analyse all the cognitive and biological, technical and physical processes that took place in any of these cases (both what concerns the apparatus and samples, and what relates to the experimenters) is a task of enormous, indeed forbidding complexity. To hide this high complexity, which calls for analyses of both technics and cognitive processes, is a way of falling into simplifications as abusive as those of old theoreticism.

The above comments may suffice to underscore once again what is specific and characteristic of a philosophy of experimentation. But I would not like to finish without making a new effort to eliminate one possible misunderstanding, and so I add a word about the intervention of theories in experiment. The thesis of a *relative autonomy* of experimentation is not a thesis against all forms of determination of experiment by theory: Certainly the celebrated thesis of "theory-ladenness," as usually presented, is very biased and incomplete, but the point is no to deny it outright. Theories and research frames do play an important role as guides of experimental research. As so many authors have emphasized, the elaboration of experimental results is too complex to be possible without the aid of maps and drafts that help to organize and simplify the work, as unilaterally as it may be. Which, however, does not turn theory into the queen of scientific activity. It is, therefore, fitting to conclude bringing to mind the general principle (which I have proposed as an elaboration on the old description of science as "experimental philosophy") that *science is both philosophy and technics, a hybrid of theorisation and experimentation*, which could not survive without the richness conferred upon it by the *mestizo* interaction of both dimensions.

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