SCIENTIFIC PHENOMENOLOGY AND SCIENCE STUDIES: GASTON BACHELARD AND THE CONCEPT OF PHENOMENOTECHNIQUE

by

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(**Abstract**)

The epistemological works of Gaston Bachelard (1884-1962), written and published between 1928 and 1953 try to make traditional philosophers of science aware of the discontinuous structure of scientific change and the dynamics of the scientific mind. Bachelard often argued that the historical and technical progress of the sciences show that the purely descriptive and classificatory features of past science are sooner or later substituted for epistemic models which rely mainly upon the scientist's power to technically construct the objects of scientific inquiry. The relationships that Bachelard saw between scientists, theories, experimentation, and scientific technology in science led him to coin the philosophical concept of 'phenomenotechnique.'

This concept reflects the historically contingent, artificial, constructed, social character of both scientific knowledge and scientific entities. Bachelard claimed the instruments are materialized theories. Just like mathematics, they are products of technique. Technique, on the other hand, is the rational expression of the scientist's world view. Scientific knowledge is what ends up being technically objectified in scientific instrumentation. Groups such as the social constructivists argue that 'phenomenotechnique' expresses their own claims regarding the strictly rhetorical nature of science. However, to Bachelard, the presuppositions behind the concept preserve the rational essence of scientific thought.

'Phenomenotechnique' is one of the most potentially rich concepts that Bachelard has to offer to contemporary science studies. The purpose of this dissertation is to offer a full account of the history and implications of 'phenomenotechnique.' Part I is an explanatory analysis of the concept as it appears in all the epistemological works of Gaston Bachelard. It also shows how 'phenomenotechnique' relates with other Bachelardian concepts such as 'technical materialism,' 'epistemological rupture,' 'psychoanalysis of scientific thought,' 'applied materialism,' and 'social consensus.' Part II deals with the intellectual and scientific context of France in the first half of the twentieth century which led Bachelard to coin the term. Finally, Part III will attempt to incorporate 'phenomenotechnique' into today's science studies.

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PART I - THE CONCEPT OF 'PHENOMENOTECHNIQUE' IN THE WORKS OF GASTON BACHELARD

Introduction

The epistemological writings of Gaston Bachelard, written and published between 1928 and 1953, try to make traditional philosophers of science aware of the specific structure of scientific explanation and the dynamics of the scientific mind. Bachelard was deeply preoccupied with what he referred to as the profound epistemological rupture between common knowledge and scientific knowledge. Moreover, he argued, discontinuity can also be found in different stages and at different levels in the evolution of scientific knowledge itself. In fact, Bachelard often pointed out that the historical progress of the sciences showed that the purely descriptive and classificatory features of past science were sooner or later substituted for epistemic models which relied mainly upon the scientist's power to technically construct the objects of scientific inquiry.

Bachelard's program included the prescriptive adoption of a particular attitude towards the study of science -- the creation of new philosophical categories that illustrate as accurately as possible the radical methodological and conceptual changes produced by contemporary physics and chemistry, as well as their metaphysical, historical, ontological and epistemological implications.[1]

Theories such as Relativity (Special and General) and Quantum theory, for instance, entail not only a redefinition of the processes involved in the construction of objectivity in the physical sciences, but also a revolutionary shift in what should count as scientific knowledge. In philosophical terms, this shift requires a renewed assessment of scientific realism itself. On the other hand, and still according to Bachelard, there is no progress in science without a reevaluation of the philosophical concepts about science, as well as a detailed analysis of the dynamic

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[1] For an overview of Bachelard's epistemological categories see Maria Teresa Castelão Pereira, Gaston Bachelard's Scientific Philosophy: an Approach to Science and Technology Studies (Virginia Polytechnic Institute and State University, 1991); Dominique Lecourt, Marxism and Epistemology: Bachelard, Canguilhem, Foucault (London: Humanities Press, 1973):25-109; Paul Ginestier, La Pensée de Bachelard (Paris: Bordas, 1968):28-121. For a general outline of Bachelard's philosophy of science and its connections with Anglo-Saxon philosophy of science, see Stephen W. Gaukroger, "Bachelard and the problem of epistemological analysis," Studies in the History and Philosophy of Science 7(1976):189-244; Mary Tiles, Bachelard: Science and Objectivity (Cambridge: Cambridge University Press, 1984); for a generic approach see Gary Gutting, "Gaston Bachelard's philosophy of science," International Studies in the Philosophy of Science 2.1(Autumn 1987):55-71, reedited in Gary Gutting, Michel Foucault's Archeology of Scientific Reason (Cambridge: Cambridge University Press, 1989):9-32.

interaction between theory and experimentation in the scientists' work. The ambiguity of Bachelard's position on the issue of scientific realism versus anti-realism connects directly with this interaction.

As a result of the relationship between particular theoretical presuppositions and experimental verifications that constitute artificial extensions of these presuppositions, mathematical physics is acknowledged by Bachelard not only for its inductive value but also as a tool which enables the scientist virtually to construct scientific objects. Bachelard's claim, particularly from the 1930s on, that science produces objects -- **scientific** as opposed to **natural** objects -- has ontological and phenomenological consequences. Rather than yielding an empirically based phenomenology, mathematical physics establishes the grounds for what Bachelard calls **phenomenotechnique**, the true scientific phenomenology. Actually, it is not only at the level of theoretical physics that a **phenomenotechnical** approach to science should be employed. The connections Bachelard sees constantly existing in science -- whether in physics or in chemistry laboratory settings -- between scientific knowledge and scientific technology are also part of the same concept.

The purpose of Part I of this dissertation is to provide an explanatory analysis of the category of

phenomenotechnique as it appears in the scientific philosophy of Gaston Bachelard. There are several difficulties connected with such a project. **Phenomenotechnique** is not a concept that shows up in Bachelard's writings clearly defined from the very start. Although Bachelard's first books in the epistemology of science -- Essai sur la connaissance approchée, Étude sur l'évolution d'un problème de physique: la propagation thermique dans les solides, and La Valeur inductive de la relativité -- were published in 1928 and 1929,[2] the word 'phénoménotechnique' appeared timidly only for the first time in the article "Noumène et microphysique," published in 1931 by the French philosophy review Recherches philosophiques. However, between 1932 and 1953, and despite Bachelard's having interrupted his works on epistemology from 1940 to 1949, the term **phenomenotechnique** became fundamental to Bachelardian discourse on scientific practices. Together with concepts such as **technical materialism**, **instrumental epistemology**, and **applied rationalism**, **phenomenotechnique** is meant to emphasize and explicate the reliance of contemporary science upon what is

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[2] Both the Essai sur la connaissance approchée and the Étude sur l'évolution d'un problème de physique: la propagation thèrmique dans les solides were theses in philosophy of science, presented to the University of Paris in 1927.

rationally constructed and embodied in scientific technology.

Understanding the full implications of **phenomenotechnique**, however, requires more than tracing out when the term starts showing up in Bachelard's works, how many times it appears in a particular book or article, or even what its basic definition might be. In fact, it is my claim that, although not always named, **phenomenotechnique** is present in practically all of Bachelard's characterizations of both scientific knowledge and scientific techniques, including in his works prior to 1931. More, it is my belief that **phenomenotechnique** should be acknowledged as the most important philosophical category created by Bachelard to illustrate the rupture of intentionalities brought about by the development of contemporary physical sciences. Nevertheless, a full assessment of **phenomenotechnique** can only be achieved by looking at the consistency and/or exhaustiveness with which it is dealt with throughout Bachelard's writings in the philosophy of physics and chemistry. As noted above, the fact that the term is often understated adds methodological difficulties to the present project.

Another problem with my analytical approach comes from the way **phenomenotechnique** is intertwined with a whole range of other Bachelardian conceptualizations. In other words,

phenomenotechnique and its implications do not stand out in Bachelard's reflections independently from a network of very diverse philosophical issues. Obviously, to single out **phenomenotechnique** from everything else would entail the virtual amputation of a rather cohesive corpus of notions regarding Bachelard's claims about how science develops and how philosophers should assess that development. This is not what I am aiming at. Although the main focus of the project will be on **phenomenotechnique**, which will be constantly connected with other Bachelardian categories, I will give the reader a full fledged phenomenotechnical account of Bachelard's epistemology of modern science. I do think that the lack of attention paid by scholars to **phenomenotechnique** has been detrimental to science studies discourse in general, and to contemporary philosophy of science in particular.

Besides a structural approach to **phenomenotechnique**, I also intend to undertake a genetic approach to the term. Thus, chronology will be an important asset to my purposes, although I will not always follow a straightforward linear analysis of Bachelard's works. It is my view that Bachelard's treatment of the conceptualizations behind **phenomenotechnique** underwent a particular ideological evolution which coincided with Bachelard's changing views on the meaning of objectivity in science. For instance, the trilogy of 1949-1953 represents a much stricter historiography of contemporary science than the works of 1928-1929, considerably focused on the history of science from previous centuries or philosophical considerations of the astounding scientific revolution at the beginning of the twentieth century. Accordingly, **phenomenotechnique** takes up different roles throughout these works, i.e., from a subtle statement about the status of instrumentation or about the necessity to expand or even break away from a certain kind of Husserlian phenomenology, to a factual claim about scientific objectivity and the intersubjective character of the use of experimental testing in science. On the other hand, Bachelard's interest in connecting phenomenotechnique with scientific methodology is gradually complemented by an interest in scientific objects, especially those provided by theoretical physics such as quantum theory.

Then there is the problem of repetitiveness. Despite the obvious value of his research on the philosophical implications of contemporary physics, Bachelard keeps emphasizing the same ideas over and over. Sometimes the content of one book is just the sequel to things he said previously. One could almost say that Bachelard's corpus of epistemological doctrines about contemporary physics written between 1929 and 1940 are just extensive footnotes to the Essai sur la connaissance approchée (1928). The examples

Bachelard gives to make his points are problematic as well. They are at times so profuse and complex that it is basically impossible for the nonspecialist in theoretical physics to fully comprehend them. On the other hand, they allow one to realize both that this very diversity is the expression of Bachelard's reflections on the evolution of the physical sciences (particularly his long-standing claim that mathematical physics and experimental physics should be considered in their dialectical implications), and where the concept of phenomenotechnique becomes truly fundamental for the philosophical illustration of the constructive work of science.

My study of **phenomenotechnique** and its implications is divided into three related sections. The first one corresponds to the time period between Bachelard's first work on epistemology of science until the book prior to the coinage of **phenomenotechnique** (1928-1929). The second section covers the time period between "Noumène et microphysique" and the last book Bachelard published before interrupting almost all of his epistemological works (1931-1940).[3] Finally, the last section is concerned with the

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[3] Bachelard wrote a book review of Stéphane Lupasco's <u>L'Expérience microphysique et la pensée humaine</u> (1941) which was published in 1942-1943 by the <u>Revue philosophique de la France et de l'étranger</u>. For purposes of structure, I will not include this book review in my division.

treatment of **phenomenotechnique** in Bachelard's epistemological books and articles from 1949 to 1953.[4]

Since I want to provide the reader with an explanatory account of **phenomenotechnique** and the way the concept connects with Bachelard's model of scientific change, I will refrain from introducing criticisms of the model in the middle of the analysis. They will, however, appear in the final paragraphs of this chapter.

The present study entailed reading in the original French sources all of Bachelard's published books, articles and other documents that Bachelard wrote on philosophy of science, as well as the published proceedings of the conferences he participated in, and that, with the exception of one in 1937 and another in 1950, were published between 1953 and 1956 in the <u>Bulletin de la Société Française de Philosophie</u>. Since not all of these sources pertain directly to my project, they will not be mentioned in the text that follows. All of Bachelard's works of epistemology, however, are listed in Appendix B to this dissertation.

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[4] Although I do not include in this division Bachelard's article "Nouvelle esprit scientifique et la création des valeurs rationnelles", published in 1957 by the <u>Encyclopédie française</u>, I will mention it at an appropriate place.

Part of the footnotes will be concerned with guiding the reader to both French and English language writings that explain and/or criticize particular categories or topics within Bachelard's epistemology. These references are particularly concerned with helping those interested in pursuing further research into Bachelard's scientific philosophy.

For purposes of structure, each section is divided into subsections. I used some of the titles of Bachelard's books as a source of inspiration for giving titles to each subsection.

Chapter 1. 1927-1929

As already noted in the introductory notes to this chapter, the conceptual and philosophical presuppositions behind 'phenomenotechnique' existed before the term itself showed up in the epistemological discourse of Gaston Bachelard. Some of these presuppositions include the claim that in science the given ('le donné') is relative to concepts that are culturally shaped. More important, it is not so much a strict empiricism that counts as a sound basis for scientific knowledge, but instead what ends up being technically objectified in scientific instrumentation.

Approximate Knowledge

Since the purpose of the Essai sur la connaissance approchée (1928) is to show, in an almost Popperian sense, the importance of subjecting scientific statements to a constant process of rectification, the book's main line of reasoning goes well beyond a mere acknowledgement of theory change in science.[5] Instead, Bachelard claims that there

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[5] For the superficial similarities between Popper, Feyerabend and Bachelard, see Roy Bhaskar, "Feyerabend, and Bachelard: two philosophies of science," New Left Review 94(1975):31-55. This article also provides an interesting

is an intrinsic correlation between the given and the methodologies that are used to describe it (Bachelard 1987, 15) and include it in a comprehensible structure. The set of methodologies used for this structuring include not only a rational network of scientifically intelligible arguments and theoretical proofs, but also particular practices and testing which are only possible through the use of scientific technology. The more the conditions of precision evolve, the more we should be led to recognize that instruments such as the optical microscope play "a primordial role in the approximate knowledge of physics" ('les connaissances approchées en physique') (Bachelard 1987, 60-61). "Instrumental epistemology" (Bachelard 1987, 77) is the expression used by Bachelard to identify the obvious dependence of scientific knowledge upon the degree of sensitivity of scientific instrumentation.

The fact that "precision in measurement . . . characterizes the scientific methods of a certain time" (Bachelard 1987, 69) enables us to say that "the history of approximate knowledge is the history of scientific systems" (Bachelard 1987, 69), and makes science subordinate to a certain form of instrumental determinism (Bachelard 1987,

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critique of Bachelard's neglect for the so-called 'analytical' tradition in philosophy of science.

65). If it is true that scientific theories of previous centuries can be recovered and applied to contemporary science, the same cannot be said of knowledge that has been acquired by means of an evolution in instrumental precision. As Bachelard says, "a conquest in detail ('minutie') immediately declassifies the experimental knowledge of a particular time" (Bachelard 1987, 69) because it constitutes a one way step further in both knowledge rectification and approximation to reality. The scientist does not work completely **ad lib**. The creativity and discovery behind scientific practices are controlled and corrected by the use of technology. On the other hand, to the degree that we admit that the study of reality is technically organized (Bachelard 1987, 160), we have to acknowledge that the content of scientific knowledge is at least as artificial as the techniques imposed on reality and used to temporarily validate particular theories.

The above considerations regarding the role of scientific technology in the construction of scientific knowledge lead Bachelard to propose new conditions for the acceptance of objectivity in science. He says that "to prove the objectivity of a phenomenon is . . . to visualize it, it is to show it and to demonstrate it. All physics apparatus tends to substitute diverse sensory references by [renewable] inscriptions [on a screen]" (Bachelard 1987, 58). The scientist is thus faced with an artificial nature (Bachelard 1987, 165) which is none other than a direct consequence of particular rational and technical scientific programs. In other words, this constructed nature, this **natura structa** to use the expression of Louis Basso quoted by Bachelard (Bachelard 1987, 161), implies that a phenomenon is technically isolated from reality, fragmented (Bachelard 1987, 165), and then transposed into a system in which it receives its overall scientific status. Since it is the research program that defines what is to be studied and how, the relationship between the phenomenon and the methodologies that 'produced' it is extremely tight. In fact, the responsibility to determine under what conditions a phenomenon can be considered a scientific fact is largely given to scientific technology:

> Existence is deduced from action. In physics, what cannot be detected by any instrument must be taken as nonexistent. Nothing allows us to suggest properties for the infinitely small, and it is already an abuse to separate a theory from the experimental means that ought to prove it. A quantity that does not count on an experience cannot disturb a theory that is precisely positioned at the level of that experience. . . . To take it into consideration would be to introduce, not only hidden qualities, but also occult quantities (Bachelard 1987, 72).

This sort of 'constructed realism' (Bachelard 1987, 187) or what Bachelard also calls 'technical empiricism' (Bachelard 1987, 162) has its own rules, as it prescribes, as we have seen, new characteristics to scientific objectivity, not to mention a new ontology for scientific entities. An example of 'constructed realism' is, for instance, the electrical current which, according to Bachelard, does not have any meaning beyond the "measurement procedures one applies to it" (Bachelard 1987, 74). Objectivity in science does not come from the possibility of integrating a phenomenon in a frame of theoretical presuppositions alone. Instead, objectivity comes from the set of instruments that, applied to the study of a particular object, "considerably reduce subjectivity in observation" (Bachelard 1987, 63). Subjectivity is often times part of what Bachelard calls **parasites**, i.e., "causes of error which at first do not seem to be relevant (notable)" (Bachelard 1987, 65). The hunting of the parasite, which thus coincides with the notion of rectification and includes things such as instrumental precision and being aware of the fatigue of the observer (Bachelard 1987, 65), is precisely one of the reasons why the scientist should be concerned with using instruments of different levels of sensitivity, as well as constantly putting each instrument and herself under suspicion (Bachelard 1987, 66).

To Bachelard, maximum rigor coincides with the total substitution of the given by the constructed (Bachelard 1987, 174). The constructed is the "arithmetic construction of the given" (Bachelard 1987, 175), that is to say, the temporary resolution, in science, of the permanent dynamic correlation between rationalism and empiricism, in which, by the way, rationalism takes over and dictates the rules. In fact, the means used by the scientist to elaborate on knowledge are "the result of . . . a whole series of validation judgements that expose the conditions of rigor [itself]" (Bachelard 1987, 169). Once again, it is not possible to achieve objective knowledge without the contextualization of a phenomenon within a set of methodological procedures in relation to which it becomes inherent to the material content of science. As already stated, these methodologies include the use of instrumentation, and are part of a much broader and regulated research program. 'Validation judgments' come directly from experimental verification, one of the most important aspects of the construction of science.

A scientific fact is totally different from a fact of ordinary experience, since, as Octave Hamelin, quoted by Bachelard, put it "the pure fact must be prepared" (Bachelard 1987, 246). Elaborating on this view, Bachelard says:

> There is no objectivity in the utilitarian perception, because [this perception] is essentially a relation between the thing and the subject, where the subject has a primordial role. One does not engage oneself in the path to objectivity unless one puts two things in

> relation with each other -- undoubtedly with the subject as mediator -- but in reducing the role of the subject (Bachelard 1987, 247).

The reduction of the role of the subject is, according to Bachelard, achieved through methodology. As we have seen, the use of scientific technology, the search for both detail and precision, and the complexification of the relations constructed between objects decrease the possibility of interference from the subject, at the same time as they maximize the process of knowledge rectification by the scientist. This process, however, does not lead to an immobile system of thought formed by absolute predications about what counts as scientific. Although it is acceptable to take knowledge of relations between objects as systematic and objective, this knowledge lacks stability:

> One starts with relations that are more or less formal, and one tries them on reality. Then, these relations manifest aberrations which demand the adjunction of new forms, less general and more numerous. The same problem happens again, but at another level. One thus progressively rectifies the logical pictures from which one has departed (Bachelard 1987, 252).

Lack of stability does not entail that science is a subjective irrational whim from which there is no escape. It is the role of history of science to help us realize that scientific knowledge in general and scientific systems of thought in particular alter positively through processes of rectification, and that criteria of objectivity in science change accordingly. The consistency with which the above-mentioned logical pictures are tested and corrected against the background of both reality and a specific system of rationality is what allows Bachelard to talk about the extreme importance of experimental research, and thus of scientific technology. To him, "the verification of a theory . . . is a provoked point of contact [between] the real and the rational," and that which basically proves the theory to be part of a system of rationality rather than "another dream in the mind of the scientist" (Bachelard 1987, 270).

Scientific theories are representations of reality that have been successfully **verified** experimentally, or even better, **instrumentally**. If there is no verification, there is no scientific representation and thus no systematic knowledge. The more detailed knowledge becomes, the more prone it is to become part of what Bachelard calls a 'micro-epistemology' (Bachelard 1987, 279). He also suggests that, since the instruments of study determine, in the phenomenon, different levels of interaction, it is necessary to bring "a new dimension . . . to phenomenology, . . . which connects with the conquest of precision, of purity, [and] of fine detection" (Bachelard 1987, 297). Just as in the philosophy of science of French physicist Pierre Duhem, the concept of truth is in the Bachelardian model, substituted for those of approximation and precision.

The Evolution of a Physical Problem

In the Étude sur l'évolution d'un problème de physique: la propagation thermique dans les solides (1928), Bachelard follows up on some of the epistemological issues presented in the Essai. As the title indicates, the book is concerned with the physical problem of the propagation of heat. The analysis of works by scientists such as Fourier (1781-1830), Poisson (1781-1840), Lamé (1795-1870), and Duhamel (1797-1872), allows Bachelard to undertake an historical survey of the way the concept of temperature evolved over time, and afterwards of showing how, even in science, the historical state overpowers the rational state (Bachelard 1973, 57). This last notion is none other than a rephrasing of the idea, also present in the Essai and basically everywhere in Bachelard's epistemological writings, that scientific concepts are culturally shaped. For our purposes, the most interesting aspect of the Étude is how Bachelard concentrates for a while on the characterization of the thermometer as a tool for verification in physics, and then how he relates it to mathematical thought.

What Bachelard considers to have been a misunderstanding prevailing in eighteenth-century physical science is the idea that the thermometer alone could determine "everything that can be known about heat. . . .

One thinks that heat is an entity that one can separate from the bodies where it develops" (Bachelard 1973, 11-12). This sort of **a priori** assumption about the scientific possibilities of an instrument constitutes an obstacle to the knowledge of caloric propagation. It is an epistemological obstacle because it does not take into consideration that a phenomenon that has been isolated and "fragmented is necessarily a conventional phenomenon" (Bachelard 1973, 97), only to be understood if incorporated into methodological principles mathematically coordinated (Bachelard 1973, 137). Bachelard refers to some of the authors mentioned above as developing precursors of the notion that the physical phenomenon is totally constructed, even if only mathematically. To Lamé, for instance, it is calculus that "should furnish the hypothesis, coordinate . . . [and] construct the phenomenon" (Bachelard 1973, 104). Bachelard also thinks that in cases like the study of thermical propagation, the intervention of mathematical thought is crucial.[6] So, two things become important in the context of physics. One is the validation coming from experience and thus technology. The other is the use of

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[6] The same notion is presented in Bachelard's article "La Richesse d'inférence de la physique mathématique," published by Scientia in 1928.

mathematics to diversify scientific questioning and to classify the relations between phenomena.

As already pointed out in the Essai, in the knowledge of the physical macroworld there has to be a fine correlation between what is logically mathematically systematized and what experimental verification tells the scientist about how 'reasonable' that system is. Scientific knowledge is not all about abstractions: it is also about quantification and measurement. Since it is the role of technology to partially validate an idea as scientific or invalidate it as a dream with no place in science, one needs to pay special attention to the problems embodied in scientific technology.

In the Essai Bachelard had already referred to the hunting of the parasite. Technology has an essential epistemic role in science, but that does not free it from inducing the scientist into error. The thermometer, for instance, possesses a 'calorific individuality' very difficult to eliminate:

> the thermometric substance, the recipient that contains it, the choice of fixed points, the value of mass itself which [disturbs] the measurements by making temperature interfere, . . . the specific powers and the conditions of conductivity. [More,] the worst obstacle [is] this frozen convention ('convention arrêtée') accepted by everyone (Bachelard 1973, 9).

The subject can bring interferences to objectivity. The Essai is clear about how objectivity in science can be achieved as long as the individual subject is removed in the name of an overemphasis given to the methodological processes that take place at both the experimental level and what I chose to call the **metarational level**, i.e., what could be considered the tacit norms of scientific rationality. In the Étude Bachelard is more forceful in explaining where the validation judgments required for both theory choice and theory change are located. Although some validation is a direct consequence of experimental verification, the construction of validation rules, i.e., what is rigorous, what is complex, what is objective, comes from the 'scientific city.' It is the scientific city that "puts the fair experience in its fair rational place" (Bachelard 1973, 68). To Bachelard, it is only accurate to talk about positivism in science if we take it to be "the positivism of a rationality more or less elaborated and not a priori" (Bachelard 1973, 68). There are rules, but these rules are contingent upon the processes involved in the constant rectification of science. We are again led to realize how strategically located scientific technology and mathematics are in the midst of theory choice and at the crossroads of experimental knowledge.

The Essai had already acknowledged the relevance of mathematics to physics by stating for instance, that the 'constructed' is the arithmetic construction of the given.[7] The importance of mathematical physics for both the suggestion of theoretical systems to be experimentally validated and the systematic organization of experimental results is given extra attention in the Étude. Bachelard is always concerned about translating the implications, for science, of the historical dependency of mathematics and experience (Bachelard 1973, 69). By a philosophical move, he ends up saying that mathematics, too, is an instrument, albeit a theoretical one, that allows the scientist to place the general phenomenon in the 'plan of possibility,' and thus to overrule direct empiricism without denying experimental validation. Both the creative aspects of mathematics and its power of synthesis are what make it such an essential tool for science. Nevertheless, despite all this optimism about the possibilities opened up for science by mathematics since the time of Newton, Bachelard is concerned about the extent to which mathematics can provide a faithful account of the physical structure of the

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[7] For a critique of Bachelard's knowledge of mathematics in books such as the Essai sur la connaissance approchée and Le Rationalisme appliqué see Roger Martin, "Bachelard et les mathématiques" in H. Barreau and others, Bachelard (Paris: '10-18,' 1974):46-61; see also Maurice Loi, "Bachelard et les mathématiques" in Gaston Bachelard: l'homme du poème et du théorème (Colloque du Centenaire - Dijon: Universitaires de Dijon, 1984):79-91.

artificial reality produced in the laboratory. In other words, "how can we explain that mathematical coordination corresponds to the hidden coordination of the characteristics [of the phenomenon] that we have singled out in experience?" (Bachelard 1973, 161).

The Inductive Value of Relativity

The explication of mathematical reasoning as an instrument for physics is further developed in La Valeur inductive de la relativité (1929).[8] This time, it is clear that Bachelard is mainly concerned with the philosophical implications of theoretical physics. What has been called in the Essai 'instrumental epistemology' is after all a more encompassing category. Bachelard had already acknowledged that technology, i.e., a network of scientific artifacts, is the main regulator and source of inspiration of theory change. What matters now is to include more forcefully, within the concept of instrumentation, things such as the mathematics of the calculus. Mathematics is an heuristic device as much as technology. Actually, mathematics is **the** rational tool par excellence of the theories of Relativity, since not only Relativity reconstructs experience but also

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[8] This book was meant to be a critique of the views of Émile Meyerson in his La Déduction relativiste (1925).

creates it (Bachelard 1929, 8). If it was accurate to talk about a context of discovery in the case of theories that can be validated through both material technology and mathematics -- and Bachelard is well aware that this validation is as artificial as the objects that it validates -- now we should instead talk of a pure context of invention.

The theory of relativity "opens up a new plan of interrogation about reality" (Bachelard 1929, 92). At this level, material experimentation as such becomes secondary since validation judgments come from the inductive possibilities provided by mathematics alone. What matters is not the reality of an effect but how much a scientific principle is conceivable (Bachelard 1929, 143). Reality does not become a limitation for thought. In the context of the theory of Relativity, thought does not depart from reality, it aims towards it (Bachelard 1929, 227; 241):

> It is by means of the possible that one discovers reality. Undoubtedly an artificial method, since it finds support [in a] supposition, [Relativity] is, precisely because of that, welcoming ('accueillant') to the inductive tendencies of the mind (Bachelard 1929, 93).

To Bachelard, relativity is not so much concerned with studying the objects of the world as it is with the study and the mathematical organization of the relations those objects maintain with each other. Reality is not antecedent to relation, but instead it is **implied** in relation (Bachelard 1929, 108).[9] Thus, Relativity as a second level of approximation to reality, constitutes a sort of "reversal ('renversement') of causality" (Bachelard 1929, 141), and a justification for why it represents a radical epistemological break with the usual cause-and-effect type of scientific picture reflected in the deductive characteristics of, for instance, Newtonianism. Relativity is a completely new way of looking at things. With relativistic theories, mathematics is a methodology in its own right, capable of transforming phenomenology into 'mathematical phenomenology' (Bachelard 1929, 217). As a method, mathematics embodies objectivity in the same fashion as technology embodies it at the first level of approximation. With mathematics as a geometrical instrument, the theory of relativity can be used to systematize the possible (Bachelard 1929, 82), and it prevents the scientist from considering immediate experience as the basis of physical theories. Relativistic principles will be posed **a priori** to experiment:

> [In Relativity] one does not extract [principles] from an examination of reality, but from a reflection about the conditions of reality. In other words, the general

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[9] For additional information on Bachelard's concept of 'relation' see Renée Habachi, "Au Commencement est la relation" in <u>Bachelard: l'homme du poème et du théorème</u> (1984):187-195.

> principles of Relativity are objectivity conditions rather than general properties about the object (Bachelard 1929, 139).

We are dealing here with concept construction alone. Conceptualization and abstraction are paths to objectivity. The observer's sensations cannot be used to validate scientific judgments, since the observer has to be systematically eliminated (Bachelard 1929, 205) from the relativistic frame. Social objectification and consensus do not come from intersubjective agreement about sensations -- that is not what the purpose of the scientific city is all about -- but from a sort of collective agreement of the minds (Bachelard 1929, 203), something I had previously called the metarational level.

Chapter 2. 1931-1940

In 1931, Bachelard published a small article in the review Recherches philosophiques.[10] Its title is "Noumène et microphysique" and there we find, for the first time in Bachelard's vocabulary on science studies, the word 'phenomenotechnique.' Actually, this event is probably the only reason why the article became important to Bachelard scholars. Its content does not provide the reader with any noticeable, crucial new information regarding mathematical physics. Nevertheless, it may be useful to analyze how the concept is developed and contextualized. In fact, I do think it is interesting to notice that 'phenomenotechnique' was born out of the need to find a concept that could translate the purpose behind the use of mathematical reasoning in microphysics, rather than the connections between scientific knowledge and scientific technology. We will see how Bachelard's standpoint on the issue changes in the course of this section.

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[10] All references to this article are to its publication in Études.

Noumenon and Phenomenon

In its treatment of mathematical physics, "Noumène et microphysique" is primarily a natural extension of both the <u>Étude sur l'évolution d'un problème de physique</u> and <u>La Valeur inductive de la relativité</u>. It starts out with the claim that mathematics is not, as it had been until the nineteenth century, a mere language capable of translating the results of direct experimentation into abstract, condensed formulations. Contemporary physics uses mathematics as a method to connect, strengthen and create relations among phenomena (Bachelard 1970, 16). 'Mathematical realism' is the expression used by Bachelard to emphasize the scientific realism demanded by contemporary physics. In microphysics, mathematics is used as generator of the instrumental conditions that allow the systematization of a hidden reality, because the **essence** of that reality **is** mathematical (Bachelard 1970a, 18). The reality constructed by microphysics "has a mathematical sense before it has a phenomenal signification" (Bachelard 1970a, 17). At the microlevel of reality, both the mathematical (functional) a priori and the physical hypothesis should be put at the same plane, since they are both **suppositions** (Bachelard 1970a, 21). As Bachelard will say later in "Critique préliminaire du concept de frontière epistémologique,"[11] scientific thought "supposes reality before knowing it and only knows it as the realization of the supposition" (Bachelard 1970a, 80).

"Physics is not a science of facts anymore; it is a technique of effects" (Bachelard 1970a, 17). If you change the method, you change the phenomenon, which makes the phenomenon "a particular instance of a method" (Bachelard 1970a, 15). This methodological reductionism makes the knowledge of reality contingent upon its own instrumental conditions, and thus a good reason to talk about what Bachelard had already called 'constructed realism' in science. In microphysics, all the 'force of discovery,' and its inductive power, is given to mathematics: "little by little it is rational coherence that comes to supplant, in strength of conviction, the cohesion of ordinary experience" (Bachelard 1970a, 15). Since the noumenal level of the microcosm is of a mathematical essence, and rational coherence becomes the touchstone of validation judgments, Bachelard dismisses normal phenomenology as incapable of making a correct philosophical assessment of what is going on in contemporary physics. Phenomenology presupposes passivity in face of the given, and the ability to describe it. In the constant dynamics of knowledge acquisition

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[11] The article was first published in 1936. I use the reference from its later publication in Études.

typical of contemporary science, it is more appropriate to talk of **creation**. Hence, instead of phenomenology, we should adopt the expression 'phenomenotechnique.'[12] Phenomenotechnique is the process through which "new phenomena are not only found, but invented, constructed piece by piece" (Bachelard 1970a, 19):

> Following the teachings of mathematical physics, we find ourselves, maybe for the first time, in the presence of a metaphysics which is positive because it experiments itself. It is the metatechnics of an artificial nature. Contemporary atomic science is more than a description of phenomena, it is a production of phenomena (Bachelard 1970a, 21).

Interestingly, it is not in physics alone that the constructivist quality behind scientific phenomena can be found.

The Philosophic Pluralism of Chemistry

In Le Pluralisme cohérent de la chimie moderne (1932), Bachelard makes similar claims about the organization and classification of chemical elements as he does earlier about physics.[13] Although the word 'phenomenotechnique' does not

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[12] A criticism of Bachelard's phenomenology, as well a comparison between the phenomenology of Bachelard with those of Hegel and Husserl can be found in Alphonse Grieder, "Gaston Bachelard - 'phénoménologue' of modern science," Journal of the British Society for Phenomenology 17.2(1980):107-123.

[13] For more information on Bachelard's epistemology of chemistry see David William Theobald, "Gaston Bachelard et la philosophie de la chimie," Archives de philosophie 45(1982):63-83; see also Robert Delhez, "Bachelard et la

appear anywhere in the book, Bachelard's reflections on the creative features of organic chemistry -- including its mathematical nature -- clearly have a lot in common with his definition of 'phenomenotechnique' in the crucial article of 1931. Bachelard had said that physics is a technique of effects. The same argument can be applied to modern chemistry, where it seems that the demarcation between nature and technique is more obvious from the start. Again, I will not concentrate on Bachelard's treatment of the historical case studies -- i.e., in what way chemists such as Mendeleef, Meyer, and Crooks contributed to the study of chemical elements -- but instead I will focus on Bachelard's epistemological discourse about the constructive features of contemporary scientific knowledge.

Realism in physics, as we have seen, is of a constructed sort. The use of both mathematical physics and technology in the systematization of the real and/or conceivable relations between physical phenomena determines the way physics develops, changes and corrects itself. There is a constructed realism -- Bachelard also calls it "rectified empiricism" (Bachelard 1973, 70) -- in chemistry as well. It is a mathematical component, i.e., mathematical

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chimie," in Bachelard (Paris: '10-18', 1984):121-133. Both authors acknowledge that Bachelard is one of the few philosophers of science concerned with chemistry.

chemistry, that allows the chemist to "explain the affinity of substances and to find a sort of systematics where the different elements receive their properties by finding their exact function" (Bachelard 1973, 9). On the other hand, the chemist works with substances by fragmenting, purifying and changing them in order to determine their structure and their details. In the laboratory, the chemist tries first of all to "provoke a halt in the evolution of a chemical structure" (Bachelard 1973, 66). This operation not only requires the use of technology to make observations, but also the fabrication of artificial substances, i.e., substances that cannot be found as such in "natural nature." This same idea is expressed in La Philosophie du non, when Bachelard says that "chemical substances are the product of technique rather than bodies found in reality" (Bachelard 1973, 45). In an article of 1934, "Lumière et substance",[14] Bachelard makes a similar point when talking about photochemistry and also the coloration doctrine. He says, for instance about coloration, that "it does not have to do with discovering, but with constructing. One constructs a color the same way as one builds a house, on a plan. . . . The real chemical problem consists of

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[14] I use the reference from the article's publication in Études.

realizing, of substantializing a law" (Bachelard 1970a, 65). In the "Critique préliminaire" Bachelard goes even further by suggesting that, in the laboratory, "certain chemical substances created by man have no more reality than the Aenead or the Divine Comedy" (Bachelard 1970a, 83).[15] It is precisely the artificial character of chemical experimentation (Bachelard 1973, 65) that determines, by extrapolation of results, the classification and understanding of the "true structure of the natural substances" (Bachelard 1973, 66). The chemical phenomenon is a provoked phenomenon (Bachelard 1973, 50), an artificial and constructed phenomenon. Bachelard uses an extensive quote from Berthelot's La Synthèse chimique (1887) to emphasize more precisely the creative character of chemistry:

> Chemistry creates its own object. This creative faculty, similar to that of art itself, essentially distinguishes it from the natural and the historical sciences. The latter have an object given to them in advance and independent from the will and the action of the scientist. . . . On the contrary, experimental sciences have the power to accomplish their conjectures ('de réaliser leurs conjectures'). These conjectures themselves are used as points of departure for the research of phenomena suited to confirm or destroy

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[15] We should not be led to think that Bachelard does not distinguish scientific accounts from literary ones. Part of his work is precisely to establish the demarcation between science and imagination. As he will late write in La Formation de l'esprit scientifique, "literary style ("aspect littéraire") is. . .often a bad sign in pre-scientific books." [Bachelard 1986, 83]

> them; in other words, these sciences pursue the study of natural laws, by creating a whole complex of artificial phenomena which constitute their logical consequences. In this respect, the procedures followed by the experimental sciences are not without analogy to those of the mathematical sciences (Bachelard 1973, 71).

If we disregard Berthelot's naive assumption that history is not a fabrication of the historian, there are two important ideas in his text. One is the acknowledgement that experimentation in the laboratory can be more than a small scale imitation of what happens in nature. Within the chemistry laboratory there is the scientist who, with the help of technology, creates new substances, and acts upon things. Bachelard, too, realizes how "nature is far from rivalling human possibilities, [and how] artificial science ('science factice') clearly extends beyond natural science" (Bachelard 1973, 69). The other idea, which for purposes of explaining Bachelard's concept of phenomenotechnique is the more interesting one, is the parallel that Berthelot sees between experimental science and mathematical science. In fact, the similarity found between methodological procedures that are presumably of different ontological status is something that Bachelard already had argued for in other writings, namely when he claims that both material technology **and** mathematical theories are techniques, instruments used by the scientist to produce phenomena. This dialectical symbiosis between reason and experience, one of Bachelard's favorite topics, is none other than an example of "applied rationalism" or "technical materialism" in chemistry, and synthesized by Bachelard in the following terms:

> We rediscover again and again this conceptual substructure that coordinates the different schemes of the homologous substances, and which leads us to take the new bodies as working drawings that we have the task of materializing. We will then construct veritable chemical machines, of delicate and multiple organs with nicely arranged functions, a whole technology pursuing [particular] ends which succeeds the first, merely ontological, chemical study (Bachelard 1973, 71-72).

The new bodies that the scientist has the task of materializing include not only chemical and physical substances, but mathematical formulae (the conceptual substructure) and scientific technology as well. We are once again faced with the scientific value of instrumentation taken in its broader sense. In the particular case of technical materialism, Bachelard will say, in both Les Intuitions atomistiques (1933) and Le Nouvel esprit scientifique (1934) that "instruments are materialized [or reified] theories" (Bachelard 1933, 140; Bachelard 1978, 16). And, indeed, if this is the case, all scientific phenomena instrumentally produced -- be they chemical substances or waves in a screen, for instance -- necessarily have a theoretical label (Bachelard 1978, 16) which those instruments are meant to justify and strengthen:

> [With instruments] we do not aim at reconstructing the phenomena as they offer themselves to us through our senses; on the contrary, we aim at a phenomenon that is precise, schematized, impregnated with theory. This phenomenon is not **found**, but instead it is **produced**. Modern science tends more and more to become a science of effects. One [even] designates these effects after the name of their inventors. Thus one speaks of the Zeeman's, Stark's, Compton's, Raman's effect. . . . (Bachelard 1933, 138-139).

Instruments are inextricably linked with the phenomena they are supposed to register. This is the case of instruments, such as Millikan's, Stern's, and Gerlach's instruments which "are directly thought in function of the electron and the atom" (Bachelard 1933, 140), and not a function of what exists "out there" in the real world. What matters is to produce the physical effects that have been predicted by mathematical physics [IA, 14], or mathematical chemistry.

Atomistic Intuitions

Bachelard's Intuitions atomistiques (1933) does not bring a lot of new elements to the understanding of **phenomenotechnique**. As in La Valeur inductive de la relativité and Le Pluralisme cohérent de la chimie moderne, the word 'phenomenotechnique' is virtually absent. But once again, almost all its conceptualizations are there, i.e., the idea that contemporary science deals with techniques that are both rational and material, and that these heuristic devices have basically the same explanatory value and the same inventive potential. It is the context in which this problematic occurs that varies in the three books.

Les Intuitions atomistiques is meant to be an historical analysis of the philosophical presuppositions behind atomistic doctrines.[16] Bachelard classifies atomism into different levels, i.e., realistic atomism and idealist atomism (positivist, criticist, axiomatic). This division corresponds to two epistemological directions in the study of the physical world: the one that corresponds to strict rationality and the one that relies mainly on experimentation. The different ways of reconciling reason and experience are seen by Bachelard as proof of the different ways each of them transforms particular intuitions into scientific arguments (Bachelard 1933, 12). He ends up explaining why the last level, axiomatic atomism (which corresponds to modern scientific atomism), constitutes an epistemological break (rupture) with the previous intuitive notions concerning the atom. Missing in the atomism of previous centuries was the realization that one had to "find the means of combining the multiple characters [of the

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[16] Together with La Dialectique de la durée (1936) Les Intuitions atomistiques is a criticism of Bergsonian intuition.

infinitely small] and, with that combination, to construct new phenomena" (Bachelard 1933, 133). In other words, ancient atomism lacked objectification. As Claude Bernard, quoted by Bachelard, would put it, now we deal with active experience and sophisticated technical conditions, which are embodiments of the role of the scientist as inventor (Bachelard 1933, 139). In "L'Idéalisme discursif", which I consider to be one of the most seductive texts Bachelard ever wrote on epistemology, he would say that, in its constant dynamics, the "fate of the scientist is a technical will" (Bachelard 1970a, 90-91)(17).[17]

"Technical will" does not imply subjectivity. We have already seen Bachelard giving attention to the dangers of confusing the coordinating and creative power of both rational techniques and material technology with lack of objectification. In "L'Idéalisme discursif", and in many other places in different works, he makes the point that "little by little, the culture of objectivity determines an **objective subjectivism**. The subject, by meditating upon the object, eliminates not only the irregular traits in the object, but also the irregular attitudes of his own intellectual behavior" (Bachelard 1933, 93). It is the subject who ultimately rectifies knowledge, not reality. It

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[17] I use the reference from the article's publication in Études.

is not reality as we perceive it directly that should constitute the (scientific) basis of our knowledge about the world. In science, we are dealing instead with **realization** of knowledge in an almost Cartesian sense, but with no God, rules of method, "res extensa" or other absolutes included. We will see how Bachelard's "objective subjectivism" has to do with what I have been calling the metarational level of validation judgments.

Despite having addressed the problem quite consistently in works such as the Essai sur la connaissance approchée and the Étude sur l'évolution d'un problème de physique, Bachelard will be concerned with issues of subjectivism in the practice of the scientist mainly in La Formation de l'esprit scientifique: contribution à une psychanalyse de la connaissance objective (1938). For the moment, let us concentrate on the connections Bachelard established between physical mathematics and experience, which also constitutes part of what is behind the concept of phenomenotechnique.

The New Scientific Mind

Le Nouvel esprit scientifique (1934) is an important book for several reasons.[18] One of them is that the word

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[18] A critical study of Le Nouvel esprit scientifique can be found in Jacques Salomon, "M. Gaston Bachelard et Le Nouvel esprit scientifique," La Pensée (janv.-mars 1945):47-55.

'phenomenotechnique' reappears in Bachelard's epistemological discourse on scientific practices. The other reason is that he sets up a more thorough analysis of the relationships between mathematics and experimentation in science. Again, Bachelard will use 'phenomenotechnique' not only as a concept for translating the coordinating and creative role of mathematics in the construction of physics, but also to capture the inclusion of technology as a sort of mediating device that allows theories to be realized, to be produced in the laboratory. In this context it is appropriate to talk of the technological axiomatics which organize research as much as scientific theories and mathematical postulates do.

Instruments are "reified theories" because they are built as a function of what they are meant to observe. And to observe, in science, is always to verify, i.e., confirm or disconfirm, previous theses:

> It is necessary that the phenomenon be sorted, filtered, expurgated, cast in the mold of instruments, produced at the level ('sur le plan') of the instruments. . . . What comes out are phenomena that carry in themselves the theoretical brand (Bachelard 1978, 16).

In the process of establishing analogies between mathematical techniques and instrumental techniques, Bachelard says, for instance, that tensor calculus is a "mathematical instrument that creates contemporary physics the same way the microscope creates microbiology" (Bachelard 1978, 58). In the same way that there would be no microbiology without scientific technology to materialize theoretical suppositions about the constitution of living matter, microphysics would be impossible without the noumenological possibilities given to science by mathematics. These notions enable us to understand better the primary role that should always be given to theory formation and the creative aspect of scientific thought reified in the technical production of scientific objects. The realization of knowledge in the physical experiment is a **mathematical** realization:

> Experiment is under the dependency of an intellectual construction; one searches on the abstract side for the coherence of the concrete. The picture of the possibilities of experience is thus in the framework ('cadres') of axiomatics (Bachelard 1978, 44).

When one considers, in microphysics, "instrumental products" such as photons, electrons, atoms, fields, and currents, it is of realization, not of reality, that one should talk. These products should then be acknowledged, not as material, substantial, but instead as logical products (Bachelard 1978, 137). They are products of theoretical science.

To Bachelard, it is the mathematics which opens up new possibilities to scientific experimentation (Bachelard 1978, 62). These possibilities coincide with the overturn of realism in science. Mathematics is not just the language in which the universe is written, as Galileo put it a few centuries before. Within **technical realism**, the mathematical order imposed on reality makes it just one particular case among what is rationally possible (Bachelard 1978, 62). The transmutation of material realism into mathematical realism makes the concept of number "an attribute . . . of the substance" (Bachelard 1978, 83). In the case of chemistry, for instance, we witness the "surreptitious passage from the chemical body to the mathematical body, where the latter is taken in its technical mathematical sense" (Bachelard 1978, 84):

> First, reality transforms itself into mathematical realism, then mathematical realism dissolves itself into a sort of realism of quantum possibilities. . . . [One accepts] to think all reality ('le réel') in its mathematical organization; better, one gets used to metaphysically measuring the real by means of the possible (Bachelard 1978, 86).

Anything in science that requires **some** form of technology has to deal philosophically with issues of both realism about theories and realism about entities. In the case of Bachelard, a theory is about entities that have a logical identity, i.e., a meaning within the frame of a particular system of thought. If scientific reality is what comes out of experiments produced in the laboratory, then electrons and currents do have a specific **onto-logical** status. They logically **belong** to an existential realm, a closed system, which is different from the "outside world" because it is artificial instead of natural, a rational construct rather than a given:

> The task of the scientific mind is clear: we should appreciate existence in terms of experience and we cannot retain valid information about the location of an object outside the experimental circumstances of location itself. For instance, if we want to **see** where one supposes the presence of corpuscles in a given instant, we will direct a beam of light, a cluster of photons. One may expect one of these photons to be deviated by one of the corpuscles; the photon deviation provoked by the **clash** against the corpuscle will sign the presence of corpuscles in the bombarded area . . . (Bachelard 1937, 34-35).

To me, the above statement connects with the concept of phenomenotechnique. Unfortunately, Bachelard does not spend any time developing philosophical considerations about the instruments that, for instance, originate the beam of photons in the first place. But the simple fact that he frames the existence of scientific objects to their own experimental situations is by itself revealing of the status of instruments in laboratory environments. On the other hand, theoretical entities such as the atom produce effects that constitute proof of its **existence in the material world**. It was only a matter of time -- "time charges itself with giving reality to the probable" (Bachelard 1978, 122) -- until something merely postulated ends up being considered part of the very infrastructure of reality.[19] In La Philosophie du non (1940) Bachelard will say the same regarding the mechanics of Dirac. Dirac's concept of negative energy was confirmed by the experimental discoveries of the positive electron by Blackett and Occhialini (Bachelard 1968, 31).

The optimism of Bachelard regarding the achievements of contemporary science is not so much the creation of interpretive systems per se, but the pedagogical potential of these systems, i.e. the possibility of understanding reality through systems that help the scientist to look at things differently. The utilization of instruments is a way of testing theories against previous systems of approximation and against reality itself. There is no question of how **real** the atom is, but how the atom constitutes a functional building block in the dynamics that constantly goes on between the mind and the world, i.e., between reason and experience.

We have already seen how Les Intuitions atomistiques is almost entirely devoted to the study of the deformation of

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[19] Additional information concerning Bachelard's rationalism can be found, for instance, in Jean-Claude Pariente, "Rationalisme et ontologie chez Gaston Bachelard," Bulletin de la société française de philosophie 79.1(1985):1-36.

the concept of atom. Other examples of concept transformation given by Bachelard are the concept of **temperature** and the concept of **space**. In La Formation de l'esprit scientifique, for instance, Bachelard says that the concept of temperature varies with the scientific context in which it appears. Talking about the "temperature of the atom" has different connotations than when we refer to "temperature" on a scale thermometer. "Designation does not imply explanation" (Bachelard 1986, 17), and "the richness of a scientific concept is measured through its power to deform itself" (Bachelard 1938, 61). The same thing happens with the concept of space. In L'Expérience de l'espace dans la physique contemporaine (1937), Bachelard says that, in microphysics, we can no longer talk about the precise location of an object, the way it had happened in pre-quantum physics, i.e., Newtonian physics. "For a microphysicist, to touch a body ('un corps') is as metaphorical as to touch a heart" (Bachelard 1937, 50). The topology of the phenomenon changed from the assumption of the object's presence in an absolute, stable space to its **probability of presence** (Bachelard 1937, 29) in a space that is also probable. A similar idea is present, for instance, in the article "La Psychologie de la raison" where Bachelard says that the characteristics of an object are in accordance with the principles of localization, and that these principles have been "shaken" by microphysics (Bachelard 1972a, 30).[20]

"[In microphysics] 'spaces' multiply themselves, and experiment divides itself also" (Bachelard 1937, 139). "Place" used to help determine the existential quality of an object (Bachelard 1937, 5; 59), because Euclidean space was taken as the only possible mathematical translation of something that was unchangeably "out there." We now shift to a sort of statistical location. There are a lot of possible "spaces," because "space" too is a human fabrication. The same idea of the probability of the presence of micro-physical objects had already been expressed by Bachelard in Le Nouvel esprit scientifique in the following terms:

> in the same way that it is impossible to know the precise position of an electron, the exact knowledge of amplitudes in each point of an area occupied by a wave is [also] manifestly impossible. Every experience of measure cannot furnish but the average value of the amplitude in an area of space and in an interval of time which cannot be reduced to a point and an instant (Bachelard 1978, 94).

Actually, these notions are not different from what Bachelard had already said in the Essai sur la connaissance approchée and later in La Philosophie du non (1940). In fact, Bachelard defines scientific concepts as elements of

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[20] I use the reference from the article's publication in L'Engagement rationaliste.

construction (Bachelard 1987, 19) or instead as obstacles that "block knowledge instead of summarizing it" (Bachelard 1968, 19], and thus as sources of both scientific error and knowledge rectification. Obviously, although these concepts are "centers around which knowledge of reality condenses" (Bachelard 1987, 19), they only have their full significance as part of the construction of knowledge and never as isolated elements. The scientist also errs if she does not consider concepts as arbitrary and constantly capable of reshaping and adaptation according to their applicability conditions:[21]

> Even if we confine a concept to pure logic, we can find a trace of arbitrariness in the limitation of understanding. A concept is . . . a real rule through which we get the characteristics we consider sufficient for recognizing an object . . . . Conceptualization is undoubtedly an effort towards objectivity, but . . . this objectivity will develop in unexpected ways [as it is] the mind that projects multiplied schemes, a geometry and even a method of rectification (Bachelard 1987, 24).

The dynamics of scientific thought involve a particular phenomenology and thus a certain notion of what should be considered as "real." To Bachelard, science, and in particular contemporary science, should only use "reality"

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[21] This idea can also be found in Bachelard's article "La Psychologie de la raison" where he argues that in contemporary science function becomes more important than structure [PR, 28]; see also "La Pensée Axiomatique." Here Bachelard says that one should not judge fundamental notions by their realism but by their function.

in two ways: as an "excuse" for scientific thought to occur (Bachelard 1978, 10) and as something that science builds itself "against." Rationality always has priority over immediate observation. This "opened rationalism" (Bachelard 1978, 179] is what allows Bachelard to talk about the incommensurability of both concepts and procedures between Euclidean geometry and non-Euclidean geometries, Newtonian and non-Newtonian mechanics, and so on.[22]

In the same way that there is no transition between Euclidean and non-Euclidean geometries, there is no continuity between the Newtonian system and the Einsteinian system (Bachelard 1978, 46). In La Valeur inductive de la relativité, Bachelard synthesizes this incommensurability in the following terms:

> We do not see in Newton's system something that would enable us to predict Einstein's and thus undermine the truly transcendental novelty of the modern system. There is no possible inference from the first to the second . . . . It is not possible to talk of approximation between both systems . . . . Even in their search for precision, these two methods are irreconcilable, as they follow two completely heterogeneous modes of thinking . . . . The Newtonian system does not neglect **part of something**, it neglects **something else** . . . . Relocated . . . at the center of the Einsteinian synthesis, Newton's construction represents nothing but a state of numerical evaluation. If we take it separately, that construction loses its realistic value. It only has a pragmatic value (Bachelard 1929, 43-48).

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[22] For more on the concept of discontinuity, see Socratis Delivoyatzis, "Le Continu et le discontinu chez Bachelard," in Bachelard: l'homme du poème et du théorème (1984):177-185.

To Bachelard, Einsteinian relativity is the example "par excellence" of an epistemological rupture between contemporary scientific knowledge and both common knowledge and classical science. This point is particularly developed between 1927 and 1940. <u>La Valeur inductive de la relativité</u>, <u>Le Nouvel esprit scientifique</u>, and <u>L'Expérience de l'espace dans la physique contemporaine</u> (1937), for instance, offer a full account of the conceptual history of relativity and its epistemological implications. Both aspects, as we have already seen, stem from (a) the identification of contemporary science as a science of relations and of "functional concepts" (Bachelard 1978, 49]; (b) the articulation of the fundamental principle of rectification through approximation to a "probabilistic phenomenology" (phénoménologie probabilitaire) (Bachelard 1978 121); (c) the methodological importance of "induction" and "synthesis" as contrasted with the use of "deduction" and "analysis" in modern scientific reasoning.

The Experience of Space in Contemporary Physics

The constructive power of the new mathematics is what basically leads Bachelard to say that contemporary physics cannot do without mathematical methodologies, and that number becomes part of the substance of scientific objects. In the same way, experience cannot do without the use of mathematical physics, because it is mathematical physics that, with the help of inductive inference, diversifies it by giving it the "plan of the possible" (Bachelard 1937, 95).

In a sense, the need for approximations and rectifications in science has to do with the degree of uncertainty and guess work that comes from the concept of "relation" itself. At the same time, the probabilistic nature of scientific objects and explanations, formulated for instance in Heisenberg's indeterminacy principle -- which Bachelard sees as the "primary principle that should systematically be put at the basis of all reasoning" (Bachelard 1937, 40-41)[23] -- does not reduce the importance of objectivity in science, but instead reshapes it into what Bachelard calls "objective indetermination" (Bachelard 1978, 126):

> [In microphysics] the primary source of objectivity is not the object, but the objective method -- it is not the content, it is the container -- it is not the final term of approximation, it is the method of approximation. The certainty values are attached more to the experimental preparation than to the experiment . . . . It is more certain to designate reality ("le réel") by means of the operations that produce the phenomenon. What we can restore ("restituer") of a well defined identity is only our experimental attitude. The objectivity about which we can understand each other is an objectivity of information, a framing objectivity ("objectivité d'encadrement") . . . . There is no sense, then, in separating the notion

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[23] See also Bachelard 1937, 73; Bachelard 1968, 88.

> of objectivity from the notion of reality (Bachelard 1937, 85).

We are dealing with mathematical possibilities that "belong" to the real phenomenon (Bachelard 1978, 60), i.e., scientific reality ("le réel scientifique") (Bachelard 1978, 176). In the "stylized nature" of the laboratory (Bachelard 1978, 170), we are looking for progressive objectification, a way to reconcile rational truth and empirical truth, rationality and realism. So, there is no sense in making "a radical distinction between the scientific mind instructed by mathematics and the scientific mind instructed by physical experiment" (Bachelard 1978, 133). Once again we return to the **phenomenotechnical** characteristics of contemporary science, i.e. the use of artificial devices (techniques and mathematical constructions) with which the scientist virtually builds the scientific worldview. Experiment becomes a matter of "axiomatic decision" (Bachelard 1937, 81). In this context, phenomenotechnique must "reconstruct at the level in which it was rediscovered by the mind the phenomenon in all of its pieces, by removing parasites, perturbations, mixtures, impurities that abound in brute and disorganized phenomena" (Bachelard 1937, 139).

Psychoanalysis of Science: The Hunting of the Break

In La Formation de l'esprit scientifique: contribution à une psychanalyse de la connaissance objective (1938) Bachelard undertakes a personal psychoanalytical reading of episodes in the history of science, including alchemy, in order to identify more accurately than before the roots of **epistemological obstacles** to the progress of scientific knowledge.[24] The book constitutes further elaboration of subjects that had been dealt with before. This is the case with notions such as "technical will" and "objective subjectivism" as presented in "L'Idéalisme Discursif," as well as an assessment of the value of instrumentation in science. We will see in the following pages how those problems connect with the concept of phenomenotechnique.

To Bachelard, it is important to acknowledge that the work of the scientist is constantly disturbed by what he calls "epistemological obstacles."[25] These obstacles make

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[24] For a critical study of La Formation de l'esprit scientifique, see Jean Leméere, "À propos d'un livre de M. Gaston Bachelard: Formation de l'esprit scientifique: contribution à une psychanalyse de la connaissance objective," Revue internationale de philosophie 1(1938):189-191.

[25] Recent applications of the concept of epistemological obstacle can be found in Joseph Chenu, "Obstacles épistémologiques: le choléra au XIXéme siècle -- errance et divagation", pp.103-119, and Montandon, Christian "Actualité d'une notion bachelardienne: l'obstacle épistémologique en psychologie sociale", pp. 121-132, in Gaston Bachelard:

the scientific mind crystallize around "intellectual habits" (Bachelard 1986, 4) which prevent it from constantly rectifying knowledge or from discovering new interpretive possibilities of the same phenomenon. Although Bachelard does not talk much about **contemporary** epistemological obstacles -- except maybe when he mentions our tendency still to correlate the shape of micro-objects with the forms we perceive with our senses (e.g., the electron is spherical) -- he devotes a lot of attention to certain concepts that were transformed into quasi-myths until the eighteenth century, and thus constituted factors for knowledge degeneration.

"Substantialist concepts" (Bachelard 1986, 87) and "animist concepts" (Bachelard 1986, 147), for instance, are epistemological obstacles. More specifically, biological concepts such as "fermentation," "coagulation," "digestion," or verbal concepts such as "sponge" and "pore" were used to make misleading generalizations. In fact, they were extended, by analogy, to the functioning of all natural phenomena. As Bachelard puts it, "it is as obstacles to the objectivity of **physical** phenomenology that biological knowledge should retain our attention" (Bachelard 1986, 149). Since they became **convictions**, these concepts

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l'homme du poème et du théorème (1984).

prevented the scientist from looking at things from a non-organic, non-substantialist point of view. Thus, the psychologic seduction for easy explanations (Bachelard 1986, 55), metaphors (Bachelard 1986, 78), first experiences, the heuristic idea of a total correlation between phenomena (Bachelard 1986, 219), personal interests, and affections (Bachelard 1986, 195) all constitute obstacles to objectivity.[26] What Bachelard suggests in an almost Weberian sense are the constant self-correcting strategies the scientist has to follow to liberate herself, by means of a "truly intellectual and affective catharsis" (Bachelard 1986, 18), from everything that, in her work, does not directly pertain to the research process.[27] Culture infects the scientific mind. Functioning as a sort of

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[26] For additional information on the importance of epistemological obstacles and error in science, see, for instance, Francesco Barone, "'Verita' ed 'Errore' nell'epistemologia de Gaston Bachelard", Revue internationale de philosophie 22(1963): 453-476.

[27] See Max Weber, Economy and Society (Berkely: University of California Press, 1978). Bachelard's psychoanalytical assumptions here are mainly Jungian. For more information on Bachelard's psychoanalysis see Dominique Lecourt, Bachelard ou le jour et la nuit (Paris, 1974); Bernard Grasset, "La Psychanalyse dans l'epistemologie?", pp. 119-147; see also the chapter "Bachelard et la psychanalyse" in Bachelard (Paris: '10-18', 1974). This section includes the following articles: Marie-Louise Gouhier, "La Rencontre," pp. 138-147; Anne Clancier, "La Psychanalyse du Feu," pp. 148-155; Gouhier, "Accomodement de la rupture," pp. 160-174; Georges Canguilhem, "Gaston Bachelard, Psychanalyse dans la cité scientifique?," Il Protagoras 24(1984):19-26.

hermeneutical devices, both psychoanalysis of scientific thought and history of science are meant to identify all cultural, intellectual and intuitive variables that constitute obstacles to the normal path of scientific thinking processes. In this respect, the influence of social factors is more an influence that has to be discovered and discarded rather than part of science's destiny.

Psychological obstacles tend to be more serious than those which are present in science for lack of technological sophistication. In fact, even with instrumentation as a mediator between reason and observation, what is observed depends mainly upon interpretative systems of thought that are loaded with preconceived, sometimes even prescientific notions, like the above ones.[28] Thus, it is in the act of knowing that most obstacles occur (Bachelard 1986, 13; 132). The task of the epistemologist, face to face with the history of science, is to psychoanalyze it and realize how objective values themselves hide a priori subjective values (Bachelard 1986, 43):

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[28] Bachelard divides history of science in three quasi-Comtean states: <u>prescientific</u> (classic Antiquity and Renaissance, plus sixteenth, seventeenth, eighteenth centuries), <u>scientific</u> (end of eighteenth, all nineteenth and the beginning of the twentieth century), and the <u>new scientific spirit</u> (from 1905 on). [Bachelard 1986, 7]

> One can read Buffon's natural history . . . by observing the observer, by adopting the position of a psychoanalyst. . . . One will understand that the portraits of animals, marked with the sign of a false biological hierarchy, are charged with traits imposed by the unconscious dreams ('reverie inconsciente') of the narrator (Bachelard 1986, 45).

For those who might think that these obstacles are directly correlated with the fact that past science is subjective and qualitative instead of quantitative and objective, Bachelard warns that, by itself, **measurement** does not prevent obstacles from forming in the mind of the scientist. As he puts it, "size" ("grandeur") is not necessarily objective and it is sufficient to leave the usual objects in order to accept the most bizarre geometric determinations, the most whimsical ("fantaisistes") quantitative determinations" (Bachelard 1986, 211). Cartesian physics is one such case. It looks like a very precise, clear, evident and objective geometry, but it lacks a doctrine of measurement. In fact, what happens is that "numerical determinations should not, **in any case**, overtake in exactitude, the means of detection" (Bachelard 1986, 216), that is to say, data provided by a particular technique or a specific technological device.

Because Bachelard only talks of phenomenotechnique in the context of contemporary physics -- which includes relativity, quantum theory and modern chemistry -- one might think that the use of mathematics as a methodological procedure cannot be found in science before the creation of these sciences. However, since we have seen Bachelard encompass within the concept of phenomenotechnique techniques that range from the purely abstract to the strictly material, the concept can also be used to identify scientific techniques before the nineteenth century. Although the word phenomenotechnique is again strangely absent in La Formation de l'esprit scientifique, the book's account of the role of instrumentation in physics allows us to say that the problematic is nevertheless present, this time under the cover of what Bachelard calls "instrumental phenomenology" (Bachelard 1986, 219).

Technology is so important in science that, according to Bachelard, it is possible to "determine the different ages of a science by [looking at] the technique of its measurement instruments. . . . Each one of the past centuries has its particular precision scales, its exact decimal groups and its specific instruments" (Bachelard 1986, 216-17). Precision has been conquered over time. The first thermometers, for instance, were very imprecise. Because there was no such thing as an "instrumental technique" (Bachelard 1986, 217), the construction of thermometers was not standardized. The knowledge obtained through the use of these instruments, i.e., their "scientific product" (Bachelard 1986, 218), lacked objectivity in the sense that there was still no conception of experimental sensitivity and thus no determination of the sensitivity levels of the scientific apparatus. On the other hand, the scientist did not perceive both theoretical presuppositions and instruments as part of "closed systems" (Bachelard 1986, 217) that have necessarily to follow the **principle of negligence** ('negligeabilité') (Bachelard 1986, 222) in order to construct frameworks of significance. These closed systems include "a complex of screens, sheaths ('gaines'), immobilizers, that keep the phenomenon imprisoned ('en cloture'). All this **built negativism** ('négativisme monté') that [constitutes] an instrument of contemporary physics, contradicts the . . . affirmation of an indeterminate possibility of phenomenological interactions" (Bachelard 1986, 223).

"Knowledge becomes objective in the proportion that it becomes instrumental" (Bachelard 1986, 218). In this context Bachelard talks again of 'technical will', and correlates science's tendency towards objectification with the awareness of the need to constantly verify an instrument's degree of precision.[29] This procedure is

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[29] For additional information on the necessity of correcting the concept of experience by incorporating the function of instruments in concept production see "Technique et expérience" in Dominique Lecourt, Pour une critique de l'épistémologie: Bachelard, Foucault, Canguilhem (Paris: Maspero, 1974):19-36.

essential since errors and perturbations may occur in the most trivial details of the apparatus itself:

> For instance, is there anything apparently simpler than the setting up, under the shape of a barometer, of the Torricelli experiment? But the filling up of the tube [alone] requires a lot of care. And the smallest fault in this regard, the smallest bubble of air that remains, determines noticeable differences in the barometric height (Bachelard 1986, 217).

By looking at the history of the development of scientific technology one is also bound to realize that a specific instrument might have been improved only to confirm/strengthen certain prescientific worldviews. This is, for instance, the case of the microscope. Since its primitive **object** of study (Bachelard 1986, 159) was life, it was applied only rarely to the observation of minerals. When this happened, the observation of the structure of the mineral would lead to an epistemological obstacle, i.e., to the notion that this structure was proof of some form of obscure life still to be 'awakened' in the mineral (Bachelard 1986, 159). In other words, animistic tendencies, present in people such as Buffon and his disciple Poncelet, prevailed for a long time in the observation of microscopical phenomena. Animistic interpretations did not come directly from what scientific instrumentation would reveal. Instead, it is the set of metaphors, values and worldviews existing in the unconscious of the scientist and embodied in theoretical assumptions which, impinging themselves upon what is observed, **distort** the phenomena by transforming them into evidence to support scientific claims about the constitution of the world.

It is easy to see how Bachelard's notion of epistemological obstacle connects "negatively" with both the idea of objectivity in science and the concept of phenomenotechnique. If we have to admit that the scientist is trapped in obstacles that are entrenched in her own scientific thinking processes, it is also true that it is impossible to undertake any kind of experimentation without some set of assumptions which define what is going to be found.[30] On the other hand, the circumstantiality behind any scientific discourse leads the scientist to constantly set in motion processes of rectification to current theoretical assumptions and instrumental precision. Constant rational criticism, to use Popper's terminology, prevents the scientist from accepting scientific absolutes and frozen postulates, at the same time as it allows her to believe in **scientific objectivity**. The constructed aspect of contemporary science comes precisely from this 'control'

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[30] There is a striking similarity between Bachelard's notion of epistemological obstacle and Popper's claims about the impossibility of observing without the use of a theoretical framework, albeit imperfect and biased. See, for instance, Karl Popper, Conjectures and Refutations (1963) or Realism and the Aim of Science (1983).

of the phenomenon given by both mathematics and instruments, i.e., techniques of objectification:

> A modern scientist seeks to limit his experimental domain more than to multiply its instances. In possession of a well defined phenomenon, he tries to determine its variations. These phenomenological variations designate the mathematical variables of the phenomenon . . . . In this mathematical coordination, reasons of variation that remained idle, extinguished or degenerated in the measured phenomenon might appear. The physicist will try to provoke them. He will try to **complete** the phenomenon, to **realize** certain possibilities that the mathematical study has uncovered (Bachelard 1986, 65).

A mathematical understanding of the phenomenon, allied with the awareness of epistemological obstacles, has to do with what Bachelard had previously called "objective subjectivism," and it raises new problems. One of them, briefly mentioned in Chapter 1, has to do with the **locus** of validation judgements within the process of knowledge accumulation, i.e., **who validates what in science**.

Besides lack of standardized instrumentation, there was something else missing in science before the nineteenth century: elaborated research programs involving teams of scientists. When comparing laboratories, for instance the laboratory of Mme. la Marquise du Châtelet in the eighteenth century, with the radioactivity laboratory of Mme. Curie, the differences are striking. One of them is the absence, in the first, of groups of scientists working under specific research programs ('un programme de recherches précis')

(Bachelard 1986, 32). The production of scientific objects -- phenomena impregnated with theory -- necessarily involves the organization of a precise program of research to justify, reify and rectify it. In contemporary science, the responsibility for this organizational aspect, as well as for all techniques of realization (Bachelard 1986, 217), whether theoretical or instrumental, comes from the "scientific city" (Bachelard 1986, 241). Thus, there are two factors involved in the connection between scientific knowledge production and the normative aspects of the scientific city: (1) the work of the scientist is not performed in isolation -- even great discoveries presuppose from now on some form of verification and validation from the community of scientists; (2) modern standardization of instruments and techniques -- as opposed to instrumental heterogeneity of the scientific and pre-scientific periods -- allows superior levels of objective precision and instrumental sensitivity.

The role of the scientific city in coming up with validation rules for science becomes paramount. This regulation is a form of **social control** (Bachelard 1986, 241), and connects directly with the purpose of objectivity in science. Together with technology and mathematics, regulations become an integral part of a constructed process for overcoming epistemological obstacles. As Bachelard says, "we propose to found objectivity on the behavior of someone else ('le comportement d'autrui') . . . to choose someone else's eye . . . to observe the shape . . . of the objective phenomenon: tell me what you see and I will tell you what it is (Bachelard 1986, 241). Being the **social** exercise of a rational conviction (Bachelard 1986, 245), the control provided by the scientific community is the measure of the acceptability of a fact as scientific. The "order of precision" and the "order of growing instrumentalization" belong to a process of socialization. Part of this process, made available and mandatory by the scientific community, is embodied in scientific technology. Measurement instruments are devices for testing rational constructs, and the verification or corroboration of theories end up being determined by the scientific city's potential to **realize** them with the help of all kinds of scientific techniques:

> If you want for example to find the width of an interference fringe and to determine, by means of connected measures, the wave length of a radiation, then you need not only apparatus and groups of crafts ('corps de métiers'), but also a theory and consequently a whole Academy of Sciences. The measuring instrument always ends up being a theory and it is necessary to understand that the microscope is more an extension of the mind than [an extension] of the eye (Bachelard 1986, 242).

This example of what Bachelard had previously called "constructed realism" directly connects with the concept of phenomenotechnique. Phenomenotechnique does not represent the mathematical realization of theories alone, but also the production, purification and isolation of phenomena. These effects are only possible through the use of instruments. Since purification is one of the traits of phenomenotechnique, we can say that the essence of purified substances, i.e., substances that have been **realized** in the laboratory, is totally rational, at least functionally (Bachelard 1986, 249). Ideally, there is no place for irrationality in the laboratory. What cannot be materialized by instruments cannot be subjected to social control. If it cannot be objectified, it is non-scientific and must be discarded as such. Instrumental phenomenology becomes a subset within phenomenotechnique, and it definitely modifies what counts as knowledge in science.

Epistemological Profiles of the Scientific Mind

La Philosophie du non: essai d'une philosophie du nouvel esprit scientifique (1940) is a curious mixture of issues that had already been dealt with in both *Le Pluralisme cohérent de la chimie moderne* and *La Formation de l'esprit scientifique*. Once again, Bachelard develops some of the philosophical implications brought about by modern chemistry, particularly what he now calls "reverse realism," in which chemical substances are normatively, methodologically, and critically described (Bachelard 1968,

48) by means of the chemist's ability to reconstruct them "under the guidance of prior theoretical views" (Bachelard 1968, 47). Reverse realism has to do with experimental systematization and thus with applied rationalism. The products of rationalized, experimental reconstructions are purified substances -- substances without irregularities (Bachelard 1968, 49). Since these normative substances are not the outcome of natural observation, Bachelard thinks we should use the concept of "experimental transcendence" (Bcahelard 1968, 9) to define the instrumental science through which rational constructions are materialized and thus objectified. Obviously, further reflections on modern chemistry -- Bachelard also calls it "technical chemistry" (Bachelard 1968, 50) and "non-Lavoisean chemistry" (Bachelard 1968, 54) -- lead us once more to acknowledge the importance of the concept of phenomenotechnique although, as happened often after its coinage in 1931, the term itself is again absent from Bachelard's epistemological discourse.[31]

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[31] In the presentation of <u>Études</u>, Canguilhem says that the category of phenomenotechnique became truly fundamental to Bachelard's epistemology especially from 1934 on. I disagree. Not only the term does not appear in books such as <u>La Valeur inductive de la relativité</u> and <u>Le Pluralisme cohérent de la chimie moderne</u>, it also 'disappears' in works after 1934. This is the case of <u>La Formation de l'esprit scientifique</u> and <u>La Philosophie du non</u>. If instead we interpret what Canguilhem said in terms of the <u>presence</u> of the presuppositions behind phenomenotechnique then there is no reason for dividing Bachelard's books into 'before' and 'after' phenomenotechnique, since those presuppositions were

Reverse realism coincides with an overturn in what counts as knowledge. The evolution of scientific knowledge implies an evolution towards "rational coherence" (Bachelard 1968, 17), and is the result of "informed experiences" (Bachelard 1968, 51). From the moment science becomes instrumentalized, objectivity redefines itself and becomes dependent upon science's methodological procedures, i.e., experimental reality. "Realization takes precedence over reality" (Bcahelard 1968, 29):

> Objective verification by means of reading off index figures merely designates as objective the thought one is verifying. And very soon the realism of mathematical substances gets to be substituted for the **reality** of the experimental curve . . . . There is a gap in objectification which is why we are justified in saying that experimentation in the physical sciences has a beyondness, a transcendence: it is not closed in upon itself . . . (Bachelard 1968, 9-10).

The ontological consequences of this epistemological attitude, which is none other than a reform of previous rational frameworks [PN, 42], is clear. It implies the postulation of new entities -- scientific as opposed to natural -- with diverse degrees of existence:

> It would be rather too pat to withdraw a total unitary realism all over again and to answer: everything is real, the electron, the nucleus, the atom, the molecule, the colloid particle, the mineral, the planet, the star, the nebula. In our estimation, everything is not real in the same way. . . **existence is not a one-toned function**; it cannot affirm itself,

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present in Bachelard's writings from the <u>Essai sur la connaissance approchée</u> forward.

> everywhere and in the same tone, all the time . . . . [The realist] must accept a laminated reality (Bachelard 1968, 46).

As we have previously seen, the reality provoked at the level of scientific experimentation presupposes the utilization, in science, of special techniques such as mathematical physics and scientific technology.

Although the importance of mathematics in the rational construction of contemporary science is still present, La Philosophie du non seems to be more concerned with expanding the philosophical issues behind the integration of technology in science, the continuation of the psychoanalysis of the scientific mind as it had been presented in La Formation de l'esprit scientifique, and also a deeper study into the transformation of scientific concepts over time. It is not surprising, then, that Bachelard refers quite often to what happens **within** the laboratory setting, and its relations with the scientific mind at work. As we have seen before, the struggle for objectification in science coincides in Bachelard with both instrumental degrees of sophistication and the over-ruling of epistemological obstacles that originate in the very act of knowing.

In the case of scientific technology, Bachelard claims that there was a historical shift in the use of instruments, i.e., from the "instrument preceding its own theory" to a reverse situation in which "the instrument . . . is a realized, concretized theory, rational in essence" (Bachelard 1968, 21). In other words, and again emphasizing the dialectical touch of the relation between reason and experiment, reverse realism entails the acknowledgement that, nowadays, instrumentation is nothing but the materialized extension of a rational construct. "In laboratory practice, the phenomenon must be prepared in order to be produced" (Bachelard 1968, 28). Entering a modern laboratory allows one to establish a direct contact with what **really** happens in the scientific city, and how the archetype of science as a precise translator of nature's features has to be changed into science as a dynamic process in which constant verifiability, rectification, reification and circumstantiality become crucial factors:

> We must impose the following methodological law: no experimental result should be proclaimed as an absolute, divorced from the various experiments which have furnished it. A precise result ought even to be stated in terms of the various operations which produced it in its final form; operations which were at first imprecise, then improved. No precision is clearly stated without some history of initial imprecision . . . no affirmation of purity can be detached from its criterion of purity or from the history of the technique of purification (Bachelard 1968, 61).

Issues such as functionality over realism, contextualization over isolationism, structure over nature, etc., have been dealt with in our discussion of Bachelard's previous works. To avoid redundancy, I will not elaborate on those problems. It is nevertheless important to notice that the purpose of Bachelard's focusing again on the constructive aspects of contemporary science in La Philosophie du non is to make us aware of some more epistemological obstacles entailed by the production of scientific knowledge itself. The detachment of a concept from the validation scale that precedes it is one such error, since "an organization is [only] rational relative to a body of notions" (Bachelard 1968, 26). The idea that an objective experimental method is totally free from subjective elements is another one:

> The scientist thinks that he can start from a mind without structure and without knowledge; . . . [he] fails to see that ignorance is a tissue of positive, tenacious, independent errors. He does not recognize that intellectual darkness has a structure and that, this being so, every correct objective experience must always entail a subjective error. But errors are not easily destroyed one by one. They are coordinated (Bachelard 1968, 8).

Psychoanalyzing the scientific mind means, among other things, gradually eliminating realist notions originally connected with the interpretation of phenomena. Overcoming epistemological obstacles such as the above allows Bachelard to analyze scientific concepts historically, and thus to determine what he calls their "epistemological profiles" (Bachelard 1968, 43).

The originality of La Philosophie du non comes largely from its descriptive study of the epistemological profiles of the concept of mass and the concept of energy, tracing them from their earlier usages to their present, almost strictly functional, attributes. In the case of the concept of mass, for instance, Bachelard detects five levels of usage, that are correlated with philosophical interpretations from the standpoints of animism, realism, positivism, rationalism, complex rationalism and dialectical rationalism (or surrationalism). Just to give some examples, the second level of the epistemological profile of mass corresponds to its association with the use of scales (Bachelard 1968, 21), and the third level "achieves its full clarity at the end of the seventeenth century when rational mechanics comes into existence with Newton . . . . The notion of mass defines itself within a **body of notions** and not merely as a primitive element of direct and immediate experience" (Bachelard 1968, 22). The fourth level corresponds to complete rationalism as exemplified in relativity. The fifth level corresponds to the loss of a phenomenon's objective individuality as dictated by quantum physics (Bachelard 1968, 77), and embodied in Heisenberg's uncertainty principle.

An epistemological profile allows us to measure "the effective psychological action of the various philosophies in the task of knowing" (Bachelard 1968, 35). In Les Intuitions atomistiques we saw Bachelard build up a similar profile for the concept of atom. In La Philosophie du non, he recapitulates the atomic problematic in order to explain better the evolution of rational criticism in science:

> It does indeed seem to us quite impossible to understand the atom of modern physics without calling forth the history of its imagery . . . . [The] atom is exactly the **sum of the criticisms** to which its first representation has been subjected. Coherent knowledge is a product, not of architectonic reason, but of polemic reason. By means of dialectics and criticisms, surrationalism somehow determines a **super-object**. This super-object is the result of a critical objectification, of an objectivity which only retains that part of the object which it has criticized. As it appears in contemporary microphysics the atom is the absolute type of the super-object. In its relationship with images, the super-object is essentially a non-image . . . . The diagram of the atom proposed by Bohr a quarter of a century ago has, in this sense, acted as a good image: there is nothing left of it (Bachelard 1968, 119).

Unfortunately, and although Bachelard acknowledges that the second level in the epistemological profile of mass coincides with the association of mass with the use of scales, he really never develops the connections between the passage from one level to the other (and thus from one philosophy to the other) with the use/evolution of particular scientific techniques, some of which to be materialized in technology. That move could have been particularly useful, as it would emphasize definitely in a renewed way Bachelard's previous claim that an instrument is a materialized theory, i.e., the material extension of a rational construct in search of practical validation and/or realization. The acknowledgement of an evolution in scientific concepts is, however a crucial aspect of Bachelard's 'phenomenotechnique'. Despite his failure to publish books on epistemology between 1941 and 1948, two articles of this period, namely a book review of Stephane Lupasco's L'Expérience microphysique et la pensée humaine , and especially the article "La Philosophie de la mécanique ondulatoire", deal with the same problematic of the technical construction of scientific facts. In the article, Bachelard says the following:

> Phenomena that man had never seen before, but which mathematical intuition discovered in its abstract formulations, did not take long before they were realized by technique. Astounding philosophical moment where the unknown mathematical being formed by the synthesis of the notion of material point and the notion of length of optical wave gives the plan to follow in order to produce a new phenomenon . . . a noumenon is here in action, and technique allied to mathematics makes us cover the metaphysical distance that goes from the noumenon to the phenomenon (Bachelard 1944, 54).

Chapter 3. 1949-1953

After the publication of La Philosophie du non (1940) there was an eight-year period during which Bachelard dedicated himself to issues regarding poetic imagination, literary criticism and esthetics like . More importantly, he devoted a lot of attention to alchemy, the transformation and creation of substances, and a general theory of images. Books such as La Psychanalyse du feu (1938), L'Eau et les reves: essai sur l'imagination de la matière (1942), L'Air et les songes: essai sur l'imagination des forces (1943), La Terre et les reveries de la volonté: essai sur l'imagination des forces (1948), and La Terre et les reveries du repos: essai sur les images de l'intimité (1948) are quite revealing of the above mentioned interests. Although not encompassed by the present study, they are nevertheless fundamental for the understanding of Bachelard's metaphysics, including his scientific metaphysics. They may even be at the basis for the changes in some of the presuppositions behind the concept of phenomenotechnique.

As we saw in Chapter 2, Bachelard did write a few articles on epistemology, namely during 1941-1942 and 1944. However, they were mainly book reviews or synthetic re-enactments of the epistemological views produced prior to

1940. From 1949 to 1953, Bachelard returned to the history and philosophy of science, intermingling this work with writings on other topics that had interested him between 1941 and 1948.[32]

A comparison of Bachelard's preoccupations from 1927 to 1940 and, afterwards in 1949, 1951 and 1953, show that they undergo structural and conceptual changes. Nevertheless, many characteristics of Bachelard's model persisted throughout his later works. In fact, the categories found in previous writings, as well as their articulation, remain much the same. Perhaps this discovery may be considered disappointing, but it should nevertheless be considered as a sign of epistemic consistency in Bachelard's philosophy of science. Another sign is the way he makes constant references to the works of the period between 1931 and 1940 -- particularly to La Formation de l'esprit scientifique and La Philosophie du non.[33]

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[32] The overlapping of subject matters -- epistemology and imagination -- did not happen in this period alone. In 1938, for instance, both La Formation de l'esprit scientifique and La Psychanalyse du feu were published. Lautréamont was published in 1939, just a year before La Philosophie du non.

[33] In the last epistemological period, Bachelard also suggests a return to some of his writings on the elements. For instance, Le rationalisme appliqué points out the importance of La Terre et les reveries du repos. Also, La Psychanalyse du feu is referred to as a seminal example of psychoanalysis of objects -- in this case fire -- that constitute epistemological objects to the progress of science.

The return to the psychoanalysis of scientific thought in Le Rationalisme appliqué (1949) and the epistemology of chemistry in Le Matérialisme rationnel (1953) provide further evidence for the mentioned inner coherence. Again, Le Matérialisme rationnel is considered by Bachelard as providing further evidence for the theses defended in both Le Rationalisme appliqué and L'Activité rationaliste de la physique contemporaine (1951). As before, Bachelard chose to emphasize certain aspects of his epistemology and relegate others to secondary status, not because they became unimportant, but because they are already developed elsewhere. He does not, however, avoid repetition. This redundancy is also one of the reasons why, structurally speaking, it is possible to find many similarities between the earlier and the later books. Bachelard's position concerning, for instance, the importance of redefining scientific realism, phenomenotechnique as a symbiosis of material and mathematical techniques, concept deformation, and so on, remains the same throughout all of Bachelard's works, although the relevance given to one aspect or other may vary quite considerably. At a more superficial level, the extreme descriptive complexity of case studies in the history of physics and chemistry, which had almost disappeared in 1938 and 1940 reappears in 1949 and becomes paramount to Bachelard's philosophical analysis of microphysics and chemistry. However, the agenda now is not so much to explore the history of a single concept, as had happened in Les Intuitions atomistiques or even in La Philosophie du non. Instead, the purpose is to present new case studies -- in which, by the way, concept deformation is always present -- to reinforce the need for a renewed epistemology of science. Bachelard refers to this epistemology as a "philosophy of the hierarchy of cultural thoughts, of active thoughts in a culture " (Bachelard 1975, 15). Part of the dynamics involved in the construction of scientific knowledge has to do with the materialization of theoretical entities postulated by the concept of phenomenotechnique. The difference between the works I analyse here and the previous ones has to do with Bachelard's interest in incorporating into phenomenotechnique the production, by quantum theory, of scientific objects. The novelties brought about by both quantum theory and Bachelard's interest in alchemy during the years between 1938 and 1948 provoked two kinds of transformations in his later philosophy of science. One has to do with the more obvious shift of focus from phenomenotechnique as the illustration of scientific methodologies to that of scientific objects. The other, correlated with the first, is a renewed emphasis on the normative role of the scientific community concerning the validation of these entities produced by theoretical physics.

This chapter will cover the trilogy of books written between 1949 and 1953, as well as providing a brief analysis of articles that were written in the same period.[34] I also discuss in this section small works that were published after 1954, but which I consider to be pure extensions of the same epistemologic block. As with all the other writings encompassed by the present project, I give particular attention to phenomenotechnique and the way this concept is articulated with the rest of Bachelard's philosophy of science.

The Concepts of Relativity

The first work considered is the article "The Philosophic dialectic of the concepts of relativity," written as a contribution to Schilpp's Albert Einstein - Philosopher Scientist (1949).[35] Although it does not

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[34] For additional information on the trilogy 1949-1951-1953 see François Dagognet, "Sur une dérnière image de la science," pp. 147-155 in Gaston Bachelard: l'homme du poème et du théorème (1984).

[35] Pp. 563-586. The article was translated into English by Forrest W. Williams, and published (in French) in L'Engagement rationaliste, pp. 120-136. Professor Suzanne Bachelard told me in a January 1992 meeting that Gaston Bachelard was far from pleased with this article. The reasons for the dissatisfaction were not pointed out, although I do agree that this is definitely not the best account offered by

address, at least directly, the issue of technology, the article has overtones of Bachelard's notion of phenomenotechnique as it had been approached in previous epistemological statements. This can be seen by the way Bachelard stresses once again the constructed aspect of physics as revealed through the dialectic coherence between its mathematical and experimental components. The article talks about the necessity, originating with Relativity, of approaching concepts such as those of space, time and velocity in a different way. These conceptual changes are directly correlated with the renewed ways in which both reality and knowledge are taken into account in contemporary physics.

The approach to scientific phenomena taken by Relativity is a direct consequence of what Bachelard calls "technological science" (Bachelard 1949a, 367), and the fact that, now, objectivity is nothing but the "aggregate of facts verified by modern science " (Bachelard 1949a, 570-71), i.e., the result of a verified instead of a given reality. The abstract-concrete character of scientific thought (Bachelard 1949a, 574), or, in other words, the coherence of its mathematical and material content, allows one to talk about a new kind of realism. This realism

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Bachelard on the philosophical implications of Relativity.

coincides with the idea, already expressed in La Philosophie du non and elsewhere, that contemporary physics is a rational construction. Taking the example of Relativity, Bachelard says:

> The laboratory technician has succeeded in **implementing** by means of the atomic pile the Einsteinian principle of inertia and energy. The reality which slumbered in his materials was **provoked** by mathematically-founded experiments. Seen from the nuclear level, one might well say that matter evokes a neo-materialism in which substance and energy are interchangeable entities. Reality is no longer nature pure and simple. It must be wrought to become the object of scientific experiment. Thus, the philosophy of contemporary science as it issued from the revolutions of the beginning of the century appears as a dialectic of enlightened rationalism and elaborated realism (Bachelard 1949a, 577-78).

"Enlightened rationalism" and "elaborated realism" are none other than notions that Bachelard had already dealt with under the names of "applied rationalism" and "constructed realism." He uses them to identify the particular philosophies of action reflected in both contemporary physics and modern chemistry.

Applied Rationalism

Le Rationalisme appliqué (1949) starts out precisely with Bachelard's usual criticism of traditional realist attitudes in science.[36] Traditional philosophies are

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[36] This criticism is thoroughly developed in L'Activité rationaliste de la physique contemporaine.

schematically positioned in correlation with applied rationalism and technical materialism (Bachelard 1975, 5):

Idealism
↑
Conventionalism
↑
Formalism
↑
Applied Rationalism and Technical Materialism
↓
Positivism
↓
Empiricism
↓
Realism

As we can see from the above chart, idealism and realism, for instance, represent the philosophical extremes of pure rationalism or pure materialism in science. Although they correspond to stages of science in its connections with common knowledge and past scientific achievements, they are completely outmoded. They do not represent in any way the necessary dialectic between reason and experience defended by applied rationalism. All other philosophies are simple approximations to the real scientific philosophy.

The above presentation is useful to Bachelard's epistemological agenda. First, it synthesizes what he attempted to explicate in earlier books. Second, it introduces the dialectic purposes of both scientific thought and scientific philosophy. Third, it enables Bachelard to return to the issue of the **artificiality of scientific reality**, and thus of the constant processes of rectification occurring in science. The discontinuous evolution of science helps us in building the historical development of reason, the history of rationalist culture (Bachelard 1975, 31) in its spiritual activity (Bachelard 1975, 37). At the same time, the study of science enables one to detect both the epistemological paths of the sciences and to determine to what extent, due to their different stages of advancement, they are bound to be interpreted by different degrees of philosophical analysis (Bachelard 1975, 10).[37]

As I said before, <u>Le Rationalisme appliqué</u> is both a return to and a reelaboration of themes that had been the particular focus of attention of books such as <u>La Formation de l'esprit scientifique</u>, i.e., psychoanalysis of scientific thought, and the necessity of understanding the cognitive processes behind both experimentation and the scientific interpretation of phenomena. <u>Le Rationalisme appliqué</u> also has something to say about instrumentation in science and the constructive frame of scientific production of knowledge embodied in the concept of phenomenotechnique. Both the new

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[37] Bachelard will return again to the problem of the role of the historian of science in <u>L'Activité rationaliste de la physique contemporaine</u>.

categories of "mutual awareness" ("interconstatation") (Bachelard 1975, 58) and "intentionality" represent Bachelard's familiar preoccupation with the subject herself as a mixture of culture, consciousness, and rationality:

> When it comes to . . . scientific thought, we cannot trust the immediacy of a "non-me" as opposed to "me." The scientific object is positioned in the perspective of its definition after the "me" was already in a particular kind of thinking and a particular kind of existence. The rationalist "cogito" that tends to affirm the thinking subject in an apodictic thought activity must function as an emergence above an already more or less empirically affirmed existence (Bachelard 1975, 50-51).

This aspect of subjectivity is just an extension of Bachelard's psychoanalytical speculations. In this respect, Le Rationalisme appliqué (1949) is not an improvement over La Formation de l'esprit scientifique (1938), although the vocabulary used in 1949 is perhaps more phenomenologically oriented than before. However, the category of "mutual awareness" is of more interest here, as it seems to give Bachelard's epistemology certain "sociological" characteristics, also reinforced by the constructive nature of scientific knowledge as entailed by phenomenotechnique:

> [Mutual awareness] forms itself before the agreement between the "I" and the "You," because in its first form it appears within the isolated subject, as a certainty of agreement with a rational other ('l'autrui rationnel') . . . . It is possible to force awareness: as I acknowledge that what I just thought is a normality to normal thought, I have the means to force you to think what I think . . . . You will think what I thought as soon as I make you aware of the problem that I have solved. We will be united in the proof as

> long as we have the guarantee of having clearly posed the same problem . . . . There is consecration of method, proof of thought efficiency, socialization of truth (Bachelard 1975, 58).

The general study undertaken in the next chapter in Husserlian phenomenology will help to better interpret the above passage, particularly the concept of intersubjectivity implicit in Bachelard's notion of "mutual awareness." For present purposes, I will stress merely Bachelard's emphasis on what he considers to be a "tacit agreement," i.e., an assumed role of the scientific community and technical city. This agreement directly connects with two qualitatively different but intertwined factors: (1) the possibility given to the scientific community of rationally assessing the degree of a theory's explanatory adequacy; and (2) the implications, for science, of the standardization of measuring instruments and scientific methodologies.

In the first case, Bachelard is once again dealing with issues that had been originally presented in works such as the Essai sur la connaissance approchée. They have to do with **validation processes** within the scientific city and with what Bachelard now calls the "surveillance" function of scientific culture (Bachelard 1975, 14).[38] This function encompasses a psychology of obstacles to knowledge

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[38] For more on Bachelard's concept of "surveillance" see Gaston Fraysse, "Bachelard et la philosophie," in Gaston Bachelard: l'homme du poème et du théorème (1984).

(Bachelard 1975, 15). It includes a reaction "against the psychism which is supported by **ordinary consensus** "(Bachelard 1975, 22), as well as an intersubjectivity that comes from "mutual admiration" towards the fecundity of rational organization (Bachelard 1975, 45), i.e., its "aesthetic character." Obviously, it is not possible to separate the notion of intersubjectivity from the notion that, to achieve "mutual awareness" one needs to temporarily accept the theoretical and pragmatic presuppositions residing inside a particular "program of experiences" (Bachelard 1975, 45). In other words, one needs to rationally accept the existence of instances of observation (Bachelard 1975, 41).

The coherence of this kind of **circumstantial objectification** in science is, for instance, represented by the use of precision scales, and aims at the complete rationalization of the scientific object (Bachelard 1975, 53). Since "the position of a scientific object . . . demands ('réclame') a solidarity between method and experience" (Bachelard 1975, 56), the intellectual surveillance brought about by individuals belonging to the scientific city extends beyond the subject and the object of knowledge, in order to also encompass scientific methodologies. "Special censorship" (Bachelard 1975, 79), which includes normative values such as control, verification, confirmation (Bachelard 1975, 59), and corroboration, connects directly with scientific consensus over scientific technology itself. This leads us to the second point indicated above, i.e., the standardization of measuring instruments and thus a sort of "technological axiomatics" (Bachelard 1975, 133) which is discussed by Bachelard under the category of phenomenotechnique. To Bachelard, the character of scientific proof cannot be separated from the technical materialism surrounding theoretical claims about phenomena. Technical materialism prevents scientific knowledge from achieving solidarity with the "imperialism of the subject" since it corresponds to the objectification of a transformed, rectified reality (Bachelard 1975, 8). With technical materialism, scientific objects become more and more substances without accidents (Bachelard 1975, 7).

The role of scientific instrumentation in this context is clear. Designed with a "theoretical destination" (Bachelard 1975, 2), instrumentation is the material intermediary necessary to construct the scientific object, the true "object of phenomenotechnique" (Bachelard 1975 ,3). This is what happened, for instance, in the beginning of wave mechanics, where, as Bachelard puts it, "one looked for a phenomenon that was the equivalent, for the electron, of the phenomenon of polarization of light" (Bachelard 1975,2).

Since now we cannot talk but of scientific reality ('le réel scientifique') and this kind of reality is strictly dependent upon technology,[39] the measure of scientificity is the measure of the progressive capacity of technology to achieve increasingly higher degrees of precision and sophistication.

Technique and its expression in technology are fundamental for both the construction of scientific reality and the rectification of scientific knowledge. The realism that comes from scientific technology implies a "derealization of common experience" (Bachelard 1975, 137), and thus a new approach to epistemological concepts such as confirmation, verifiability, and objectivity. Technical materialism changes what "reality" means to the scientist.[40] Again, objects produced by the material extension of theoretical presuppositions allowed by technology are indirect scientific objects of a phenomenotechnique:

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[39] In "L'Invention et technique: réalites et possibilités" Bachelard will say that "there is no science without technique the same way that there is no technique without science anymore." [IT, 78]

[40] For additional information on technical materialism see Dagognet, "Brunschvicg et Bachelard", Revue de métaphysique et de morale 70(1965):43-54 (especially 50-52); see also David William Theobald, "Eclectism in the philosophy of science," Technology and Society 5.2(1969):56-59 (especially 57-58).

> The single fact of the **indirect** character of the determinations of scientific reality ('réel scientifique') places us in a new epistemological realm. For instance, as long as what mattered, in the positivist mind, was to determine atomic laws, the technique -- no doubt a very precise one -- of the 'scale' was enough. But when, in the twentieth century one picks up ('tire') and "weighs" isotopes, an indirect technique is needed. The **mass spectroscope**, indispensable for this technique, is founded upon the action of electric and magnetic fields. It is an instrument that one should rightly qualify as **indirect** if compared with the scale. . . . The electrical phenomena of atoms are **hidden**. One needs to instrumentalize them in an apparatus that does not have a **direct** signification in ordinary life. . . . It is the noumenal preparation of phenomena technically constituted. The trajectories that allow the separation of isotopes in the mass spectroscope do not **exist** in nature; one has to produce them technically. They are reified theorems . . . [which] are not even a **natural** sequence from natural phenomena (Bachelard 1975, 103).

Compared with what Bachelard had already said about instrumental epistemology, these notions are not totally new. He seems to go a step further in the development of the conceptualization behind phenomenotechnique when he says that, within phenomenotechnique, the "temporal succession of phenomena often develops itself following the causality of thought" (Bachelard 1975, 78). But this assumption can also be seen as a straightforward consequence of Bachelard's previous claims about scientific techniques being both of a material and a mathematical order, and also the philosophical presupposition that instruments are materialized theories. It is, nevertheless, interesting to see Bachelard spending more time with these issues in Le

Rationalisme appliqué and L'Activité rationaliste de le physique contemporaine than anywhere else in his work.

Le Rationalisme appliqué ends with speculations about machines like the X-ray apparatus which, according to Bachelard, do not possess any trace of irrationality in them (Bachelard 1975, 178), since they are strictly artificial products of rationality. We have seen Bachelard talking about scientific objects as tending towards pure rationality before. The impurity and irrationality of nature is now also annihilated in the gradual perfecting of scientific technology:

> Even the matter that enters in the realization of a modern machine is rationally guarded by such a body of rational precautions that it does not stay, in terms of rationality, behind the most detailed geometrical adjustments (Bachelard 1975, 178-79).

In "phenomenotechnique . . . everything develops itself in the direction of applied rationalism" (Bachelard 1975, 169). Objectivity in science becomes **objectivity of what is abstract** (Bcahelard 1975, 175). This does not in the least undermine the role of technology in science. On the contrary, the character of proof in science will depend on the "mutual awareness" coming from scientific observation that was made possible through the use of both instrumental and mathematical techniques. **What cannot be realized technically does not belong to science**. Only scientific techniques allow the testing of theories and thus the tacit agreement concerning what counts as knowledge.

Curiously, the idea of tacit agreement within the technical and scientific city and entailed by the collective intellectual surveillance of the scientist's ego is more systematized in the works of 1949 to 1953. However, two factors prevent these notions from collapsing into pure sociology: (1) the classification of the new sciences as revolutions of reason (Bachelard 1975, 45); and (2) the notion that the scientific city is a **social** city, but nevertheless has to establish itself independently of the rules of the larger social framework:

> If the spirit formed itself **directly** inside the scientific city, we could do an economy of psychoanalysis of psychologism and directly establish the principles, not of reason . . . but the principles of the rational organization of scientific culture. But that is not the case. The scientific city is established at the margin of the social city. So the scientific city must fight against a particular psychology in order to create a new psychologism. [The scientific city] will always transcend not only common knowledge, but also the primary knowledge of culture ('connaissance de première culture') (Bachelard 1975, 22-23).

Rationalism in the Scientific City

We cannot neglect the epistemological consequences of Bachelard's orientation towards an analysis of scientists' and technicians' relationships. To Bachelard, the scientific city became a "social" reality at the beginning of the nineteenth century. As he says in L'Activité rationaliste de le physique contemporaine (1951),[41] by then the scientific city was embodied in the "Great Scientific Schools under the Revolution [and] the Institute under . . . the Empire" (Bachelard 1951, 42). From then on, scientific knowledge required the theoretical and technical organization of hierarchies of scientists working under particular research programs.

"Constructive realism," as Bachelard also calls it, establishes criteria for knowledge objectification, theory choice and permanent instrumental verification. More, the rational and technical coherence of the scientific city is seen by Bachelard as a trait of the progressive history of the socialization of science. This point was made, for instance, in his article of 1951 "L'Actualité de l'histoire des sciences," and is particularly clear in L'Activité rationaliste de la physique contemporaine when Bachelard says:

> Following contemporary physics, we abandon nature to enter the factory of phenomena. Rational objectivity, technical objectivity, social objectivity, are from now on three strongly connected aspects. If we forget but one of these aspects of modern science, we enter the domain of utopia (Bachelard 1951, 10).

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[41] For a critical study on this work see, for instance, Raymond Ruyer, "L'Activité rationaliste de la physique contemporaine," Revue de métaphysique et de morale 57(1952):83-92.

The aspects mentioned in the above quotation also include second level assumptions: (1) the intersubjective quality of scientific thought gives it an undeniable social character (Bachelard 1951, 6; and 10); (2) the progress of science is proof of its social character (Bachelard 1951, 6); (3) scientists' work depends on codified objectivity values (Bachelard 1951, 7); (4) science and technology are from now on inextricably intertwined (Bachelard 1975, 9). All of these assumptions are consistent with what Bachelard had stressed in his earlier books about "social objectification," "tacit agreement," and phenomenotechnique in science. As we have said, the social aspects Bachelard finds in scientific thought are not meant to trivialize the content of science or equate it with just one more socially determined phenomenon. Science should rely on its own concepts. Bachelard's purpose is to bring out contemporary characteristics of science: collectiveness, communication and specialization. The acceptance of theories is more a matter of rationality than of negotiation. The determinism in science is **rational** and **technical** rather than purely social, if we take social as meaning more than a micro-level agreement upon what is rationally objective.

In terms of scientific technology, Bachelard stresses from the very beginning of L'Activité rationaliste de la physique contemporaine that scientific thought is essentially instrumental:

> Scientific phenomena of contemporary science only truly start from the moment one turns the apparatus on ('met en marche'). Here, the phenomenon is a phenomenon of apparatus . . . . First, one prepares the apparatus ('met l'appareil en état') . . . . The eye behind the microscope has totally accepted instrumentalization; it has itself become an apparatus behind an apparatus (Bachelard 1951, 5).

What Bachelard calls later on in the book "cogito d'appareil" resembles his previous claims about the crucial importance, for scientific progress, of standardized technology, as well as the notion that science itself cannot **think** without using coordinated instrumentation as intermediary between scientific theories and their material objectification. **Facts** cannot be seen outside their "theoretical framing "(Bachelard 1951, 45). They are **not** natural events. Thus, science becomes a network of rectifiable rational constructs. This rational materialism is the trade mark of contemporary physics and chemistry. It also corresponds both to the expression of theories in terms of mathematical physics (or mathematical chemistry), and to their verification through instrumental techniques.

As Bachelard will say in his article of 1957 "Le Nouvel esprit scientifique et la création des valeurs rationnelles":

> It is necessary to rigorously coordinate the experiences that do not belong to the natural nature,

> but that are rationally constituted from veritable theorems expressed in a rigorous mathematics. Final realization shows up as a concretization of rational values (Bachelard 1972a, 13).

"Realism rationally conditioned" (Bachelard 1951, 15) is a direct consequence of both theory rectification and the creation, through technology, of scientific entities postulated by mathematical physics and embodied in the 'new' mechanics: relativistic, quantum and wave mechanics (Bachelard 1951, 23). Scientific entities are seen by Bachelard as inventions rather than discoveries. The laboratory originates the circumstances which allow entities such as the neutron to become 'real' (Bachelard 1951, 114). The same thing happened with the neutrino (Bcahelard 1951, 117), the meson (Bachelard 1951, 122), the spin (Bachelard 1951, 163) and the magneton. Scientific entities rationally postulated do not have to exist in nature. The world of science is conditioned by both "technical experience" (Bachelard 1951, 82) and theoretical frameworks outside which scientific reality, i.e., the ontological status of constructed phenomena, loses its meaning.

Corpuscles are "human." As Bachelard says, they appear at a very precise point in the history of science. "Maybe with the exception of the precursor electron . . . they are 'very twentieth century'" (Bachelard 1951, 87). They are constructed by the technician and are thus dependent upon "technical organization" (Bachelard 1951, 65). Scientific objects are "**objects of thought** that afterwards become **objects of technical experiments**" (Bachelard 1951, 79). It is in this context that Bachelard once again raises the issue of phenomenotechnique:

> The most typical corpuscles: electron, proton, positron, neutron, all show up within a technique of electrical phenomena. Neutrons themselves, which do not have electrical charge, which are insensitive to the electro-magnetic field, are the indirect products of electrical techniques . . . . Thus, one cannot undertake a study of all these corpuscles of modern physics unless this study is "phenomenotechnical." In the phenomenotechnique [in which contemporary physics works] no phenomenon shows up **naturally** . . . . It is necessary to **constitute** it and to read its characteristics **indirectly**, always with the alert consciousness ('conscience éveillée') of an instrumental and theoretical **interpretation** (Bachelard 1951, 91-92).

The consciousness of instrumental interpretation is also consciousness of the **indirect nature** of scientific facts and thus of the enlargement of the ontological status of scientific entities. Correspondingly, realism in contemporary science is a circumstantial, elaborated, technical, **constructed realism**. It actually coincides with Bachelard's notion of applied rationalism and technical materialism, both expressions of the action of phenomenotechnique in physics and in chemistry.

In L'Activité rationaliste de la physique contemporaine, Bachelard synthetically defines applied rationalism for the first time. He says that it is "a constant fusion of the system of theoretical reasons [with that of] technical experiments" (Bachelard 1951, 111). This is not the same as conventionalism, i.e., the use of scientific theories and entities as comfortable working hypotheses. Instead, with applied rationalism, scientific entities such as corpuscles are **relative beings** (Bachelard 1951, 112). The positron, for instance, is constructed rationally and technically. Although this is not Bachelard's expression, I think we can talk of **technical ontology** as the new metaphysics for science. What counts as knowledge is what has been instrumentalized, articulated and realized by the dynamics of scientific thought. Here, rectification means the possibility of completely redefining what counts as scientific reality, i.e., what is, within a particular axiomatics, rationally and technically **possible**. One has to substitute **probability** for realism (Bachelard 1951, 207).

Spin and the magneton are also taken by Bachelard as examples of **functional beings**, rational objects that **become concrete** (Bachelard 1951, 171), real, by their gradually more extensive use in a diversity of domains (Bachelard 1951, 172), i.e., by their consequences rather than through their possibility of becoming observables. The meson, too, is considered as a good example of constructed realism, since it had been "essentially a mathematical hypothesis and not an image in connection with experience" before its existence was confirmed experimentally (Bachelard 1951, 122). What all these entities have in common is that none of them would have been conceivable without the "joint work of organizing rationalism and technical materialism" (Bachelard 1951, 114). They are inferred entities, and their ontology is provisional. They can thus become the focus of disagreement among scientists, who may attribute different characteristics to them -- that is why one can talk of "Fermi's, Louis de Broglie's, Kronig's and Jordan's neutrinos" (Bachelard 1951, 121) -- or deny their existence altogether (Bachelard 1951, 127). They are **provoked** beings, the products of what Bachelard will call '**technical causality**' (Bachelard 1951, 121).

Obviously, technical causality changes with the perfecting of both theories (which constantly demand new technology), and instruments (which become increasingly more precise):

> [The technician] will set aside ('écartera') parasites, dominate over perturbations, [he] will eliminate impurities; he will aim at the system ('régime'), the regular movement, the more and more exhaustive agreement between the instrument and the scientific law (Bachelard 1951, 218).

If we are to talk of determinism in science, we should add that it is **instrumental determinism**, basically founded upon what Bachelard calls 'conscience de l'appareil', without which the technician would "lose the very basis of his technical certainties " (Bachelard 1951, 218).

Although *L'Activité rationaliste de la physique contemporaine* emphasizes epistemological notions in basically the same way as before, it is curious to notice that now Bachelard is more deeply concerned with the rational and technical construction of **contemporary entities**. He seems to have suspended his earlier considerations regarding the inclusion of mathematical physics in the conceptualization of phenomenotechnique to concentrate mainly on its more technical, material features. On the other hand, the case studies presented in the book are not so much the descriptions of how particular machines function in the laboratory, but instead consider the implications, for philosophy of science, of the new scientific phenomena virtually **produced** by machines. This allows Bachelard to return to four of his favorite topics: (1) realism in science and how it should be transformed in order to encompass the entities postulated by theories and subsequently realized by technology; (2) the epistemological rupture between scientific knowledge and common knowledge; (3) concept deformation as paramount to science; (4) the need for a radical reform in both philosophy and history of science.

Philosophers should go to school with scientists because science has created a form of philosophy of which both philosophers and scientists are still unaware. The task of philosophy must be to inflect its language in order to translate contemporary scientific thought in its flexibility and mobility. It is history of science that constitutes evidence of scientific progress, particularly the "historical rupture in the evolution of modern science " (Bachelard 1951, 24), and the dialectic of epistemological obstacles and epistemological acts (Bachelard 1951, 25).[42]

Normativity had already been pointed out in Bachelard's earlier writings as a fundamental trait in the new history of science. It allows the detection of epistemological obstacles and it accounts for the dynamics of science as a dialectical process where experience is just a condition within the possibilities offered by mathematical rationalism and technical materialism. As the normal process of science involves a gradual separation between rational life and oneiric life, abstraction and imagination, the psychoanalysis of scientific thought is again intrinsic to

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[42] For additional information on the role of the historian of science in Bachelard's epistemology, see Lecourt, (1978)(1st ed. L'Épistémologie historique de Gaston Bachelard (Paris: J. Vrin, 1969); see also Canguilhem, "L'Histoire des sciences dans l'oeuvre épistémologique de Gaston Bachelard," pp. 173-207 in Études d'histoire et de philosophie des sciences (Paris: J. Vrin, 1983).

epistemology. However, psychoanalysis can only be performed with the material provided by the selective works of the historian of science. History shows us continuities and ruptures, evolution through error and revolution, concept rectification and reality approximation. These studies also imply an awareness of the technical methodologies of science -- including the technical construction of scientific phenomena -- and the procedures of theory acceptance through rational tacit agreement.

Rational Materialism

The third element of the epistemological tripthic of 1949-1953 is Le Matérialisme rationnel, published in 1953.[43] It is Bachelard's last book in the epistemology of science, and it is one of his few books almost totally dedicated to epistemology of chemistry (the other being Le Pluralisme cohérent de la chimie moderne). In this book, we are again faced with a sort of re-enactment of themes that have by now become fundamental to Bachelard's epistemological discourse on scientific practices. Among these are: (1) the rupture of intentionalities between science and immediate observation; (2) the social character

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[43] For a critical study of Le Matérialisme rationnel see Ruyer, "Le Matérialisme rationnel selon Gaston Bachelard," Revue de métaphysique et de morale 50(1953):413-422.

of modern science as embodied in both the rules and the instrumental procedures temporarily effective in the scientific/technical city; (3) the verified ontology of scientific entities as a factor for the acceptance of a directed, critical, instrumental realism; (4) applied rationalism as the dynamic philosophy of contemporary chemistry and physics, as well as the illustration of the new phenomenology of science. Again, the next pages of this chapter will be concerned with making the connection between the above-mentioned themes and the concept of phenomenotechnique, this time seen as the philosophy of "creating technique" ('technique créante') (Bachelard 1953, 198).

The epistemological discontinuity between science and common knowledge entails what Bachelard calls a "rupture of intentionalities" between the rational and strict consciousness ('conscientiel') (Bachelard 1953, 208) (embodied in the immediate experience of the senses). At the level of history of science, this rupture represents the difference between descriptive knowledge, i.e., a first level of approximation to reality, and the dynamic construction of phenomena, which includes the network of theoretical and practical assumptions regulated and sanctioned by the scientific community which give it scientific meaning.

The rupture of intentionalities is marked in the scientific neo-language required for the reformation of scientific concepts. This happens when we compare, for example, "the concept of temperature of the laboratory and the notion of 'temperature' of a nucleus" (Bachelard 1953, 216):

> The concept of 'temperature of the atomic nucleus' totalizes two reforms. First it analyses, within a new domain, the kinetic notion of temperature as it had been introduced in science by classical thermodynamics; [second] it transposes the scientific concept into a sphere of application where the classic concept does not normally apply. One sees the structuring of diverse stages in the conceptualism ('conceptualisme') of science: the 'temperature' of the nucleus is a sort of concept of a concept, a concept which is not a concept of first abstraction (Bachelard 1953, 217).

On the other hand, since scientific knowledge becomes contingent upon the progress of scientific rationality and its embodiment in scientific technology, Bachelard sees contemporary science -- in this case modern chemistry -- as an illustration of the subversion of what, in science, counts as **objective knowledge**. This claim is far from new. The revolutionary meaning that the specialized works of physics and chemistry have for philosophy of science is pointed to throughout Bachelard's epistemological writings almost ad nauseam. It does seem, however, become more central than before in Le Matérialisme rationnel, especially if we see it as what I think to be the result of an ideological shift in Bachelard's progressive conceptualization of phenomenotechnique, or what he now calls "directed phenomenology" (Bachelard 1953, 65).

This ideological shift in Bachelard's concept of phenomenotechnique has to do with an extra-emphasis -- which had started already in works such as Le Rationalisme appliqué -- given to the role of the scientific city in the determination of what counts as scientific, as well as the reliance of scientists upon **technique** in general and scientific technology in particular. Bachelard sees them both, I think, as proof of what he now calls the "social character of modern science" (Bachelard 1953, 175). In fact, science cannot be separated from the cultural conditions that originated it in the first place. What Bachelard calls "social objectivity" in chemistry only started at the end of the eighteenth century, when **experimentation** in the "institutional" context of the term overruled the value of personal, isolated experiments as the ones still performed at the time by people such as Lamarck (Bachelard 1953, 219). Experimentation becomes a social act, in the sense that it is performed within a "constituted city." In the scientific and technical city, specialization and cooperation among scientists becomes the touchstone for the scientific validation of theoretical entities -- "the realism of the objects of an epoch " (Bachelard 1953, 115) -- as well as the "social guarantee" that all measurement techniques must receive (Bachelard 1953, 77). In the case of chemistry, "each epoch of science, in its modern development, has established a sort of corpus of reactants constituted at a well determined level of purification . . . . There are diverse social ages for materialistic purity" (Bachelard 1953, 78).

Although statements such as the above may sound like a defense of relativistic and skeptical attitudes regarding the work of science, this is **not** Bachelard's intention. His point is that science is a process of constant knowledge rectification. What is taken as absolute in science is just a sort of temporary functional **a priori** that allows the construction of knowledge without letting it collapse into some form of perennial, unchangeable representations of reality. We need frameworks -- both practical and theoretical -- but they are points of departure rather than arrival points. It is not derogatory, then, to identify certain theoretical entities as social. On the contrary, their social character **coincides** with the intersubjective agreement which gives them the status of scientific facts:

> Hydrogen and oxygen are, in many respects, . . . social gases, gases of high civilization . . . . Substances studied by instructed materialism are not exactly . . . **natural givens** any more. Their social label is a profound mark. Instructed materialism is inseparable from its social status (Bachelard 1953, 31).

....

> Rectified, ordered reality is a human interdependent ('solidaire') reality from the technical city of an epoch of well defined progress. . . . Thus, the problem of the knowledge of the external world cannot be separated from its cultural characteristics (Bachelard 1953, 198).

The chemical laboratory is loaded with technology, of purification utensils which constitute **technical givens** (Bachelard 1953, 78). These artificial givens determine to a large extent the avenues of research available to the scientist. In other words, research is integrated in the present state of technological and scientific advancement, which in turn is culturally circumscribed.[44] Since chemical realism is a realism of socially constructed science, a science that constructs its objects (Bachelard 1953, 115), technological systems allow an openendedness not only in the study of matter, but also in the creation of matter unknown to nature. This is something that had already been acknowledged in the nineteenth century by people such as Auguste Laurent, when he says that "today's chemistry has become the science of bodies that do not exist" (Bachelard 1953,22):

> The proof of the chemical rationalism of color [can be synthesized as follows]: before the construction, in a molecular genesis, of chromosomatic groups, what was color? Nothing. The color of a molecule does not

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[44] Taking into consideration his notion of epistemological break, I think Bachelard is talking of scientific culture, not of human culture as a whole.

> belong to isolated atoms . . . . Color is the result of construction; it is a fact of structure . . . . In lots of cases, the chemist builds color piece by piece . . . . The chemist thinks color within the very working drawing ('épuré') that guides its creation. And there, [color] has an indestructible objective reality, an objective reality that is communicable, a negotiable social reality (Bachelard 1953, 202).

The constructed essence of chemical substances coincides, thus, with both the elimination of contingency (and irrationality) in science, and the possibility of negotiation of knowledge (and practice) in the scientific city (i.e., what counts as knowledge). **Objectivity in science is possible only because science is of a social, constructed nature.** In this context, the category of phenomenotechnique shows up again as the philosophical translator of humanized, realized, technical science. It represents the transposition of the mathematical determination of science ('conditionnement géométrique') (Bachelard 1953, 197) into material productions allowed by the dialectic interdependency of reason and technique.

Conclusion

The main purpose of this chapter was to undertake a descriptive analysis of Gaston Bachelard's concept of phenomenotechnique. As we have seen, the category itself appeared for the first time in a small 1931 article, and basically was meant to translate the importance of the use of mathematics in microphysics. The idea that mathematics is an **instrument**, a technique used by science, pervades all of Bachelard's works whether they are concerned with the epistemology of physics or the epistemology of chemistry. The **attribution of agency** to mathematical thinking was thus subsumed under phenomenotechnique. Once Bachelard became aware of the characteristics of quantum theory and its implications for scientific realism concerning entities, phenomenotechnique became a more encompassing term. Bachelard soon realized that scientific practice entails **fabrication**, invention, and that phenomena are **provoked** by instrumental techniques. He thus starts to use phenomenotechnique in a more pragmatic sense, i.e., connecting it with scientific technology and its power to determine and validate scientific theories. In both chemistry and physics, we are not dealing with natural nature anymore, but with **products of technique**. Bachelard often refers to both technique and technology. In the course of this chapter I use both terms almost interchangeably because Bachelard does not seem to demarcate the theoretical products of scientific rationality from its instrumental validation. He sees instruments as materialized theories. In other words, to Bachelard technology is a form of applied science. Technique thus involves both mathematics and technology. Technology is thus the reification of technique, i.e., the set of tools which the scientist uses to test theories.[45]

Mathematics is a technique, but instruments are embodiments of technique as well. The study of reality becomes technically organized. It does not really matter that mathematics is more theoretical than instrumental techniques. As Bachelard says, "there is no sense in making a radical distinction between the scientific mind instructed by mathematics and the scientific mind instructed by physical experiment" (Bachelard 1978, 133). From the moment instruments are materialized theories -- i.e., they are used to confirm the functional truth of a theory or disprove it as non-scientific -- there is no qualitative differentiation between the products of mathematical physics or chemistry and the products of scientific technology. This

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[45] For more information on the difference in the French context between 'technologie' and 'technique', see Jean-Claude Beaune, La Technologie introuvable: recherche sur la définition de l'unité de la technologie à partir de quelques modèles du XVIIIe et XIXe siècles (Paris: J. Vrin, 1980). According to Beaune, there are three types of technology: cultural, scientific and economic. The definition of scientific technology, which I see as corresponding to what Bachelard considers to be technology in the context of science, says the following: "Scientific technology reduces technique to laboratory and measurement instruments, [it reduces them to] experimental protocols; it makes of technique a sort of 'applied science', whose logical structure depends upon science (especially natural and exact sciences, preferably mathematics and mathematical physics). . . ." (p. 13). See also chapter III of the same work (Technologie et Sciences).

constructed, technical realism is what makes Bachelard claim that the use of technique in science is the touchstone for **objectivity in science**. More, technical objectification has to do with perfecting precision conditions in instrumentation as much as with the attempt to demarcate the universe of the subject as observer from that of the scientist as constructor.

When phenomenotechnique reaches its full conceptual development, one realizes that most of the presuppositions behind phenomenotechnique were a constant presence in Bachelard's epistemological writings, including those prior to the coinage of the term. It seems thus that Bachelard was looking for a term that would give the full account of the "constructivist" features of science. Phenomenotechnique could only be that term if it included more than the role of mathematics in physics. Another interesting aspect of phenomenotechnique, now in its more encompassing sense, is the notion that the rules of objectivity and validation judgments involve the normative activity and the censorship established by the scientific city. Scientific knowledge becomes contingent upon both instrumental conditions **and** the circumstantiality of scientific culture in a particular state of its progressive evolution ("corpuscles are very twentieth century"). Since it is not possible to separate, in Bachelard's terms, knowledge from technique, the development of technological conditions is at the basis of the development of science. The fabrication of artificial substances in organic chemistry, for instance, is pointed out by Bachelard as proof of the phenomenotechnical character of contemporary science. Technique allows the **realization of scientific entities** such as electrons and mesons, which may or may not be considered by scientists themselves as **existing** outside the theoretical and material framework that gave them epistemic significance in the first place. Technology becomes fundamental for both the construction of **scientific reality** and the **rectification of scientific knowledge**, both of which are only approximate and circumstantial.

The constructed character of scientific objectivity and the social control embodied in the scientific city do **not** lead to the consideration that phenomenotechnique is a way of expressing the dependency of science upon power struggles, or the result of negotiations among scientists. Bachelard was always very clear about the need to demarcate the rules of the scientific city and the rules of the larger societal frame. Part of the purpose of psychoanalysis as applied to scientific knowledge is to eradicate the larger cultural context and thus reduce social and subjective interests in the practice of science. I think Bachelard was unsuccessful at establishing such a demarcation. However, it is obvious from my study that phenomenotechnique is not, to Bachelard, part of an epistemological agenda to take science off its pedestal and see it as a strict social construction where the quest for certainty in science is nothing but an illusory anomaly.

Despite the complexity and depth of Bachelard's epistemological discourse on science, this discourse is by no means flawless. It is obvious that any attempt to incorporate phenomenotechnique into today's studies of science cannot ignore the contributions to the field by people who are ideologically opposed to Bachelard's philosophical purposes. The way science is built and subjected to influences such as economic, political and social cannot be left out of the picture if we wish to obtain models of scientific explanation that are not biased from the very start. Bachelard's model of scientific change is definitely biased. Although very much concerned with history, his model only deals with history of science, and never with the influence of the larger historical context upon science. In other words, because it tries so hard to demarcate science from non-science, Bachelard ends up offering a strictly internalist perspective on scientific work. Social, political, and economic motives are totally absent from Bachelard's explication of the way knowledge is constructed. Considering that Bachelard himself perceived science as a circumstantial product of culture, this fact is hard to justify. If "big science" as one knows it at present did not exist in Bachelard's time, it is nevertheless true that scientific and technological achievements were long since shaped by non-scientific motivations. An analysis of science, albeit philosophical, should not disregard the action of forces that guide research and determine what is accumulated and counts as knowledge. Since Bachelard opens up avenues of research for science studies -- in the realms of psychology, history, philosophy, and sociology -- it is not unreasonable to think that these studies should also involve the use of disciplines such as politics, and economics. On the other hand, Bachelard's focus on particular areas of the physical sciences make is model of scientific change a reductionist one.

Another general criticism of Bachelard's scientific philosophy is that he unquestionably assumes -- no doubt under the influence of his own culture -- that whenever science changes, it changes for the better. Science is the proof of the progress of reason, in the same way as history was the proof of the progress of reason to Hegel. Despite the fact that Bachelard is a defender of discontinuity, and thus the idea that systems of thought are incommensurable to each other, he still thinks that it would not have been possible to create Relativity without first having to go through Newtonianism. This sort of whiggish attitude is further emphasized when Bachelard seems to assert that, once superseded, past scientific knowledge is **not scientific** anymore. Instead, it becomes a sort of archeological evidence for humanity's pseudo-scientific attempts at understanding the world. Of course, Newtonian science does not belong to this category, since it still holds for certain areas of physics. Since scientific mistakes are gauged by contemporary science, Bachelard's attitude is far from relativistic, although definitely very similar to the kind of pervading attitude among scientists themselves. Also, his claim that the scientific mind did not yet reach its full maturity allows one to think that what Bachelard saw then as the expression of science, will be on non-scientific by future standards.

Another problem with Bachelard's model as a whole is his assumption that everything that happens in science must necessarily have the brand of rationality. The more rational science becomes, i.e., the more scientists succeed at preventing cultural prejudices, metaphors, their own subjectivity, etc., from interfering with the scientific process, the more prone they are to construct objective theories. Since Bachelard claims that physical concepts are very much the product of a specific scientific culture, it seems contradictory to somehow tacitly convey the notion that some scientific cultures could be free from prejudice, subjectivity, and the larger social environment. But this is not what Bachelard argues. Actually, what he says is that degrees of specialization in science do not mean that we are "getting closer to the truth," but only approximating rationality. In other words, truth does not exist except in relation to something. Further, when he says that certain technologies do not possess any trace of irrationality, he is saying that pure rational knowledge is not true knowledge. In other words, we are not supposed to apply science, but instead to focus on making it increasingly rational. To Bachelard, science -- particularly microphysics -- is not concerned with reality anymore but with exploring our potential to construct rational possibilities of reality.

In the context of what was just said, it is obvious that the concept of phenomenotechnique can be seen by the social constructivists as very useful for emphasizing their social relativism. To say that science is a rational possibility created at a certain time by a particular culture is almost the same as saying that science is a social product. However, the explanation of phenomenotechnique presented in this chapter shows that Bachelard cannot take the step to social relativism. The conceptualization behind phenomenotechnique incorporates the use of scientific technology for the testing of scientific theories. Even when we agree to say with Bachelard that instruments are materialized theories, it is obvious that the information provided by a particular apparatus would be a social construction (what Bachelard would have called the unfeasible dream in the spirit of the scientist) if and only if it could not be replicated. Instrumentation thus becomes the gauge of the objectivity of scientific hypotheses. Intersubjective consensus does not mean that everyone agrees with the same social construction. It means that the criteria of objectivity of a particular time in the history of a particular science allowed the scientific community to assert the validity of certain theoretical constructs that seem to have some sort of correspondence in the world.

Phenomenotechnique is one of the most potentially rich concepts that Bachelard has to offer to contemporary science studies. It is an analytical tool and a heuristic device. Part III will be devoted to incorporating phenomenotechnique into disputes among experts in science studies.

PART II

THE SCIENTIFIC PHENOMENOLOGY OF GASTON BACHELARD AND THE FRENCH INTELLECTUAL AND SCIENTIFIC MILIEU IN THE FIRST HALF OF THE TWENTIETH CENTURY

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Introduction

The epistemology of Gaston Bachelard (1884-1962) in general and the concept of phenomenotechnique in particular did not originate "ex nihilo." In fact, and despite their undeniable originality, the works of Bachelard, whether literary or scientific, are the result of a very specific milieu which produced some of the most revolutionary explanatory models in areas such as art, science, and philosophy.

Much was happening in France at the end of the nineteenth and the first half of the twentieth century: The implementation of a republican educational program; the increasingly pragmatic orientation of scientific institutions (connected with the "revanche" obsession that beset the French after defeat in the Franco-Prussian war (1870); the new scientific paradigms implied by relativity and quantum physics; and the intellectual (and anti-intellectual) horizons opened by anti-scientism, irrationalism, phenomenology, and psychoanalysis.

Although I am aware of the inextricability of all these developments, this Part is only superficially concerned with political-institutional history. It is thus a semi-internalist approach, since it focuses on the intellectual and scientific context of France in the first half of the twentieth century. More specifically, the following represents a reconstruction of the network of scientific and intellectual events which led Bachelard to his scientific philosophy. This Part also presents the argument that phenomenotechnique is (1) the direct philosophical result of the development of theories such as relativity and quantum theory in the first decades of the twentieth century, and (2) an idealist rebuttal to the interpretations of science as provided by traditional philosophers of science such as Émile Meyerson and the French nineteenth century materialism and rationalism.

In France, Henri Poincaré, Henri Bergson, Léon Brunschvicg, and Gaston Bachelard devoted considerable attention to the philosophical implications of some unusual characteristics of relativity and quantum theory. In particular, the implications of modern physics for the understanding of the scientific mind at work were the focus of the analytical studies of science made by Brunschvicg, Bachelard and Bergson -- although as we will see his concerns were more directed towards the refutation of

Darwinism. Both Brunschvicg and Bachelard thought that phenomenology should incorporate the dynamic and technical relationships established between reality, the scientific microworld, and the rational subtleties of scientists' thought processes.

Characteristically, people inside the scientific community of theoretical physics also addressed some of the philosophical problems which originated with the new sciences. Max Planck, Albert Einstein, Niels Bohr, Louis de Broglie, Werner Heisenberg, and others, wrote extensively on the consequences of their own discoveries for understanding both reality and the limitations of scientific knowledge. Causality, determinism, the structure of the scientific mind, models of scientific change, and theory choice were definitely among their philosophical preoccupations. The phenomenological consequences of microphysics, for instance, were discussed by both Einstein and Bohr, and they constituted the root of the scientific and epistemological conflict Einstein saw between relativity and quantum theory. In the case of Bohr, the significance of instrumentation for the construction of scientific facts was obvious, and it constituted a basic assumption within the Copenhagen Interpretation of quantum theory, in its turn challenged by people such as Louis de Broglie. Meyerson accepted relativity only if it could be seen as being continuous with

Newtonian mechanics. He rejected quantum theory not only because of its logical incommensurability with relativity but because of quantum theory's indeterministic characterization of physical phenomena.

One cannot help but notice that Bachelard often cites the works and the issues raised by these physicists. For instance, he also refers to the works of Ernst Mach, Henri Poincaré, Hans Reichenbach, to name only a few. Undoubtedly these contacts -- which included chairing some of Louis de Broglie's conferences at the **Société Française de Philosophie** --were at the root of Bachelard's interest in developing more sophisticated approaches to science. A deeper analysis of these and other issues provide, as we will see, useful insights into the understanding of Bachelard's concept of phenomenotechnique. Since the scope of this project is fairly limited, I do not analyse but a few among those which Bachelard refers to in his works and which seem to have inspired certain aspects of his epistemology.

The works of Henri Bergson and Edmund Husserl are also relevant in this context. Bachelard's L'Intuition de l'instant: étude sur la "Silöe" de Gaston Roupnel (1932) and La Dialectique de la durée (1936) are mainly refutations of Bergson's reflections on continuity, intuition, time, and duration. Although Bachelard refers more often to Bergson in the context of his more metaphysically oriented books than in the epistemological ones, the fact remains that the influence of Bergson on Bachelard's philosophy of science is pervasive. A brief overview of Husserl's phenomenology and its impact on Bachelardian thought is also necessary. Bachelard refers to Husserl mostly in the context of his works on imagination -- even to the point of considering, at the end of his life, that phenomenology is the tool **par excellence** of the study of poetic imagery. It is therefore only speculation to argue that phenomenotechnique has something in common with a position that is, at least apparently, quite antiscientific.

The present study includes a broad overview of the state of French philosophy between 1900 and 1947. A picture of what was happening in the narrower field of the philosophy of physics is also undertaken. The philosophical writings of scientists such as Louis de Broglie and Niels Bohr, and the philosophies of science of philosophers of science such as Émile Meyerson, Léon Brunschvicg, Dominique Parodi, Abel Rey and André Lalande will also be considered since some of them are crucial to the understanding of Bachelard's concept of **instrumental epistemology**. Some attention is also devoted to philosophers such as William James, Herbert Spencer, and Hans Reichenbach, who seemed to have anticipated some of the epistemological issues later developed by Bachelard.

Part II of this dissertation is thus divided into two related chapters. Chapter One deals with the **intellectual** context of France in the first half of the twentieth century, and presents an overview of influential philosophies at that time, particularly Bergsonism and Husserlianism. Chapter Two concentrates on the contemporary **scientific** context, as well as on some philosophies of science which seemed to have had a greater impact on Bachelard's epistemology.

Chapter 1 - German Thought, Anti-intellectualism, Science and Phenomenology: Their Impact in French Philosophical Circles (The General Picture)

According to republican philosopher Abel Rey, despite the novelty of the 'intuitive' phenomenology of Henri Bergson, the general trend in French philosophical thought in the early 1930's can be seen primarily as a return to Aristotelianism, Cartesianism, and Kantianism (Rey 1931, 1). To some extent, this was true. The ideological consequences of a classical revival are not surprising. One particularly interesting consequence of this position was the philosophical reduction of diversity in scientific practices and achievements to an illusory and mythical unity. Bachelard was most critical of this kind of reductionism. He claimed that the study of science cannot be done using only extreme philosophical positions such as realism, positivism, and materialism. Rather, he argued that an adequate analysis of science required a pluralistic philosophy adapted to the different stages in the evolution of the different sciences. A second consequence of a return to traditional philosophical models was the idea that an analysis of science through its history undeniably showed a continuous process of knowledge accumulation within unchanging mental structures **à la Kant**. Again, the position of Bachelard on the issue is that, within scientific practices, the scientific mind goes through constant changes which simultaneously adapt themselves and provoke the challenges imposed by theory formation and experimentation. However, to say that the picture of French philosophy can be synthesized as Rey did is basically to neglect the enormous intellectual vitality that pervaded the French milieu during the first decades of the twentieth century.

If we focus on the account revealed by an analysis of The Philosophical Review regarding the larger intellectual picture of France around 1912, we see that divergent doctrines (all against materialism) coexisted: positivism, idealism, partisans of intuition, mystics, traditionalists (Lalande 1913, 358). When we look for constants in terms of subject matter in French philosophical publications in the first quarter of the twentieth century, they encompass what the academic institutions considered as fundamental to the curricula: ethics, history of philosophy, logic, mathematical philosophy, philosophy of law, psychology, sociology, philosophy of religion and philosophy of science.[46] Some of these disciplines reflected

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[46] During World War I there seemed to be a renewed interest for the publication of works in the history of philosophy.

preoccupations of the time. For instance, "most of the republican intellectuals were interested in developing a nonreligious morality for the republican educational system" (Paul 1979, 137-138). Hamelin, Rey, Bergson, Parodi, and Brunschvicg are some of the names connected with this particular movement. The contents of the Philosophical Review also reveal that history of science was represented by intellectuals such as Léon Robin.[47]

Many philosophies of science were present throughout the first decades of the twentieth century. Interestingly, in 1906, six years after Planck's quantum of action and a year after Einstein's theory of relativity, Lalande reports that "[there are] an increasing number of philosophical works by professional scientists." Poincaré, Hadamard, Duhem, Langevin, Meyerson and Brunschvicg belong to this group of scientists with philosophical preoccupations.

In 1924, the Philosophical Review reflects how important the union of science and philosophy had become in France. For instance, Meyerson's Identité et réalité (1909) and De l'Explication dans les sciences (1912) were perceived as classics in philosophy of science. From 1921 on, Meyerson and Brunschvicg -- as well as Bergson -- appeared in all

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[47] Bachelard's Les Intuitions Atomistiques (1933) used some of the studies presented in Robin's La Pensée Grecque et les Origines de l'Esprit Scientifique.

scenarios concerning the philosophical implications of sciences such as physics and biology (in the case of Bergson). The same preoccupations were reflected in the works of Duhem, Édouard Le Roy and, after 1927, Gaston Bachelard.

Brunschvicg's L'Expérience humaine et la causalité physique appeared in 1922, coinciding with Einstein's visit to Paris. The work constitutes a sort of demarcation, within philosophy of science between the positivist philosophy taught at the Sorbonne and Brunschvicg's defense of a new form of idealism, later to be developed by Bachelard. Slowly, the Sorbonne became more open to the integration of philosophies aside from positivism and Thomism. The popularity of Brunschvicg's lectures during the 1930s, which were published in his Les Âges de l'intelligence some time later, are quite revealing in this respect. According to Dominique Parodi, the philosophical works of both Meyerson and Brunschvicg, and the teachings of André Lalande were the ones which, after Émile Boutroux's death, most deeply influenced French intellectual circles between 1918 and 1925 (Parodi 1925, 489). Still, for some Brunschvicg was considered to be one of the crucial French philosophers of the period of transition between Maine de Biran and Henri Bergson (Hamelin 1907, 467). Among his most notable characteristics were his "reflexive method, his subtle and severe passion for the history of philosophy and the sciences, a critical idealism founded upon an epistemology finally at the level of the sciences from which it takes inspiration" (Hamelin 1907, 467). Some of these qualities became important to Bachelard's epistemology of science. In fact, although not interested in the descriptive analysis of aspects of science that had been the focus of attention of Brunschvicg, Bachelard succeeded in maintaining the idealistic and methodologically oriented program of his intellectual forerunner were dealing with the most materialistically oriented sciences of the time. In his concept of phenomenotechnique, for instance, Bachelard speculates on the constructive importance of scientific technology. He does this while considering technology, along with mathematical physics, as the manifestation of human technique, rather than something independent from the purposes of scientific rationality.[48] Also, the interest that both Brunschvicg and Bachelard had for methodology was probably the only thing that they had in common with the sociologists.

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[48] For more on Bachelard's epistemological idealism, see the extremely critical account of Michel Vadée in Gaston Bachelard ou le nouvel idéalisme épistémologique, especially chapters I and V. The book is a rebuttal of the materialistic image of Bachelard's epistemology as conveyed by people such as Dominique Lecourt and Louis Althusser, who were responsible for the suggested connection of Bachelardianism with Marxist ideology.

Was There any Place for Sociology?

The status of sociology in early twentieth century France was quite interesting, considering that it was the political system initiated by the Third Republic that allowed the discipline to 'conquer' the Sorbonne. The more conservative circles would accuse republicanism of creating the **Nouvelle Sorbonne**, i.e., the university reforms which allowed the incorporation of what they considered to be a pseudo-science into the academic curricula (Lepenies 1988, 47). Sociology was also seen as part of the anti-intellectual trend besetting French academia. It was seen as a **nouveau riche** discipline which conquered a place at the Sorbonne "through nepotism and by promising to serve the republicans as a foundation of ideology" (Lepenies 1988, 48). One of the criticisms against sociologists was that they seemed to be obsessed with only two things: "critical catalogues of sources and commentaries on them" (Lepenies 1988, 49); and methodology.[49]

The interest showed by sociologists for **methodology** was probably the influence of the French critical movement during the 1930s which had been developed as a reaction

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[49] See Lepenies (1988), Between Literature and Science: The Rise of Sociology, especially Part I, which deals with the development of sociology in France.

against the German obsession with **system** (Lalande 1932, 18). Bachelard was also aware of the importance of both anti-intellectualism and methodology of science but he did not consider his philosophical program to be sociology oriented (Mansuy in Colloque 1984, 239). He did belong, with Bergson, Boutroux and Brunschvicg, to the movement in philosophy of science known in France as **la critique de la science**. His emphasis on methodology was an extension of Brunschvicg's return to idealism (even if Bachelard's idealism is of the 'instrumental' genre) together with the refusal of building models of science and rationality which, like the one defended by Meyerson when he claimed that science is the search for identities in nature, are presented as stable and evolutionary endeavors. It was **not** primarily an influence coming from sociology. When Bachelard talks about theoretical entities as social he is not advocating the relativization of standards defended by the sociologists, but instead his focusing on the rational contingency of scientific practices within the scientific community which, to him, is independent of the larger social context. To study science, Bachelard chose instead methodologies coming from disciplines that were becoming increasingly influential: notably, phenomenology and psychoanalysis, but also German idealism, existentialism, and surrealism. What most influenced the epistemology of

Bachelard was the astounding array of philosophies and movements that were characteristic of French culture in the first half of the twentieth century.

The Impact of German Thought in France

French intellectual life has always been divided into sharply distinct modes of thought. Bergson's influence on French culture, for instance, was not as minor as the republican Abel Rey appears to think. Rey refused to accept the ideas that German culture was invading, and that the French intelligentsia was less adverse to Germany republican institutions than the orthodox French epistemology seemed to convey. On the other hand, Bergson had been extremely influential in all areas of French culture.[50] On the other hand, the impact of German philosophy, art and science in France had been very strong, especially at the end of the nineteenth century, and was far from negligible even after the War. Thus, both the philosophy of Bergson and German thought always remained a more or less obvious presence throughout the whole period of time with which we are dealing. They undoubtedly helped shape some of the new anti-intellectual and idealist trends in French philosophy. As we will see, the reaction of Bergson to science was not

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[50] For instance, he received the 1927 Nobel Prize in Literature.

always negative. His criticisms were basically directed against the positivistic image of science.

The admiration of German classics in philosophy, especially Kant's work, was the consequence of its introduction in France by Renouvier and the neo-critical school. The admiration for Kantianism was somewhat weakened with the influence of Bergson's intuitionism and James' pragmatism (Lalande 1916, 524). But this does not mean that the acceptance of German things became problematic. On the contrary, it was with great discomfort that those who were **against** its influence tried to account for the enthusiasm over German thought that appeared to be congenial to the French. In 1918, Lalande would say, referring to the acceptance of German thought before the war:

> Pleasantries in a foreign tongue (when one finally understands them) seem much better than in the original language. So philosophical ideas seem more profound, perhaps because one is pleased with oneself by grasping them . . . . The French easily become infatuated with a neighboring people, as for example with Italy of the sixteenth century, or England of the eighteenth. Then, too, Germans gave themselves out as the philosophers **par excellence**, and in the end people simply took their word for it . . . . Today the reverse is true. It is not possible to give here a bibliography of all the criticisms of German philosophy. They are everywhere (Lalande 1918, 455).

The acceptance of the new German intellectual models led people such as Pierre Duhem and G. Papillant to publish anti-German science propaganda on the grounds that the

German attraction for a "mythology of abstractions and categories" was radically different from the procedures behind the establishment of laws inherited from Descartes, Condillac and Claude Bernard (Lalande 1918. 456). As we will see later, the visit of Einstein to Paris in 1922[51] was regarded with skepticism by a great portion of the scientific and intellectual community. At the same time, one of the most interesting reactions -- Lalande would call it **infatuation** (Lalande 1924, 542) -- to Einstein's presentation to that community was the number of works published some time later concerning both the scientific and the philosophical implications of relativity. An important consequence of the relativity theory had to do, for instance, with the questioning of previous models of scientific explanation as well as a new approach to the theory of knowledge. Philosopher-scientists such as Meyerson, Bergson, Brunschvicg and later Bachelard were keenly interested in the philosophical implications of relativity in terms of their own claims concerning the construction of scientific knowledge in particular and the dynamics of human thought in general.

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[51] The war had prevented him from following up on Langevin's invitation of 1914.

Bergson and French Philosophic Culture: The Undisputable Link

An analysis of articles on French philosophy in The Philosophical Review between 1900 and 1941, the year Henri Bergson died, shows how enormously influential he had been for almost a half century of French culture. Especially notable were his attacks on mechanism, positivist thought and the scientific dogmatism of the age. For instance, he claimed with Le Roy that the closer science was to organization and crystallization, the more it would distance itself from reality (Lalande 1906, 241).[52] The influence of Bergson in France dated from the end of the nineteenth century as a spiritualist reaction against **scientism** and **materialism**. In fact, the approach to life and science as represented in the writings of Bergson was a sort of reification of the anti-intellectualist trends that were being formed in France and elsewhere. Still, his attitude towards science was not one of denial. He acknowledged that the physical sciences were useful and effective, and that they were the expression of some sort of inevitability of the human mind to build knowledge of the world according to what he called the logic of solid bodies. What he criticized was the tendency to use these models in the life sciences.

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[52] Bergson's general attitude towards science was in this regard quite similar to that of Edmund Husserl (1859-1938).

For instance, Bergson's phenomenological works such as Essai sur les données immédiates de la conscience (1889)[53] and Matière et mémoire (1896) fueled the antagonism towards the positivist notion of determinism (in the Essai) and materialism (in Matière et mémoire)(Charlton 1983, 296). His criticisms would later extend to the mechanical concepts of evolution in L'Évolution créatrice (1907)[54] and to the concept of time in relativity in Durée et simultanéitée (1922).

Again, according to The Philosophical Review, in 1900 Bergson's philosophy was "debatable, although it had attracted a great deal of attention" (Paulhan 1900, 45). By 1905 his doctrines had been "welcomed and regarded with lively sympathy" (Lalande 1905, 437). In the subsequent years and up to 1914 his publications or echoes of his intellectual influence are **always** referred to by Lalande and Rey. Even Lalande, considered the very image of **the philosopher** during the Third Republic (Belaval 1974, 473),

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[53] The Essai was Bergson's doctoral thesis. Its English title is Time and Free Will.

[54] In a letter to Bergson, William James referred to L'Évolution créatrice in the following terms: "a real wonder in the history of philosophy. . . . There is so much that is absolutely new that it will take a long time for your contemporaries to assimilate it." As quoted in Thomas Hanna, ed., The Bergsonian Heritage (New York: Columbia University Press, 1962):15-16.

could not deny Bergson's importance. Referring to the state of French philosophy in 1926 and 1927, Rey said:

> French philosophy has changed its orientation appreciably in the last twenty years. Twenty years ago the searching and illuminating criticism of M. Bergson had completely shaken the reigning doctrines -- the positivism of Comte, Littré, Taine, Renan and Ribot; the neo-criticism of Renouvier, Pillon, Brochard, Hamelin and the **critique philosophique**, and the Kantianism or eclectic rationalism of the greater part of the French university. (Rey 1928, 527).

The renaissance of positivism at the end of the nineteenth century, with its prominent adherents -- Littré, Cournot, Renouvier, Meyerson and Duhem in France, Mach in Vienna and Ostwald in Germany -- exerted an enormous influence upon models of scientific explanation (realism, materialism) and theory choice on the one hand, and French intellectual styles on the other. One of the beneficial effects of the **école positiviste**, later to be developed by Brunschvicg and Bachelard, was that this school progressively recognized the importance of history -- including history of science -- as a useful explanatory tool.

It is important to realize that philosophers with different philosophical tendencies from the French positivists also attributed a fundamental role to history in the understanding of science. Dilthey, for instance, was one of the defenders of this position. To him, as well as to Bergson and the adherents of an **organic** conception of reality the world cannot be seen as a machine but rather as "life in action" (Bochenski 1951, 92). It is difficult to ascertain whether Bachelard expanded the positivistic historiographic tendencies into his own epistemology or instead chose to use the historical model of Dilthey or Bergson. In fact, he was so critical of positivism in other respects that the importance of history for the understanding of science might have come from a different source. But the permeability of French society to positivism was very strong, and might have led Bachelard to a sort of eclectic attitude, that is, the selective use of positivistic claims outside their reductionist context. As we will see later on, there is a great ambiguity in Bachelard's attitude towards positivism. Although he spends a lot of time reflecting upon scientific language, or with emphasizing the conceptual richness of the physical sciences, he never refers to positivistic trends such as the one developed with the formation of the Vienna Circle around 1928. This is even more strange when we realize that Bachelard was familiar with the scientific philosophy of Hans Reichenbach, one of the founders of the Berlin Circle.[55]

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[55] Bachelard's familiarity with Reinchenbach's philosophy is probably the consequence of Reichenbach's talks at the Société française de philosophie.

Positivism could be seen as the official philosophy in France, and to a great extent the official philosophy of Republicanism. But there were reactions against positivistic thought, not all of which came from Bergson and the defenders of the organic model. Generally speaking, they had at least two philosophical routes: idealism (which incorporated Bergson and German irrationalism) and neo-Thomism. They also came from more literary circles. As Weber puts it:

> By 1886, when Le Figaro published the Manifesto of Jean Moreas, explaining decadence as the perception that reality was best apprehended by intuition and best expressed by illusion, poets like Baudelaire, Verlaine and Rimbaud, painters like Puvis de Chavannes, Gustave Moreau, and Odilon Redon, had produced all or much of their work. To the would-be objective and scientific theories of Naturalism, these men opposed a subjective and poetic point of view in which the artist played the part of a magician, delving into and exalting the importance of the unconscious that French and German scientists like Hartmann and Charcot were exploring at about the same time (Weber 1986, 146).

Anti-scientific trends were germane to French art at the end of the nineteenth century. In philosophy, Bergson's attitude became crucial in two key respects: first, it probably constituted one of the most influential and systematic attacks on the positivistic image of science; and second, together with the philosophy of William James, it definitely contributed to the weakening of the influence of Kantianism in France (Lalande 1916, 524).

In 1928, when Bachelard came to the French cultural debates in philosophy of science defending the idea that scientific thought is in a state of constant renewal and that this implied that thought structures **themselves** go through constant change, Bergson had already established a precedent. However, we should not stretch the similarities too far. Bergson and Bachelard dealt with philosophical concepts which presuppose very different metaphysics. For instance, one of Bergson's concepts, **stream of consciousness**, was a recognition of the permanent flux in which reality and all its objects are immersed. While Bergson considered the **élan vital** as a universal property of nature, Bachelard considered instead that vitalism and animism were **epistemological obstacles** for the progress of science.[56] On the other hand, contrary to Bachelard's discontinuist thesis, Bergson was a devout continuist. Also, both disagreed on the nature of time and causality. Bachelard's La Dialectique de la durée (1936) is a criticism of Bergson's notion of causality. Moreover, whereas Bachelard considers that the physical sciences are the ones which achieved, with relativity and quantum theory, full

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[56] For an appraisal of Bachelard's criticism of vitalism as an epistemological obstacle to scientific objectivity see, for instance, his **La Formation de l'esprit scientifique: contribution à une psychanalyse de la connaissance objective**, chapter VIII ("L'Obstacle animiste").

maturity, Bergson sharply criticized the transfer of models from physics to essentially different sciences such as biology.

The criticisms of Bergson and his attacks upon scientific reductionism started well before Bachelard came into the picture. As already noted, what Lalande called "the evolution of French thought as the renunciation of the **scientisme**" was in full development between 1880 and 1914 (Lalande 1919, 448). Thus, it is clear that Bachelard used elements from the anti-intellectualist view of science while trying, as a good positivist would, to **restore** the reputation of the scientific effort to build a comprehensive account of physical reality.[57] Also, whereas one of Bergson's purposes was to reestablish the place of metaphysics in science and liberate philosophical speculations from subservience to scientific methods (Lalande 1942, 2),[58] Bachelard claimed that there is no philosophy **outside** philosophy of science.

Another development that helps to contextualize both Bachelard and Bergson is the return to subjectivism and irrationalism at the end of the nineteenth century. For

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[57] The positivists's image was being questioned by Husserl, and also by existentialists such as Martin Heidegger, whose Time and Being was published in 1928, the same year that Bachelard published his Essai sur la connaissance approchée.

[58] Also one of Husserl's purposes.

instance, in books such as La Formation de l'esprit scientifique: contribution à une psychanalyse de la connaissance objective (1938), Le Rationalisme appliqué (1949) or La Psychanalyse du feu (1938), Bachelard often refers to the necessity of using parts of the psychoanalytical method to uncover the epistemological obstacles that linger in the scientific mind and prevent the scientist from achieving objectivity. For his part, Bergson criticizes the traditional assumptions regarding scientific objectivity which also shaped the French way of philosophizing. As Émile Bréhier points out:

> With Bergson's reflection, it is the ways we face questions that has changed: general suspicion of conceptual construction, of discussions concerned only with the meaning of words, the temporary and practical character of all classifications; but especially, the philosopher is not seen just as a pure spirit living in a purely intellectual atmosphere; Kantian criticism had [gotten us used] to a certain subject-object relation [that made possible the idea of] an exact description of human knowledge (Bréhier 1950, 109).

Interestingly enough, despite the fact that Bergson's lectures at the Collège de France were extremely popular and despite his undeniable influence in philosophical and scientific circles, Bergson was not highly regarded by the neo-Thomists. His work was put on the Catholic Index in June 1914, after his philosophy had been attacked in 1913 by people like Jacques Maritain (Tint 1964, 140-141). In La Philosophie bergsonienne, Maritain says that the doctrines of Bergson "constitute the strangest abdication of intelligence and of reason" and that, that being the case, "there is no possible [conciliation] between Christian philosophy and the enemies of intelligence." (Maritain in Parodi 1925, 327) Although praising Bergsonianism for its attacks on scientism and materialism, Maritain saw it as a reification of the intellectual nihilism that was plaguing French thought.[59]

Besides Bergsonian philosophy, other anti-intellectualist trends such as Marxism and existentialism were at the time competing with both the positivistic ideas of the nineteenth century and the renaissance of Catholic thought (Charlton 1983, 300). For instance, from the end of the nineteenth century on, French positivism had to compete, even at the educational level, with Thomism, considered the "official philosophy of Catholicism" and the most orthodox group of French Catholic intellectuals (Lalande 1938, 1; Charlton 1983, 300). As a result of trying to accommodate itself to science, French Catholicism underwent a considerable expansion. The recent scientific advancements

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[59] The attitude of French academics towards Bergson's philosophy changed gradually over time. As Maurice Merleau-Ponty pointed out, by the time Bergson was "retired from teaching and nearly silent during the long preparation of the Two Sources, [he was] already considered by Catholicism as a light rather than a danger, already taught in classes by rationalist professors." (Hanna 1962, 133)

clearly involved a new conjugation of **fides** and **ratio** or, in other words, they implied a particular relationship -- if any at all -- between scientific developments and their metaphysical significance (Paul 1979, 12 and 192). Also, neo-Thomism did not seem to be easily adaptable to modern science. Although Thomist thought in general was seriously opposed to irrationalism, it included different currents, like modernism and fideism. While the first emphasized the possible reconciliation between church doctrines and positivistic science (Wright 1981, 304), fideists would argue that "an attempt to integrate the conclusions of science into Thomism endangered the claim of metaphysics to be independent of changing scientific hypothesis" (Paul 1979, 192). It was biology, with its new evolutionary theory and the fight over Darwinism (Paul 1979, 24), that provoked more discussions.

Bergson's position in the debates over Darwinism was curious. His detailed knowledge and interdisciplinary integration of biology, psychology and physics led him to reject Darwin's and Lamarck's theories on the grounds that evolution is not the outcome of a blind and automatic process of selection, but instead a "process of essentially active adaptation of an **intellectual effort** ('effort intellectuel') which, in breaking away the with (...) inadequate forms of our intelligence and in organizing its structure, leads to a better understanding of reality" (Capek 1956a, 210). In general, he was against Thomism, and in favor of an adaptation of metaphysics to the new sciences. Just like Herbert Spencer (1820-1903), whom Bergson studied closely, and despite Spencer's pro-Darwinist views, Bergson claimed that theory of knowledge and theory of life are **inextricable** (Capek 1956a, 194).[60] To Bergson, the adaptive character of knowledge entailed a refusal of mathematical models -- used in what he called **science rigide** -- in favor of biological, psychological and sociological models.

Bergson's L'Évolution créatrice (1907) is a rebellion against the imposition of logical and materialistic models of explanation of reality. As far as physics is concerned, Bergson thought that the mathematical form of physical laws is artificial (Bergson 1944, 238), and that the standards of measurement are conventions and thus "foreign to the intentions of nature" (Bergson 1944, 239). Nature presents us with the moving continuity of things. What science gives us is "an artificial reconstruction which is in practice [the equivalent of this continuity], and has the advantage

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[60] Bergson disagreed with Spencer on fundamental grounds. For an idea of the Bergson/Spencer conflict see, for instance, M. Capek, "La Théorie biologique de la connaissance chez Bergson et sa signification actuelle," Revue de métaphysique et de morale 64.2(1956):194-211.

of being easily handled" (Bergson 1944, 358). The book also contains an expression of Bergson's rejection of Darwinism:

> The Darwinian adaptation by automatic elimination of the unadapted is a simple and clear idea. But, just because it attributes to the outer cause which controls evolution a merely negative influence, it has great difficulty in accounting for the progressive development and, so to say, rectilinear development of complex apparatus . . . . The struggle for life and natural selection can be of no use to us . . . for we are not concerned here with what has perished, we have only to do with what has survived . . . We see that identical structures have been formed on independent lines of evolution by a gradual accumulation of effects. How can accidental causes, occurring in an accidental order, be supposed to have repeatedly come to the same result, the causes being infinitely numerous and the effect being infinitely complicated? (Bergson 1944, 63-64)

Bachelard not only advocates discontinuity in conceptual development, he also was not opposed to mathematical models. He even claimed that mathematics, more than a language, had the potential to actually **create** science. Also of interest is how some of his attitudes concerning the construction of scientific knowledge were similar to the organic view suggested by Bergson. Like Bergson, when referring to the sciences Bachelard sees that traditional philosophies of science are insufficient to account for the **mobility of the scientific mind**. With Bergson, he also thinks that the complexity of the physical world cannot be accounted for by the strictly empirical, positivistic models of theory choice. They both seem to think that a strictly empirical view of the world does not illustrate the richness of the rational processes that are necessarily involved in the perception of the world, the understanding and systematization of both subjective and objective reality, and their permanent dynamic relationships. However, whereas Bergson claims that science does not describe the complexity of the physical world, particularly phenomena such as that of duration and time, Bachelard still sides with those who believe in the explanatory power of scientific models.

Although not usually addressed by French intellectuals of the period, the philosophical debate between Bergson and Einstein regarding the concept of time definitely helps to understand the contact established between French culture and the scientific controversies of the first half of the twentieth century. These controversies generated in both scientists and philosophers a renewed interest in philosophy of science. One of the most important aspects of Bergson's philosophy has to do with his concepts of time and space as related to both physical reality and the individual. He argued that it is necessary to make a distinction between **knowledge by intelligence** (whose purpose is action in the world) and inner experience (which corresponds to 'intellectual intuition'). Time, too, can be divided into **time** which is artificially measured and **duration** ('la durée') which is the inner experience of the flowing of time

(Charlton 1983, 297). The book Durée et simultaneitée (1922) constitutes Bergson's attempt at confronting his own philosophical views with the philosophical conclusions of relativity theory (Gunter 1969, 123). There he claims that relativity corresponds to a new way of thinking and that it definitely abolishes the fiction of an universal, static reference system (Gunter 1969, 124). However, Bergson was not prepared to agree with the postulate of the existence of different times. Like the relativity of simultaneity, the concept of multiple-time series is an artificial device, a scientific convention, rather than the representation of what **really** happens in the world. There is only **one real time** which is not an illusion or an "effect of perspective" (Gunter 1969, 124). The task of the philosopher is precisely to discern what is conventional from what is not.

Einstein's rebuttal to Bergson's criticism of the multiplicity of times implicit in relativity theory was that a distinction should be made "between the time studied by the psychologist and the time studied by the physicist." As Gunter reports:

> The concept of universal time is indeed, Einstein agrees, derived from the psychological experience of simultaneity and is a first step towards objectivity. But our capacity to deal with the high propagational velocity of light reveals to us that the concept of simultaneity derived from ordinary perceptual experience leads to contradictions. In relativity theory we discard psychological time in order to attain to the objective time of objective space and thus

> overcome our originally subjective impressions (Gunter 1969, 127).

The problem associated with relativity, which was at the origin of the chief question of Durée et simultaneité, was to know "what sort of **reality** should be accorded to the various opposed observers who disagree in their measurement of time" (Lalande 1924, 543). Besides, according to Bergson, there are important elements that do not show up in the relativistic description, in fact in any scientific description, of time:

> Aging and duration belong to the order of quality. No work of analysis can resolve them into pure quantity. Here the thing remains separate from its measurement, which besides, bears upon a space representation of time rather than upon time itself (Bergson 1965, 160).

Just as he had not shown any interest in the epistemological conflict between Einstein and Bohr, Bachelard did not seem to be particularly interested in the details of the debate between Einstein and Bergson.[61] He was, however, deeply concerned with metaphysical issues such as that of scientific realism, i.e., the objective validity of the theoretical entities postulated by both relativity and quantum theory. Furthermore, Bachelard's claim that scientific entities are constructs **within** an experimental,

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[61] For additional information on this debate see, for instance, P. A. Y. Gunter, Bergson and the Evolution of Physics (1969) chapter on "Henri Bergson, Albert Einstein and Henri Piéron."

laboratory context is a sort of follow-up to Bergson's notion that the task of the philosopher of science is to differentiate between the real and the conventional. Referring, for instance, to relativity's concept of time, Bergson says:

> We are no longer dealing with an experienced time; we shall be before a conventional time, an auxiliary magnitude introduced with a view to calculating real magnitudes. It is perhaps for not having first analyzed our mental view of the time that flows, our feeling of real duration, that there has been so much trouble in determining the philosophical meaning of Einstein's theories, that is, their relation with reality (Bergson 1965, 64).

Later on in the same work, Bergson claimed that "there is a single real time, and the others are imaginary" (Bergson 1965, 77). As I said in Part I, to Bachelard the reality of a scientific concept lies in its contextualization and in its **technical determination** more than in its correspondence with nature. There is thus no sense in talking about conventions in the midst of a context that is by definition already **opposed** to natural things, i.e., things which do not have the imprint of rational or material techniques and thus do not have scientific status. In this regard, Bergson and Bachelard do not seem to fundamentally disagree. They seem to define "conventionality" of scientific concepts differently, but they are both quite aware of the artificial character of theory choice and theory justification. Despite the fact that Bachelard could have derived the notion of conventionality of scientific theories and entities from the works of Poincaré, Bergson seems to have the same view. Gunter synthesizes Bergson's acknowledgment of the conventional character of science in the following terms:

> Bergson's explanation of the appearance of conventional elements in relativity theory is the same as his explanation of the appearance of conventional elements in scientific thought generally: that is, it is caused by the scientists' inability to give a completely adequate description of change, motion, and variability (Gunter 1969, 124).

Both Bergson and Bachelard acknowledge that some of the products of the scientific mind have to do with habitual modes of thinking which are culturally and cognitively determined, i.e., they represent the direct epistemic outcome of our perceptions of the world.[62]

Bergson's 1911 conclusion that the idea of a corpuscle conceived as a locus of movement is nothing but "a simple concession to the [entrenched] habits of our visual imagination" (Capek 1956a, 206) was later considered by people such as de Broglie as an astounding anticipation of the fusion of substance and movement in modern physics that

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[62] For synthetic studies on the comparison between Bachelard and Bergson, see for instance, S. Goyard-Fabre, "Bachelard et Bergson: 'deux grandes pensées'," pp. 93-107 in Guy Lafrance, ed., Gaston Bachelard: profils épistémologiques (Canada: Presses Universitaires d'Ottawa, 1987), 93-107; see also Mary McAllester, "Bachelard contre Bergson: vers une pensée de la différence," pp. 241-247 in Gaston Bachelard: l'homme du poème et du théorème, (Colloque de Dijon, 1984).

took place much later.[63] In his article "Les Conceptions de la physique contemporaine et les idées de Bergson sur le temps et sur le mouvement" (1941), for instance, de Broglie said:

> since [my] youth [I was] very much taken by the most profoundly original ideas of Bergson concerning time, duration and movement. More recently, having [reviewed] those famous pages and reflecting on the progress achieved by science . . . [I was] taken aback by the analogy of certain new conceptions of contemporary physics with some of the stunning ('fulgurantes') intuitions of the philosopher of Duration ('le philosophe de la Durée')(de Broglie 1941a, 242).

Some of the ideas de Broglie referred already appeared in Bergson's Essai sur les données immédiates de la conscience (1889), some forty years before Bohr's and Heisenberg's interpretation of quantum mechanics. In that book, Bergson suggests that "there is nothing in space but parts of space and, anywhere where one considers the mobile, **one will not obtain but one position**" (Bergson in de Broglie 1941a, 250).[64] The nature of our intellectual and visual

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[63] Milic Capek also found "similarities between the ideas of Henri Bergson and the conceptual changes introduced into physics by quantum theory and the theory of relativity." (Gunter, 1969)

[64] For a primary source on the relations of Bergson's philosophy with physics see, for instance, Louis de Broglie, "Les Conceptions de la physique contemporaine et les idées de Bergson sur le temps et sur le mouvement" in Revue de métaphysique et de morale LIII.4(1941):241-257; for a brief secondary study of Bergson as seen through the eyes of De Broglie, see M. Capek, Bergson and Modern Physics: a Reinterpretation and Evaluation, chapter 13.

habits as taken by Bergson in works such as L'Évolution créatrice is somewhat similar to what Bachelard saw as **epistemological obstacles** to the progress of scientific reasoning. Bergson argued that intellectual habits, which also include certain association of ideas, become so familiar that they become solid and unbreakable.[65] Rationalists "endowed them with the status of **a priori** principles constituting the immutable or, as Kant called it, 'transcendental' structure of the human mind which no future experience can challenge" (Capek 1971, 56). In L'Évolution créatrice (1907), Bergson had already established the persistent influence of what he called "the logic of solid bodies" throughout the history of science:

> We shall see that the human intellect feels at home among inanimate objects, more especially among solids . . . that our concepts have been formed on the models of solids; that our logic is, pre-eminently, the logic of solids . . . . But from this it must also follow that our thought, in its pure logical form, is incapable of presenting the true nature of life, the full meaning of the evolutionary movement (Bergson 1944, xix-xx).[66]

Mechanism and atomism are examples of intellectual habits, which Bergson also criticizes in his Matière et mémoire (1896):

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[65] This position is similar to Husserl's as illustrated in his concept of "sedimentation" and developed in the Crisis of European Sciences.

[66] For an excellent study of Bergson's connection with these matters see Capek, chapter 5.

> But the materiality of the atom dissolves more and more under the eyes of the physicist. We have no more reason, for instance, to represent the atom to ourselves as solid rather than as liquid or gaseous, nor for picturing the reciprocal actions of atoms by shocks rather than in any other way. Why do we think of solid atoms, and why of shocks? Because solids, being the bodies on which we clearly have the most hold, are those which interest us most in relations with the external world; and because contact is the only means which appears to be at our disposal in order to make our body act upon other bodies. But very simple experiments show that there is never true contact between two neighboring bodies; and, besides, solidity is far from being an absolute state of matter. Solidity and shock borrow then, their apparent clearness from the habits and necessities of practical life; images of this kind throw no light on the nature of things (Bergson 1959, 195).

As we saw in the last chapter, Bachelard followed up on some of Bergson's claims concerning atomism in works such as Les Intuitions atomistiques. On the other hand, L'Intuition de l'instant (1932) constitutes Bachelard's criticism of Bergson's concept of **instant** as artificial -- Bachelard sees the instant as real -- and in La Dialectique de la durée (1936) Bachelard rejects Bergson's continuity thesis.[67] Two of the chapters of La Dialectique de la durée are devoted to duration and causality, and constitute an ideal illustration of Bergson's and Bachelard's contrasting views. Bachelard uses the recent discoveries of microphysics to criticize Bergson's causality thesis. To Bachelard,

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[67] Roch Smith reports that later in his life Bachelard would say that he accepted everything in Bergson's philosophy except Bergson's continuity thesis (Smith, 1982:3).

microphysics had already shown that Bergson's notion that we think according to the 'logic of solid bodies' is incorrect. On the other hand, determinism and causality are also challenged by quantum theory. In fact, determinism is "an almost immediate consequence of the character of quantum measurements." (Bachelard 1950, 63), and "the easy trajectories [of time] was totally ruined by microphysics" (Bachelard 1950, 67). Bachelard would also say:

> We have attained a level of knowledge where the scientific objects are what we make them be. . . The history of a laboratory phenomenon is the history of the measurement of the phenomenon. . . . Causality is in a way solidified by our instruments. Objectivity becomes the more pure as it ceases to be passive and become active, the more it ceases to be continuous in order to become clearly discontinuous (Bachelard 1950, 63).

Nevertheless, it is clear that Bergson's contributions to both the development of philosophy of science in France and to Bachelardian epistemology should definitely be taken into consideration. Bachelard picked up some of Bergson's favorite issues, which were also the crucial problems besetting French anti-intellectual philosophical circles, and developed them to their ultimate consequences. Instead of staying at the phenomenological level of scientific knowledge acquisition, i.e., the analysis of the repercussion of the object in the flowing consciousness of the subject as expressed for instance in

Bergson's La Pensée et le mouvant (1938),[68] Bachelard claimed that there is also an inextricable dynamic interchange between the mobile spirit of the scientist and phenomena which, at the level of theoretical physics and modern chemistry, are not only imprecise but often times the result of scientists' rational and material constructions. It is at this level that Bachelard introduced the concept of phenomenotechnique.

Other Philosophical Connections

Part of what Bachelard considers to be fundamental in the new sciences is the dialectical character of knowledge construction. This dialectic is what led him to claim that the metaphysics of the new scientific mind is a mixture of **technical materialism** and **applied rationalism**. Scientific practices strive for the realization, in the laboratory context, of entities postulated by scientific theories. The dialectic intervenes at the level of the materialization of those entities through experimental, technical procedures. Technique determines what is to be found, but rationality constructs the verification techniques. On the other hand, technique (whether mathematical or instrumental) serves as

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[68] For a nice overview of Bergson's phenomenology, see C. A. Van Peursen, "Henri Bergson, phénoménologie de la perception," Revue de métaphysique et de morale 3(1960):317-326.

the intermediary between scientists' speculations and reality, that is, between scientific theoretical entities and their reification. If reification is impossible, it is because the entity does **not** correspond to something in the world. If an instrumental technique reveals a postulated entity or an entity which had not been contemplated by the theory, then new pieces of the scientific system of thought are invented (a contextualized, artificial world view).

Some might be tempted to read the heavy hand of Hegel into Bachelard's concept of dialectic. But to say that Bachelardian dialectics was inspired by Hegelian dialectics is almost as farfetched as to say that Marxist scientific materialism influenced Bachelard's instrumental realism. Obviously, Hegelianism and Bachelardian epistemology can be related at a different level, i.e., cultural. Hegel, as well as Kant, and later on Bergson, Husserl and Merleau-Ponty, devoted much attention to phenomenology and to the traditional philosophical and psychological analysis of the demarcation between the subject (or spirit, or consciousness) and the objects of perception. Through the popularity of works such as The Phenomenology of the Spirit (1807) and Reason in History : A General Introduction to the Philosophy of History (1837) and others, Hegelian philosophy can be seen as much a part of the cultural background of France as Kantianism. Hegelianism was part of the intellectual history of Europe until the middle of the nineteenth century, only to recede with the rise of positivism, finally to reemerge in the first decades of the twentieth century (Hartmann 1931, 289).

It is difficult to determine the influence of Hegelianism on Bachelard's scientific phenomenology. True, there are many problems posed by Hegel that seem to resurface in Bachelard's works, but problems such as the relation between objects and consciousness and intentionality are common to **any** phenomenology. In The Phenomenology of the Spirit, Hegel said that dialectics is the "successive manifestation of constantly renewed views ('aperçus') about the object and, with relation to our intuition, [dialectics] truly represents a sort of movement of the object" (Hartmann 1931, 290). He also claims that each object of study develops different methodologies. Each object requires a specific method of approach, since it is impossible to generalize methodologies (Hartmann 1931, 291), or even to create them in advance, without first taking the context into consideration (Hartmann 1931, 293). This position is similar to that of Bachelard when he defends a pluralistic philosophy of science, ultimately represented by applied rationalism, on the grounds that each science has its own objects and its own methodologies. The methodological differentiation has to do with the diversity of objects of study in each science, but it is also the result of the different stages of maturity reached by each one of them.

In Reason in History, the study which Hegel devotes to history as an illustration of the progress of Reason (Hegel 1953, 8), Hegel puts forth views quite similar to Bachelard's claim that the study of science allows us to determine the progress of rationality. Bachelard's classification is also similar to Spencer's in his La Classification dans les sciences[69] and especially the classification of Brunschvicg in his Les Âges de l'intelligence. The notion expressed by Hegel that "the very essence of spirit is action, it makes itself what it essentially is; it is its own product" (Hegel 1953, 89) is programmatically realized in Bachelard's claims about the constant dynamics of the scientific spirit, and its ability to phenomenotechnically produce scientific objects. But again, the attempt to trace some of the problems encompassed by Bachelard's philosophy of science back to Hegelianism makes more sense if we first realize that these problems reveal Bachelard as a phenomenologist of science rather than as a developer of Hegelian philosophy of history.

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[69] This similarity is even more revealing if we realize that Spencer's classification was a criticism of Comte's, and that Bachelard was also deeply critical of Comtean positivism.

Bachelard's knowledge of the works of William James (1842-1910), one of the few American philosophers read widely in Europe (Holton 1992, 33), is undeniable.[70] In L'Essai sur la connaissance approchée (1928), for instance, he uses material from James' Précis de psychologie in a chapter which Bachelard devoted to concept rectification (Bachelard 1987, 19-29), and from James' L'Idée de vérité in his chapter on "Epistemological continuity and progressive verification" (Bachelard 1987, 258-271). Curiously enough, Bachelard's concept of "approximation" and the idea that scientific entities or theories do not have any meaning if taken out of the context in which they are recognized and validated is nothing but James's pragmatic theory of truth applied to science. In Pragmatism: a New Name for Some Old Ways of Thinking (1907), James would say the following:

> **True ideas are those that we can assimilate, validate, corroborate and verify. False ideas are those that we cannot** . . . . The truth of an idea is not a stagnant property inherent in it. Truth **happens** to an idea. It **becomes** true by events. Its verity is in fact an event, a process: the process namely of its verifying itself, its veri-**fication**. Its validity is the process of its valid-**ation** . . . . This marriage of fact and theory is endlessly fertile. What we say is here already true in advance of special verification, **if we have subsumed our objects rightly**. Our ready-made ideal framework for all sorts of possible objects follows from the very structure of our thinking. They coerce us; we must treat them consistently, whether or not we like the results . . . . Between the coercions

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[70] In his works, Bachelard also refers to John Dewey, another pragmatist.

> of the sensible order and those of the ideal order, our mind is thus wedged tightly. Our ideas must agree with realities, be such realities concrete or abstract, be they facts or be they principles . . . (James 1981, 92-96).

The approximative, dialectical character of knowledge as seen by James is thus very much the same as Bachelard's epistemological considerations about the formation of scientific knowledge. But the fact that Bachelard uses notions that are so close to pragmatism does not really mean an unconditional allegiance to the pragmatic view as applied to science. In fact, the philosophical orientation towards action can also be found in many other non-pragmatic philosophies to which Bachelard was also exposed.

The functional role attributed to scientific knowledge by Bachelard is similar to some aspects of the philosophies of Émile Boutroux and Herbert Spencer. In works such as _L'Idée de loi naturelle_ (1894) Boutroux defended a double mobility: the mobility of things and the mobility of our mind. Science is an artificial creation, and the scientific mind shapes itself by following the creation and the progress of science (Parodi 1925, 183). On the other hand, siding with both Bergson and Bachelard, Boutroux argues that it is "through abstraction and artificial construction that we isolate a world of atoms and mechanical forces and consider themselves as self-sufficient" (Parodi 1925, 179). In _Les Principes de psychologie_ (1855) Spencer would say that the "constant development and increasing complexity of sensitivity is parallel to the evolution and increasingly more complicated relations between the living being and its environment" (Parodi 1925, 195). Bachelard too sees knowledge complexification as the direct result of a dialectic between the scientific mind and the experimental context. But there is a fundamental difference between Bachelard's and Spencer's views regarding rationality. In fact, whereas Spencer sees reason in its present form as **static**, as the definitive result of evolution (Parodi 1925, 211), Bachelard sees it as still undergoing stages of development, which are intrinsically **dynamic** and relatively independent from biological evolution in the strict sense of the word. On the other hand, whereas Spencer claimed that non-Euclidean theorems are inconceivable (Capek 1971, 24), because to him Euclidean geometry is the only direct counterpart of the human organism's perception of space, Bachelard saw in non-Euclidean geometries the living proof of the nature of scientific reasoning as constructing itself "against" nature.

Bachelard's attitude regarding the relations between philosophy and science was similar to Spencer's and Bergson's in the sense that, to him, philosophy should stand up to the new demands of scientific thought. However, instead of a Bergsonian **métaphysique positive**, Bachelard followed Brunschvicg's steps in giving importance to mathematics as the tool **par excellence** of the new scientific mind. Also, instead of being carried away by the claims of an evolutionary, socio-biological model for rationality, Bachelard chose to demarcate rationality from biological determinism and concentrate on its socio-rational aspects (which are more revolutionary than evolutionary). The proto-functionalist attitude obvious in the philosophies of Spencer and Bergson were with all probability an important source of inspiration for Bachelard's philosophy of science, particularly Bachelard's criticism of the nature of scientific realism and Kantian-oriented descriptions of the scientific mind at work. This criticism is also an attack on positivism.

Bachelard mentions some of Ernst Mach's works first in his Essai sur la connaissance approchée, then in L'Évolution d'un problème de physique and in L'Activité rationaliste de la physique contemporaine. He also refers to Hans Reichenbach's The Rise of Scientific Philosophy in books such as La Formation de l'esprit scientifique, and to both Mach and Reichenbach in La Valeur inductive de la relativité. Nevertheless, Bachelard has a certain contempt for the intellectual contributions of the positivists to the philosophy of science. This does not mean that he simply ignored the scientific contributions of Mach and other

> The Vienna Circle evolved in 1923 out of a seminar led by Professor Moritz Schlick and attended, among other students, by F. Waismann and H. Feigl . . . . Many of the participants were not professional philosophers. Schlick, for example, had specialized in physics. . . . Among the other active members we may mention: Hans Hahn, mathematician; Otto Neurath, sociologist; Victor Kraft, historian; Felix Kaufmann, lawyer; Kurt Reidemeister, mathematician . . . Phillip Frank . . . the psychologist E. Kalia . . . . The most decisive and rapid development of ideas began in 1926 when Carnap was called to the University of Vienna . . . (Feigl in Joergensen 1970, 848).

Initially, the program of the Circle was to develop the contributions of Ernst Mach (1838-1916) to the philosophy of physics,[72] to study logic and "the clarification of the meaning of propositions" (Holton 1992, 46). Later they also became interested in the analysis of the works of Bertrand Russell (1872-1970), Alfred N. Whitehead (1861-1947), and Ludwig Wittgenstein (1889-1951), just to name a few. The Circle was constituted by people especially interested in the philosophical foundations of science. They "centered about the foundations of logic and mathematics, the logic of empirical knowledge, and only occasional excursions into the philosophy of the social sciences and ethics" (Feigl in Joergensen 1970, 849). According to Carnap, all the members of the Circle were acquainted with modern logic, and agreed

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[72] A similar group formed around Hans Reichenbach, Alexander Herzberg and Walter Dubislau in Berlin. Later this group developed into the Society for Empirical Philosophy. There was also a group of logical empiricists in Warsaw. For more information on the Berlin group, see Joergensen, 1970: 894-900; for the Warsaw group, see Joergensen, pp. 900-901.

on crucial points. For instance, all rejected traditional metaphysics (Carnap in Schilpp 1963, 21). In general, they thought that, contrary to Neurath, "logic, including applied logic, and the theory of knowledge, the analyses of language, and the methodology of science, are, like science itself, neutral with respect to practical aims" (Carnap in Schilpp 1963, 23). Neurath, however, had different views concerning the social aims of the unification of the natural sciences and the social sciences under a common language. As Carnap wrote in his autobiographical notes:

> For Neurath the aim of a unified science was of vital importance. The sharp distinction between natural sciences and . . . humanities, which was strongly emphasized in German philosophy, was in his view an obstacle on the road towards our social goal, because it impeded the extension of the empirico-logical method to the social sciences. Among the possible forms of science he gave strong preference to a physicalist language as against a phenomenalistic one (Carnap in Schilpp 1963, 23).

Still, an exposition of the purposes of the Vienna Circle were published in Wissenschaftliche Weltauffassung: Der Wiener Kreis (1929) claimed that the aim was, just like Neurath argued, to form a "unified science comprising all knowledge of reality . . . without dividing it into separate, unconnected special disciplines, such as physics and psychology, natural science and letters, philosophy and the special sciences" (Joergensen 1970, 850). The periodical Erkenntnis, founded in 1930 by Carnap and

Reichenbach, was also the result of a wide curiosity for the ideas of the Circle.

The interest for a "unified science" -- this term was introduced into the positivists' discourse by Neurath -- was still present well into the thirties, both among philosophers of science and scientists alike. In June 1936, for instance, the session at the Second International Congress for the Unity of Science in Copenhagen devoted to "The Causality problem" included in the audience, among others, Karl Popper, Niels Bohr, and Carl Hempel (Holton 1992, 50).[73] The connections between the positivists and the scientists of the first half of the twentieth century was quite strong and led to the renewal of philosophical models of scientific justification.

Despite Bachelard's knowledge of some of the works of the logical positivists, he never attempted to differentiate that positivism from the positivism of Comte, and made little effort to understand the main arguments of the logical positivists. It may be possible to argue that the reason for the lack of knowledgeable references to the

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[73] Karl Popper had contacts with the Vienna Circle. People such as Carnap thought that Popper's philosophical attitudes were similar to those of the Circle. Apparently, Popper did not think the same way, and was quite critical of the positivists in his works, especially of Schlick, Neurath and Reichenbach. For more on this, see Carnap in Schilpp, 1963: 31-32.

positivists. But the fact remains that, if we were only to rely on Bachelard's account of the history and philosophy of science of his time, we would never be aware of the impact of Machian positivism on the scientific works of people such as Albert Einstein,[71] or the high regard which scientists such as Lorentz, Heisenberg, Pauli, James and others had for both his science (particularly in acoustics and optics) and Mach's philosophy (Holton 1992, 27). Gaston Bachelard's neglect of positivism is at first surprising when we realize the influence of positivism in the interwar period, precisely the time when Bachelard's first works on epistemology were published. It is also surprising if we take into account Bachelard's own program of enhancing the picture of science, which was very much like the one presented by the positivists themselves. I think that there are at least two complementary explanations for this neglect. They both have to do with the ideological structure and aims of the European positivists.

The Vienna Circle

The origin of the Vienna Circle was described by Herbert Feigl in the following terms:

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[71] For more on this see Einstein's Autobiographical notes," pp. 3-95 in Schilpp, 1949.

positivism of the Vienna and the Berlin Circles in Bachelard's books had more to do with the French context in the period between wars than to lack of intellectual curiosity on the part of Bachelard. However, a strictly externalist account is insufficient to justify Bachelard's detachment from the Circle's views. In fact, quite divergent records of the contacts of the positivists with the rest of the intellectual world are available. According to Holton, for instance, the Vienna positivists were interested in seeking allies, but were more successful in Great Britain and the United States. Contrary to France, in these countries "the ground had been prepared by the work of Peirce, James, and to some extent John Dewey and others" (Holton 1992, 50). This view seems to coincide with the stereotype of the animosity of most of French academia towards the novelties, both scientific and philosophical, coming from German-speaking Europe. But the stereotype is problematic. People such as Joergensen had a different perspective concerning the impact of the positivists outside Austria. According to him, the Unity of Science congresses held by the Vienna Circle took place in Prague (in 1929), Königsberg (in 1930), Paris (in 1935, under the heading "Congrès international de philosophie scientifique"), Copenhagen (in 1936), again Paris (in 1937 under the heading "Congrès Descartes"), Cambridge, England (in 1938), and

Cambridge, Massachusetts (in 1939).[74] Moreover, German-language philosophers such as Husserl, and Heidegger, for instance, were immediately read. If there was animosity towards Germany, there would be no reason for it to be selective.

It is thus reasonable to think that Bachelard's lack of interest for the positivists was more intellectual than strictly political. The animosity towards positivism by people like Poincaré and Couturat are, in this respect, good examples. On the other hand, Bachelard spend most of this epistemological period analyzing the philosophical implications of the scientific novelties coming from Austria and other countries -- Bachelard did **not** pursue the positivistic route, because he decided to focus on different aspects of the scientific enterprise. This leads to the second reason for Bachelard's neglect of "German" positivism. While striving to understand the logical structure of the new sciences, the positivists ended up neglecting aspects of the scientific enterprise which were fundamental to Bachelard. One is at the origin of Bachelard's concept of phenomenotechnique and has to do with an idealistic reaction against the materialistic, anti-metaphysical positions taken by the positivists. Also,

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[74] For more information on these congresses, see Joergensen, 886-894.

intersubjective agreement within the scientific community, and the postulation of 'socially constructed' entities, while not alien to the conventionalists, seemed to be quite apart from the rest of the positivists' claims concerning the way science is structured. Furthermore, they were not interested in the analysis of what Reichenbach called "the context of discovery," but strictly in the "context of justification." For his part, Bachelard was not so much interested in what science **is**, but instead in what science is constantly **becoming**. He also disagreed with earlier positivists such as Reichenbach on the issue of continuity. Reichenbach would say that "science requires a reinterpretation of the knowledge of everyday life, because knowledge is ultimately of the same nature whether it concerns concrete objects or the constructs of scientific thought" (Reichenbach 1954, 177-178). Bachelard claimed instead that common knowledge and scientific knowledge belong to different systems of thought and are thus discontinuous with each other. Moreover, the discontinuity is not just in terms of thinking processes, but also in terms of the objectification of scientific thinking processes (technique) in scientific technology.

What about Husserl?

Bachelard's concept of phenomenotechnique started out as an adaptation to the study of scientific practices of the concept of phenomenology as defined by Husserl (1859-1938). As we saw in Part I, Bachelard thinks that the direct application of the phenomenological method to science is incorrect. He argues that phenomenology, which can be defined as "the **method** of the critique of cognition" (Husserl 1964, 3), i.e., the analytical study of consciousness in relation to its objects of experience, does not account for the dynamics in both the scientific processes of cognition and in the constant creative dialectic between the scientist and the scientific facts or objects (which, in modern science, are **constructed** by scientists themselves). Phenomenotechnique implies that the **essence** of scientific objects is mathematical, and that instruments are **materialized theories** which serve to test the material reality of what are at first exclusively rational postulates about the structure of the world. Phenomenology deals with essences too, but these essences are directly apprehended by consciousness. In other words, they are not the product of rational **elaboration**, but instead the result of the "passive" structure of both our consciousness and the objects that we perceive, whether these objects actually exist outside our consciousness or not. According to Bachelard, direct apprehension only happens in the context of ordinary knowledge and in the realm of poetic imagination, **not** in the acquisition and construction of science. Thus, phenomenology needs to be reshaped in order to be the correct approach to scientific methodology.[75]

It not enough, however, to say that phenomenotechnique is an extension of classical, Husserlian phenomenology, since the ideology behind both methodologies is quite different (Margolin 1983, 58). On the other hand, it is not easy to determine the **real** impact of Husserlianism on Bachelard's epistemology. Although Bachelard uses concepts that can only be found in Husserl's discourse, such as the concepts of intersubjectivity and intentionality[76] he does not give them their original meaning. Instead, just as he did with Freudian psychoanalytic discourse or Hegelian dialectics, rather than develop the epistemological consequences set up by their original usage, Bachelard

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[75] An interesting synthesis of phenomenology can be found in J. M. Edie, <u>Edmund Husserl's Phenomenology: a Critical Commentary</u> (Bloomington, Indianapolis: Indiana University Press, 1987); for selection of primary sources see also R. O. Elveton, ed., <u>The Phenomenology of Husserl: Selected Critical Readings</u> (Chicago: Quadrangle Books, 1970).

[76] The term 'intentionality' was coined by the philosopher Franz Brentano, whose lectures in Vienna Husserl attended.

might have known about them at the time of their publication in Paris in 1931 under the title **Méditations cartésiennes**. On the other hand, it is possible that Bachelard had had further contact with Husserlian phenomenology by means of Husserl's commentators or through the reading of German originals.[79] Works such as the Crisis, for instance, only became public in the fifties (Landgrebe, 1964:366). Also, the preface to the 1973 edition of Husserl's Experience and Judgement (1938) tells us that its publishing house (the Academic-Verlag of Prague) was closed soon after the annexation of Czechoslovakia, thus preventing almost all if its copies from becoming public. Only two hundred copies were available; these were sold in England and in the United States, and thus remained basically unknown in continental Europe. It is however possible that Bachelard had had earlier contact with other works of Husserl through his friend Jean Cavaillès, who was, together with Jean-Paul Sartre, one of the few intellectuals responsible for the enthusiasm for Husserl's works in France during the forties (Bachelard 1960a: 354). It is also possible that the connection with Husserlianism might have been established

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[79] Bachelard's daughter was a Husserl scholar of international reputation around 1957, some time after Bachelard suspended his interest in publishing epistemological works. To be more precise, his last book on philosophy of science was published in 1953, although he taught history and philosophy of science at the Sorbonne from 1940 to 1955.

through Merleau-Ponty, who taught at the Sorbonne at the same time as Bachelard.[80] Unfortunately, all the possible connections of Bachelard with Husserlianism mentioned in the above paragraph are speculations on my part.

Nevertheless, the orientation of Bachelard towards pure phenomenology appears in the context of his later concerns over poetic imagination. In works such as La Poétique de l'espace and La Poétique de la reverie, for instance, he even concludes that, compared to the psychoanalytical method, phenomenology (including Minkovski's phenomenological concept of "reverberation")(Bachelard 1964, xii) is the **ideal method** for understanding the impact of poetic imagery in our minds (Granger 1974a, 21),[81] since it postulates a sort of intuitive, direct, non-intellectual apprehension of its essence:

> Only phenomenology -- that is to say, consideration of the **onset of the image** in an individual consciousness -- can help us to restore the subjectivity of images and to measure their fullness, their strength and their transsubjectivity . . . . At the level of the poetic image, the duality of subject and object is iridescent, shimmering, unceasingly active in its inversions. In this domain of the creation of the poetic image by the

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[80] Merleau-Ponty taught at the Sorbonne between 1944 and 1954.

[81] For more on the use of phenomenology to study imagination see E. S. Casey, "Imagination and the phenomenological method", in F. A. Elliston, and P. McCormick, eds., Husserl: Expositions and Appraisals (Notre Dame: University of Notre Dame Press, 1977).

> poet, phenomenology, if one dare to say so, is a microscopic phenomenology (Bachelard 1964, xv).

Bachelard sees the task of poetic imagery as contrasting with that of the specificity of rational consciousness, i.e., its **constructive** purposes (Bachelard 1964, 212). The demarcation established in the works of Bachelard between science and art is objectified in his claim that phenomenology is the methodological tool for the analysis of artistic creativity, whereas phenomenotechnique is the analytical tool for the study of science.[82] He keeps pointing out, for instance in his 1953-1954 lectures, that "phenomenologists want consciousness to always be consciousness of something" (Lescure 1963a, 121). By so doing, they fail to analyze the act of consciousness itself, and thus to realize that consciousness can invent its own objects rather than just describe what is presented to it (Lescure 1963a, 121).[83]

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[82] It is important to notice in this context that the theoretical demarcation established by Bachelard between these two modes of expression does not hold in practice. In La Poétique de l'espace, he claims that one reason for the adequacy of phenomenology (and not psychoanalysis) for the study of poetry is that the poetic image is **independent from causality**. Obviously, the epistemological novelties brought about by the new sciences, including the rejection of strict causality, allow for the application of phenomenology to the study of science. This is what Bachelard does through the concept of phenomenotechnique.

[83] Suzanne Bachelard subsequently used Bachelard's category of **applied rationalism** to undertake a phenomenological study of science. Inspired by both

Bachelard's return to phenomenology in his later works is quite different from the treatment he gives it in the context of his epistemological writings. The explanation for this is both ideological and historical. We have seen that Bachelard does not think that the classic phenomenological method is accurate in terms of being aware of the implications, for the mind, of scientific knowledge and the way it progresses. Actually, his criticisms are addressed to the commentators and followers of Husserl (possibly Max Scheler) more than to Husserl himself. Bachelard thinks that the popularizers of Husserl's works have distanced themselves from the original phenomenological source by forgetting an important aspect of consciousness, i.e., its **instrumental character** (Granger 1974, 212).

Although both Husserl and Bachelard are very critical of psychologism positivism,[84] and the traditional division between empiricism and rationalism, the phenomenological program established by the later Husserl is the result of what he sees as the crisis of European sciences and culture

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Bachelardian epistemology and Husserlianism, she called this study **epistemological phenomenology**. See, for instance, Suzanne Bachelard's "Phenomenology and mathematical physics," pp. 413-440 in Kockelmans and Kiesel, 1970.

[84] For more on Husserl's anti-psychologism see E. Stroker, "The Role of psychology in Husserl's phenomenology", in H. Silverman, J. Sallis, and T. Seebohm, eds., Continental Philosophy in America (Pittsburgh: Duquesne University Press, 1983).

(Husserl 1970, 6), i.e., their inability to address the real problems of humanity.[85] He criticizes the sciences on the grounds that they strive to alienate the investigator from subjectivity and metaphysics. Since the Renaissance it is the belief in a scientific truth that provoked the spread of ideologies and philosophies which disrupt societal values (Husserl 1970, 7), i.e., the original purity of what Husserl calls in the Crisis of European Sciences the **lifeworld**. The physical sciences are those responsible for the establishment of world views (theoretical approaches to the world) (Husserl 1970, 281) which, by becoming **traditions**, are perceived as the objective truth about the world. He termed this as the process of "sedimentation," which corresponds to Polanyi's notion of "tacit knowledge" (Harvey 1989, 51).

Bachelard's position regarding European science is completely different. In an interview with Gilles-Gaston Granger, he would say, "Don't you see how everywhere scientific culture is asphyxiated ('étoufée'), discouraged, baffled ('déroutée')? Although each one pretends to render it honor, our world is such that a thousand obstacles [are

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[85] Later to be systematized by existentialists such as Heidegger and Sartre. For the relation among these three authors, see Landgrebe, "Husserl, Heidegger, Sartre. Trois aspects de la phénoménologie," Revue de métaphysique et de morale 69.4(1964): 365-380.

constructed against] objective thought" (Granger 1947a, 55). Moreover, Bachelard's purpose is not to claim with Husserl that society is "infected" with ideologies which are the natural outcome of positivistic science, but on the contrary that science has to learn to liberate itself from traditional philosophies of science (including positivism) and common-sense ways of thinking which, by invading both the scientific mind and past scientific practices, constitute epistemological obstacles to the progress of scientific rationality. Nevertheless, there are many ideas in Bachelard's scientific phenomenology which can be connected with the views of Husserl as expressed in works such as Ideas: General Introduction to Pure Phenomenology (1931), and which Bachelard seemed to have known directly (Margolin 1983, 57).

In Ideas, Husserl criticizes empiricists on the grounds of what he sees as their hostility towards concepts such as 'Idea', 'Essence' and 'Knowledge of the Essential Being' (Husserl 1967, 81). "The fundamental defect of the empiricist's argument," he says, "lies in this, that the basic requirement of a return to the 'facts themselves' is identified or confused with the requirement that all knowledge shall be grounded in **experience**" (Husserl 1967, 83). On the other hand, Husserl argues that the positivists "confuse at one time the cardinal distinctions between the types of intuition, and at the other . . . are yet **not willing**, being bound by their prejudices, to recognize more than one of these as valid, or indeed as present at all" (Husserl 1967, 87). But more important than the criticism of these philosophies is the study which Husserl undertakes on concepts that are later on akin to Bachelard's philosophy of science. I am referring, for instance, to the concepts of **intersubjectivity** and **intentionality**, which, as already noted, Bachelard considers to be fundamental to the processes of validation, objectification, and thus tacit agreement concerning scientific entities and theories within the **cité scientifique**.

In Ideas, Husserl says the following about intersubjectivity:

> Whatever holds good for me personally, also holds good, as I know, for all other men whom I find present in my world-about-me . . . . For each, again, the fields of perception and memory actually present are different, quite apart from the fact that even that which is here intersubjectively known in common in different ways, is differently apprehended, shows different grades of clearness and so forth. Despite all this, we come to understandings with our neighbours, and set up in common an objective spatio-temporal fact-world . . . (Husserl 1967, 105).

Some of Bachelard's claims in his later epistemological works seem to be instantiations of Husserl's phenomenological claims about intersubjective agreement relative to the content of the world as it is perceived. The only difference is that Bachelard applies this kind of intersubjective awareness to science, whereas Husserl generalizes it to one's surroundings.

Furthermore, Husserl also refers to intentionality and the way it relates to experience, which again seems to be one of the high points of Bachelard's scientific phenomenology.[86] In his Ideas, Husserl said the following about intentionality:

> Under **experiences** in the **widest sense**, we understand whatever is to be found in the stream of experience, not only therefore intentional experiences, **cogitations** actual and potential taken in their full concreteness, but all the real phases to be found in this stream and in its concrete sections. For it is easy to see that **not every real phase** of the concrete unity of an intentional experience has itself the **basic character of intentionality**, the property of being a "consciousness of something." This is the case, for instance, with all **sensory data**, which play so great a part in the perceptive intuitions of things (Husserl 1967, 120).

Even without the need for further analysis into these two Husserlian concepts, it is already quite obvious that there are both similarities and divergences between Bachelardianism and Husserlianism. As I said above, Bachelard's concept of "intersubjectivity" is a re-enactment of Husserlian intersubjectivity applied to science. In fact, he thinks with Husserl that there is a way in which subjects achieve consensus in the form of an "objective

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[86] For a good approach to Husserl's "intentionality" see H. Dreyfus, ed., Husserl, Intentionality and Cognitive Science (Cambridge, Massachusetts: MIT Press, 1982).

spatio-temporal fact-world." If we transpose this image to Bachelard's notion of consensus about theories within the scientific community, the general process of validation is basically the same.

The substantial difference between Husserlian phenomenology and Bachelard's scientific phenomenology lies in their consideration of the implications for knowledge of intentionality. For Husserl intentionality requires the consciousness of something that is pre-given. For Bachelard intentionality within science implies the constructive ability of scientific rationality, and thus the establishment of both theoretical entities (which may be reified by technique) and what counts as knowledge. Husserl does not account for the possibility of directing phenomenology towards the reflexive study of scientific entities which are virtually created by the "scientific consciousness." It is precisely at this point that phenomenology per se seems to be somewhat inadequate, and that Bachelard's concept of phenomenotechnique takes over. It is here too that we should finally consider the originality of Bachelardian phenomenology. The scientific phenomenology of Gaston Bachelard is truly the first explanatory model in philosophy of science to take into consideration the import of scientific technology or technique (whether mathematical or instrumental) in the construction of the sciences of the twentieth century. True, some scientist-philosophers (theoretical physicists mainly) were at about the same time speculating about the epistemological consequences of the new sciences, but only Bachelard constructed a set of concepts which truly represent the first integrative analysis of scientific practices in the history of philosophy of science.

Chapter 2. A New Philosophy for the New Sciences: The Copernican Revolution in Epistemology?

Freud's Interpretation of Dreams (1900), Rutherford's discovery of alpha and beta rays in radioactive atoms, Planck's Laws of Radiation (1901), Marie Curie's research on radioactive substances (1904), Einstein's formulation of the special theory of relativity in the Annalen der Physik (1905), the impact of Darwinism in biology, De Broglie's works on wave mechanics (1923), Heisenberg's creation of quantum mechanics (1924), were just some of the events which stimulated European intellectuals to consider new approaches to the construction and validation of scientific knowledge. These and other scientific breakthroughs produced Henri La Science et l'hypothèse (1902), Dilthey's Experience and Poetry (1905), Henri Bergson's L'Évolution créatrice (1907), Brunschvicg's Expérience humaine et causalité physique (1922), Meyerson's La Déduction relativiste (1925), to mention only a few examples. Bachelard summarized that dynamism in science in the following terms:

> We will set up the exact era of the new scientific spirit in 1905, at the time when Einsteinian relativity came to deform primordial concepts that were thought to be forever immobile. From then on, reason multiplies objections, it dissociates and relates fundamental notions, it rehearses the most audacious abstractions. Thoughts, of which just one would suffice to illustrate

> a century, show up in twenty five years, signals of an astounding spiritual maturity. This is the case of Louis de Broglie's quantum wave mechanics, Heisenberg's physics, Dirac's mechanics and soon, no doubt, the abstract physics that organizes all the possibilities of experience (Bachelard 1938, 7).

Undoubtedly, the indeterminacies that scientific discoveries suggested, particularly in theoretical physics, demonstrated the need for redefinitions of realism and new metaphysical attitudes in art, literature and, of course, philosophy. A reevaluation of traditional models of scientific explanation, heavily reliant upon what Gagey called "Laplacian certainties" (Gagey 1969, 38). For example, mechanical and deterministic models of explanation of the physical world, suddenly became imperative. Models such as the electromagnetic world-picture, as already represented for instance in Maxwell's equations, were substituting the mechanical one. This was partly the result of the influence of the positivist approach to science that had been advocated since the 1870s by people such as Ernst Mach (Miller 1986, xiv), who in his Science of Mechanics (1883) had criticized the "Newtonian absolute space and time as idle metaphysical conceptions" (Miller 1986, 378). In the physical sciences, relativity and quantum theory seemed to be the dominant factors that caused new epistemological concerns over the character of scientific knowledge and its relations to reality, at the same time that they became increasingly more alien to the popular empiricist school. The sometimes paradoxical conclusions produced by the new sciences regarding the limitations of knowledge forced both scientists and philosophers to choose between the classic version of concepts such as determinism and objectivity and the rejection of what was until then intrinsic to the philosophical principles of science.

The idea of determinism in classical physics was basically the belief in the **principle of causality**, which postulated, for instance, that every phenomenon is the total effect of a previous cause. Since the cause is conserved in the effect, every physical phenomenon can be predicted (Kojève 1990, 43).[87] More specifically, **strict** Newtonian causality asserted that, "given the precise position **and** velocity of a particle at a given time, and if [one] knows all the forces acting on it, then you can predict from Newton's laws the precise position and velocity of that particle at a later time" (Pais 1986, 15). The notion that the principle of causality is the possibility condition of science still held in the 1920s, with scientists such as

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[87] I highly recommend Kojève's L'Idée du déterminisme dans la physique classique et dans la physique moderne. The book deals with the concepts of determinism and causality and contextualizes them as concepts in the midst of other philosophical problems challenging the physical knowledge of reality; see also Ullmo's La Pensée scientifique moderne, chapters IV, V, and VI for a briefer account.

Planck and Moritz Schlick (Kojève 1990, 43-44). However, causal determinism -- which could easily be connected with continuism (Kojève 1990, 137) -- was challenged as early as 1900. Curiously, Planck was one of the instigators of this **Copernican revolution** in epistemology. In fact, his work on black body radiation introduced the hypothesis that "physical action has a discontinuous structure" (Kojève 1990, 138). As Kojève suggests, other scientists helped emphasize the same notion of the discontinuity of matter:

> Following the experiments of Millikan, Perrin, Rutherford and others, the hypothesis of the atomic structure of matter seems to have been raised to the level of definitely proved truth. The electronic theory (Lorentz, etc.) introduced the notion of discontinuity in the physics of electricity. With the photon hypothesis (Einstein) physics returns in some sense to the corpuscular theory of light (ibid., 139-140).

Although it is the triumph of twentieth century science to have found exceptions to the principle of causality as defended by classical physics, it may well have been the creativity allowed by nineteenth-century mathematics that set the stage for what was later to be considered a scientific revolution in physics.

It is possible to see the theories of general relativity and quantum mechanics as natural and applicable consequences of non-Euclidean geometries discovered by Gauss, Riemann, Lobachevski and Bolyai in the years of 1878-1880. Scientists such as Durhing, Renouvier, Dodson (Lewis

Carroll) and Frege saw these geometries as products of a pluralistic philosophy (Toth, 1983:263)[88] and, contrary to Brunschvicg's and Bachelard's analyses, as completely unacceptable. The reaction to the new geometries and the creation by Cantor (1845-1918) of the theory of groups, also stemmed from other factors connected with their anti-intuitive character: the apparent impossibility of accepting two opposed systems of propositions as simultaneously true, their lack of practical or scientific utility outside the realm of pure mathematics, and their anti-Kantian character. To question the foundations of mathematics and accept different axiomatics meant accepting not only that scientific knowledge is just an approximation of reality but also that the character of this approximation is historically, rather than epistemically, contingent. On the other hand, the rejection of a unitary view of science meant accepting what Julien Benda, referring to the philosophy of science of Bergson, Boutroux, Le Roy, Brunschvicg and Bachelard, called the doctrine of the permanent mobility of scientific thought (Benda 1950). The denial of unchangeable mental structures directly implied that both reason and knowledge could not to be considered in

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[88] Toth's article "La Révolution non Euclidienne," pp.241-265 in La Recherche en histoire des sciences (1983) offers a very good account of the historical and philosophical implications of the invention of nonEuclidean geometries.

absolute terms. Euclidean geometry should not be seen as the mathematical correlate of knowledge architectonically arranged in **a priori** structures. Instead, it became the geometry of everyday life, which could be apprehended intuitively, but which became clearly insufficient as the ideal mathematics for the new physical theories.

The new geometries systematically introduced a relativity factor into all mathematical operations thus enabling scientists such as Poincaré and philosophers such as Le Roy -- who later influenced some areas of Bachelard's epistemology -- to reaffirm the dependency of knowledge upon conventional axiomatics. Time, space and velocity, not to mention causality and determinism, as defined by Kant and omnipresent in both Euclidean geometry and Newtonian mechanics, had to be redefined, especially when Einstein and other theoretical physicists emphasized the non-Euclidean character of space/time. At the same time, the new discoveries provided by contemporary physics gave ontological status to these non-Euclidean geometries, and they appeared to reject Comtean positivism as a reasonable approach to scientific thought. Instead of traditional realism, a new form of idealism was much closer to the epistemological needs of the new sciences. The capacity for abstraction required by theoretical physics had already been recognized very early by Planck. But the shift in the

philosophical attitudes concerning science was not only a consequence of the new levels of abstraction provided by mathematics. This shift also corresponded to the introduction of scientific technology capable of producing the effects and entities postulated by theoretical physics. The interrelationships of these two factors led Bachelard to talk about concepts such as **technical materialism** and **phenomenotechnique**.

The necessity of a conceptual reconstruction in science and epistemology had then its roots at the turn of the nineteenth-century, when "theories involving particles and discontinuous change appeared in a number of fields which had been dominated previously by the conception of the continuity of matter and of change." (Mason 1962, 549) Although influential philosophers such as Henri Bergson tried to preserve continuism in the biological and the physical sciences, people like Bachelard stressed instead the importance of the concept of discontinuism as part of a renewed epistemological vocabulary for the new sciences like quantum physics, again directly connected with their anti-natural, strictly constructed, and paradoxical character. Whereas scientist-philosopher Émile Meyerson would argue that relativity was continuous with Newtonian mechanics, gaining with this and other claims the admiration of Einstein himself, to Bachelard the new sciences represented instead an all-encompassing structure of knowledge to which classical physics belonged as a particular case among equally pertinent cases.[89] Contradicting Einstein, to whom science was a refinement of everyday thinking, Bachelard argued instead that there is a radical rupture between empirical and scientific knowledge. He also argued that the new sciences -- including relativity -- had nothing in common with classical descriptions of reality. This contradicted Einstein's assumptions on the relationships between classical science and modern science. In fact, Einstein acknowledged scientists such as Newton, Maxwell, Mach, Planck and Lorentz as his precursors (Pais 1982, 13). For instance, referring to Lorentz's equations, he said to Abraham Pais that "without Lorentz he would never have been able to make the discovery of special relativity" (Pais 1982, 13). Indeed, the influence was more than strictly scientific. Einstein also believed that, even within relativity, physical reality was ruled by the principle of **strict** causality, which to him was only to be challenged by quantum theory. He said:

> It is only in the quantum theory that Newton's differential method becomes inadequate, and indeed strict causality fails us. But the last word has not yet been said. May the spirit of Newton's method give us the power to restore unity between physical reality

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[89] What Foucault, influenced by Bachelard, would later call **systems of thought**.

> and the profoundest characteristic of Newton's teaching -- strict causality (Einstein in Pais 1982, 15).

As pointed out earlier, philosophers were not the only intellectuals concerned with understanding the epistemological consequences of sciences such as relativity and quantum mechanics. Lucien Poincaré, for instance, noticed at the beginning of the century that scientists confronted epistemological problems raised by contemporary science as vigorously as the more traditional philosophers (Lalande 1907, 361). At about the same time, Abel Rey, one of the French representatives of the school of **scientific criticism** (Bochenski 1951, 102) and probably the philosopher who influenced Bachelard's concept of approximate knowledge and the notion that philosophy **is** philosophy of science, said that a considerable number of physicists were doing philosophy of physics, i.e., reflecting on science's problems, methods, and future (Rey 1907, iii). The conventionalist Henri Poincaré (1853-1912), author of La Science et l'hypothèse (1902) was one of them. The empirico-criticist Mach (1838-1916), with works such as The Science of Mechanics (1883) and Knowledge and Error (1905) and Duhem (1853-1916) in books such as L'Évolution de la méchanique (1903) and The Aim and Structure of Physical Theory (1904) also belonged to the same category.

The Scientific Construction of Philosophical Knowledge

Until the end of the nineteenth century, "the prevailing scientific world picture was formed by mechanical hypotheses" (Jungnickel and McCormick 1986, 212). Interestingly, most of the works mentioned above represent a criticism of mechanical explanations of physical phenomena. Although working within the continuist paradigm, Max Planck distinguished the laws of thermodynamics, which should be rooted in our universal experience of nature, from its mechanical view (Jungnickel and McCormick 1986, 215). Mach considered that "a mechanical theory of matter is historically intelligible, but nonetheless an artificial conception" just like mathematics is only a functional tool and atoms only artificial conventions instead of "realities behind phenomena" (Jungnickel and McCormick 1986, 212). By the same token, Henri Poincaré, whose epistemology was rooted in inductivism (Miller 1986, 56) did not consider scientific theories as representations of the world, but rather as useful and economical conventions. Science is **not** about **prediction**, but about explanation. The purpose of science is to establish **relations** among things. The study of the relations between objects in the real world is embodied in the axioms of geometry. As he pointed out in <u>Science and Hypothesis</u>:

> The geometrical axioms are therefore neither synthetic **a priori** intuitions nor experimental facts. They are conventions. Our choice among all possible conventions is guided by experimental facts; but it remains free, and is only limited by the necessity of avoiding every contradiction, and thus it is that postulates may remain rigorously true even when the experimental laws which have determined their adoption are only approximate (Poincaré in Miller 1986, 57).

It is interesting that, despite the fact that Bachelard was influenced by Poincaré, he never completely accepted the view that the construction of theoretical entities did not entail **any** correspondence between those entities and reality. Although it is true that he was ambiguous at some points in his work when referring to the ontological value of scientific entities as postulated by theoretical physics, by claiming, for instance, that some entities are products of a particular stage in science's development, Bachelard held that these entities **do exist out there**, even if what he means by **out there** is just the context of the laboratory. His indecision on the issue of realism versus anti-realism with respect to theoretical entities was probably the result of what Arthur Fine called, in the context of the philosophical disputes originated by quantum theory, the "schizophrenia over realism" (Fine 1986, 151).

Nevertheless, Bachelard's epistemology is in many respects similar to that of Henri Poincaré. It is of an approximative and inductivist nature. To him, mathematics is a fundamental tool that helps determine the relations between scientific objects. The creative power attributed to mathematics by Bachelard also goes beyond the capacity to construct abstract representations of reality. Mathematical physics has an ontology of its own, one that allows for the creation of scientific objects themselves. However, Bachelard differs from Poincaré in terms of two intertwined notions original to his philosophy of science: a) the **historical** rather than strictly conventional character of scientific theory choice; and b) the **dialectics** between scientific knowledge and scientific technology, which, to a large extent, determine the historical character of scientific entities. Also, Bachelard did not seem to have adopted Poincaré's skeptical attitude towards relativity as a universal principle. Poincaré's skepticism was due to his interest in the electromagnetic world picture (Miller 1986, 30). It is not difficult to argue that these are also some of the epistemic reasons that led Bachelard to reject traditional philosophies such as realism and positivism as the metaphysics of contemporary science. He believed that the possibilities opened up to theoretical physics by logical mathematics were incommensurable with the requirements of absolute experimental certainty as defended by the French positivists.

In 1905, the Annalen der Physik published Einstein's article "On the electrodynamics of moving bodies." This was also the year in which light quanta entered physical theory (Jungnickel and McCormick 1986, 183). Although Bachelard sees 1905 as a landmark for philosophy of science, the acceptance of both relativity and quantum theory by the scientific community was far from immediate. Neither special relativity nor quantum theory -- which had been in the air at least since the works of Planck on black body radiation in 1900 and, in the case of relativity, since the article by Poincaré, "Sur la dynamique de l'éléctron" (1905) (Miller 1986, 30) -- were received by the scientific community as theories which would open up a new era for physics (Pais 1982, 27). In the case of special relativity, for instance, both Lorentz and Poincaré "had difficulty recognizing that this was a new theory of kinematic principle rather than a constructive dynamic theory" (Pais 1982, 27). Interestingly enough, at least by way of corroborating today's prevailing view of the existence of an Einsteinian revolution, the demarcation between the views of Lorentz and Einstein only came to the scientific scene after Ehrenfest's foundational analysis of a rigid body (Miller 1986, 238). In Germany, despite the positive attitude towards relativity by major scientists such as Planck, the special theory was only "fairly widely accepted by 1910 or

1911" (Jungnickel and McCormick 1986, 247-248). It was only after 1919 that Einstein became popular outside the German-speaking countries (Pais 1982, 308). This popularity might be explained through the interest of English scientists in the phenomenon of the bending of light. According to Pais, this interest "developed soon after copies of Einsteins' general relativity papers were sent from Holland by de Sitter to Arthur Stanley Eddington at Cambridge (presumably, these were the first papers on the theory to reach England)" (Pais 1982, 304). Experimental verification of the deflection of light as predicted by Einstein, (by an expedition sent to the Portuguese island of Príncipe and elsewhere), definitely showed, even to the skeptics, that Einstein's gravitational law should be taken seriously.

Still, in countries like France, the philosophical implications of the new sciences, i.e., the decline of absolute causality, were not easily accepted by academia. As late as 1922, French philosophical circles were still confused by the implications of relativity, as can be seen by works such as Bergson's Durée et simultanéité: à propos de la théorie d'Einstein. As already pointed out, Henri Poincaré never accepted Einstein's formulation. Bergson, Brunschivcg, and Bachelard were perhaps the only French philosophers of science to acknowledge that relativity implied a redefinition of the strictly Comtean, and later

Machean, sense of scientific objectivity. Bachelard's attitude towards some of the philosophical implications of relativity coincided with Einstein's later demarcation between Mach's positivism and his own particular brand of scientific realism. With relativity, objectivity was no longer the correspondence between theories and physical phenomena, but the "equivalent to **invariance** of physical laws" (Margenau 1949, 253). In 1933, Einstein would say the following:

> The natural philosophers of those days [18th and 19th centuries] were . . . most of them possessed with the idea that the fundamental concepts and postulates of physics were not in the logical sense free inventions of the human mind but could be deduced from experience by 'abstraction' -- that is to say by logical means. A clear recognition of the erroneousness of this notion really only came with the general theory of relativity, . . . the fictitious character of the fundamental principles is perfectly evident from the fact that we can point to two essentially different principles, both of which correspond with experience to a large extent. . . (Einstein in Frank 1949, 273).

The reasons that led part of the French scientific community to receive relativity with large doses of skepticism were deeper than the general reasons pointed out above and which, in any case, were felt by the great majority of physicists everywhere, particularly before 1916.[90] Those reasons were also of a political and

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[90] For a developed study of the reception of Relativity in Europe and elsewhere, see Thomas Glick, ed., The Comparative Reception of Relativity (Dordrecht: D. Reidel Publishing Company, 1987). For the impact of Relativity in

cultural order. In fact, the cultural background provided by the **École Républicaine** had much more to do with applied science and traditional modes of thought than with the new philosophical and scientific trends coming from Germany. One of the problems with the acceptance of relativity in France was, for instance, the question of Einstein's nationality and the polemics concerning the compatibility of Einstein's theory with the French **esprit de finesse**. It is perhaps useful in this context to point out that, regardless of Einstein's pacifist activism, he was nevertheless "claimed by the German establishment as one of their most prominent members" (Pais 1982, 315). On the other hand, and now in a more scientific context, the strong influence on science and philosophy of science of people such as Duhem and Henri Poincaré, not to mention Bergson, might have also contributed to the slow incorporation of relativity in philosophical and scientific circles -- which to a large extent were basically the same.

Curiously, in 1922, Poincaré's former student Paul Langevin tried to propagate relativity, arguing how "it found its support in Henri Poincaré's ideas" (Biezunski 1983, 274). Langevin's enthusiasm for the coincidence he found between Einstein's theory and his own works already

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France, see Michel Paty, "The Scientific reception of relativity in France," pp. 113-167 in Glick.

had led him to lecture on the principles of special relativity in his courses at the Collège de France since 1906. Nevertheless, between 1919 and 1921, and despite the enormous success of Einstein in other countries, the French Academy of Sciences was still divided between the partisans of the scientific value of relativity and those who preferred the notion that "all principal laws of science have [already] been discovered" (Biezunski 1983, 272). Again, the visit of Einstein to France in 1922, following Langevin's invitation, raised the issue of Einstein's incompatibility with the French spirit:

> In fact, the silent majority of French scientists . . . has not really been persuaded by the visit of Einstein of the importance of relativity to physics. This theory was only introduced in the teaching curricula in a belated and slow way, since it was necessary to become insurmountable by the nuclear physicists of the 1930s . . . and this time [only] because its formulas had become operational (Biezunski 1983, 291).

Thus, the slow acceptance of relativity in France was more than the consequence of some sort of cautious scientific skepticism. It also included nationalistic reasons.[91] The distinction, made by Pascal and used by Duhem in his The Aim and Structure of Physical Theory (1906), between the **esprit de finesse** and the **esprit**

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[91] For a nicely written account of the public and academic reaction to the visit of Einstein to Paris, see the complete article of Michel Biezunski "Einstein à Paris", pp. 267-293 in La Recherche en histoire des sciences (1983).

géométrique naturally led to the perception that there was a clear distinction between **French science** and **German science**. Duhem was even more forceful in claiming that there is an inborn difference between these two ways of thinking in his last book, La Science allemande (1915). There, he accuses German scientists of being obsessed with detail and thus losing sight of the general picture (Duhem 1915, 85), and with not worrying about the damage created in the physical sciences by the disproportionate use of mathematical models. In fact, the geometric mind strives to reduce sciences such as mechanics, geometry,and physics to algebra (Duhem 1915, 14). Furthermore, German scientists "have this excessive confidence towards deductive reasoning, they distrust intuition which comes from common sense" (Duhem 1915, 20). The disregard for common sense, which appears to be one of the main characteristics of the French **esprit de finesse**, appears as the main culprit of the German's tendency towards both skepticism and idealism. Duhem then extends the criticism of German thought to German history which, according to him, distorts reality to fit theoretical corollaries instead of the other way around (Duhem 1915, 60).

Anti-German propaganda was also the main purpose of G. Papillant's Science française et science allemande (1915). The plan of the book is an organized attack on the methodologies of German science and its contrast with the laborious, richly conceptual, causality-oriented French scientific works. The Germans are portrayed as preferring vague abstractions, of "making explanatory entities out of conceptions imperfectly understood, hypothesizing actions and relations, etc." (Lalande 1918, 456). There are other things to take into consideration regarding skepticism of French academia towards the products of German theoretical physics. For instance, at the time that Einstein visited France in 1922, the country was still dealing with post war reconstruction. Not only was there an understandable animosity towards the Germans, but the interests of the French government in science at that time were much more pragmatically oriented. The philosophy of the Third Republic (1870-1940) included, among other things, the stabilization of the French economy, the development of industry, the emphasis on research and applied science, and the construction of modern technology.

As already noted, it was the negative attitude towards German science demonstrated by a large proportion of French academia, that led to the rejection of relativity by influential scientist-philosophers such as Duhem (1861-1912). Duhem refused to accept this theory, an obvious product of the **geometric mind**. He saw relativity as an unacceptable discontinuity with the old Newtonian and Kantian categories of absolute time and space.

However, to say that the reception of relativity in France was purely negative does not really present the whole picture. Oblivious to both the pragmatic requirements of the Third Republic and to the skepticism with which relativity had been generally received, there was also a representative circle of French scientists and philosophers who received relativity with enthusiasm. Curiously, the philosophical interpretations of relativity among the scientists who accepted it tended to be quite diverse, if not totally opposed. The cases of Meyerson, on the one hand, and of Brunschvicg and Bachelard, on the other, are interesting in this respect.

Émile Meyerson and the Search for Identity in Nature

Émile Meyerson (1859-1933) exemplified the scientist who accepted relativity, but only on the condition that it had to be continuous with classical physics.[92] This philosophical position had the advantage of safeguarding causality and determinism in physics. More importantly, it corresponded to a large extent to Einstein's epistemological

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[92] For a synthesis of Meyerson's philosophy see, for instance, L. Brunschvicg, "La Philosophie d'Émile Meyerson," Revue de métaphysique et de morale 33(1926):39-63.

position. In an appendix to the English edition of Meyerson's La Déduction relativiste (1st ed. 1925), Einstein said that "this book was written by a man who has grasped the pathways of thought of modern physics and who has penetrated deep into the history of philosophy and the exact sciences with the sure eye for psychological motives and interrelationships" (Meyerson 1985, 252). Actually, Meyerson and Einstein contacted each other several times, especially on scientific issues. At the meeting that took place at the **Société française de philosophie** on the occasion of Einstein's visit to Paris gathered many eminent French scientists around him. There were physicists such as Paul Langevin, Jean Perrin, and Paul Painlevé (Minister of Education during World War I) and mathematicians Jacques Hadamard and Elie Cartan. Meyerson was at this meeting, too, along with other influential philosophers: Henri Bergson, Léon Brunschivcg, and Édouard Le Roy (Pais 1982, 163). Apparently, one of the topics of discussion had to do with Einstein's criticism of Mach's phenomenalism, which had led the latter to reject "the existence of atoms despite the overwhelming experimental evidence for it" (Meyerson 1985, 252). On the other hand, Meyerson was interested in knowing Einstein's attitude towards the spatializing interpretation of relativistic space as presented by Minkowski (Meyerson 1985, xxxviii). These and other issues were published later in Meyerson's La Déduction relativiste, written a few years after the encounter with Einstein. This book was an attempt to develop the epistemological consequences of relativity, by maintaining, among other things, Newtonian causality, the philosophical position that reason is immutable, and the notion that one of its most important epistemic operations is deduction.[93] Most of the epistemological positions taken by Meyerson in La Déduction relativiste were restatements of issues already present in Meyerson's first writings in the philosophy of science. The explanatory and deductive power of science had already been expressed in works such as De l'Explication dans les sciences (1921) and Identité et réalité (1908). To Meyerson, science is the search for identities in nature, and it is the rationality embedded in all physical phenomena which allow scientists to build accurate representations of reality. As he says in Identity and Reality:

> Comte and Mach affirm that all science is established only for the purpose of action and prevision. We prove that the principle thus put into play, the principle of lawfulness (**legalité**) is not enough, that science attempts equally to **explain** phenomena, and that this explanation consists in the identification of the antecedent and the consequent. It is from this principle, the principle of **scientific causality**, that the atomic theories are derived (Meyerson 1930, 9-10).

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[93] For similarities between Kantianism and the critical study of reason by Meyerson see J. Loewenberg, "Meyerson's critique of pure reason," The Philosophical Review XLI.4(1932):351-367.

On the other hand, scientific knowledge is continuous with common sense because both are the result of similar thinking processes. In fact, "the scientists of the beginning of the twentieth century continue to build up atomic theories just as their predecessors have done" (Meyerson 1930, 391). The continuity thesis, together with mechanism, may be seen as characteristic of models of scientific explanation until well into the nineteenth century, is still present within Meyerson's philosophy of science, **despite** the new attitudes towards theory change opened up by relativity. As stated above, Einstein himself seemed to have endorsed continuism. The fact that Meyerson already defended the continuity thesis **before** relativity started to have a significant impact on physics, as well as the influence on him of scientists such as Pierre Duhem and Henri Bergson might help explain this fixation on continuism. Interestingly enough, although aware of Bachelard's works, de Broglie seemed to side with Meyerson on the issue of relativity, and with both Meyerson and Bergson on the issue of continuity.

Although a continuist and a mechanist, Meyerson was well aware not only of the artificial character of scientific theories but also of the implications, for research, of scientists' reliance on traditional systems of explanation. The natural tendency to use the principle of causality to find analogies between "the order of ideas and the order of things" (Meyerson 1930, 403) misleads the scientist, if she relies only on the principle itself:

> what is really **a priori** in science is in the first place the series of postulates which we need for empirical science -- that is, in order to be able to formulate this proposition: nature is ordered and we can know its course. Nevertheless, this rigorously empirical science is an **artificial** creation, and **science** is not exclusively empirical; it is also the application to nature, in successive phases, of the principle of identity, the essence of our understanding (Meyerson 1930, 402).

Since there is no complete agreement between thought and nature (Meyerson 1930, 407), science is constantly being perfected. There is thus some sort of teleology embedded in Meyerson's picture of science which also shows up in Bachelard's epistemology, particularly in his concepts of **approximation** and **phenomenotechnique**. As we will see later on, Bachelard considers Meyerson as the main representative of a basically outmoded philosophy of science. However, the exposure to Meyerson certainly left its marks on Bachelard's more general attitudes towards science: an almost unconditional admiration for the theoretical achievements of science, an internalist attitude, the notion that previous models of explanation and theories may constitute sources of scientific error, and need somehow to be exorcised by reason. It is also curious that these attitudes do not differ much from the positivist attitudes, although Meyerson seems to be much more aware of this philosophy of science than was Bachelard. In fact, Meyerson spends a lot of time trying to explain why is it that, although correct in some aspects, philosophies such as positivism are insufficient to account for the works of contemporary science. As he pointed out in 1916:

> A science that would correspond to the positivistic ideal, that would [thus] be really phenomenalist, by applying [so much of itself] to find relations between sensations, would not be able in any way to dedicate itself to the creation of new **things** (Meyerson 1916a, 210).

Still, according to Meyerson, it is not correct to see mathematics as more than a set of procedures that are "products of historical development" (Meyerson 1916a, 205). As an example, he tries to show that geometry is embedded in substantialism and that, as Poincaré argued, geometry must be seen as a representation of physical reality rather than as its correspondent (Meyerson 1916a, 210). In fact, and again following Meyerson, one of the reasons why science does **not** conform to the positivist's dream is that science is ontological. In other words, it needs the concept of **thing** as much as common sense knowledge does (Meyerson 1916a, 211). On the other hand, theories such as mechanism and atomism do not have their origin in science. The atomism of Democritus, for example, started out as a **philosophical**, not a scientific attitude. Systems like these are prior to scientific thought. At the same time they are responsible for the tendency that science has to construct and validate similar models of explanation (Meyerson 1916a, 215). There are also doctrines which seem to be strictly scientific. Energetism is one such case. According to Meyerson, energetism introduces discontinuity in nature. But to him, the acknowledgement of definitive discontinuities such as the decomposition of things into atoms and molecules (Meyerson 1916a, 219-20) does **not** imply that philosophy of science should become discontinuist.

In De l'Explication dans les sciences, Meyerson is also concerned with criticizing positivism in science, and with explaining the functioning of scientific reason (Meyerson 1921, vii) and the basic features of scientific knowledge in its relation to physical reality. Again, scientific explanation is seen as the attempt to construct **deductive** mathematical systems. Meyerson is convinced that it is possible to determine how reason in all domains works if one looks at how science is rationally constructed (Meyerson 1921, viii).[94] Adopting John Stuart Mill's position, Meyerson says that the positivists' renunciation of the search for causes, which is taken to be metaphysical in

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[94] See also Meyerson, "La Science et les systèmes philosophiques," Revue de métaphysique et de morale XXIII.1 (1916): 203-242.

nature, has as its substitute a focus on **phenomena** alone, for example **sensations**, and their pure non-ontological relationships (Meyerson 1921, 19). Following the histologist Nagotte, Meyerson argued that this sort of ontological detachment did not represent what happens in scientific observation, since scientists have the tendency even to **forget** that "the microscopic images are not at the same scale of the objects which surround them" (Meyerson 1921, 25). Thus, the ontological attitude towards physical objects does not change in the passage from common sense to science. Science is constrained to **create** new **things** (Meyerson 1921, 29). But the fact that these new objects destroy the ontology postulated by common sense does not mean that **ontology** itself is also destroyed (Meyerson 1921, 30). In fact, all sciences presuppose an ontology. These and other assertions led Meyerson to advocate a sort of scientific realism which is quite similar to that of Einstein when the latter wrote around 1949:

> Being is always something which is mentally constructed by us, that is, something which we freely posit (in the logical sense). The justification of such constructs does not lie in their derivation from what is given by the senses. Such type of derivation (in the sense of logical deducibility) is nowhere to be had, not even in the domain of pre-scientific thinking. The justification for the constructs, which represent 'reality' for us, lies alone in their quality of making intelligible was is sensorially given (Einstein 1949a, 669).

Curiously, an analysis of some of the most important features of Meyerson's philosophy of science shows that Bachelard sided with Meyerson in his critique of positivism in science. From the claim that science cannot be reduced to a system of observational sentences, Bachelard concluded that positivism is incapable of an objective, unbiased analysis of scientists' activities. He also agrees with Meyerson on the issue of the historicity of mathematics and, as we saw above, the notion that certain scientific principles may constitute what Bachelard called **epistemological obstacles** to the progress of scientific thought. He also seemed to agree with both Einstein and Meyerson on the issue of what the latter called "chosisme" in science. Thus, regardless of what Bachelard's emulators say about his personification of Meyerson as the representative of a conservative, inadequate, philosophy of science, we must be aware that both Meyerson and Bachelard use much of the same criteria to construct their methodologies of science. Obviously, it is important to recognize that there are also fundamental differences between Meyerson and Bachelard. As we already saw, Bachelard sees 1905 as the year of the scientific revolutions that demanded a differentiated epistemology. Although we can criticize Bachelard on the grounds that special relativity is an evolutionary, not revolutionary, outcome of nineteenth century physics and mathematics (non-Euclidean geometries, Poincaré's mathematical physics, and Lorentz's equations, for instance), it is certainly true that the discovery of the absolute speed of light, overthrew some of the most cherished classical scientific paradigms and opened the way for the extraordinary development of theoretical physics. Whether one should talk of continuity or discontinuity of systems becomes almost irrelevant in the case of both special and general relativity. Einstein was correct when he says that the philosophical problem of the overthrow of strict Newtonian causality only becomes relevant in the realm of quantum theory. Only then do issues such as the denial of causality and determinism announce that the transition from classical to modern physics is complete.

It is crucial in this context to acknowledge the fundamental role of Einstein in the creation of quantum theory. As Bohr once said:

> Important progress in the development of quantum theory was made by Einstein himself in his famous article on radioactive equilibrium in 1917, when he showed that Planck's law of thermal radiation could be simply deduced from assumptions conforming with the basic ideas of the quantum theory of atomic constitution (Bohr 1949a, 204).

Einstein's contributions to quantum theory started in 1909 with his analysis of fluctuation in black body radiation. This led him to become "the first to state, long before the discovery of quantum mechanics, that the theory of the future ought to be based on a dual description in terms of particles and waves" (Pais 1982, 20). Similarly, Planck's work had already by then shown that there was something wrong with the foundations of classical physics (Pais 1982, 27). Einstein's observation, in 1916, of "a lack of Newtonian causality in spontaneous emission" (Pais 1982, 20) made him realize that causality was being questioned, at least temporarily, at the level of microphysics. Thus, if we are to talk about discontinuities and revolutions in science, it is probably more correct to connect them with Einsteinian general relativity, which apparently did **not** have any scientific ancestors, or with quantum theory as developed by Einstein and later by Bohr, which represented a clear challenge to deterministic worldviews.

Although Meyerson was aware of the nature and implications of relativity, microphysics was a somewhat different issue. His reaction to quantum theory was curious, because even then he tried to safeguard determinism and "chosisme" in science (Meyerson, 1933a). He did it with almost the same eagerness that led Einstein to affirm that the construction of a unified field theory should be possible. Meyerson's **realist** doctrine was to be somewhat weakened by the dominance of the "Copenhagen Interpretation" of quantum mechanics, which prohibits the traditional demarcation between the scientific object, scientific technology, and the observer. He refused to admit that the mathematics involved in quantum theory implied the annihilation of what for him was part of the scientist's natural quest for identification and objectification. Thus, he claimed, for instance in Du Cheminement de la pensée, that quantum theory did not represent the triumph of subjectivism:

> When the scientist makes an observation, he always considers that some other scientist, by looking at the same phenomenon, will make an analogous observation. In doing this, he manifestly supposes that outside himself and the other's self there is something objective that is independent of the two selves (Meyerson 1931, 70).

A. Metz, who wanted to justify Meyerson's refusal of the indeterminist model ended up saying that quantum theory does not really deny determinism. Instead, it requires a new definition of indeterminism. For instance, "the wave-corpuscle duality is something inexplicable, **irrational** in Meyerson's sense" (Metz 1949, 151).

Bachelard was aware of the philosophical implications of quantum theory, which basically led him to create the concept of **phenomenotechnique**. But he does not seem to be aware of some important aspects of the scientific context which we are in the process of analyzing. Since the overall purpose of Bachelard's scientific philosophy is to find discontinuities and epistemological breaks in the process of science formation, he considers that, although doctrines such as the atomism of Democritus are **prior** to science, modern atomism is **already** in sharp **discontinuity** with such doctrines. On the issue of relativity, I am uncertain as to whether Bachelard disregarded Einstein's epistemological positions or simply was not aware of them. It is certainly true that he frequently stressed the need for philosophers to go to school with scientists. But at the same time he made clear that very often scientists are not aware of the philosophical implications of their own discoveries. He neglected Einstein's views on relativity altogether, as much as he disregarded de Broglie's continuist thesis and return to deterministic models of explanation. In this context, I argue that this kind of neglect has to do with the influence of the philosophy of Brunschivcg (1869-1944).

Léon Brunschvicg's Discontinuist Idealism

Between 1920 and 1937, Brunschvicg's idealism was extremely influential in French thought, almost as much as the anti-intellectualist philosophy of Bergson (Bochenski 1951, 78). Brunschvicg was probably one of the first philosophers to defend the discontinuist thesis.[95] Not only did he apply this thesis to scientific thought but he also directed it toward human thinking as it developed over the ages. In Les Ages de l'intelligence (1947)(Winter lectures at the Sorbonne during 1932-1933), for instance, he tried to establish the stages of thought maturation from primitive mentality -- using anthropological data provided by Lévy-Bruhl's research and the works of the epistemologist-psychologist Jean Piaget -- to the present, as revealed for instance in the sophistication of mathematical thinking. In Brunschivcg's terms, error in science can only be recognized and analyzed retrospectively since the present state of knowledge represents a "superior state in the progress of human reason" (Brunschvicg 1947, 11). This is why he said that we understand Newton better after Einstein's work. Bachelard's presentism is similar to that of Brunschvicg. He, too, thinks that the analysis of the development of science enables us to study the historical development of reason, and that the rational stage achieved by modern science is proof of the assimilation of previous systems of thought into discontinuous dynamic anti-Kantian structures.

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[95] For a basic account of Brunschvicg's philosophy see, for instance, Raymond Aron, "La Philosophie de Léon Brunschvicg," Revue de métaphysique et de morale 50(1945):127-140.

Most commentators on Bachelard's epistemology correctly associated some of its themes with the philosophy of science of Brunschvicg. Although Brunschivcg was very much concerned with systematic approaches to the history of philosophy in works such as La Modalité du jugement (1897) and Le Progrès de la conscience dans la philosophie occidentale (1927), he, too, was interested in the philosophical implications of the development of sciences such as physics and mathematics. His Les Étapes de la philosophie mathématique (1912) and L'Expérience humaine et causalité physique (1922) are studies in the philosophy of science and deal with many issues that Bachelard later developed in his own works. Some of Bachelard's general philosophical points of view, too, are similar to those of Brunschvicg: the criticism of French positivism in general, the idea that Meyerson's continuist philosophy of science is not adequate to account for the recent developments in theoretical physics, the notion that mathematics represents the "highest degree to be attained by human thought" (Brunschvicg in Bochenski 1951, 79), that psychologism is the wrong approach to the analysis of the scientific mind.[96]

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[96] For a comparison between Brunschvicg's and Bachelard's epistemologies, see François Dagognet, "Brunschvicg et Bachelard," Revue de métaphysique et de morale 70(1965):43-54.

In <u>Les Étapes de la philosophie mathématique</u> (1912), Brunschvicg already recognizes that scientific facts "are conditioned by theories" (Brunschvicg 1912, 453), and that we must also take into consideration the limitations imposed on the scientist by the technical conditions of the experiment (Brunschvicg 1912, 518). These thoughts were further developed in <u>L'Expérience humaine et la causalité physique</u> (1922), particularly in their connection with relativity. In fact, Brunschvicg saw twentieth century as living proof that the principles of scientific thinking are not fixed. Also, it is with relativity that our representational systems suffer a radical overturn (Brunschvicg 1922, 404). This overturn leads him to compare the Einsteinian revolution with the revolutions provoked by Copernicus, Kant and Rey (on the study of elliptical functions) (Brunschvicg 1922, 404). According to Brunschvicg, the special theory of relativity shows that measuring instruments do not give us absolute results since they are not independent from what they are supposed to measure. Furthermore, the radical difference between Newton and Einstein is that "with Newton the thing to measure possesses an absolute content, which is in fact situated outside the [sphere of direct interference from] man" (Brunschvicg 1922, 407). For Einstein, on the contrary, there is "reciprocity between the conditions of measurement and the reality which is measured" (Brunschvicg 1922, 407).

It is the power of both mathematical analyses and experimental technique that allow for the progress of scientific thought. Using in this context Duhem's thesis of the impossibility of an **experimentum crucis**, Brunschvicg says:

> What one generally takes as the detailed and precise observation of phenomena, of certain facts, are . . . coincidences which are revealed through the measuring apparatus . . . . These coincidences [signify nothing] outside [our] interpretation of them, which supposes the set of physical theories accepted by the observer . . . . . Facts cannot be taken as givens imposed from outside . . . facts are **fabricated**, undoubtedly with the collaboration of nature, but by means of an artificial intervention of the scientist which characterizes them as scientific facts . . .(Brunschvicg 1922, 445).

In "La Relation entre le mathématique et le physique" (1923), Brunschvicg analyses in further detail the role of mathematical physics in science. He concludes that this role is basically the expression of the unity that can be found, in contemporary science, between mathematical theory and physical application (Brunschvicg 1923a, 355). The intellectual habits inherited from immediate representation and Newtonian mechanics have to be replaced by the "numerical world that relativity theory invites us to contemplate, a four dimensional non-Euclidean multiplicity" (Brunschvicg 1923a, 357). On the other hand, technological inventions such as the microscope and the spectroscope pose new challenges for the problem of objectivity in science (Brunschvicg 1923a, 359). The root of the problem lies in the direct **liaison** between what is human and what is objective. In other words, scientific objectivity cannot be separated from the human component of the scientific process.

In his lectures of 1932-1933, Brunschivcg develops the issue of the relationships between the subjective and the objective in science in connection with relativity. Referring for instance to Einstein's interpretation of Lorentz's equations he said:

> [It becomes obvious] that man cannot obtain real ('véritable') knowledge of the universe without having reflected about his position of **subject** in relation with the **object**. This reflection is no longer a simple metaphysical view, which would not have any repercussion on the scientific treatment of the problem (Brunschvicg 1947, 120).

Brunschvicg also takes into consideration the philosophical impact of the scientific possibilities allowed by the degree of precision attained by experimental techniques in contemporary science. Interestingly enough, he sees in the new relations between the subject (which includes the subject's use of instrumentation) and the object (at the level of microphysics) the descent of "idealism from the philosophical to the scientific plan" (Brunschvicg 1947, 121). The return to idealism in Brunschvicg's philosophy of science, which is in broader terms a criticism of materialist interpretations of science, has to do with what he saw to be one of the most fundamental consequences of Heisenberg's uncertainty principle. In fact, the uncertainty and indeterminacy embedded in the methodology of theoretical physics imply that objectivity in science cannot be demarcated from subjectivity. In other words, that at the atomic level, objective and subjective interfere with one another in a way that is "at the same time inevitable and inextricable" (Brunschvicg 1947, 123).

It is important to realize that some of these conceptions are close to what Bachelard said about the new epistemological profiles of the new sciences. In fact, the reasons why Bachelard considers relativity to be sharply discontinuous with Newtonianism, for example, are Brunschvicg's reasons. Bachelard acknowledges that Brunschvicg had said already that relativity is a "sort of more sensitive physics that makes theory and experiment [become] more solidary" (Bachelard 1945a, 80). In fact, there are profound dialectics, within scientific work, between "experimental information and rational information" (Bachelard 1945a, 81). In actuality, the idealism suggested by Brunschvicg was only taken to its ultimate consequences in the scientific philosophy of Bachelard, particularly through the concept of phenomenotechnique. Although it is true that a lot of philosophical premises concerning the physical sciences had been established by both Meyerson and Brunschvicg (e.g., the fabrication of scientific facts), Bachelard was definitely the first to connect the historical and sociological contingency of scientific practices (which had started with people like Auguste Comte) with both the creation of entities by science and the normative role of the scientific community. In doing so, he set the stage for a new vocabulary concerning the relationships between scientific knowledge and scientific technology, a view which previously had not been articulated by philosophers of science. However, by restricting himself to the criticism of traditional philosophies of science and their outmoded way of studying science, he ends up disregarding a great number of philosophical controversies that were largely left to scientists themselves.

Bachelard neglects an important segment of the deep philosophical conflicts that were part of the scientific community of his time, and seems to be satisfied with just proving that there is no functional link between Newtonian mechanics and relativity. I think that, without realizing it, Bachelard, like Brunschvicg before him, **extended** some of the philosophical consequences of quantum theory **to** relativity. His search for the similarity in the philosophical consequences of relativity and quantum theory, i.e., the consideration that both are examples of the discontinuities that shape the progress of science, led him to overlook crucial features of the ongoing process of the acceptance of quantum theory as postulated by Werner Heisenberg and Niels Bohr. I refer here to the epistemological conflict between Einstein and Bohr, which, at least in Einstein's terms, expressed the fundamental philosophical incommensurability between relativity and quantum theory. At this point, Bachelard's assessment is largely reductionist. True, this reductionism does not really alter the importance of his epistemological concepts. However, at least in historical terms, lapses such as these are far from negligible. I will return to this epistemological conflict. For now, and since Bachelard's concept of phenomenotechnique is, among other cultural reasons, what I think to be the direct derivation of some of the most important scientific premises behind the theory, a brief description of some of the scientific and philosophical implications of quantum theory will be the focus of the next paragraphs.

The Epistemological Consequences of Quantum Theory

Despite the contributions of Max Planck, with his **quantum of action** (Bohr 1929a, 92; Heisenberg 1958, 30; Bernstein 1973, 191; Hermann 1971, 2) and Einstein, to the development of quantum theory, the scientists who are usually associated with it are Niels Bohr and Werner Heisenberg. In the texts concerning the interpretation of quantum theory by the Copenhagen school, these scientists acknowledged its peculiar character in the following terms:

> [the quantum theory] imposes a rupture with the modes of thinking associated with classical physics, representation habits, categories of understanding, certain logical laws and the relation between words and things (Chevalley 1989, 157).

It is true that, from the very start, the scientific and philosophical novelties brought about by quantum theory had no clear historical or philosophical antecedents. If it was still reasonable to claim with Einstein that relativity did not challenge determinism -- and this philosophical stand was accepted by Bohr himself (Bohr 1958a, 2) -- with quantum theory the case was much different. So different that, according to people such as LeRoy it ultimately contributed to the "dissolution of the old epistemological categories" (LeRoy 1935a, 152). He could not have been more accurate. In fact, Heisenberg's uncertainty principle, one of the most characteristic aspects of the methodology of quantum theory (Bernstein 1973, 193) gave "the precise limitation to measurements on the atomic scale" (Bernstein 1973, 193). At this level of scientific observation it is no longer possible to talk in terms of causality, but instead in terms of **probability of occurrence**. In other words, the observer can never make exact predictions or discover the exact causal chains between atomic phenomena, as the theory implies the "impossibility of any sharp demarcation between the behavior of atomic objects and the interaction with the measuring instruments which serve to define the conditions under which the phenomena appear" (Bohr 1949a, 209). In the framework of classical physics it was not necessary to take this demarcation into account. As Bohr pointed out:

> the quantal features of the phenomenon are revealed in the information about the atomic objects derived from the observations. While, within the scope of classical physics, the interaction between object and apparatus can be neglected or, if necessary, compensated for, in quantum physics the interaction thus forms an inseparable part of the phenomenon. Accordingly, the unambiguous account of proper quantum phenomena must, in principle, include a description of all relevant features of the experimental arrangement (Bohr 1958, 4).

The acknowledgement that at the level of atomic experiments the apparatus interacts with, and thus alters the object of observation, leads to extremely interesting questions concerning the objectivity of scientific experimentation. In fact, it is undeniable that the introduction of quantum uncertainty forced a redefinition of previous scientific concepts. It also questioned the traditional separation, not only between measurement instruments and objects measured, but, maybe even more important, in terms of epistemological consequences, between the object and the subject, i.e., "the content of our consciousness and the background loosely referred to as 'ourselves'" (Bohr 1960a, 12-13). The issue thus turns not just on realizing that there is an intrinsic, human, impossibility of grasping what is out there. It also has to do with the fact that "the richness of conscious life demands in various situations a **different placing of the section between subject and object**" (Bohr 1960a, 13)(emphasis added).

It should now be obvious that one of the sources of inspiration for some aspects of Bachelard's philosophy of science, including the latter, more encompassing, concept of phenomenotechnique lies in the discoveries -- or inventions -- produced by theoretical physics, particularly quantum theory. In fact, phenomenotechnique alerts us, among others things, to the importance of the inextricability of the theory/verification relation within the experimental context, the constructive power of scientific technology, the fact that the notion of 'scientific object' is in radical contrast with the objects of everyday experience, and the artificial, socially constructed nature of the new sciences. What Bachelard basically did was to name a set of problems that were raised in the context of quantum theory and then extend them so that it became a crucial part of his discourse on scientific methodology. Bachelard's notion that **instruments are reified theories** is also important.

Furthermore, he insists that there is sometimes no point in making a distinction between what is strictly theoretical, such as mathematical models, and other scientific tools such as material instruments. To him, both are extensions of the scientific mind. When a scientist performs an experiment, she is confirming a rational construct. If the rational construct does not have a counterpart in both the results obtained with the help of technology and the mutual agreement among the individuals of the scientific community, then **it is not real** . In other words, it cannot be accepted as **scientific fact**.

In this context, it is also interesting to notice how some of the concepts of the sociology of scientific knowledge resembles some of Bohr's ideas regarding the impossibility of an exact differentiation between objects and subjects in science.[97] Sociologists of knowledge are certainly aware not only of their heavily relying on the epistemology of theoretical physics, but also that these implications were, **from the very introduction** of quantum mechanics, **intrinsic to scientists' beliefs**. It is important to reiterate that neither Bachelard nor the

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[97] For more on this, Pais, Niels Bohr's Times in Physics, Philosophy and Polity is the best account I came across. Pais provides an exhaustive and elaborate description of the science, the history, and the philosophy of Niels Bohr, including the epistemological and sociological implications of quantum theory.

quantum theorists saw in this epistemological attitude an obscure subjectivism in the work of the scientists. Heisenberg, for instance, is quite clear when he says:

> Of course the introduction of the observer must not be misunderstood to imply that some kind of subjective features are to be brought into the description of nature. The observer has, rather, only the function of registering decisions, i.e., processes in space and time, and it does not matter whether the observer is an apparatus or a human being; but the registration, i.e., the transition from the "possible" to the "actual", is absolutely necessary here and cannot be omitted from the interpretation of quantum theory (Heisenberg 1958, 137).

Although Bachelard used many notions from both relativity and quantum theory, his works never directly address the **conflict** between Einstein and Bohr on the issue of complementarily. I have not been able to determine whether he was cognizant of the conflict or not. But I am almost sure that he did know about it. In fact, Bachelard's conceptualizations behind his latter definition of phenomenotechnique parallel the philosophical claims made by the proponents of the Copenhagen Interpretation of quantum theory, which was the **official view** on quantum mechanics among professional physicists (Davies and Brown 1986, 26). There is definitely a striking similarity between the Copenhagen view of reality, according to which "**on its own** an atom or electron or whatever cannot be said to 'exist' in the felt, common-sense, notion of the word" (Davies and Brown 1986, 24) and some of Bachelard's claims regarding the

social, twentieth-century character of some of the new entities created by physics and chemistry. Bohr's claim that "no elementary quantum phenomenon is a phenomenon until it is brought to a close by an irreversible act of amplification by a detection such as the click of a Geiger counter or the blackening of a grain of photographic emulsion" (Davies and Brown 1986, 61) is also consonant with Bachelard's notion of **technical materialism** and the reification of scientific objects through the use of scientific technology.

Throughout the nineteenth century German's physicists had had a fascination for particle theories. In this regard, I believe that there is a greater affinity between Bachelard's views and what was happening in Germany than events in France, particularly the relevance given by German scientific institutions to theoretical physics. Despite his concerns with the psychological and phenomenological rather than the logical character of scientific knowledge construction, Bachelard's views are very close to those of the **École Allemande de logique quantique**, which originated between 1925 and 1927, and its preoccupations with "trying to reconcile . . . the logical difficulties of quantum theory with the explanation of their consequences at the level of a theory of knowledge" (Chevalley 1989, 153). Of course, it is difficult to know the extent to which French academia was aware of all the novelties produced by German-oriented trends in science and philosophy. Furthermore, I have no evidence to support a direct correlation between Bachelard's epistemology after the 1930s and the école Allemande. It was impossible to obtain conclusive information concerning Bachelard's relations with German institutions. However, I dare to speculate that Bachelard knew more about 'German' reflections on the new sciences than his works may lead us to assume. I believe that this contact was almost certainly established **via** de Broglie. This important scientist, whom Bachelard met in conferences organized by the **Société française de philosophie**, was definitely aware of them. It is my belief that de Broglie contributed to the acceptance of 'German' science by the French scientific community almost as much Langevin had done with relativity. One should however realize that the philosophical position of de Broglie towards quantum theory also went through changes. In fact, the 1923 quantum formulation was still deterministic.

Louis de Broglie and the Philosophy of Contemporary Science

It was with Louis de Broglie, Erwin Schrödinger and others that, from 1923 on, quantum theory underwent considerable expansion (Marcel 1932, 7). In fact, de Broglie, one of Langevin's students, was the first to develop the principle of wave-particle duality as a principle applicable to the structure of particles such as the electron, a discovery that won him the 1929 Nobel Prize in physics (Abbott 1984, 45). As should be expected, de Broglie had close contact with the scientific community outside France. Via Langevin, de Broglie had his 1924 doctoral thesis read by Einstein, who was impressed with the young French physicist's interpretation of the Bohr-Sommerfeld quantum rules (Pais 1982, 436). Also, de Broglie had been one of the scientists present at the Fifth Physical Conference of the Solvay Institute held in Brussels in 1927 and dedicated to the theme "Electrons and Protons" (Bohr 1949a, 211). At this congress, "Bohr and Heisenberg characterized 'quantum mechanics' as a complete theory, the fundamental physical and mathematical hypotheses of which are not susceptible to modifications'" (Jungnickel and McCormick 1986, 370). As Bohr reported:

> The discussions centered on the question of whether the quantum-mechanical description exhausted the possibilities of accounting for observable phenomena or, as Einstein maintained, the analysis could be carried further and, especially, of whether a fuller description of the phenomena could be obtained by bringing into consideration the detailed balance of energy and momentum in individual processes (Bohr 1949a, 213).

Einstein's interpretation of quantum mechanics acknowledged its value as a purely statistical theory but rejected it as insufficient as a theory for individual particles (Jungnickel and McCormick 1986, 370). Despite Einstein's view, de Broglie, Pauli, and Schrödinger could not come to an agreement over the interpretation of quantum mechanics. In fact, they were at that point responsible for the "epistemological flavor" of the whole meeting (Jungnickel and McCormick 1986, 370). Bachelard surely knew about these discussions.

Interestingly enough, and again confirming my claim that Bachelard was in contact with the scientific controversies of his time, de Broglie wrote extensively on the philosophical aspects of contemporary physics. The list of articles he published in the Revue de métaphysique et de morale, in which Bachelard also published, is a good example of the kind of material that could easily be assessed by French academics.[98] The similarity between the content of some of these works and some of Bachelard's claims about science is evident, although it is difficult to determine who said what first. The idea that scientific conceptions

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[98] De Broglie's articles include "Détérminisme et causalité dans la physique contemporaine" (1929), "Relativité et quanta" (1933), "Récents progrès dans la théorie des photons et autres particules" (1933) (cited by Bachelard in La Philosophie du non), "Souvenirs personnelles sur les débuts de la mécanique ondulatoire" (1941), "La Métaphysique et la science: au delà des mouvantes limites de la science" (1947), "L'Espace et le temps dans la physique quantique" (1949), "Un Nouveau venu en physique: le champ nucléaire" (1951). De Broglie also wrote books on the topic, for instance, La Physique nouvelle et les quanta (1937) and Continu et discontinu en physique moderne (1941).

are only approximations is a case in point. Referring to geometric laws, de Broglie argued that despite the fact that those and other laws are verified with precision, further discoveries make their approximate nature reveal itself (De Broglie 1936, 19). Bachelard had been claiming precisely the same thing since his <u>Essai sur la connaissance approchée</u>. There are many other points in common between these two authors. For instance, referring to the status of scientific observation, de Broglie says:

> experimental discovery is constantly guided by theoretical conceptions. If these conceptions are not always to the point of allowing us to exactly predict the phenomena to discover, they are nevertheless the ones who tell the experimenter what is the path towards which he should guide his research and how he should interpret the results. Most of the time, what makes an experimental result valuable is the way in which we interpret it . . . .
>
> Theoretical conceptions, where scientific imagination always intervenes to a larger or lesser extent, are indispensable to the interpretation of experimental results, but their role is not limited to this, since it is obvious that the experimental apparatus was also combined with them . . . [the experimental discovery], far from being a passive acknowledgment ('constatation'), carries the brand of our spiritual activity (de Broglie 1940, 73-74).

Bachelard makes almost the exact same comments about the heuristic value of theories, i.e., that all experiments are in principle theory-laden. He is also constantly arguing for the importance of considering scientific knowledge as the result of a **dynamic** activity, part of which is reflected in the construction of scientific objects and the verifiability value attributed to scientific technology.

The analysis of Louis de Broglie's writings also allows us to identify his philosophical positions regarding quantum theory (including his deterministic, indeterministic, and back-to-deterministic philosophical attitude), as well as his knowledge and deep understanding of the historical details behind the creation of the new physics. He, too, was amazed at the consequences of quantum mechanics. Already in his 1929 article he ends with the following note:

> Causal laws substituted for by probability laws, physical individuals well located and whose movement is well defined substituted for by physical individuals that refuse to let themselves be represented in a simple way and cannot be but half described: these are the surprising consequences of the new theories . . . whatever might be the fate of the new doctrines, it is infinitely interesting to philosophers that physicists were led, if only momentarily, to doubt the determinism of physical phenomena and the possibility of describing completely in the 'cadre' of space-time (de Broglie 1929a, 442-443).

By 1933, six years after the Fifth Solvay Conference, de Broglie was convinced that no definitive solution had yet been found for the problem of how to reconcile between relativity and quantum theory (de Broglie 1933a, 279). The historical recollection presented in Continu et discontinu en physique moderne (1944) led him to say that the new sciences brought about the "crisis of determinism" (de Broglie 1944, 72) and that, with Heisenberg's uncertainty relations and Bohr's claim that these relations are transcendental to space-time, it is no longer possible to accept causal determinism in the old sense of the expression (de Broglie 1944, 72-73). Still, there are chronological issues to take into account. The reference to people such as Louis de Broglie, Heinsenberg, and Bohr, for instance, show up quite frequently in Bachelard's books, but mainly after 1934, i.e., after the Copenhagen Interpretation of quantum theory had become standard acceptance within the scientific community. On the other hand, this is also the time when the conceptualizations behind the concept of phenomenotechnique include a strong focus on the creation of scientific entities in microphysics.

A curious fact, which never caught the attention of Bachelard's commentators, is that there is no record of de Broglie's being interested in the philosophy of science of Gaston Bachelard. They met on several occasions, namely at the meetings of the Société française de philosophie, some of which were chaired by Bachelard himself. However, de Broglie's interest seemed to be directed to the comparison of the new discoveries of quantum mechanics with what he thought to be the anticipatory philosophy of Bergson, rather than with the echoes of his own discoveries in the works of Bachelard.

The Epistemological Conflict Between Einstein and Bohr

The conflict between Einstein and Bohr was extremely important for the philosophy of science. In fact, the definition of what counts as **physical reality** at the level of the micro-world and its lack of correspondence with the events of the macro-world was one of the problems that led Einstein, von Laue and Schrödinger, among others, to reject the Copenhagen interpretation of quantum theory -- which incorporated Heisenberg's 'indeterminacy principle' (Heisenberg 1958, 49 and 129). This led to Einstein's epistemological conflict with Bohr. The 1935 article "Can Quantum-mechanical description of physical reality be considered complete?" by Einstein, Podolsky and Rosen, for instance, was an overt criticism of Bohr's substitution of the correspondence principle (1922) for the complementarily principle (1927). An outgrowth of Heisenberg's **uncertainty principle** (1925), the complementarily principle contains the idea that "the investigation of an experimental situation, such as the position of an electron, automatically excludes the possibility of looking at the other side, such as the momentum or velocity of the electron" (Davies and Brown 1986, 58). The rejection of quantum mechanics by Einstein, Podolsky and Rosen had to do with what they perceived to be its incomplete character. The abstract of the above article made the following claims:

> In a complete theory there is an element corresponding to each element of reality. A sufficient condition for the reality of a physical quantity is the possibility of predicting it with certainty, without disturbing the system. In quantum mechanics in the case of two physical quantities described by non-commuting operators, the knowledge of one precludes the knowledge of the other. Then either (1) the description of reality given by the wave function in quantum mechanics is not complete, or (2) these two quantities cannot have simultaneous reality. Consideration of the problem of making predictions concerning a system on the basis of measurements made on another system that had previously interacted with it leads to the result that if (1) is false then (2) is also false. It must therefore be concluded that the description of reality as given by a wave function is not complete (Einstein, Podolski, Rosen 1935a, 777).

A few months later, Bohr wrote the article "Quantum-mechanical description of physical reality", which is intended to refute the positions of Einstein and his colleagues on the issue of the completeness of quantum mechanics. Bohr tries to show that those positions contain an ambiguity, and that the introduction of the complementary principle in the description of quantum phenomena fulfills, "within its scope, all rational demands for completeness" (Bohr 1935a, 696):

> In the phenomena concerned we are not dealing with an incomplete description characterized by the arbitrary picking out of different elements, but with a rational discrimination between essentially different experimental arrangements and procedures which are suited either for an unambiguous use of the idea of space location, or for a legitimate application of the conservation theorem of momentum. Any remaining

> appearance of arbitrariness concerns merely our freedom of handling the measuring instruments, characteristic of the very idea of experiment. In fact, the renunciation in each experimental arrangement of the one or the other of two aspects of the description of physical phenomena, -- the combination of which therefore in this sense may be considered as **complementary** to one another, -- depends essentially on the impossibility, in the field of quantum theory, of accurately controlling the reaction of the object on the measuring instruments . . . (Bohr 1935a, 699).

Both the mathematical formalism of the quantum-mechanical description and its assumption regarding the "impossibility of controlling the reaction of the object on the measuring instruments" (Bohr 1935a, 697) require a radical revision of "our attitudes towards the problem of physical reality" (Bohr 1935a, 697). The results, however, still have to be expressed in classical terminology:

> It is decisive to recognize that, **however far the phenomena transcend the scope of classical physical explanation, the account for all evidence must be expressed in classical terms.** The argument is simply that by the word "experiment" we refer to a situation where we can tell others what we have done and what we have learned and that, therefore, the account of the experimental arrangement and of the results of the observations must be expressed in unambiguous language with suitable application of the terminology of classical physics (Bohr 1949a, 209).

Although indifferent to the epistemological debate between Bohr and Einstein, it is possible to recognize the scientific elements that led Bachelard to talk about things such as scientific intersubjectivity, the importance of the theoretical and instrumental context that shapes the experimental situation, and the need to realize that scientific concepts mean different things in different contexts. One of his claims was that philosophers had to find a new vocabulary to accurately express what is happening inside physics. Definitely, the great motivation for a renewal in philosophy of science was the scientific milieu provided by the impact of theoretical physics in Germany and in France. It was this extremely rich and complex environment, of which only a glimpse was given in the above paragraphs, which led Bachelard to coin the concept of phenomenotechnique as described in Part I.

Conclusion

The richness of Bachelardian philosophy of science relates closely to the specific time in the history of the sciences and the cultural history of France in the first half of the twentieth century. The reactions against scientism, the ideological consequences of the war with Germany, the anti-intellectual trends illustrated by the phenomenologies of Husserl and Bergson, the rational revolutions in philosophy provoked by the new sciences were some of the aspects dealt with in the above chapters. Quite a number of issues that also helped shape Bachelard's scientific philosophy were not discussed: surrealism (which more than anything influenced Bachelard's works on poetic imagination and artistic creation) and the return to subjectivism epitomized by the works of Freud and Jung. To incorporate these issues would require a work more extensive as this one. For our part, it is important to realize that the writings of Bachelard are, more than anything else, extraordinary works of **cultural synthesis**.

Bachelard was a product of his time, but he also went a step further by introducing in philosophy of science concepts such as phenomenotechnique. I have not ignored the flaws I found in Bachelard's account of science. Parts I and II are both descriptive as well as critical accounts. Bachelard's disregard of epistemological conflicts between Einstein and Bergson, and Einstein and Bohr, for instance, are awkward, albeit peripheral to Bachelard's philosophical program. More than a cultural synthesis, which cannot be said to be fundamental to contemporary science studies except in historical terms, the epistemology of Bachelard contributed to the recognition that science and scientific technology maintain an interdependent relationship which basically determines the viability of the extent varieties of scientific realism. Phenomenotechnique is thus the concept that should be rediscovered and adjusted to the construction of an ideal model for science studies. This task will be undertaken in Part III.

PART III

'PHENOMENOTECHNIQUE' AND SCIENCE STUDIES: THE CONNECTION

PART III - 'PHENOMENOTECHNIQUE' AND SCIENCE STUDIES: THE CONNECTION

Introduction

Part I of this dissertation presents a developed account of the concept of 'phenomenotechnique' and its articulation with other concepts crucial for the understanding of the scientific philosophy of Gaston Bachelard. Part II is the first attempt in the literature of science studies at building a picture of the intellectual and scientific context in the first half of twentieth century France which was at the basis of Bachelard's model of scientific change. The purpose of Part III is to claim that the incorporation of the concept of phenomenotechnique into the heart of current debates over the construction of scientific knowledge is crucial to the development of models of science that emphasize the **dialectical connections between scientific theories and experimentation.**

In what follows I will argue that the use of phenomenotechnique as an analytic tool and heuristic device for the analysis of scientific practices allows a better understanding of the role of both theory and experimentation in science. At the same time, it forces us to redefine traditional perceptions of science. This study will also show that scientific relativism, supported by the social constructivists and the more Foucauldian oriented intellectuals -- both of whom deny any demarcation between science and non-science -- should be carefully assessed before being accepted as the truth about the way science is constructed.

After a brief overview of some works in the philosophy of science, philosophy of technology and sociology of science and technology which used or failed to use the concept of phenomenotechnique, an analysis will be undertaken of the main arguments of two such works. Each work will be the focus of a separate chapter. Because the epistemological concerns of their authors are philosophically close to those of Bachelard, the books selected were Representing and Intervening: Introductory Topics in the Philosophy of the Natural Science (1983) by Ian Hacking, and How Experiments End (1987) by Peter Galison. In fact, both Galison and Hacking are most critical of the traditional separation between theory and experiment, and both focus their attention on the specific characteristics of the experimental context of theoretical physics. Thus, the purpose of this analysis is not merely to provide a destructive criticism of the way in which the issue of the relationships between theory and experiment. It will be demonstrated, however, that, once we incorporate the concept of phenomenotechnique into science studies, some of their epistemological claims need further refinement if they are not to be overturned. The integration of phenomenotechnique in science studies is a crucial step in the construction of a new model of scientific change. Hopefully, this work will provide the reader with insights on how that construction can be achieved.

Review of Literature

In the Anglo-American countries, most of the contemporary references to Bachelard's scientific philosophy in connection with experimentation come from sociologists and historians rather than philosophers of science and technology. Superficial references to Bachelard can be found, for instance, in The Evolution of Technology (1988), by George Basalla, and Walter Vincenti's What Engineers Know and How They Know It (1990). But the people who cite Bachelard more often or who virtually integrate his categories into their own studies of science are mainly the social constructivists and the sociologists, of which The Craft of Sociology: Epistemological Preliminaries (1991)(first French edition 1976) by Pierre Bourdieu, Jean-Claude Chamboredon and Jean-Claude Passeron is an example. More than half a century after Bachelard's breakthroughs in the philosophy of science, American intellectuals became interested in transposing Foucault's discontinuist historicism into science studies and in using European phenomenology to understand the cognitive features of science overlooked by the logical positivists. They continue, however, to be unaware of the epistemological works of Bachelard. Moreover, most of them do not realize that Bachelard's scientific philosophy is being filtered into science studies through the interpretations of the social constructivists. They distort Bachelard's applied rationalism and misleadingly connect it with the 'received view' in the sociology of science.

The work that refers to Bachelard's philosophy of science most frequently is perhaps Bruno Latour and Steve Woolgar's <u>Laboratory Life: The Construction of Scientific Facts</u> (1986). One of the subtitles for the second chapter of that book is "The 'Phenomenotechnique'." After pointing out that the results provided by instrumentation in a laboratory setting tend to make scientists relegate the material processes to the realm of the merely technical (Latour and Woolgar 1986, 63), Latour and Woolgar continue:

> It is not simply that the phenomema **depend on** certain material instrumentation; rather, the phenomena are **thoroughly constituted** by the material set of the laboratory. The artificial reality, which participants describe in terms of an objective reality, has in fact been constructed by the use of inscription devices. Such a reality, which Bachelard terms the 'phenomenotechnique,' takes the appearance of a

> phenomenon by virtue of its construction through material techniques (Latour and Woolgar 1986, 64).

Although I believe that an understanding of Bachelard's categories 'qua' categories was achieved in this context, the **philosophical presuppositions** behind these categories are mostly ignored. To Bachelard, the determinism of science is rational and technical rather than social in the sense in which this term is used by the constructivists. To Bachelard, the characteristics of science's collectiveness and technical specialization do not make it less objective. In fact, it only means that we should redefine 'objectivity' in such a way as to include the new requirements of the scientific city. In other words, now that we are dealing with highly organized research programs, our analyses of scientific practices must incorporate things such as sophisticated technology and its influence upon what counts as scientific knowledge. Thus, while social constructivists such as Latour and Woolgar argue that the acceptance of scientific findings is based on 'inscription devices' that reinforce the scientists' rhetorical manoeuvres -- what Andrew Pickering, in his <u>Constructing Quarks: A Sociological History of Particle Physics</u> (1984) called "opportunism in context" -- Bachelard argues instead that these instrumental results are what allow scientists to accept, within the framework of experimentation, higher degrees of explanatory adequacy and standards of scientific objectivity.

Steve Fuller's Social Epistemology (1988) also refers to Bachelard's concept of phenomenotechnique and Bachelard's theory of scientific experimentation. Describing the general features of Bachelard's The New Scientific Spirit (1934), Fuller acknowledges that the author saw instrumentation in science as providing a "reconstitution of one's perceptual horizon" as well as the epistemological rupture with the previous worldview "is gradually closed as the instrument is integrated in the lifeworld." The example used in this context is the telescope, which Fuller sees as the embodiment of Bachelard's phenomenotechnique. Entailed by this concept, and by what Fuller considers to be an extension of Bachelard's notions, is the idea that the telescope's artificiality is "'repressed' in order to be used unproblematically" (Fuller 1988, 125).

Almost none of Fuller's claims noted above can be extrapolated from what Bachelard said about scientific technique. It is true that Bachelard, along with Émile Meyerson and Léon Brunschvicg, was perhaps one of the first philosophers of science to point out the existence of phenomena like transparency or closure -- notions dear to writers such as Steve Shapin, Simon Schaffer, Andrew Pickering and Peter Galison -- which occur in scientific technology and which may prevent the scientist from acknowledging the artificial nature of her own constructs. However, one will not find in the works of Bachelard the references pointed out by Fuller. First of all, the category of 'lifeworld' is not Bachelardian. As we have seen in chapter two, it is a category introduced in phenomenology by Edmund Husserl in the context of a critique of the influence of the natural sciences upon common knowledge. A criticism of the physical sciences is the last thing Bachelard could possibly have in mind. Second, although Bachelard mentions quite often the "technical character of scientific determinism" (Fuller 1988, 111) he does not do it, at least in The New Scientific Mind, in the context of Galileo's telescope (Husserl was more likely to do so, since he undertook a complete critique of Galileo's science). Bachelard's book has a chapter devoted to astronomy (chapter five), but it only includes an understanding of its mathematical and geometric nature. Finally, Bachelard never talks of "repression" of an instrument's artificiality in order for it to become "unproblematic." The use of psychoanalytical categories by Bachelard takes on a very different tone. It is basically concerned with bringing to the surface all preconceived notions that constitute epistemological obstacles to the advancement of science. We may deduce that 'old' technology can be included in those notions, but it is farfetched to extend it to what Bachelard says about the uses of modern technology.

The Uses of Experiment: Studies in the Natural Sciences (1989), edited by David Gooding, Trevor Pinch and Simon Schaffer, also contains references to Bachelard's philosophy of science. In the Introduction the editors write:

> Bachelard used the term 'phenomenotechnique' to pinpoint this aspect of instrumentation: effects are realized through active instrumental work, rather than recovered by a passive observer from an all-powerful nature. In just this sense, instruments come to embody the theories they are used to support (Gooding et al. 1989, 4).

This non-passivity of the scientist in the act of knowing is rather important to Bachelard, and it definitely constitutes an epistemological problem. In fact, Bachelard did not see the subject-observer as being 'located' outside the realm of what is perceived, since the 'given' -- which in science is a 'technical given' -- is already interpreted in the frame of a specific scientific context. Particularly in science, the dialectics between these two components of cognition is what leads Bachelard to create the concept of 'applied rationalism.' Although it is recognized in The Uses of Experiment that Bachelard is a philosopher of science, he is grouped with sociologists of experiment such as Harry Collins and Trevor Pinch, his contributions being considered of a sociological import rather than of a philosophical one. Also, Simon Schaffer uses Bachelard's famous line that "instruments are reified theorems" in the beginning of his article "Glass-works: Newton's prisms and the uses of experiment" (Gooding et al. 1989). But again the context is one of what I would call rhetorical authority of one scientific argument over another, i.e., "how experimental instruments play a central role [in] the using of experiment in reaching agreement between disputants" (Gooding et al. 1989, 67). As with all other cases described, the expressions and notions from Bachelard's works were taken out of their philosophical context to serve particular ideological purposes.

Despite the social constructivists's use of Bachelard's epistemological categories, they at least found interest in the recovery and incorporation of some of Bachelard's categories into science studies. The same cannot be said of the philosophers of science. A proper assessment of the concept of phenomenotechnique would be of great value as a means of elaborating the early conceptual framework of some of the studies in the history and philosophy of experiment, and thus the history and philosophy of scientific technology. One may think of works such as How Experiments End (1987), by Peter Galison, or The Neglect of Experiment

(1986) and Experiment: Right or Wrong (1990), both by Allan Franklin.

Although it is of course acceptable that the name of Bachelard does not appear in the above-mentioned books, some reference to Bachelard's phenomenotechnique might have been expected in works that claim to give historical accounts of the philosophy of science and technology, and in phenomenological matters connected with science. But there was no indication of even a superficial knowledge of Bachelard's contributions to such issues. Ian Hacking's Representing and Intervening: Introductory Topics in the Philosophy of Natural Science (1983) ignores the analysis that large parts of the contributions of French epistemology to the study of scientific experimentation and in grappling with scientific realism. Pierre Duhem is an honorable exception to this, but we have seen that, taken in isolation, clearly misrepresents French philosophy of science. When Hacking deduces, after a long study of the use of the microscope, that the chief role of experiment is the creation of phenomena (Hacking 1983, 220) and that "to experiment is to create, produce, refine and stabilize phenomena" (Hacking 1983, 230), he fails to mention that the historical antecedents for such ideas are located in the third decade of the twentieth century, and thus not the result of a new insight into the study of scientific activities.

Instrumental Realism: The Interface Between Philosophy of Science and Philosophy of Technology (1991), by Don Idhe, is meant to be a historical survey, but it also fails to mention Bachelard's works. Among the points of interest to us is, for instance, Idhe's notion that now "we acknowledge the transformational non-neutrality [of technology]" (Ihde 1991, 73). He also points out, following not Bachelard but Latour and Galison, that "post-modern science, techno-science, becomes a productive science in precisely the laboratory/factory sense" (Idhe 1991, 134). These accounts, however, are historically inaccurate, since Idhe, Latour and Galison are not the creators of such categories. On the other hand, and despite its interest, Idhe's 'phenomenology of instrumentation' is a re-enactment of most of the technical presuppositions behind Bachelard's concept of phenomenotechnique.

Idhe's notion that "we are a very long way from the old philosophy of science" (Idhe 1991, 135) because we finally came to understand that "science constructs its 'realities'" (Idhe 1991, 116) is again misleading. It is not a prerogative of post-modernism to give science studies this sort of constructivist model of scientific explanation. As we have seen in chapter one, Bachelard constantly emphasized this point in his works. We are in the position to claim that the distance between Idhe's old philosophy of science and what he considers to be recent perceptions in the philosophy of technology is practically nil. Idhe's historical survey is incomplete. Although it refers to phenomenologists such as Husserl, it does not include figures in the Continental tradition such as Bachelard, whose epistemology is much more similar to Idhe's new program in science and technology studies. Thus, his philosophy of science cannot be presented as a contrast with what he calls the old philosophy of science. It may be a contrast in respect to the Anglo-Saxon tradition, but there is more to philosophy of science than logical positivism.

Robert Ackerman's <u>Data, Instruments and Theory: A Dialectical Approach to Understanding Science</u> (1985) mentions Bachelard 'en passant.' The author acknowledges, for instance, that Bachelard's discussions on the evolution of scientific concepts were brilliant (Ackermann 1985, 98). He also mentions that "according to Bachelard, the creative dreaming of the scientist occurs because of the layered concepts, like that of mass, which are instruments of experimental check" (Ackerman 1985, 99), adding that "the Bachelardian view helps to capture the complexity of scientific autonomy" (Ackermann 1985, 99). Thus Ackerman recognizes that, for Bachelard, "reality comes at the end of science, and not at the level of everyday discourse" (Ackermann 1985, 100). These are reasonably good interpretations of what Bachelard meant to say regarding conceptual change in science. But Ackerman says nothing about phenomenotechnique. This neglect is awkward, especially in a book that is mainly preoccupied with the intrinsic relationships between instruments, data, and with the role of theories in experimental verification. Ackerman says that it is possible to construct the history of science by just looking at the parallel history of scientific technology, but he never acknowledges that it was Bachelard who first said so in the first decades of this century.

Within the philosophy of science proper, Mary Tiles in Bachelard: Science and Objectivity (1984) provides the most accurate account of the concept of phenomenotechnique. Since the purpose of her work is to connect Bachelard's philosophy of science to some of the preoccupations of contemporary American analytical philosophers, it is obvious that the treatment of phenomenotechnique is fairly superficial. Gary Gutting's "Gaston Bachelard's philosophy of science" (1987) and Mary McAllester's The Philosophy and Poetics of Gaston Bachelard (1989) are both interesting, but only generic accounts.

The misrepresentation of the philosophical presuppositions behind Bachelard's concept of phenomenotechnique is part of the motivation for the following chapters. But as was said in the introduction, the main purpose of this project is not to chastise those who misused Bachelardian categories. In fact, it is probably important that the social constructivists realized that phenomenotechnique is full of rich implications for the analysis of contemporary scientific practices. Still, the term should not be seen through the eyes of the sociologists of science alone. As we saw before, phenomenotechnique was born as a philosophical category. If properly explored, it can be used with advantages by American philosophers of science and technology. It is this feeling of a missing link that is at the basis of the present work.

Chapter 1. Representing and Intervening: Hacking's Theory of Scientific Praxis

In Representing and Intervening: Introductory Topics in the Philosophy of Natural Science, Ian Hacking claims to be a scientific realist, i.e., one who believes that "the entities postulated by theoretical physics are real" (Hacking 1983, 1). The purpose of Hacking's book is to demonstrate that this metaphysical position concerning scientific entities is the result of a renewed analysis of science, one which sees action as its most important feature. Given his penchant for scientific practice rather than scientific theorizing, his perspective is that of a pragmatic realist (Hacking 1983, 2).

To Hacking, the traditional dichotomy between theory and experimentation in science is artificial. He also claims that this dichotomy is at the basis of many misconceptions concerning the role of scientific theories and their dependence upon experimentation and thus scientific technology. Hacking is also determined to show that, in many instances, scientific experimentation has a life of its own, independent from theoretical presuppositions. Furthermore, he argues that the metaphysical issue of scientific realism in regard to entities cannot be solved by just looking at the role that those entities have in theories (Hacking 1983, 31). Also, the failure of the project suggested by people such as Bertrand Russell to reduce all scientific statements which dealt with theoretical entities to logical statements free of such entities (Hacking 1983, 49) is proof that realism in science has to be approached differently. Thus, Representing and Intervening proposes a new form of scientific realism. According to Hacking's model, the criteria for the acceptance of the existence of entities is determined by the way they can be manipulated, the way they provoke changes in observable phenomena, and also their influence upon instrumental systems regulated by different physical characteristics. As we will see, Bachelard's concept of **phenomenotechnique** fits easily into the discourse on science as it is defended by Hacking's model, at the same time as it entails the correction of some of Hacking's claims concerning the priority of experiment over theoretical systems of representation.

Half of Representing and Intervening is a sustained criticism of traditional models of scientific explanation and change which rely too much on the role of theories and thus representation in science. To Hacking, groups such as the logical positivists and theory-oriented philosophers of science such as Karl Popper, Bas van Fraassen, Rudolf Carnap and others, see science almost as strictly theoretical enterprise and are thus curiously oblivious of the role of experimentation in the constitution of knowledge. They tend to consider theories as the only relevant part of science. Observation and experiment have only a subservient and secondary role, because they are always built within the context of a particular theoretical situation, and thus are always theory-laden. For the positivists, **representation**, **description**, and **explanation** are what science is all about. On the other hand, Hacking thinks that, in science, truth should no longer be "correspondence to a mind-independent reality" (Hacking 1983, 58). Once concepts like Thomas Kuhn's incommensurability and paradigm came into the picture and "unintentionally inspired a crisis of rationality" (Hacking 1983, 2), the relevance of what counts as rational in science has to be put into historical and sociological perspective:

> The old-fashioned nominalist of times gone by held that our systems of classification are products of the human mind. But he did not suppose that they could be radically altered. Kuhn has changed all that. The categories have been altered and may be altered again. We can hardly avoid approaching nature with our present categories, problems, systems of analysis, methods of technology and of learning. We are in fact empirical realists: we think as if we are using natural kinds, real principles of sorting. Yet in the course of historical reflection we realize that the inquiries most dear to us must be replaced (Hacking 1983, 110).

Kuhn's historical model of scientific change confronts traditional philosophers of science with the fact that "there is no concept of the right, final representation of the world" (Hacking 1983, 110). On the other hand, science is not a strictly rational activity. Paradigm shifts, for instance, do not necessarily have a strong rational or even experimental justification behind them. Changes in scientific world views are instead the result of gestalt switches, where the same phenomenon is analyzed and interpreted in a totally different way. Although we could see this as being the product of a shift in what Putnam calls 'styles of reasoning,' Hacking seems to see it as the result of the role of irrationality in scientific change. Another point made by Kuhn's Structure of Scientific Revolutions which coincides with what Hacking thinks to be fundamental to take into account in the construction of a model for modern science. Referring to the role of instrumentation inside established paradigms, Kuhn says:

> The decision to employ a particular piece of apparatus and to use it in a particular way carries an assumption that only certain sorts of circumstances will arise. There are instrumental as well as theoretical expectations, and they have often played a decisive role in scientific development (Kuhn 1970, 59).

Kuhn's notion of 'instrumental expectation' is further developed by Hacking, particularly when the latter sees scientific technology as shaping what counts as scientific knowledge. Although Kuhn deals only tangentially with the issue of scientific technology, he nevertheless considers that an anomaly can be something in the experimental context which theories within current paradigms are unable to account for. By the same token, Hacking claims that, very often, instruments themselves are responsible for the overturn of theories.

Hacking also says that the traditional dichotomy between thinking and acting is false (Hacking 1983, 130). This dichotomy is the consequence of "a single-minded obsession with representation and thinking and theory, at the expense of intervention and action and experiment (Hacking 1983, 131). According to Hacking the truth is that, when scientists experiment, they "create, produce, refine and stabilize phenomena" (Hacking 1983, 230). Furthermore, intervention and the power of scientific technology in the construction of science is of such magnitude that it can, by itself, validate the scientists' ontological claims concerning theoretical entities. Thus, a redefinition of traditional concepts such as that of 'representation' is at place. Hacking thinks that the sole criterion for representation is that of its "public likeness" (Hacking 1983, 133), i.e., "complicated speculations which attempt to represent the world" as well as all kinds of physical artifacts such as statues, pictures, and so on. Thus, representations, portrayals (Hacking 1983, 138) should not be confused with what they are supposed to re-present. Moreover, reliance on strict representation is insufficient, especially when one becomes aware of the existence of alternative systems of representation (Hacking 1983, 139).

One example pointed to by Hacking is Heinrich Hertz's presentation in his Principles of Mechanics (1894), of "three different ways to represent the . . . knowledge of the motions of bodies" (Hacking 1983, 143). Issues of realism become even more critical when one deals with the experimental sciences. If theories are not enough to prove that entities exist, the focus should shift from scientific representation to scientific practice. Hacking sets the stage for this reorientation in the philosophy of science -- which he calls the 'Back-to-Bacon movement' (Hacking 1983, 150) -- by postulating that theoretical entities exist if the scientist can manipulate them and use them to provoke changes and thus alter the world (Hacking 1983, 149). As he puts it "if you can spray them, then they are real" (Hacking 1983, 22). Emphasis on experimentation has the advantage of solving the problems surrounding scientific realism once and for all. What is real in science is not the outcome of a rational choice among equally good explanatory theories, but instead what can be instrumentally demonstrated. The concept of 'instrumental determinism' suggested by Bachelard fits nicely into Hacking's notion that instrumentation in science gauges the objectivity of scientific entities which do not exist in the natural world but only in the context of experimentation.

Just as with representation and experimentation, observation has to undergo a process of redefinition. The traditional concept of 'observation' implies that observation is always entailed by theories or itself theory-laden. Within the context of experimentation, however, observation is more than merely reporting. It also means "to get some bit of equipment to exhibit phenomena in a reliable way (Hacking 1987, 167). More than a language, observation is a **skill** (Hacking 1983, xii). It presupposes ability, know-how. Skills can be independent from the theoretical knowledge that justified the observation in the first place. "Persistent attention to an oddity . . . is what leads to new knowledge" (Hacking 1983, 167). In other words, you can be a great experimentalist without knowing what theory or what entities you are testing. As Hacking says:

> An assistant can be trained to recognize [those tracks] without having a clue about the theory. [He may be] extraordinarily skilful with the apparatus, but also quickest at noting an oddity on for example the photographic plates he has prepared from the electron microscope . . . its sense need not be necessarily entangled in some particular theory, so that every time

> you say 'that's a positron' you somehow assert the theory (Hacking 1983, 179).

As one small aspect of the experimentalist's work, observation entails sensitivity and alertness. It makes "an experiment go, detecting the problems that are making it foul up, debugging it, noticing if something unusual is a clue to nature or an artifact of the machine" (Hacking 1983, 185). It is not clear how, by Hacking's account, experimenters who are clueless about what they are looking for in theoretical terms are perfectly able to distinguish between artifacts and 'clues to nature.' The technician may be able to learn how to detect 'oddities' and 'quirks' but that does not necessarily mean that her skills guarantee that what she 'sees' is real, or that she is doing science without realizing it. The scientific meaning of observation can only be ascertained once the observed entity is conveniently contextualized within explanatory bodies of theory. This does not mean that the theory-oriented philosophers of science are right and the experiment-oriented ones are wrong. It only means that, in science, concepts such as 'observation,' 'experiment,' 'theory,' and so on, undergo what Bachelard would have called 'deformations.' Conceptual redefinitions such as the above reflect the dynamic nature of scientific methodologies. Also, they introduce scientific technology into philosophical models of science, at the same time as they allow for a reconsideration of both the boundaries which divide theory and practice or the elements which unite them. This is one of the reasons why we have to extend the concept of observation:

> It is commonplace in the most rarefied reaches of experimental science to speak of 'observing' what we would naively suppose to be unobservable -- if 'observable' really did mean using the five senses almost unaided (Hacking 1983, 182).

What scientists say that they 'see', then, does not correspond anymore to the orthodox notion that one can only guarantee the existence of things which one sees with the 'naked eye.' Now, there is instrumentation. Technology redefined what 'to see' actually means, and it coincides with the changing role of experimentation in science.

Hacking provides several historical examples to emphasize that practice often precedes theory. One such case is given by the experimenter R. W. Wood's research. Hacking says that, regardless of Wood's ignorance of and skepticism about quantum mechanics, he nevertheless made "fundamental contributions to quantum optics" (Hacking 1983, 158). Other cases can be found in the history of thermodynamics, which "is a history of practical invention that gradually leads to theoretical analysis" (Hacking 1983, 162-3). Technicians, too, can be put under the category of inadvertent contributors to the development of scientific theories by means of their own laboratory practices. They are able to interpret what they see, i.e., separate what is a mere artifact or a disturbance produced by an instrument from what is a quality of the object under study without having to know about the theoretical context which surrounds it. As **observants**, scientists also have this ability. They are not constantly worried about testing and refuting or falsifying theories in order to be doing good science. Instead, they rely upon what technology shows about what is being observed. The ability to observe, i.e., knowing how to 'see' with a microscope, is taken by Hacking as fundamental to the construction of scientific knowledge.

Although Hacking defines himself as a pragmatic realist, he only gives peripheral consideration to what Charles Sanders Peirce saw as a fundamental test of the objectivity of scientific theories: the role of the community of inquirers in the establishment of what is truth (Hacking 1983, 60). He acknowledges, however, having been influenced by John Dewey, particularly by Dewey's notion of warranted acceptability (Hacking 1983, 61). By the same token, he seems to partially accept Putnam's notion of truth as "universal agreement among reasoning people" (Hacking 1983, 99). However, whereas Putnam refers to warranted rational assertability in regard to theories, Hacking is interested in entities. In this respect, **phenomenotechnique** would have been more useful to him than pragmatism or an adaptation of the Putnamian model to entities, since the conceptualization behind phenomenotechnique incorporates scientists' agreement that, if conveniently scrutinized, the products of scientific technology are what count as scientific reality. In other words, phenomenotechnique would have allowed Hacking to introduce the problem of consensus in the scientific community in regard to entities without having him weaken his original claim that scientific knowledge is not strictly theoretical. Taking technology as the locus of the reification of theoretical entities is to accept that part of what counts as real in science is also determined within the context of experimentation. Obviously, it also implies that scientific **praxis**, in contrast with scientific **theory choice**, is responsible for the verification of the entities created and recreated by the scientist as technician.

To Hacking, Karl Popper is wrong. Conjectures and refutations are not the only justification for testing and experimenting in science (Hacking 1983, 154). In fact, "none of the traditional values of prediction, explanation, simplicity, fertility and so forth" (Hacking 1983, 143) can be used as criteria for judging if a particular theory constitutes the right representation of the facts. Falsifiability models are constructed under the assumption that, in science, theories are prior to action and, consequently, that "deliberate experimentation is dominated by theory" (Hacking 1983, 157). The scientist does not experiment unless she is trying to falsify a theory. Karl Popper, quoted by Hacking, said in the 1939 edition of The Logic of Scientific Discovery:

> The theoretician must long before [the experimenter] have done his work . . . he must have formulated his questions as sharply as possible. Thus it is he who shows the experimenter the way. But even the experimenter is not in the main engaged in making exact observations; his work is largely of a theoretical kind. Theory dominates the experimental work from its initial planning up to the finishing touches in the laboratory (Popper in Hacking 1983, 155).

Since Hacking suggests that, in science, experimentation may precede or be independent from theorizing, he does not think that Popper's oversweeping claim that observation statements are "interpretations in the light of theories" (Hacking 1983, 155) can be taken at face value. Counterexamples to refute Popper's model are, for instance, the development of early optics between 1600 and 1800, which relied heavily on "simply noticing" (Hacking 1983, 155), and the work of chemist Humphrey Davy (1778-1829). Hacking demonstrates that Davy's technique of electrolytic chemical analysis (Hacking 1983, 153) was the consequence of his "imaginative trials required for the perfection of the technology" (Hacking 1983, 164) rather than his profound theoretical knowledge of chemistry.

even if the models suggested by the 'representing' part of the book have some heuristic value, as is the case of models provided by philosophers such as Hilary Putnam and Bas van Fraassen, they do not really help in the case of ascertaining the existence of entities which are materially created by science, and which 'owe' their existence to the scientific context of a particular stage in science's development. The same happens with Feyerabend's model. Despite his criticism of the idea that there is just one 'scientific method' (Hacking 1983, 152), Hacking sees it as illustration of the notion that theories are more important than experimentation and observation.

Theoretical entities can only be given an ontological status if the role of experiment becomes more important than that of theory. In other words, their existence can only be ascertained through scientific praxis. Instead of using the concept of 'real entities' in the context of pure theory, Hacking says that we should "count as real what we can use to intervene in the world to affect something else, or what the world can use to affect us" (Hacking 1983, 146). This is the reason for Hacking's program of denying the historical and practical bias in favor of the theoretician (Hacking 1983, 150). It is also his **leitmotif** for introducing the notion that scientific technology is an essential factor in the validation of scientific knowledge, particularly as it pertains to entities. In Hacking's words:

> Often the experimental task . . . is less to observe and report, than to get some bit of equipment to exhibit phenomena in a reliable way . . . . A good experimenter is often the observer who sees the instructive quirks or unexpected outcomes of this or that bit of the equipment . . . . Sometimes persistent attention to an oddity that would have been dismissed by a lesser experimenter is precisely what leads to new knowledge (Hacking 1983, 167).

As Hacking said before, the concern of the philosopher of science should be with "those entities we can use in experimental work" (Hacking 1983, 29) and not with the correspondence between theories and the world. The shift from representation to intervention is interesting, because it also entails Hacking's rejection of traditional conventionalism in regard to entities, which argues that entities are useful intellectual tools with no correspondence in the world. His position is similar to the constructive empiricism of Bas van Fraassen. In fact, van Fraassen suggests that the acceptance of theories involves a criterion of empirical adequacy (Hacking 1983, 51). For his part, Hacking argues for a similar kind of empirical adequacy for the acceptance of theoretical entities, which could be formulated very simply in the following terms: scientific instruments determine which entities are real.

Hacking disagrees with Putnam's claim that objects have ontological status only "within a theory or description"

(Hacking 1983, 93). But he concurs with Putnam's rejection of meaning-incommensurability, which basically says that "whenever a theory changes, we cease to talk about the same thing" (Hacking 1983, 80). In the case of theoretical entities, it is unlikely that Hacking would agree that entities exist and, at the same time, accept that the historical change of scientific theories entails the rejection of entities that had been proved, through instrumentation, to be real. Instead, he admits that technology can be corrected and lead to more sophisticated types of observation. Entailed by the use of technology, scientists become more capable of differentiating between background effects and real objects. As in the case of Putnam's concept of stable extension of theories, Hacking would say that the notion of electron, for instance, is **extended** to incorporate newly discovered properties. Bachelard would say that the concept endures successive **deformations** which correspond to the constant process of approximation to rationality that scientific knowledge/technique goes through. The importance of both Bachelard's and Hacking's positions is that they incorporate scientific technology into the study of science. However, their epistemological standpoints concerning the relationships between theories and scientific technology is quite different.

Two problems implicit in the above quotation are not properly addressed by Hacking. One has to do with the 'independent' nature of experimentation **vis à vis** theoretical presuppositions. The idea is that the context of experimentation -- which includes the set of physically differentiated techniques used by the experimenter -- is by itself capable of **producing** scientific **knowledge**. In other words, it advocates that know-how is similar to knowledge and that experimentation does not presuppose the use of interpretive frames of reference except the ones provided by the experimental circumstances themselves. Such a standpoint does not shed any light on the philosophical, the historical and sociological understanding of the nature of scientific activity. Moreover, it may even lead philosophers of science to fall again into the trap of giving priority to one aspect of science over the other. The only difference is that now the situation is reversed: instead of claiming that theory comes first, one would say that practice comes first. This situation only helps to understand why philosophers of science have spent so much rhetoric on a 'chicken-and-egg' kind of issue.

As already stated, Hacking makes the claim that different physical systems allow one to distinguish between an observational artifact and what the object of observation **really** is. Referring to scientific observation in blood biology, he says that it is possible to determine whether bodies observed with the electron microscope inside the blood cell are part of it or just artifacts of the instrument. Again, it is experimental action rather than theory that allows for the **crucial test** concerning the reality of those bodies.

The idea that different instruments help distinguish artifacts from reality coincides with the views expressed by Allan Franklin in The Neglect of Experiment in the realm of the physical sciences. It also parallels Bachelard's claim in the Essai sur la connaissance approchée that, in physics and in chemistry, the hunting of the parasite (which corresponds to Hacking's 'artifact') is one of the reasons why the scientist should be concerned with using instruments of different levels of sensitivity, as well as putting each instrument constantly under suspicion (Bachelard 1987, 66). The agreement of these authors concerning the role of instruments as detectors leads us to the second problem entailed by Hacking's 'philosophy of experimental science' (Hacking 1983, 185).

Contrary to Bachelard's epistemology, and because it wants to 'liberate' experiment from subservience to theory, Hacking's view neglects to mention that experimental check is often an **interpretation** of data, i.e., groups of statements which validate experimental and theoretical assumptions, in this case about the 'real structure of the cell.' More often than not, experiments are theory-loaded. Counterexamples to the priority of theories over empirical observations do not really demonstrate that experiments have a life of their own. Instead, they demonstrate that the question of scientific realism cannot be solved unless one accepts that the relationship between theory and observation is of a **dialectical** nature.[99] Furthermore, **it is not a dialectic between different things, but a dialectic between different aspects of the same thing**.

Hacking realizes that sometimes scientists have to disrupt the structure of what they are observing (Hacking 1983, 191) in order to gather data about the object of observation. The awareness of some of the disturbing effects connected with scientific technology and thus with the experimental context as such should have led him to relativize the claim that instrumentation has the last word on the issue of scientific realism. The perfecting of technology is thus more than getting rid of background effects. Hacking does not emphasize this point, because he is too concerned with 'saving the experiment'. Unfortunately, this concern makes his model be self-

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[99] On the dialectical nature of science see for instance Robert J. Ackermann's Data, Instruments and Theory: a Dialectical Approach to Understanding Science (1985).

contradictory. At different places in Representing and Intervening he says that: a) it is unreasonable to separate between thinking and action; b) experiment has a life of its own; and c) "I make no claim that experimental work could exist independently of theory" (Hacking 1983, 158). Were he to acknowledge the special dialectical character of theory and observation (or thinking and acting), no contradictions would be found, since, by definition, there can be found no special priority of experiment over theory or vice versa. Scientific realism would thus become a matter of **phenomenotechnique**, i.e., one that takes into account the contributions of **both** theoretical and practical presuppositions in the construction of science, without letting itself be led to take an extremist position, alleging that theory (or action) comes first.

Phenomenotechnique solves the above contradiction. It avoids the reductionism of a type of scientific realism which relies upon instrumental verification alone or upon theoretical representation alone. It incorporates Hacking's emphasis on experimentation and scientific technology as having the power to determine with accuracy whether particular theoretical entities exist. On the other hand, and contrary to Hacking's model, phenomenotechnique does not neglect the idea that theoretical systems have an essential role in the construction and validation of those very entities. Bachelard's concept recognizes that even the more technical aspects of scientific production originate within the context of theorizing. To Bachelard, technique is both of an abstract order (mathematical calculus, for instance) and of a practical one (illustrated in scientific instrumentation). The practical aspect of technique is regulated and validated by the theoretical side and vice versa. The dialectic established, within technique, between theory and practice is the most important aspect of phenomenotechnique. Phenomenotechnique illustrates the scientific mind at work. The last word in science comes from reason because even practice is the material expression of scientific rationality. Hacking seems to be saying the contrary, i.e., that scientific rationality is the abstract expression of scientific practice. As we have seen before, these positions have more in common than what Hacking is prepared to accept.

Many of Hacking's claims about how science should be analyzed philosophically are not completely alien to Bachelard's model. Both argue that scientific technology is the landmark of modern science and that scientific entities are virtually constructed by the experimental contexts of the diverse developmental stages of science. Both Bachelard and Hacking agree with the value of scientific practice in comparison with that of theory construction. The difference between the two positions lies on **how they relate scientific action** (experimentation, observation) **with scientific knowledge as a whole**, i.e., how they conceptualize the **role of technique** in science.

In Representing and Intervening, Hacking refers to 'technique' or 'physical technique' (Hacking 1983, 200) only a few times, and in those cases 'technique' basically means **experimental ability**. In referring, for instance, to the use of microscopes in biology, Hacking says:

> No one needs to have any ideas what the dense bodies **are**. All we know is that there are some structural features of the cell rendered visible by several techniques. Microscopy itself will never tell all about those bodies (if indeed there is anything important to tell). Biochemistry must be called in (Hacking 1983, 201).

By the same token, he uses the concept of 'technique' to explain why the experiments of people like Aristarchus, Cavendish, Fizeau and Millikan represent truly exceptional ways of doing science:

> They are admired for at least two reasons. First, they were extraordinarily accurate . . . . Secondly, each individual produced a brilliant new technique. Each experimenter had the genius not only to conceive a brilliant experimental idea, but also the gift to get it working, often by inventing numerous auxiliary experimental conceptions and technological innovations (Hacking 1983, 236).

In both of the above cases, **scientific practice** is reduced to **engineering**, that is, to a set of skills of diverse levels of complexity which allow the scientist as experimenter to fabricate science. One of the last paragraphs of Hacking's book says, for instance, that "engineering, more than theorizing, is the best proof of scientific realism about entities" (Hacking 1983, 274). Thus, scientific practice becomes a set of physical techniques (Hacking 1983, 201) and ideas become 'experimental ideas', not mere products of theoretical reasoning. On the other hand, it is practice which "creates the ability to distinguish visible artifacts of the preparation (or the instrument), and the real structure that is seen with the microscope. This practical ability breeds conviction" (Hacking 1983, 191). Since the role of theories is minimized, the problem becomes one of **belief** in the power of experimental techniques rather than one of testable objectivity. It is technique that allows the scientist to "see through a microscope" (Hacking 1983, 189), not her prior knowledge of what is being currently observed.

Hacking defends the idea that what is seen with the microscope has a particular meaning **per se**, i.e., independently from specific theoretical frames of reference. But one has to admit that, contrary to Hacking's view, the determination of **what counts as real** cannot only be the direct result of what one sees through the microscope. It also comes from particular theoretical and practical presuppositions which are largely determined by the historical development of science. In the context of observation, the technician can acknowledge what is a mere artifact of the instrument or a physical characteristic of the object under study. This, however, does not turn technicians into scientists. It only makes technical ability one of the scientist's most essential tools. If one would completely separate the scientist and the technician, no technician would produce science. All by herself, the technician lacks the kind of knowledge necessary to articulate the information acquired by observation with the current, more encompassing, paradigm. It is acceptable to "push the concept of seeing" (Hacking 1983, 207) and that of "direct observation" (Hacking 1983, 183) as far as possible. In fact, one 'sees' through an electron microscope as one 'sees' a brain in the screen of a computer. 'Seeing' is a matter of practice (Hacking 1983, 191). However, the meaning of what is observed, i.e., its scientific interpretation, is still conditioned by the scientific mind at work. A technician is able to interpret data not because scientific observation can be done with pure know-how, but because the technician herself exhibits some form of scientific expertise. Only in this sense can we talk of the "extension of the idea of observation" (Hacking 1983, 182).

The emphasis given by Hacking to the manipulation of entities as proof of their existence seems to allow him to easily solve the traditional problem of scientific realism:

> Experimental work provides the strongest evidence for scientific realism. This is not because we test hypotheses about entities. It is because entities that in principle cannot be 'observed' are regularly manipulated to produce new phenomena and to investigate other aspects of nature. They are tools, instruments not for thinking but for doing (Hacking 1983, 262).

The solution lies in a renewed establishment of the connection between experimentation -- which includes the realist convictions of the scientist (Hacking 1983, 208) -- and scientific entities. As we have seen, this connection is supposed to allow the philosopher of science to realize that the issue of scientific realism has to be discussed in the context of practice rather than that of theory. Still, Hacking is careful enough to point out that realist attitudes of scientists concerning entities cannot be used as proof that those entities are **real** (Hacking 1983, 208). The ontological validity of scientific entities only comes from the scientists' ability to manipulate them and interfere with them (Hacking 1983, 209).

One of the problems with the above remarks regarding the testing of the ontological status of theoretical entities through 'interference' and 'manipulation' is that it transforms scientists into engineers. This mutation, which basically reiterates the value of action as opposed to that of theory, requires Hacking to **redefine** both the concept of 'scientific activity' and that of 'phenomenon'. Only redefinitions such as these allow Hacking to incorporate within his pragmatic model notions that were considered beforehand as products of theorizing. Thus, his "tripartite division of activities" includes things such as "speculation, calculation and experimentation" (Hacking 1983, 212). These subsets of scientific action work together to achieve what Hacking considers to have been for too long neglected by philosophers of science, i.e., the creation of phenomena by science. As he puts it "scientists create the phenomena which then become the centerpieces of theory" (Hacking 1983, 220). On the other hand, a 'phenomenon' is something noteworthy, discernable, it is "commonly an event or process of a certain type that occurs regularly under definite circumstances [or] a unique event that we single out as particularly important" (Hacking 1983, 221). This definition too is ambiguous, since it does not allow one to distinguish, for instance, between natural and artificial (scientific) phenomena.

The problem with Hacking's experimental realism is that it can easily be held by those who, contrary to Hacking, think that theories come first and that experimentation is solely generated for the testing of theories. The case of Bachelard is in this respect illuminating. One of the presuppositions behind the concept of phenomenotechnique is that instruments are materialized theories, i.e., the **material embodiment of rational assumptions** about the structure of matter. Hacking says that "most of the phenomena of modern physics are manufactured" (Hacking 1983, 228). Similarly, Bachelard would say that modern physics and chemistry laboratories are factories of phenomena and that scientific technology allows for the reification of substances which have nothing to do with 'natural nature'. In other words, the fact that one can **use** theoretical entities (Hacking 1983, 262) does not entail that experiment is more important than theory. Although it may be correct to claim with both Hacking and Bachelard that technology creates phenomena which do not have any correspondence in the world, this does not allow one to argue that the role of theory in the determination of what is real is secondary.

The difference between Hacking and Bachelard on this point is subtle, but by no means negligible. Hacking sees technique as an essentially practical component of science. He also reduces rational processes such as speculation and calculation to expressions of engineering. Technique is something that can only be expressed through technology. Bachelard says instead that there are two kinds of techniques, i.e., theoretical techniques and instrumental techniques, both of which are the result of scientific rationality. Thus, "there is no sense in making a radical distinction between the scientific mind instructed by mathematics and the scientific mind instructed by physical experiment" (Bachelard 1978, 133). In other words, and once again going a step further than Hacking, phenomenotechnique asserts the inextricable dialectics established between reason and observation, theory and instrumentalization.

Conclusion

To Bachelard, scientific technology creates new phenomena. At the same time, he argues that the power to decide the reality of these entities is not given to instrumental techniques alone. To him, experiment does **not** have a life of its own. The criterion for determining what counts as scientific is primarily given to rationality and to the consensus of the scientific community regarding what counts as real. Since technology is built to test theories, the ontological validation of entities is embodied in instruments themselves. The role of the scientific community in the determination of what is significant for science is also fundamental in Bachelard's account of scientific change. Hacking completely disregards the social aspects of science. He sees instruments as neutral devices capable of determining what is truth independently of an epistemological context, at the same time as he makes one believe that scientific technique means the same as engineering.

The incorporation of phenomenotechnique into Hacking's project offers a less radical and also less contradictory view of scientific practices. While maintaining that experimentation and thus scientific technology are essential for the redefining of scientific knowledge, phenomenotechnique emphasizes the dialectical relationship between thinking and acting which exists at all levels of the scientific enterprise. In L'Activité rationaliste de la physique contemporaine, for instance, Bachelard says that there is a "constant fusion of the system of theoretical reasons [with that of] technical experiments" (Bachelard 1951, 111). Scientific entities are products of "technical causality" (Bachelard 1951, 121), products of technique, but 'technique' is more than engineering.

Hacking's experimental realism tries to make philosophers of science focus their attention on traditionally neglected areas of science such as experimentation. Through the concept of phenomenotechnique, Bachelard argues instead that scientific techniques, whether theoretical or material, are the outcome of the scientific mind at work. Despite the fact that scientific entities are artificial creations of the instruments, both instruments and entities are theory-dependent materializations of explanatory theoretical models. To Bachelard, scientific realism is "rationally conditioned" (Bachelard 1951, 15), since scientific entities are not natural givens. They are instead the technical (mathematical and material) product of rational constructs.

Hacking strongly emphasizes that it is the skill to manipulate theoretical entities, our ability to interfere with them and provoke changes in other systems and in the world that allows one to say that entities are real. Bachelard's standpoint is similar. He considers scientific entities as "functional beings" (Bachelard 1951, 171), and says that they **become real** by their gradually more extensive use in a diversity of domains (Bachelard 1951, 172). They are real because of their consequences rather than their possibility of becoming observables. Scientific entities are relative beings. They are relative to technology, relative to theories that "provoked" them in the first place, and relative to the developmental stages of each science. Because, for Bachelard, technique is the expression, both speculative and material, of scientific rationality, 'technical ontology' is not proof that the philosopher of science should undermine the role of theory with respect to experiment. The realism of Hacking concerning theoretical entities does not add much to what scientists themselves know about their scientific practices.

To Hacking, scientific technology allows the scientist or the technician to 'see' things accurately and to discover things that had not been predicted or taken into account by theory. On the other hand, the testing of the images provided by instrumentation, i.e., by the context of experimentation, relies more on technology itself than on theoretical frameworks. This is one of the reasons that led Hacking to ascertain that the test for distinguishing between an artifact of the instrument and a quality of the object being observed is still done in the realm of experimentation. Instead of theories, it is the **reliance upon instruments** with different physical properties and diverse degrees of sophistication which constitutes the ultimate test for the existence of scientific objects. Referring to the observation of cellular physiology with the electron microscope, Hacking says:

> On the basis of the instruments and densities of [these] bodies in various stages of cell development or disease, it is guessed that they may have an important part to play in blood biology. On the other hand they may simply be artifacts of the electron microscope. One test is obvious: can one see these selfsame bodies using quite different physical techniques? . . . Two physical processes -- electron transmission and fluorescent re-emission are used to detect the bodies. These processes have virtually nothing in common between them. They are essentially unrelated chunks of physics. It would be a preposterous coincidence if, time and again, two completely different processes produced identical visual configurations which were, however, artifacts of the physical processes rather than real structures in the cell (Hacking 1983, 200-1).

By the same token, it is insufficient to rely on models such as the one suggested by Imre Lakatos. Besides the fact that Lakatos is trying to safeguard rationality in the midst of historical change in science -- and Hacking seems to be against the idea that there is some permanent form of rationality that pervades all scientific knowledge -- Lakatos' methodology of scientific research programmes is only able to judge retrospectively whether a programme was progresssive or degenerating (Hacking 1983, 121). True, Lakatos says that there is "no firm theory-observation distinction" (Hacking 1983, 115). However, his analysis is only useful for "a particular kind of knowledge produced by a particular kind of reasoning" (Hacking 1983, 127), i.e., the mathematical (Hacking 1983, 127), hypothetico-deductive model. Hacking seems to prefer Putnam's claim that the progress of science entails the development of styles of reasoning (Hacking 1983, 111) which cannot therefore be understood by means of reductionist models.

As we have seen, the general reason for Hacking's criticism of the above positions is that he is more interested in the issue of scientific realism in regard to entities than in the issue of realism in respect to theories. The philosophical study of science cannot be solely devoted to the analysis of scientific statements, their semantics, logic and referential meanings. To him, The focus on action rather than on theory, does not solve the problem of scientific realism at all. Bachelard thinks that the main focus of attention is on reason rather than on action, and his conclusions regarding the artificiality of scientific entities are not very different from Hacking's.[100] The advantage of Bachelard's concept of phenomenotechnique over Hacking's concept of pragmatic realism is that the former acknowledges that scientific practices -- whether theoretical or instrumental -- are always expressions of rationality and of the mutual interdependency of thinking and acting. Contrary to what Hacking says, experiment does not have a life of its own, and the creation of scientific entities does not prove that science is the same as engineering.

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[100] The same applies to Don Idhe's 'instrumental realism' as expressed for instance in his Instrumental Realism: The Interface between Philosophy of Science and Philosophy of Technology (1991).

Chapter 2. How Experiments End: Galison and the Problem of Experimental Autonomy

Peter Galison starts his book How Experiments End (1987) with a criticism of the way science studies have been conducted by philosophers, historians and sociologists of science. Historians of science, for instance, seem to think that a complete understanding of scientific change can be achieved by looking at the evolution of scientific concepts alone. Typically, they neglect the idea that conceptual change in science also has to do with what happens in the context of laboratory practices (Galison 1987, ix). Abstract interpretations of science are the result of an asymmetry in historical analysis, one that overdetermines the normative value of scientific theories at the expense of an underdetermination of both the role of experimentation and the social structure of the scientific community. In other words, despite the increasing importance of experiment in the construction of the physical sciences, most science studies tend only to emphasize the determinant role of theory in the construction and validation of scientific knowledge.

Philosophical and historical models of science which are constituted on the basis of the above kind of internalist conceptual analysis give the false impression that (a) practice is just a way of testing theories, or that (b) experimentation is always theory-laden. Also, models of scientific change that come out of traditional historical and philosophical perspectives on science lead to misunderstandings concerning the ways in which scientific change occurs and how scientific arguments are resolved. First, they neglect the fact that scientific knowledge is the result of both theoretical and practical presuppositions. Second, they undermine the contribution of scientific technology and technical skills in the resolution of controversies. Finally, they do not take into account the crucial importance of the social context, i.e., "the inextricable social component of scientific teamwork" (Galison 1987, x) and its influence in the closure of experiments and arguments.

The purpose of Galison's book is thus to offer an alternative view of science, one which tries to establish the symmetry in the philosophical, historical, and sociological analysis of the high energy physics laboratory after 1926. It describes the ways in which scientists test, destroy, justify, accept, and communicate their claims about the constitution of the microphysical world. According to Galison, the advantage of studying what physics has been from the end of the twenties on is that it allows us to look at the development of branches of physics other than the traditional positivist focus on special relativity and non-relativistic quantum mechanics. On the other hand, it shows how what counts as science changes, and how the sources used to legitimize scientific knowledge are determined by the historical, instrumental, theoretical, and even metaphysical stages of the development of science. The case studies chosen for analysis are the experiments based on macroscopic forces and effects, the small-scale scattering experiments built around cosmic rays and radioactive materials, and the giant accelerator-based experiments of the later twentieth century (Galison 1987, 13-14).

Although Galison recognized Hacking's contribution to the shift of attention from theory to experiment, I argue that Galison is not so much interested in defending the notion that "experiments have a life of their own." More important to him is to look at what happens in the physics laboratory and demonstrate, among other things, that "the road to experimental commitment has changed direction over the twentieth century" (Galison 1987, 13). He denies Hans Reichenbach's distinction between the context of discovery and the context of justification in science (Galison, 1987, 277). Galison thinks that the heterogeneous and complex web of practices, theories, presuppositions, argumentations, and collaborations that happen in laboratory practices rules out reductionist models. To use his own words:

> From a historical perspective the question of how experiments end commands our interest because it directs attention to that fascinating moment in the activities of the laboratory when instrumentation, experience, theory, calculation, and sociology meet. To understand how ends are constructed, we must narrow our historical gaze in order to identify the arguments, evidence, skills, and hardware that drive the investigators themselves to feel confident that they have gold in their pans, not pyrite (Galison 1987, 1-2).

The contrast between Galison's attitude and the ones expressed by the received view in the philosophy of science, the historical description of science as rational construction of theories (which is a postulate of positivism as well), or the studies of science done by the social constructivists is striking. In fact, Galison's perspective is an eclectic incorporation of some of the aspects of the previous views on science into a picture that illustrates the different levels of theory, experience, and instrumentation, as well as their dynamic interaction. Moreover, the relationships among these elements do not exist in the actual context of practice alone. They also exist at the more speculative, **a priori** stages of science, i.e., in the form of past **practical and theoretical presuppositions**.

One will notice that part of Galison's claims regarding the way science develops is similar to the views of Gaston

Bachelard. Bachelard believed in the dynamical, interdependent relationships among apparently diverse aspects of scientific work. Moreover, he also argued that (a) the philosopher of science has to go to the laboratory in order to actually observe the scientific mind at work; (b) the study of science implies the study of history of science and technology; and (c) the criterion for determining what counts as evidence and what counts as valid knowledge in science comes largely from scientific criticism and from the rules established by scientists themselves, instead of coming from mind-independent, transcendental truths. Bachelard's concept of phenomenotechnique is to a large extent, the illustration of most of these philosophical assumptions.

Both Bachelard and Galison realize that only interdisciplinary models of science account for the dynamics of scientific thought and practice, and the inextricability of all of its heterogeneous levels, i.e., theoretical, experimental, technological, and sociological. Phenomenotechnique is Bachelard's conceptual representation of such claims. It constitutes part of the first program in the epistemology of science to incorporate **avant la lettre** Galison's (and also part of Hacking's and Ackermann's) views concerning the need to focus on the role of scientific technology in the determination of what counts as scientific, at a time when the models suggested by the Anglo-Saxon positivists were accepted as the mainstream philosophy of science. In some respects, and because Galison has the advantage of a larger view of the development of theoretical and experimental physics, his explanatory model for scientific development is an important complement to that of Bachelard. Whereas Bachelard sees only one profound change in the status of scientific experimentation, i.e., from an individual pursuit to sets of standardized procedures and increasing degrees of specialization, Galison goes further than that. He shows that, within the standardized acceptance of experimental procedures, changes occur. These changes transform experimentation into a complex web of integrated levels which make experimentation **per se** from being insufficient to justify theory choice. For his part, Bachelard seemed to shift back and forth constantly between the view that experiments accurately test the validity of theories (and thus that theory and experiment are qualitatively independent from each other) and the view that experiments are material extensions of theory. Before analyzing the advantages and disadvantages of both thinkers, I now turn to the development of some of their main arguments.

What makes Galison suspicious of the traditional view in the philosophy of science is the fact that an analysis of experimental activities shows that the role of deductive and inductive arguments is insufficient to justify the closure of experiments. Even if the "textbook" version of science makes it seem that all the steps in the construction of scientific arguments are logically consistent, the fact remains that there are always elements within the context of scientific choice which escape logical analysis. Although focusing on practical rather than theoretical issues, this view is very similar to Kuhn's when referring to the underdetermination of rational mechanisms for theory choice inside particular paradigms and, especially, of decisions which involve paradigm shifts. Following Duhem, Quine, and Putnam, Galison says that "there is no **strictly logical** termination point inherent in the experimental process" (Galison 1987, 3), and thus the knowledge that an experiment achieved its natural conclusion. On the other hand, the choice of closing an experimental controversy is not totally optional either (Galison 1987, 4) because the laboratory context itself imposes sets of constraints on the experimenter:

> [The] asymmetry between experiment and theory is often hidden because experimentalists express their public claims in a language that suggests experimental results are independent of the researcher's judgments, experience, or skills. Reading an article, one could conclude that an effect would follow from an experimental setup with the inexorability of logical implication. But lurking behind the confidence of the experimental paper lies a body of work that relies on a

> kind of subtle judgment that is notoriously ill-suited for the prose of hypothesis and deduction (Galison 1987, 244).

The study of some of the constraining elements that shape the construction of scientific theories and experiments is perhaps one of Galison's most important contributions to science studies. It accidentally parallels quite closely some of Bachelard's claims about the need to construct a psychoanalysis of scientific objects. Bachelard always stressed that the philosopher of science has to bring to the surface the hidden elements that prevent science from progressing. He called these elements 'epistemological obstacles.' Within these obstacles Bachelard includes things such as past science, outmoded metaphysics of science, common knowledge, and beliefs. To Galison, the logical underdetermination of experiments has to do with sets of antecedent conditions that shape and direct what happens within the experimental context which is, by definition, heterogeneous. The problem underlying these conditions is that they have been "partially buried" (Galison 1987, 6) and only rise to the surface in times of controversy, i.e., when "the experimentalists themselves are forced to ask explicitly which of their data ought to be kept and which discarded" (Galison 1987, 6). Still, despite the experimentalists' ability to create devices that enable them to get rid of background effects, there is never the guarantee that the artifacts used in experimentation are totally free of disturbing elements.

The acknowledgment that the closure of experiments cannot be justified with rational arguments or the discarding of background effects alone implies that the consensus of the scientific community over experimental closure relies on other factors. Moreover, these factors are historically contingent in the Kuhnian sense, i.e., they depend upon the status of knowledge and the status of experiment, which in turn influence what counts as evidence:

> Our discussion of how experiments end means nothing if we are not faithful to the possible alternatives that were considered reasonable **at the time**. And at the time [of the experimental works of Anderson and Neddermeyer on the muon] there were **two** alternatives: quantum mechanics was correct and the particles were protons, or quantum mechanics was incorrect and the particles were electrons. The former appeared to be ruled out. **No one discussed new particles.** Quantum electrodynamics had been put to the test at high energies. It had failed (Galison 1987, 106).

Curiously enough, Galison's criticism of traditional philosophers and historians of science does not have so much to do with their neglect of experiment as with their naive assumptions regarding the adjucatory role of experimentation in theory choice. Carnap, for instance, thought that theory confirmation is assured by means of "progressive accumulation of observation reports" (Galison 1987, 7). By the same token, Popper's falsifiability criterion entails "the possibility of matching competing theories with the clear results of experimental evidence" (Galison 1987, 7). In fact, Popper argues that the demarcation between science and non-science rests in the capacity of scientific theories to be refuted by testing. Despite the fact that observations are already 'selective' instead of pure (according to Popper they incorporate the scientist's 'horizon of expectations', reference frames, interests, etc.), they are always unproblematic.[101] Verificationists like Carnap and falsificationists like Popper believe that experiments can accurately test theories. In both cases, the underlying assumption is that the interpretation of experiments does not suffer from the kinds of uncertainties (Galison 1987, 110), ambiguities and imprecisions inherent in scientific theories, and thus that they have adjudicatory power (Galison 1987, 8). Moreover, it also implies that experiments are 'provoked' by, but nevertheless independent from, theories. Hacking went even further when he said that experiments are not necessarily shaped by the theoretical context in which they occur. Galison's position regarding this issue is more complete than those offered by Hacking, Popper, and Carnap. Hacking's model of science, for instance, can be criticized on the ground that it does not

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[101] For more on the 'selective' character of scientific observations, see Karl Popper's Conjectures and Refutations: The Growth of Scientific Knowledge (1963).

account for the inextricability of the practical and the theoretical aspects of science. On the other hand, Popper's falsification criterion should not be applied to theories alone. In certain circumstances, it is "theory [that] seems . . . to disagree with experiment" (Heitler in Galison 1987, 103) and not the other way around. Other times, theory and experiment are "on a collision course" (Galison 1987, 106). Moreover, since the focus of the above philosophers is on scientific rationality, all of them neglect the psychological and sociological components of the scientific enterprise.

To Galison, Kuhn seemed to be on the right track when he reacted against the views of the logical positivists and offered a model of science which incorporated non-rational elements such as beliefs in the constitution of paradigms. He did not, however, provide a developed account of the psychological mechanisms behind scientists' conversions to new paradigms. As he puts it:

> I would like to suggest a different interpretation of the way that theory influences the outcome of experiment, one that depends neither on Gestalt-like selection nor on the large spread of random errors. The task of understanding the relation of theory to experiment is contingent on grasping the different **levels** of theory that are involved in the experimenter's work and on analyzing the various mechanisms that connect experimental work with elements of theory (Galison 1987, 69).

The sociologists of science, on the other hand, provided innovative insights into the non-cognitive aspects of science which, I argue, are elaborations of Kuhn's claims regarding the theoretical groundedness of paradigms. Writers such as Steven Shapin, Barry Barnes, Andrew Pickering, and David Bloor gave an account of scientific activities which, like Kuhn's, is also incommensurable with the idea that science can be explained by rational models.[102]

Barry Barnes thinks that an interest-theory account is sufficient to explain the way scientific practices are structurally and epistemically organized. The interest-theory thesis "denigrates the role of nature and supposes that scientists' presuppositions -- bolstered by their interests -- condition the admissible phenomena in such a way as to render a particular theory and its associated experiments closed and self-referential" (Galison 1987, 10). Pickering advocates the same kind of socially determined relativism in science. Like most of the social constructivists, he does not seem to take into account the contingencies of the physical world -- which would by itself

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[102] The same can be said of the sociologists Bruno Latour and Steve Woolgar in their Laboratory Life: The Construction of Scientific Facts (1979), and Bruno Latour in works such as Science in Action: How to Follow Scientists and Engineers Through Society (1987).

be adjudicatory in relation to theories -- but only the rhetorical tools used by the scientist to make pseudo-objective claims about the structure of matter. Obviously, the purpose of scientists' claims is to convince the rest of the scientific community that a particular theory or entity has more explanatory power than rival world views. Galison criticizes this kind of sociological fundamentalism by saying, for instance, that interest-theory accounts overestimate the flexibility of theories. Also, they "do not attend to the constraints of experimentalists' conclusions that are imposed by the skills and techniques of their work [which] do not suddenly alter all at once upon the introduction of new theory" (Galison 1987, 11).

As already stated, Galison makes an important contribution to the philosophy of science by taking the technical and material constraints imposed on experimentation into account. It constitutes a definite improvement over Hacking's philosophical attitude regarding the power of scientific experimentation and technology in issues such as those of scientific realism. Like the rationalists, Hacking implies that the experimental outcomes can provide the scientist with elements which are unambiguous and precise enough to produce changes in scientific theories that had no connections with current experimentation in the first place. In Galison's case, the interest is not to defend that it is theory which influences experiment or vice-versa. As it had happened in the case of Bachelardian epistemology, the treatment of the relations between experiment and theory is more sophisticated. Both Bachelard and Galison see the construction of science as the dynamic interplay of theory, practice and instrumentation.

In examining the ways in which theories influence the context of experiment, Galison is definitely adverse to the idea that theoretical presuppositions infect experiments by not allowing the scientist to be critical of what she observes. In an almost Popperian sense, theories are to Galison **fundamental** for the interpretation of practice:

> Theoretical presuppositions that "subvert" the full autonomy of experimental procedure are more than mere "biases" interfering with an otherwise clear view of nature. Without presuppositions, experiments can neither start or finish. . . . They embody what experimentalists know -- from the home truths of phenomenological laws and the empirically gained craft knowledge of materials to the metaphysical principles that the community may invoke to the admissibility of physical laws (Galison 1987, 12-13).

The constant presence of theory in laboratory practice is essential in several respects. First, theories are heuristic devices. They direct and help scientists find what they are looking for. On the other hand, it is impossible to conceive of any experiment which does not include, at least implicitly, theoretical presuppositions of some sort. In general terms, this view is not very different from the one advocated by Popper. But Galison's views on the qualitative diversity of elements which compose the experimental context is essential for understanding his claims that the reasons for why experiments end are not solely dependent upon the agreement of experiments with theories, or because experiments can be self-sufficient. For example, Galison synthesizes the reconstruction of the history behind the acceptance of the muon in the following way:

> Bit by bit theorists and experimentalists had to piece together their calculational techniques, their instruments, their data-reduction methods, and their underlying beliefs about theory to allow room for the new particle. As we follow the construction of their questions and disputes, we will be able to trace the construction of persuasive experimental arguments (Galison 1987, 17).

The reconstruction of the theoretical and practical assumptions behind experiments is fundamental because each stage in the development of physics implies layers of theory, experiment, and instrumentation. The shift of observational sensitivity from macroscopic to atomic and particle effects (Galison 1987, 15) is one such case. Microphysics itself was transformed by experimental discoveries after 1895: "cathode rays, X-rays, the Zeeman effect and radioactivity" (Galison 1987, 22). On the other hand, the historical circumstances underlying the heterogeneity of those layers in experimental situations makes them qualitatively and quantitatively different from the ones that are both consciously and tacitly accepted by today's scientific community.

Galison's arguments concerning the congenial relationships of theory and practice as illustrated above are not substantially different from the ones found in Bachelard's concept of phenomenotechnique. The similarity of some of their conceptions is even more striking when we connect them with the role of scientific technology in the determination of scientific reality.

As we have seen, to Galison there is a direct correlation between the status of experiment and the development of scientific instrumentation (Galison 1987, 25). Like Bachelard when referring to the usefulness of reconstructing the history of scientific techniques for a deeper understanding of science's progress, Galison thinks that the analysis of several centuries of 'material culture' of microphysics allows for the understanding of the segmentation of experiment and the changes in the "dynamics of experimental argumentation" (Galison 1987, 23). On the other hand, coordination (Galison 1987, 76) segmentation, fragmentation, and recombination of experiments are nothing but a consequence of both technical and sociological developments (Galison 1987, 19). The evolution and diversification of scientific equipment provokes alterations in the experimental context, which in turn results from the need to objectify scientific arguments. Galison thinks that machines themselves carry "the material embodiment of earlier theoretical presuppositions" (Galison 1987, 252). Similarly, to Bachelard "instruments are materialized theories." In other words, scientific technology is the **embodiment** of scientific postulates which help to construct the material and theoretical coherence of the experiment.

The idea that experimentation test theories and that theories influence the outcome of experiments is akin to both Bachelard's and Galison's philosophies of science. The experimentalists' dilemma, provoked by the influence of theory in the shaping of experimentation, is presented by Galison in the following terms:

> Without theory, [experimentalists] frequently will have neither a guiding qualitative sense of where the interesting physics lies nor any quantitative prediction about how large the effect ought to be. With their goal shrouded in darkness they may not be able to find the effect sought or to dissociate it from disturbances. In this sense, theories -- better, the many layers of theory -- provide essential, self-consciously imposed **constraints** on acceptable data. . . . On the other hand, given a quantitative prediction, the experimentalist is eventually forced to declare (at least implicitly) that there are no more systematic errors (Galison 1987, 73-74).

Theoretical presuppositions influence the treatment of data (Galison 1987, 50) and thus the experimental results (Galison 1987, 68). The assumptions of Einstein and de Haas on their confirmation of Ampère's hypothesis of the electric electron (Galison 1987, 47) are an example of theories which constitute 'self-consciously imposed constraints' on the experimentalist. These two scientists shared a theoretical belief: "that the current whirls were orbiting electrons -- which translated into a definite **quantitative** prediction" (Galison 1987, 69). Obviously, these beliefs shaped the way they constructed the experimental context.

Theories also include presuppositions that are not totally verbalized and could almost be seen as varieties of Polanyi's 'tacit knowledge'. In fact, Galison includes within the category of 'theory', levels of complexity that, with the exception of Bachelard and maybe Kuhn, most ordinary philosophers of science would not be willing to accept. Whereas the traditional approach to scientific theories consider theories as incomplete, imperfect, falsifiable, but somehow always correctable, Galison adds elements to their composition which do not allow the scientist to subject them to the same processes of rectification. These elements, which Galison considers to be fundamental for the understanding of the relations between theory and experiment, range from beliefs to metaphysical assumptions and issues of scientific realism regarding scientific entities.

The analysis undertaken by Galison of the ways in which "prior experimental and theoretical beliefs narrow alternatives of what the experimentalist takes to be reasonable beliefs and actions" (Galison 1987, 246) allows him to find three types of constraints. Long-term constraints include, for instance, metaphysical commitments, teleological explanations (Galison 1987, 247), kinds of experimentation (Galison 1987, 248), use of certain kinds of instrumentation, and argumentation (Galison 1987, 249). Middle-term constraints include particular theoretical and experimental programmatic goals and practices, and technological presuppositions (Galison 1987, 251). Short-term constraints include particular theories and models (Galison 1987, 252) and specific phenomenological laws (Galison 1987, 253). Galison thinks that there was, for example, a strong connection between Millikan's religious beliefs, his scientific realism (he "believed that he was measuring the charge on real electrons") (Galison 1987, 88), and "his theory of the origin of the elements" (Galison 1987, 86). Millikan's theoretical presuppositions included his "long-term commitment to the role of an ever-active, ever-present God, beliefs about the end of the universe, adherence to a particular kind of instrumentation, and the persuasive force of different types of experimental evidence" (Galison 1987, 17). Another example is Einstein's search for a unified field theory:

> Einstein had a vision of physics that demanded larger guiding principles to shape the kind of theory he thought worth pursuing. Principles of unity and simplicity were not ancillary qualities to the theories he would support -- they were the sine qua non of a **true theory** (Galison 1987, 50).

Theoretical presuppositions themselves are embodiments of everything, from theories and beliefs to past scientific practices. They dictate what counts as knowledge and influence the decision to end an experiment. As Galison synthesizes:

> By asking how experiments end, we are forced away from a mere recounting of results and toward the many strands of instrumental and theoretical beliefs that are locked in the practice of experimentation. We discover which instruments are new and unfamiliar, and which are practically extensions of the craftsman's hand. Among the levels of theories that set the constraints on experimentation are beliefs ranging from the grand, overarching metaphysical principles to the detailed models that soon fall by the way (Galison 1987, 74).

This point of view is similar to Bachelard's claim that epistemological obstacles influence scientific practices and prevent progress in science from occurring. Bachelard also said it is the set of metaphors, values, and world views existing in the unconscious of the scientist and embodied in theoretical assumptions which, influencing what is observed, **distort** the phenomena by transforming them into evidence to support metaphysical claims such as animism. One could then say that presuppositions take various forms and are intrinsic to the methodological and cognitive assumptions about the physical structure of the world. They dictate the relations between objects and how to interpret what is observed. These interpretations become naturalized, are assumed as objective and thus taken as independent from the material and epistemic context which produced them in the first place. The difference between the position of Bachelard and that of Galison is that Bachelard does not locate practical and theoretical constraints inside scientific technology. One of the obstacles to knowledge referred by Galison is the existence of errors in testing. **Ambiguity in experimentation** is an essential element of Galison's model of scientific practices. To the normal source of errors that come from lack of technological sophistication, Galison adds new sources of error which is very similar to Bachelard's concept of epistemological obstacles as applied to theories. In fact, just like Bachelard, he says that large number of errors are due to the existence of prior practical and theoretical commitments on the side of the experimenter. Going a step further, Galison argues that instruments themselves are embodiments of commitments. As a consequence, experimental traditions and "technological presuppositions" exert constraints on the interpretation of data. More than that, they can even be **"built into the apparatus itself"** (Galison 1987, 251). As Galison points out:

> The construction of a large-scale instrument must compromise between suitability for the physics at hand and flexibility for adaptation to the unknown . . . once they have built an instrument, experimenters have no choice as time goes on but to pursue their new problems within the material constraints of the given apparatus (Galison 1987, 265).

The other reason for error in science has to do with the existence, in instrumentation, of background effects. When random, they are fairly easy to detect and eliminate (Galison 1987, 69). The problem lies in what Galison calls 'systematic errors' which come from a variety of different sources, and leave "lighter footprints" (Galison 1987, 70). The detection by scientists of systematic background effects which are inherent to instrumentation becomes a matter of technique and is intrinsic to experiment: "controlling the background is not peripheral to the experimenter's craft; it is constitutive of the activity itself" (Galison 1987, 71). Hacking argues that one of the ways to distinguish between an artifact of the instrument and what is a scientific entity is by using instruments of different physical characteristics. By the same token, Galison thinks that "the consistency of different data-analysis procedures can persuade the high-energy physicist that a real effect is present . . . . Under sufficient variation any artifact ought to reveal itself by causing discrepancy between the different 'subexperiments'" (Galison 1987, 219). Thus, he thinks that it is not necessary to make use of Kuhn's concept of 'Gestalt switch' to explain changes in the acceptance of what counts as science. Once systematic errors are made visible and eliminated, epistemic changes occur naturally. In this context, the construction of instrumentation for the detection and controlling of errors becomes paramount. In fact, "once the instrument was tested against other instruments and against calculations of its performance, the device came to have a reliability of its own" (Galison 1987, 251). Rather than episodes of religious conversion to new paradigms, "increasing **directness** of measurement and growing **stability** of the results" (Galison 1987, 259) is a better explanation for why scientists **believe** in particular effects and entities.

Redesigning experiment and building instruments was always part of the scientific enterprise. In the realm of high-energy physics the task is much more difficult and costly than beforehand, so much so that "the drive to eliminate backgrounds would have to shift in part from the machinery itself and toward the reduction of data" (Galison 1987, 72). On the other hand, it requires different kinds of instrumentation, which in turn provoke changes in both the status of physical experimental methodologies and the problem of scientific realism in regard to entities (Galison 1987, 75). These issues can be easily related to Bachelard's phenomenotechnique, since this concept incorporates the idea that the study of reality is technically organized (Bachelard 1987, 160).

Technical innovations and instrumental complexities such as those represented by the construction of cloud chambers (Galison 1987, 112) and accelerators, which allowed for the production of new particles (Galison 1987, 125), can be seen as examples of phenomenotechnique. To Bachelard, they would constitute the "concretization of rational values" (Bachelard 1972, 13). To Galison, they are the **material embodiment** of theoretical and practical assumptions which, by coordinating and directing experiments, "helped fashion the nature of persuasive evidence" (Galison 1987, 131), and thus exerted constraints upon scientific knowledge. Elementary particle physics, for instance, was the result of the combination of theoretical assumptions (the quantum field theory by Richard Feynmann, Julian Schwinger, and Shinichiro Tomonaga) (Galison 1987, 126) and instrumental techniques (the construction of new accelerators). To Bachelard, phenomenotechnique implies that theoretical and material techniques, i.e., scientific theories and scientific technology, produce entities. He also thinks that in modern science one should talk in terms of technical determinism and constructed realism. These claims are not different from the claims made by Galison when referring to the fact that alternatives posed to the experimenter depend on prior instrumental and theoretical commitments (Galison 1987, 132).

Both Bachelard and Galison hold similar views in respect to the partial adjudicatory role of the scientific community in issues such as that of realism concerning entities. Whether referring to the discovery of the muon or to the acceptance of neutral currents, Galison says that, crucial to the closure of collective scientific arguments, that they are "satisfactory to the experimental community as a whole" (Galison 1987, 132). He also talks about the need for groups of scientists to reach consensus (Galison 1987, 131), and of "disciplinary control over disclosures" of scientific information (Galison 1987, 185). Consensus over the closure of experiments is to Galison the result of an "expanding circle of belief" (Galison 1987, 274). For his part, Bachelard talks of the "social exercise of a rational conviction" (Bachelard 1938, 245), 'consensus' within the scientific city, "social control" (Bachelard 1938, 241), 'mutual agreement', and "special censorship" (Bachelard 1975, 79).

Both Bachelard and Galison avoid talking in terms of certainty and truth. They both argue that changes in scientific knowledge are connected with the historical rather than conventional character of scientific practices (including theories and entities). Bachelard said, for instance, that, because of things such as technical organization, "corpuscles are very twentieth century" (Bachelard 1951, 87), and that realism is historically, and "rationally conditioned" (Bachelard 1951, 15). Galison rejects Barnes interest-theory and the Kuhnian Gestalt switch approach. He claims that, in science, changes occur because of the detection of systematic errors, the gradual stabilization of experimental results, and the gradual increase in the compatibility of practical and theoretical layers which overlap in the experimental situation. He also says that the recognition of the social interactions and the social structure embedded in the organization of science should not "force us to take a radically relativist stance towards experimental conclusions" (Galison 1987, 267).

Although referring to the notion of specialization as embodied in precise research programs, Bachelard does not raise the issue of competing groups within the scientific community. On the contrary, Galison claims, for instance, that the acceptance of particles such as the muon resulted from the collaboration of two research traditions with different layers of theory, kinds of demonstration (Galison 1987, 225), programmatic, experimental, and instrumental commitments: "the birth cry theory in the west, quantum theory in the east and Europe . . ., electroscopes and cloud chambers in the case of the West Coast, counters and coincidence circuits in that of Europe and the East Coast" (Galison 1987, 131). When competing groups such as these reach consensus, i.e, decide to end an experiment, it means that they "were at once rendering a verdict on instruments, experiments, high-level theories, and specific models" (Galison 1987, 131).

To Galison, the above issues are ones which belong to the sociology of experiment. Other issues raised by him in the same context are, for instance, science policy decisions (Galison 1987, 270), and particularly the division of labor for data analysis (Galison 1987, 265), i.e., the existence of sub-groups with different constraints, techniques, expertise that shape the way they approach data. To him, the division of labor is "where the sociological and the demonstrative structures of the experiment overlap" (Galison 1987, 267). These elements, not dealt with in depth by Bachelard, are nevertheless crucial, since they allow us to undertake a more complete analysis of the inextricability of cognitive and sociological components in the construction of scientific arguments.

Conclusion

To Bachelard, theoretical entities are 'social' in the sense that they depend upon the historical and contextual development of scientific theories and scientific technology.[103] Curiously enough, and as we have seen, Bachelard does not establish a rigid demarcation between science and technique. He believes that technology is the material expression of knowledge, and that knowledge is the abstract result of experimental verification. Technique is thus both theoretical and practical.[104] Instruments are materialized theories, because they embody the scientific rationality of a particular scientific culture. This allows Bachelard to claim that science produces its objects. To him, realism is only "the realism of the objects of an epoch" (Bachelard 1953, 115). Phenomenotechnique incorporates the notion that science is practice and technique, i.e., it depends upon technical (material and theoretical) expertise. The idea that the correct metaphysics for science is the intertwining of applied rationalism and technical materialism only reasserts the notion that technology is the embodiment of rationality. As a consequence, it also embodies the sets of rules which dictate what is to be taken as science. The criteria for determining what counts as scientific are established

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[103] The reasons why the social constructivists used some of the concepts of Bachelard has to do with the sociological overtones of some of his concepts.

[104] The demarcation between science and technique was also criticized by Jacques Ellul in his <u>The Technological Society</u> (1967)(1st French ed. 1954).

through consensual agreement by the scientific community. They also suffer from the contingencies inherent to scientific work. They are heuristic devices which go through change and are open to refutation. **Embodiment** is thus one of the most important notions embedded in Bachelard's claim that instruments are materialized theories. It is also one of the strongest connections between phenomenotechnique and the arguments behind Galison's explanation of how experiments end.

There are many similarities between Bachelard's and Galison's views about science. Given that Bachelard is mainly concerned with theory and the expression of technique in instrumentation, and Galison focuses on the context of experimentation, one might expect their conclusions to be quite different. But, as I tried to show in the above paragraphs, the views of Bachelard and Galison are complementary, indeed strikingly similar. The main difference lies in the motivations underlying their analysis of scientific practices, and the role they attribute to theoretical and practical presuppositions. Bachelard sees them as potential epistemological obstacles. Galison also sees them as something that shapes the way science is done, but not as something that the scientist can escape from, because they exist inside technology itself, i.e, **they have a life of their own**, just like Hacking had suggested in regard to experiment. In fact, Galison sees the several layers of constraints upon experimentation as something that science students should analyze in order to construct a more complete account of the way science is done. But he is a technological determinist. Bachelard's claims, on the other hand, are **normative**. He sees epistemological obstacles as intrinsic to the scientific world view, but argues that the task of the philosopher of science is to make scientists aware that they are, for instance, using metaphysical systems which no longer correspond to the philosophical implications of modern science.

The idea that the context of experimentation deserves special attention is no longer new. Works such as Franklin's The Neglect of Experiment, Ackermann's Data, Instruments and Theory, Hacking's Representing and Intervening were all concerned with this issue. The originality of Galison's work lies on his having inserted theoretical and practical presuppositions not only into the interpretation of data but also **inside scientific technology itself**. Whereas people such as Hacking argued that experiment has a life of its own, and that technology 'decides' which entities are real, Galison is interested in analyzing in depth the layers of theory that constrain the outcome of experiments and determine their closure. In other words, although Galison says in the beginning of the book that the theory-oriented philosophers are mistaken for neglecting the role of experimentation in the formation of scientific concepts, <u>How Experiments End</u> can be seen as representing a new way of emphasizing the importance of theory in the construction of science, one which does not demarcate theory from experiment but instead sees them as part of the same demonstrative process. In this respect, we can find strong connections between Galison's claims about the intertwining of theory, practice, and sociology, and Bachelard's concept of phenomenotechnique. Bachelard advocates a similar kind of intertwining by considering both technology and mathematics as manifestations of technique. As we have seen before, he claimed that "there is no sense in making a radical distinction between the scientific mind instructed by mathematics and the scientific mind instructed by physical experiment" (Bachelard 1938, 133).

The levels of theory and the sorts of experimental and instrumental presuppositions found by Galison are more elaborate than the ones presented by Bachelard. The former synthesizes his views on the closure of experiments in the in the following way:

> Experiments begin and end in a matrix of beliefs. Some are metaphysical, others programmatic, and yet others no more general than a formal or visualizable model. But laboratory work also exists amid practical constraints that may have little in the way of theory to support them: beliefs in instrumental types, in programs of experimental inquiry, in the trained,

> individual judgments about very local behavior of pieces of apparatus or the tracks, pulses and counts recorded everyday (Galison 1987, 277).

In this respect, the fundamental difference between Bachelard and Galison does not lie alone in the fact that the first makes normative claims and the second descriptive ones. The analysis of Galison's book shows that, just like Duhem and Kuhn, theory choice and the choice of closing an experiment is a matter of metaphysics and belief: belief in technology, methodologies, and so on. Galison's model is very sophisticated and he successfully showed how one needs to discard both the fundamentalist relativism of the social constructivists and the positivist idea that closures are logically grounded. However, he does it at the expense of rationality and objectivity. The case of Bachelard is somewhat different. He often said that epistemological obstacles are part of science. His concept of phenomenotechnique is the expression of his view that scientific technology is the materialization of theories, and the acceptance of entities produced by technology is a matter of commensurability between instrumentation, theoretical presuppositions, and the consensus of the scientific city. However, he never argued that technology embodies theoretical and experimental constraints in the same way as Galison does. Bachelard considered that the study of science implies the analysis of scientific practices and that, at the atomic level, subjectivity and objectivity interfere with one another. Still, this interference does not imply that rationality in science is no longer the criterion for theory and experiment choice.

Curiously, most of the claims that Galison makes in regard to the development of high-energy physics and particle physics are similar to the ones Bachelard makes in regard to quantum theory, relativity and modern chemistry. Phenomenotechnique implies that scientific entities are produced in the laboratory context, and that these entities are the consequence of the material embodiment of theories by technology. Thus, "objectivity in science becomes objectivity of what is abstract" (Bachelard 1949, 175), and scientific objects are "objects of thought that afterwards become objects of technical experiments" (Bachelard 1951, 79). Technique determines what is to be found, but rationality constructs the verification techniques. Contrary to what the social constructivists might assume, this does not mean that all rational constructs can be reified by technology. Just like Hacking and Galison, Bachelard too thought that background effects can be detected with technologies of different degrees of sensitivity, and that scientific techniques allow the scientist to distinguish between artifacts of the machine and objects of observation. As Bachelard pointed out:

> Little by little, the culture of objectivity determines an **objective subjectivism**. The subject, by meditating upon the object, eliminates not only the irregular traits of the object, but also the irregular attitudes of his own intellectual behavior (Bachelard 1970, 93).

In the case of Bachelard, the work of science is one of increasing rationality. Accounting for the experimental context, the constructive aspect of technological systems, and the epistemological obstacles which exist in the mind of the scientist and in the lack of technological sophistication do not imply that there are constraining agents independent from the scientist. Since technology is the materialization of theories and theories are created by the scientist, lack of rationality in science can only be attributed to the scientist.

Testing of theories by technology is still testing of theories against physical reality. Interest-theory accounts of science are not enough to explain why and when consensus is reached, or how is it that technology redefines the meaning of objectivity. Hacking's pragmatic realism does not take into account the inextricability, in the experimental context, of layers of theoretical and technical constraints. Galison, on the other hand, managed to achieve symmetry in science studies, by incorporating layers of theory, instrumental commitments, sociology and policy in his model. However, his claims concerning the beginning and the closure of experiments are still too close to Kuhn's and

Duhem's metaphysical assumptions, at the same time as they can be used successfully by the advocates of interest-theory accounts of science. On the other hand, if introduced into Galison's model, the concept of phenomenotechnique would help weaken the claim that the beginning and closure of experiments are strictly matrixes of beliefs; it would also preserve the historical rationality of scientific systems.

PART IV

GENERAL CONCLUSIONS

PART IV - GENERAL CONCLUSIONS

The epistemological works of the French philosopher of science Gaston Bachelard are of crucial importance for those who want to put science studies into historical and philosophical perspective. Bachelard was, in fact, the first philosopher of science to acknowledge that contemporary physical sciences such as relativity theory and quantum mechanics entail a renewed assessment of longlasting philosophical debates about science concerning issues such as realism, and positivism. He was also one of the few philosophers of science who developed the epistemic consequences of the discovery that, in certain areas of science, the demarcation between subject and object is much more fluid and ambiguous than both the traditional wisdom and the "received view" in the philosophy of science lead one to assume. To Bachelard, the new sciences, as well as the new technologies created for those sciences, question the orthodox models of what counts as knowledge, proof, and thus scientific objectivity.

The model of scientific change proposed by Bachelard takes into consideration aspects of the philosophical analysis of science that had been dealt with only peripherally by previous scholars. People like Henri

Poincaré argued that scientific theories are conventions rather than representations of physical reality, and Pierre Duhem pointed out that the concept of truth in science should be substituted for that of approximation. Auguste Comte argued for the historical contingency of society's norms, and Léon Brunschvicg claimed that the progress of science is discontinuous and that mathematics is one of the most important heuristic tools of theoretical physics. Bachelard, in his turn, developed these issues to their ultimate consequences. At the same time, he differentiated his model from the ones suggested by his intellectual antecedents by postulating that the relativistic picture of science represents at the same time an increasing conquest of objectivity and instrumental precision. On the other hand, he was the first to fully develop the implications, for science, of both scientific technology, and the restrictions imposed on experimentation and theory by the scientific community.

As I tried to show in the historical part of this dissertation, Bachelard's assessment of the implications, for philosophy of science, of theories like quantum mechanics relates directly to his knowledge of the scientific and philosophical speculations of scientists such as Werner Heisenberg, Niels Bohr, and Louis de Broglie, just to name a few. Among the concepts created by Bachelard to illustrate the constructive aspect of the physical and chemical sciences, 'technical determinism', 'constructed realism', and 'phenomenotechnique' are the ones which I consider to be most significant for contemporary philosophy of scientific experimentation and the philosophy of scientific technology. The present work was developed around the concept of phenomenotechnique, but it also connected the concept with other interrelated aspects of Bachelard's epistemology.

There were several reasons for the election of phenomenotechnique as the main focus of my research. One of them was its fairly frequent use by contemporary sociologists of science. Although apparently coherent in its overall application, the usage of the term by the social constructivists leads one into thinking that phenomenotechnique illustrates the constructivists' claims about the totally subjective, negotiated character of science, as well as it seemed to reinforce interest-theory, and actor-network theory accounts of scientific experimentation and theory choice. The other reason, which ties in with the analysis undertaken in Part III of two recent studies in the epistemology of science, is that phenomenotechnique can be almost considered as the ideal illustration of the highly complex, dialectical character of the relationships between theory and experiment in science.

The analysis of all the epistemological works of Gaston Bachelard showed that the concept of phenomenotechnique appeared at a particular time in Bachelard's philosophy of science and went through a specific conceptual evolution, both of which coincided with some of the most recent, revolutionary events in the history of the physical and chemical sciences. The first time that phenomenotechnique appeared in Bachelard's discourse, it was meant to illustrate the inductive, i.e., creative, power of mathematical physics. This was in 1931, in an article dealing with some of the epistemological and methodological implications of microphysics. Bachelard's concern for methodology gradually developed into an interest in the analysis of scientific entities, such as the ones produced with instruments for theoretical physics. The challenge that these objects represented in terms of their anti-intuitive character was similar to the one originated by the application of non-Euclidean geometries to relativity. Bachelard soon realized that some of the claims that he had made when dealing with the theory of relativity, i.e., that it represents a rupture with common sense and classical physics, could be extended to entities like the proton and the electron spin. By 1940 and from then on, phenomenotechnique became a more encompassing concept, one which included microphysical entities created in the laboratory. As materialized theories, instruments too started having a more fundamental role in the objectification of these entities, and thus in the redefinition of scientific realism. Bachelard also spent some time with the production of chemical substances. His works of 1932 and 1953, represent the first effort from a French philosopher of science to build an epistemology for chemistry. There too, the concept of phenomenotechnique appears to reflect the scientist's ability to purify and provoke 'unnatural' changes in the elements.

Bachelard's interest in the elements and in alchemy might have contributed to his shift of attention from scientific methodologies to scientific objects. Still, the fact remains that his interest for methodology never disappeared from his works and constitutes one of the signs of their epistemic consistency. Also, his notion of 'constructed realism' was already present in L'Essai sur la connaissance approchée (1928), and thus the idea that, as he put it, maximum rigor "coincides with the total substitution of the given by the constructed" (Bachelard 1987, 174). But the concept of phenomenotechnique became richer, since it became, as I said, more encompassing, in order to include the fabrication of entities, as well as the normative role of the scientific community.

The claim that science constructs its own objects and that these objects lose their meaning unless they are an integral part of a theoretical system entails a reflection on the characteristics of experimentation, and on the relations between theories on the one side, and scientific technology on the other. Moreover, it raises the question of whether or not the social constructivists legitimately linked phenomenotechnique to their claims regarding the strictly social, contingent, negotiated value of scientific practices.

The first set of issues was the one which provoked my analysis of the works of Hacking and Galison on the epistemology of scientific experiment. For instance, I tried to show that the dialectic between theory and experiment postulated by phenomenotechnique allows a less radical analysis than the one offered by Hacking in his Representing and Intervening. Hacking rightly criticized the neglect, by traditional philosophers of science, of the practical, instrumental aspects of science. But by focusing too much on the independence of the experimental context from the theoretical context he ended up falling into the same trap of prioritizing one aspect over the other. My claim was that, because most of Hacking's pragmatic realism coincided with the concepts behind phenomenotechnique, the incorporation of the concept into his philosophy of experiment could make his account more convincing, since it gave a dialectical, more symmetrical view of the scientific environment.

The analysis of Galison's *How Experiments End* was the consequence of the same preoccupation. The book is an excellent analysis of the way layers of theories influence experimental outcomes, and an important development of some of the problems that Bachelard had left behind, like science policy and the relations between groups of researchers with different methodologies of approach, instrumental commitments, and beliefs. But Galison's claim that experiments start and end in a matrix of beliefs was somewhat unsatisfactory because it led to the idea that science's theoretical and experimental outcomes are not rational. In this respect Galison's position is very similar to that of Duhem when he tried to separate metaphysics from science, but had to recognize that, when scientists are confronted with two equally good theories, the choice of one over the other is a matter of belief. It seems that Galison is making a similar point when he says that, once scientists chose particular kinds of instrumentation, both the metaphysical assumptions of the scientists and the beliefs embodied in technology direct the outcome of scientific practices. My claim was that, because there are so many similarities between Galison's epistemology and that of Bachelard, the incorporation of phenomenotechnique into Galison's model is legitimate and prevents it from being the expression of Galison's technological determinism.

The problem of the association of phenomenotechnique with the social constructivists, however, is not easy to solve. In fact, it is almost impossible to draw the line between the constructivists' claim that scientific inscriptions are not different from literary ones, and Bachelard's idea that "certain substances created by man have no more reality than the <u>Aenead</u> or the <u>Divine Comedy</u>" (Bachelard 1970, 83). On the other hand, Bachelard's substitution of the concept of objectivity for that of intersubjectivity seems to further emphasize the social character of science. Husserl used 'intersubjectivity' to explain the kind of consensus which exists among people regarding the world, but he also admits that this consensus does not prove that the world exists independently from the subjects. By the same token, if we transpose this idea to the claims of Bachelard concerning the intersubjective consensus in the scientific community about entities, we have to admit that, by itself, consensus does not guarantee that the things that are agreed upon actually exist. Obviously, the consequence of the above remarks is that phenomenotechnique has been correctly applied by the social constructivists, since because Bachelard himself was a social constructivist **avant la lettre**. But this label would be anachronistic. Bachelard's concept of intersubjectivity makes his philosophy of science closer to a variety of transcendental idealism, than of social constructivism.

Paradoxically, however, Bachelard's model is one of rationality and renewed objectivity. In this respect he sided with Heisenberg when the latter claimed that "the introduction of the observer must not be misunderstood to imply that some kind of subjective features are to be brought into the description of nature" (Heisenberg 1958, 137). Moreover, there is the clash of theories with reality. As Galison said when he criticized interest-theory advocates, the flexibility of theories cannot be overestimated. It is not the case that all theories can be embodied in instruments, because instruments themselves are subjected to the laws of the physical world. Both Galison and Franklin suggest that part of the criterion to distinguish artifacts from intrinsic characteristics of observables is to use technology which relies on different types of physical systems. Bachelard had made a similar point when he talked about the importance of instrumental scrutiny, and thus the constant state of awareness in the part of the observer for possible parasites or background effects. Instrumental testing is to him part of the strategy to determine if scientific hypotheses are, as he said, just dreams in the mind of the scientist, or have instead epistemic and explanatory value.

Bachelard's relativistic stance, i.e., both his claims that scientific entities are fabrications rather than representations of real things, and that instruments are materialized theories, are counterbalanced by his metaphysics of constructed realism. It is not the case that theoretical entities always remain such, i.e., with no impact on the physical world. Moreover, he thinks that the physical sciences of the present reached a degree of maturity unknown to past scientific practices. Thus, he is not an anti-realist in regard to theories either. The concept of phenomenotechnique, on the other hand, cannot easily be applied to the philosophical analysis of any science. The term was only meant to illustrate the creative potential of mathematical physics, and also the fabricated, instrumental character of certain scientific micro-objects. If it is correct to extend Bachelardian concepts like 'epistemological rupture' to sciences such as biology -- and in this respect Georges Canguilhem is a case in point -- the same cannot be said of phenomenotechnique.

Bachelard only applies phenomenotechnique to very restricted areas of science, i.e., theoretical physics, and contemporary chemistry. Interestingly enough, the social constructivists have applied the term to areas of macrophysics as well, leading one to believe that this was an integral part of Bachelard's constructive attitude towards science. But his views are again very similar to that of Werner Heisenberg. Just like the latter, Bachelard does not argue that the unavoidable mechanisms of interference of the measuring instruments in the observation of micro-entities makes science a subjective enterprise. He also believes that this interference does not have to be taken into account in the study of the macroworld.

Interestingly enough, both Hacking's pragmatic realism in regard to entities, and Galison's view that experimental outcomes are the result of layers of beliefs, metaphysical standpoints, and past instrumental commitments, are, as we have seen, quite close to the views of Bachelard concerning the dialectic between theory and experiment. However, as I have shown in Part III, the position of Hacking, for instance, is too radical. Since his preoccupation is to criticize the neglect, by philosophers of science, of the crucial role of experiment, he collapses into the extreme opposite of the position he attacks, thus neglecting the heuristic role of theories altogether. As a consequence, science becomes to him a form of engineering, since the ontological status of theoretical entities is gauged by the scientist's ability to manipulate them.

Galison's position is more balanced than that of Hacking. It offers a picture of theoretical physics where the role of theories in the experimental context is acknowledged as fundamental. However, his claim that beliefs are part of the layers of theory and that metaphysics is embodied in instruments, seems to postulate a form of technological determinism which weakens the rational status of scientific practices. The position defended by Bachelard, on the other hand, contributes to the enhancement of both the symmetry requirement postulated by Galison, and Hacking's pragmatic realism, without letting them underdetermine requirements of scientific rationality and objectivity.

Appendix. EPISTEMOLOGICAL WORKS OF GASTON BACHELARD

This bibliography was adapted from Jean Rummens, "Gaston Bachelard: une bibliographie," Revue Internationale de Philosophie 56(1963):492-504; Paul Ginestier, La Pensée de Bachelard (Paris: Bordas, 1968):18-21; Jacques Gagey, (1969). Gaston Bachelard où la conversion à l'imaginaire (Paris: Marcel Rivière et Cie, 1969):278-290; Vincent Therrien, La Révolution de Gaston Bachelard en critique littéraire, ses fondements, ses téchniques, sa portée (Paris: Klincksieck, 1970):359-385; Jean-Claude Margolin, Bachelard (Paris: ed. du Seuil, 1974):173-182.

Books

If different, the dates of the editions and/or the publishing houses consulted for this dissertation are shown in brackets at the end of the reference.

(1928) Essai sur la connaissance approchée. Paris: Vrin. [1987]

(1928) Étude sur l'évolution d'un problème de physique: la propagation thèrmique dans les solides. Paris: Vrin. [1973]

(1929) La valeur inductive de la relativité. Paris: Vrin.

(1932) Le Pluralisme cohérent de la chimie moderne. Paris: Vrin. [1973]

(1933) Les Intuitions atomistiques: essai de classification. Paris: Boivin.

(1934) Le Nouvel esprit scientifique. Paris: Alcan. [Presses Universitaires de France, 1978]

(1937) L'Expérience de l'espace dans la physique contemporaine. Paris: Presses Universitaires de France.

(1938) La Formation de l'esprit scientifique: contribution à une psychanalyse de la connaissance objective. Paris: Vrin. [1986]

(1940) La Philosophie du non: essai d'une philosophie du nouvel esprit scientifique. Paris: Presses Universitaires de France [English edition, New York: Orion Press, 1968]

(1949) Le Rationalisme appliqué. Paris: Presses Universitaires de France [1975].

(1951) L'Activité rationaliste de la physique contemporaine. Paris: Presses Universitaires de France.

(1953) Le Matérialisme rationnel. Paris: Presses Universitaires de France.

(1970) Études (posthumous recollection, presentation by Georges Canguilhem). Paris: Vrin.

(1971) Bachelard, epistémologie (selection of texts by Dominique LeCourt). Paris: Presses Universitaires de France.

(1972) L'Engagement rationaliste (posthumous recollection, presentation by Georges Canguilhem). Paris: Presses Universitaires de France.

Articles and miscellanea

"La Richesse d'inférence de la physique mathématique," Scientia 8(1928):37-39, reedited in (1972) L'Engagement rationaliste, Paris: Presses Universitaires de France, pp. 109-119.

"Noumène et microphysique," Recherches philosophiques 1(1931-1932):55-65, reedited in (1970) Études. Paris: Vrin, pp. 11-24.

"Essai de classification des intuitions atomistiques," Revue des cours et conférences, 1932. [not consulted]

"Physique et mathémathique," Septima Spinozana: Acta conventus oecumenici in memoriam Benedicti de Spinoza diei natalis trecentesimi Hagae Cornilis habiti. La Haye, Nijhof (1933):74-84. [not consulted]

"Le Monde comme caprice et miniature," Recherches philosophiques 3(1933-1934):306-320, reedited in (1970) Études. Paris: Vrin, pp. 25-43.

"Lumière et substance," Revue de métaphysique et de morale 41(1934):343-366, reedited in (1970) Études. Paris: Vrin, pp. 45-75.

"Pensée et langage," Revue de synthèse 8(1934):81-81 and 237-249.

"Valeur morale de la culture scientifique," 1934, *Actes du Congrès international d'éducation morale*, Cracovie [not consulted]

"Idéalisme discursif," *Recherches philosophiques* 4(1934-1935):21-29, reedited in (1970) *Études*. Paris: Vrin, pp. 87-97.

"Critique préliminaire du concept de frontière épistémologique," *Actes du huitième Congrès internationale de philosophie* 2-7 September 1934, Prague: Orbis (1936): 3-9, reedited in (1970) *Études*, Paris: Vrin, pp. 77-85.

"Le Surrationalisme," *Inquisitions* 1(1936), reproduced in (1972) *L'Engagement rationaliste*. Paris: Presses Universitaires de France, pp. 7-12.

"Logique et épistémologie," *Recherches philosophiques* 6(1936-1937):410-413.

"Un Livre d'un nommé R. Descartes," *Archeion* 19(1937):161-171, reedited in (1972) *L'Engagement rationaliste*. Paris: Presses Universitaires de France, pp. 15-26.

"La Psychologie de la raison," *Les Conceptions modernes de la raison*, (Entretiens d'Été, Amersfoort, 1938), 849, Paris, Herman, "Actualités scientifiques et industrielles", 1939, reedited in (1972) *L'Engagement rationaliste*, pp 27-34.

"La Psychanalyse de la connaissance objective," *Études philosophiques*. Gand: École des Hautes Études (1939): 3-13. [not consulted]

"Univers et réalité," *Travaux du IIéme Congrès des sociétés de philosophie française et de langue française*, Paris, Neveu (1939): 63-67, reedited in (1972) *L'Engagement rationaliste*. Paris: Presses Universitaires de France, pp.103-108.

"La Pensée axiomatique," *Études philosophiques* (1940):21-22. [not consulted]

"Compte rendu de Stéphano Lupasco, *L'Expérience microphysique et la pensée humaine*," *Revue philosophique de la France et de l'étranger* 132(1942-1943):155-158.

"La Philosophie de la mécanique ondulatoire," Neuchatel, Suisse: ed. des Trois Collines. 20éme Anniversaire de la

Mécanique Ondulatoire, Plaquette Commemorative publie sous les auspices du comité Louis de Broglie, 1944.

"La Philosophie scientifique de Léon Brunschvicg," *Revue de métaphysique et de morale* 50(1945):77-84, reedited in (1972) *L'Engagement rationaliste*. Paris: Presses Universitaires de France, pp 169-177.

"La Philosophie Dialoguée," *Dialectica: International Review of Philosophy of Knowledge* 1(1947):11-20. [later to be part of (1949) *Le Rationalisme appliquée*]

"Préface," in Cavaillès, Jean (1947). *Sur la Logique et la théorie de la science*. Paris: Presses Universitaires de France.

"The Philosophic dialectic of the concept of Relativity." Pp. 563-580 in Paul A. Schilpp, ed., (1949) *Albert Einstein: Philosopher-Scientist*. Evanston: The Library of Living Philosophers. Reedited (in French) in (1972) *L'Engagement rationaliste*. Paris: Presses Universitaires de France, pp 120-136.

"De la Nature du rationalisme," *Bulletin de la Société française de philosophie* 44.2(1950):45-86, reedited in (1972) *L'Engagement rationaliste*. Paris: Presses Universitaires de France, pp 178-190.

"Idonèisme ou l'exactitude discursive," *Études de philosophie des sciences (mélanges Ferdinand Gonseth)* (1950):7-10.

"Le Problème philosophique des méthodes scientifiques," *Actualités industrielles et scientifiques* 1126(1951), reedited in (1972) *L'Engagement rationaliste*. Paris: Presses Universitaires de France, pp 35-44.

"Les Taches de la philosophie des sciences," *Information philosophique* I.1(1951):1-19.

"L'Actualité de l'histoire des sciences," Paris, ed. du Palais de la Découverte, 1951, reedited in (1972) *L'Engagement rationaliste*. Paris: Presses Universitaires de France, pp. 137-152.

"L'Invention téchnique: réalités et possibilités," *Revue de synthèse* 74(1953):69-54. [From 1951, but not published until 1953.]

"La Vocation scientifique et l'ame humaine," Conférence du 3 septembre 1952 aux Rencontres Internationales de Genève, in _L'Homme devant la science_, Neuchatel, La Baconnière, et Bruxelles, Office de Publicité, pp 11-29. [not consulted]

"Le Nouvel esprit scientifique et la création des valeurs rationnelles," _Encyclopédie française_, t.XIX (1957): 19-14-12 to 19-14-15, reedited in (1972) _L'Engagement rationaliste_. Paris: Presses Universitaires de France, pp. 99-121.

"Préface" in Duhem, Jules (1959). _Histoire des origines du vol à réaction_. Paris: Nouvelles Éditions Latines.

"La Vie et l'oeuvre d'Édouard le Roy (1870-1954)," Académie des sciences morales et politiques, 15 février 1960, Paris, Firmin Didot, 1960, reedited in (1972) _L'Engagement rationaliste_. Paris: Presses Universitaires de France, pp. 43-46.

"Préface" in Cavaillés, Jean (1960). _Sur la logique et la théorie de la science_. Paris: Presses Universitaires de France.

WORKS from the _Société française de philosophie_ and published in the _Bulletin de la Société française de philosophie_

"La Continuité et la multiplicité temporelle," 37(1937):53-81. [not consulted]

"De la Nature du rationalisme," 44(1950):45-86. [not consulted]

With Alquié, Ferdinand. "Structure logique et structure mentale en Histoire de la Philosophie," t. XLVI (1953):89-132, exposition and discussion.

With Broglie, Louis de. "La Physique Quantique restera-t-elle indétérministe?," t. XLVI (1953):136-159, exposition and discussion.

With Dufrenne, Mikel. "Signification des 'a priori'," t. XLVII (1956):97-132, exposition and discussion.

With Favez Boutonier, Juliette. "Psychanalyse et philosophie," t. XLVII(1955): 1-41, exposition and discussion.

With Gueroult, Martial. "Brunschvicg et l'Histoire de la Civilization," t. XLVII (1954):1-36, exposition and discussion.

TAPED CONFERENCES and INTERVIEWS of Gaston Bachelard (O.R.T.F. Archives) [not consulted]

"Le Merveilleux scientifique," 13 dec. 1947. 8mn 20s.

"Les Sciences en 1848," 21-26 juin 1948. La science au XIX siècle, l'action sociale, le climat philosophique. Émission Les Grandes Conférences. 25mn.

"Les Sources multiples de l'invention," 21 mars 1951. 14mn.

"La Vocation du savant," 8 nov. 1952. Déclaration de Gaston Bachelard, Jean Lescure et Jean Amrouche. 25mn 30s.

"L'Homme et la science," 15 nov. 1952. Suite de l'entretien précédent. 27mn 25s.

"La Philosophie," 21 dec. 1952. Déclaration de Gaston Bachelard sur la philosophie contemporaine et l'apport de la science. 25mn environ.

"L'Alchimie au Moyen Age," 17 mai 1954. 12mn 30s.

"L'État des sciences au XIIe siècle," 24 mai 1954. 12mn 15s.

"Les Aspects philosophiques de la Relativité," 23 avril 1955, Hommage à Albert Einstein. 11mn 25s.

Bibliography

Revue de métaphysique et de morale

Aron, Raymond (1945), "La Philosophie de Léon Brunschvicg," 50: 127-140.

Bachelard, Gaston (1945), "La Philosophie scientifique de Léon Brunschvicg," 50:77-84.

Berthelot, René (1930), "Sur quelques philosophies des sciences dans la France contemporaine," 37:181-221.

Brunschivcg, Léon (1920), "L'Orientation du rationalisme. Représentation, concept, jugement," XXVIII.3:261-343.

---------. (1922), "Le Temps et la causalité," XXIX:1-13.

---------. (1923), "La Relation entre le mathématique et le physique," XXX.3:353-363.

---------. (1926), "La Philosopie d'Émile Meyerson," 33:39-63.

Capek, Milic (1952), "La Genèse idéale de la matière chez Bergson: la structure de la durée," 3:325-348.

---------. (1956), "La Théorie biologique de la connaissance chez Bergson et sa signification actuelle," 64:194-211.

Dagognet, F. (1965), "Brunschvicg et Bachelard," 1:43-54.

De Broglie, Louis (1920), "Determinisme et causalité dans la physique contemporaine," XXXVI:433-443.

---------. (1933), "Relativité et quanta," XL:269-279.

---------. (1940), "Récents progrès dans la théorie des photons et autres particules," LII:1-16.

---------. (1941), "Controversies Bergsoniennes. Les conceptions de la physique contemporaine et les idées de Bergson sur le temps et sur le mouvement," LIII.4:241-257.

---------. (1941), "Souvenirs personneles sur les débuts de la mécanique ondulatoire," LIII:1-23.

---------. (1945), "Léon Brunschvicg et l'évolution des sciences," 50:73-76.

---------. (1947), "La Métaphysique et les sciences au delà des mouvantes limites de la science," 52:277-289.

---------. (1949), "L'Espace et le temps dans la physique quantique," LVIII.2:113-125.

---------. (1951), "Un Nouveau venu en physique: le champ nucléaire," 56.2:117-127.

Delbos, Victor (1930), "La Préparation de la philosophie moderne," 37:289-335.

Gilson, Étienne (1956), "Souvenir de Bergson," 64.2:129-140.

Guillaume, E. (1920), "La Théorie de la relativité et sa signification," XXVII.4:423-469.

Hartmann, N. (1931), "Hegel et le problème de la dialectique du réel," XXXVIII.3:285-316.

Landgrebe (1964), "Husserl, Heidegger, Sartre. Trois aspects de la phénoménologie," 69.4:365-380.

Le Roy, E. (1953), "Ce que la microphysique apporte ou suggère a la philosophie," XLII.2:151-184.

Le Senne, R. (1927), "La 'Philosophie contemporaine en France' et la pensée de M. Parodi," XXXIV.1:81-114.

Metz. A. (1949), "La Physique des quanta et l'épistémologie Meyersonienne," LVIII.2:139-162.

Meyerson, E. (1916), "La Science et les systèmes philosophiques," XXXIX.1:203-242.

---------. (1924), "Le Relativisme, théorie du réel," XXI.1:24-48.

---------. (1930), "Le Sujet et le prédicat," XXXVII:150-180.

Parodi, D. (1924), "De l'Explication dans les Sciences par Émile Meyerson," XXXI:585-597.

Rey, Abel (1911), "Pour le réalisme de la science et de la raison," XIX:561-567.

Rosenfeld, L. (1972), "Le Conflit épistémologique entre Einstein et Bohr," 67:147-151.

Touini, V. (1962), "Nouvelles tendances réalistes dans l'intérprétation des théories physiques modernes," 67.2:152-162.

Tresmontant, Claude (1956), "Deux métaphysiques Bergsoniennes?", 64:180-193.

Ullmo, J. (1949), "Physique et axiomatique," LVIII.2:126-138.

Van Peursen, C.A. (1960), "Henri Bergson, phénoménologie de la perception," 3:317-326.

Wahl, Jean (1962), "Physique atomique et connaissance humaine," 62. 2:251-262.

Watanabe, S. (1951), "Le Concept de temps en physique moderne et la durée pure de Bergson," 56.2:128-142.

The Philosophical Review (arranged chronologically)

Paulhan, Fr. (1900), "Contemporary philosophy in France," IX.1:42-69.

Lalande, André (1905), "Philosophy in France," XIV.4:429-454.

---------. (1906), "Philosophy in France (1905)," XV.3:241-266.

---------. (1907), "Philosophy in France (1906)," XVI.4:357-386.

Le Roy, E. (1908), "Philosophy in France (1907)," XVII.3:291-315.

Lalande, André (1910), "Philosophy in France (1909)," XIX.4:373-394.

---------. (1911), "Philosophy in France, 1910," XX.5:515-534.

---------. (1912), "Philosophy in France in 1911," XXI.3:279-302.

---------. (1913), "Philosophy in France in 1912," XXII.4:357-374.

---------. (1915), "Philosophy in France, 1913-1914," XXIV.3:245-269.

---------. (1916), "Philosophy in France in 1915," XXV.4:523-545.

---------. (1918), "Philosophy in France, 1917," XXVII.5:443-468.

---------. (1919), "Philosophy in France, 1918," XXVIII.5:439-464.

---------. (1924), "Philosophy in France, 1922-1923," XXXIII.6:533-559.

----------. (1926), "Philosophy in France, 1925," XXXV.6:499-521.

Rey, Abel (1928), "Philosophy in France in 1926 and 1927," XXVII. 6:511-556.

Lalande, André (1929), "Philosophy in France in 1928," XXXVIII.6: 511-538.

Rey, Abel (1931), "Philosophy in France, 1929," XL.1:1-31.

---------. (1932), "Philosophy in France, 1930," XLI.1:1-36.

Lalande, André (1933), "Philosophy in France, 1931," XLII.1:1-30.

---------. (1934), "Philosophy in France, 1932," XLIII.1:1-26.

---------. (1935), "Philosophy in France, 1933-34," XLIV.1:1-23.

---------. (1936), "Philosophy in France, 1934-1935," XLV.1:1-25

---------. (1938), "Philosophy in France, 1936-1937," XLVII.1:1-27.

Parodi, D. (1940), "Philosophy in France, 1937-1938," XLIX.1:1-24.

Lalande, André (1946), "La Philosophie en France, 1942-1945," LV:1-23.

---------. (1948), "La Philosophie en France, 1946-1947," LVIII:35-53.

Books and articles

Abbott, David, (ed.) (1984), Physicists. London: Blond Educational.

Ackermann, R. (1985), Data, Instruments and Theory: a Dialectical Approach to Understanding Science. Princeton: Princeton University Press.

Bachelard, Gaston (1942), L'Eau et les rêves: essai sur l'imagination de la matière. Paris: José Corti.

---------. (1943), L'Air et les songes: essai sur l'imagination des forces. Paris: José Corti.

---------. (1948), La Terre et les rêveries du la volonté. Paris: José Corti.

---------. (1948), La Terre et les rêveries du repos: essai sur les images de l'intimité. Paris: José Corti.

---------. (1964 [1958]), The Poetics of Space. Boston: Beacon Press.

---------. (1972 [1936]), La Dialectique de la durée. Paris: Presses Universitaires de France.

---------. (1972 [1938]), A Psicanálise do Fogo. Lisboa: ed. Cor.

---------. (1979 [1932]), L'Intuition de l'instant. Paris: Gonthier.

Bachelard, Suzanne (1968), A Study of Husserl's Formal and Transcendental Logic. Evanston: Northwestern University Press.

---------. (1970), "Phenomenology and mathematical physics." Pp. 413-425 in Joseph J. Kockelmans, and Theodore J. Kisiel, eds., Phenomenology and the Natural Sciences: Essays and Translations. Evanston: Northwestern University Press.

---------. (1970), "The Specificity of mathematical physics." Pp. 426-440 in Joseph J. Kockelmans, and Theodore J. Kisiel, eds., Phenomenology and the Natural Sciences:

Essays and Translations. Evanston: Northwestern University Press.

Basalla, George (1988), The Evolution of Technology. New York: Cambridge University Press.

Beaune, Jean-Claude (1980), La Technologie introuvable: recherche sur la définition et l'unité de la technologie à partir de quelques modèles du XVIIIe et XIXe siècles. Paris: J. Vrin.

Belaval, Yvon (dir.)(1974), Histoire de la philosophie. Paris: Pléiade, Gallimard.

Benda, Julien (1950), De Quelques constantes de l'esprit humain: critique du mobilisme contemporaine (Bergson, Brunschvicq, Boutroux, Le Roy, Bachelard, Rougier). Paris: Gallimard.

Bergson, Henri (1944 [1907]), Creative Evolution. New York: The Modern Library.

----------. (1959 [1896]), Matter and Memory. New York: Anchor Books.

----------. (1965 [1922]), Duration and Simultaneity (With Reference to Einstein's Theory). New York: The Bobbs-Merrill Company, Inc.

----------. (1990 [1938]), La Pensée et le mouvant : essais et conférences. Paris: Presses Universitaires de France.

Bernstein, Jeremy (1973), Einstein. New York: The Viking Press.

Biezunski, Michel (1983), "Einstein à Paris." Pp. 267-293 in Thuillier et al. La Recherche en histoire des sciences. Paris: ed. du Seuil.

Bourdieu, Pierre, Jean-Claude Chamboredon, and Jean-Claude Passeron, (1991), The Craft of Sociology: Epistemological Preliminaries. New York: Walter de Gruyter.

Bochenski, I.M. (1951), La Philosophie contemporaine en Europe. Paris: Payot.

Bréhier, Émile (1950), Transformation de la philosophie française. Paris: Flammarion.

Brunschvicg, Léon (1897), La Modalité du jugement. Paris: Félix Alcan.

----------. (1912), Les Étapes de la philosophie mathématique. Paris: Félix Alcan.

----------. (1922), Expérience humaine et la causalité physique. Paris: Félix Alcan.

----------. (1927), Le Progrès de la conscience dans la philosophie occidentale. Paris: Félix Alcan.

----------. (1947), Les Âges de l'intelligence. Paris: Presses Universitaires de France.

----------. (1929), "Revue Critique: Étude sur l'Évolution d'un Problème de Physique: la Propagation Thèrmique dans les Solides, et Essai sur la Connaissance Approchée par Gaston Bachelard," Revue philosophique de la France et de l'étranger CVII:92-101.

Bohr, Niels (1961 [1929]), "The Quantum of action and the description of nature." Pp. 92-101 in Niels Bohr, Atomic Theory and the Description of Nature - Four Essays with an Introductory Survey. Cambridge: Cambridge University Press.

----------. (1935), "Quantum-mechanical description of physical reality," Physical Review 48:696-702.

----------. (1949), "Discussion with Einstein on Epistemological Problems." Pp. 201-241 in Paul A. Schilpp, Albert Einstein: Philosopher-Scientist. Illinois: The Library of Living Philosophers.

----------. (1963 [1958]), "Quantum Physics and philosophy: causality and complementarity." Pp. 1-7 in Niels Bohr, Essays 1958-1962 on Atomic Physics and Human Knowledge. New York: Interscience Publishers.

----------. (1960), "The unity of human knowledge." Pp. 8-16 in Niels Bohr, Essays 1958-1962 on Atomic Physics and Human Knowledge. New York: Interscience Publishers.

Boll, Marcel (1932), L'Idée générale de la mécanique ondulatoire et ses premières applications. Paris: Librairie Scientifique Hermann et Cie.

Canguilhem, Georges (1983), Études d'histoire et de philosophie des sciences. Paris: J. Vrin.

Capek, Milic (1961), The Philosophical Impact of Contemporary Physics. New Jersey: D. van Nostrand Company, Inc.

----------. (1971), Bergson and Modern Physics: a Reinterpretation and Evaluation. Dordrecht: D. Reidel Publishing Co. (Boston Studies in the Philosophy of Science, vol. VII).

Charlton, D.G., ed. (1983), France: a Companion to French Studies. London and New York: Methuen.

Chevalley, Catherine (1989), "De Bohr et Von Neumann à Kant: l'École allemande de logique quantique." Pp. 151-179 in Pierre Jacob, (dir.), L'Âge de la science: lectures philosophiques. Paris: ed. Odile Jacob.

Cline, Barbara Lovett (1987), Men Who Made the New Physics: Physicists and the Quantum Theory. Chicago: The University of Chicago Press.

D'Abro, A. (1951), The Rise of the New Physics. New York: Dover.

Davies, P.C.W.and J. R. Brown, (1986), The Ghost in the Atom: A Discussion of the Mysteries of Quantum Physics. New York: Cambridge University Press.

De Broglie, Louis (1936), La Physique nouvelle et les quanta. Paris: Flammarion.

----------. (1941), Continu et discontinu en physique moderne. Paris: Albin Michel.

----------. (1953), The Revolution in Physics: A Non-Mathematical Survey of Quanta. New York: The Noonday Press.

----------. (1955), Physics and Microphysics. New York: Pantheon Books.

Digeon, Claude (1959), La Crise allemande de la pensée française. Paris: Presses Universitaires de France.

Dirac, Paul, A.M. (1971), The Development of Quantum Theory. New York: Gordon and Breach Science Publishers.

Duhem, Pierre (1954 [1904]), The Aim and Structure of Physical Theory. Princeton: Princeton University Press. (2nd ed. with appendix, 1914).

Einstein, Albert (1949), "Autobiographical notes." Pp. 3-95 in Paul A. Schilpp, Albert Einstein: Philosopher-Scientist. Illinois: The Library of Living Philosophers.

-----------. (1961), Relativity: the Special and the General Theory. New York: Crown Publishers, Inc.

----------. (1963), Out of my Later Years. New Jersey: Littlefield, Adams & Co.

----------. (1973), Ideas and Opinions. New York: Laurel.

Einstein, Albert, B. Podolski, and N. Rosen, (1935), "Can Quantum-mechanical description of physical reality be considered complete?" Physical Review 47:777-780.

Frank, Philipp G. (1949), "Einstein, Mach, and logical positivism." Pp. 271-286 in Paul A. Schilpp, Albert Einstein: Philosopher-Scientist. Illinois: The Library of Living Philosophers.

Franklin, Allan (1986), The Neglect of Experiment. New York: Cambridge University Press.

-----------. (1990), Experiment: Right or Wrong. New York: Cambridge University Press.

Feyerabend, Paul K. (1984), "Mach's theory of research in its relation to Einstein," Studies in the History and Philosophy of Science 5.1:1-22.

Fine, Arthur (1986), The Shaky Game: Einstein, Realism and the Quantum Theory. Chicago: The University of Chicago Press.

French, A.P., ed. (1979), Einstein: A Centenary Volume. Cambridge, MA: Harvard University Press.

Fuller, Steve (1988), Social Epistemology. Bloomington: Indiana University Press.

Galison, Peter (1987), How Experiments End. Chicago: University of Chicago Press.

Gillispie, Charles C. (1990), The Edge of Objectivity: An Essay in the History of Scientific Ideas. Princeton: Princeton University Press.

Gooding, David., Trevor Pinch, and Simon Schaffer, (1989), The Uses of Experiment: Studies in the Natural Sciences. New York: Cambridge University Press.

Gunter, P. A. Y., ed. (1969), Bergson and the Evolution of Physics. Knoxville: The University of Tennessee Press.

Gutting, Gary (1989), Michel Foucault's Archeology of Scientific Reason. Cambridge: Cambridge University Press.

Hacking, Ian (1983), Representing and Intervening: Introductory Topics in the Philosophy of Natural Science. Cambridge: Cambridge University Press.

Hamelin, O. (1907), Essai sur les éléments principaux de la représentation. Paris: Félix Alcan.

Harvey, Charles W. (1989), Husserl's Phenomenology and the Foundations of Natural Science. Athens: Ohio University Press.

Hegel, G.W.F. (1953), Reason in History: A General Introduction to the Philosophy of History. New York: The Bobbs-Merrill Co.

Heisenberg, W. (1958), Physics and Philosophy: The Revolution in Modern Science. New York: Harper & Brothers Publishers.

Herman, Armin (1971), The Genesis of Quantum Theory (1899-1913). Cambridge, MA: The MIT Press.

Holton, Gerald (1992), "Ernst Mach and the fortunes of positivism in America," Isis 83.1:27-60.

Husserl, E. (1963), Recherches logiques. Paris: Presses Universitaires de France.

---------. (1964), The Idea of Phenomenology. The Hague: Martinus Nijhof.

---------. (1965), Phenomenology and the Crisis of Philosophy: Philosophy as Rigorous Science and Philosophy and the Crisis of the European Man. New York: Harper Torchbooks.

---------. (1970), The Crisis of European Sciences and Transcendental Phenomenology: an Introduction to Phenomenological Philsophy. Evanston: Northwestern University Press.

---------. (1977), Cartesian Meditations: an Introduction to Phenomenology. The Hague: Martinus Nijhoff.

Idhe, Don (1991), Instrumental Realism: The Interface between Philosophy of Science and Philosophy of Technology. Bloomington: Indiana University Press.

James, William (1981), Pragmatism. Indianapolis: Hackett Publishing Company, Inc.

Jammer, Max (1966), The Conceptual Development of Quantum Mechanics. New York: McGraw-Hill Book Company.

Jorgensen, J. (1970), "The Development of logical empiricism." Pp. 847-946 in Otto Neurath, Rudolf Carnap, and Charles Morris, eds., The Foundations of the Unity of Science: International Encyclopedia of Unified Science Vol. II Nos 1-9.

Jungnikel, C. and R. McCormmach, (1986), Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein, vol. 2. Cambridge, MA: The MIT Press.

Kerszberg, Pierre (1987), "The Critical philosophy of relativity," The Review of Metaphysics XLI.1.161:23-52.

Kojève, Alexandre (1990), L'Idée du détérminisme dans la physique classique et dans la physique moderne. Paris: Le Livre de Poche.

Krips, H. (1986), "Atomism, Poincaré and Planck," Studies in the History and Philosophy of Science 17.1:43-63.

Kuhn, Thomas (1970), The Structure of Scientific Revolutions. Chicago: University of Chicago Press.

Lakatos, Imre (1980), The Methodology of Scientific Research Programmes. Cambridge: Cambridge University Press.

Latour, Bruno, and Steve Woolgar, (1986), Laboratory Life: The Construction of Scientific Facts. Beverly Hills: Sage Publications.

Lepenies, Wolf (1988), Between Literature and Science: The Rise of Sociology. Cambridge: Cambridge University Press.

Loewenberg, J. (1932), "Meyerson's critique of pure reason," The Philosophical Review XLI.4:351-367.

Lupasco, Stéphane (1941), L'Expérience microphysique et la pensée humaine. Paris: Presses Universitaires de France

Mach, Ernst (1976 [1905]), Knowledge and Error. Dordrecht: D. Reidel Publishing.

Margenau, Henry. (1949), "Einstein's concept of reality." Pp. 245-268 in Paul A. Schilpp, Albert Einstein: Philosopher-Scientist. Illinois: The Library of Living Philosophers.

Margolin, Jean-Claude (1983), Bachelard. Paris: ed. du Seuil.

Mason, Stephen F. (1962), A History of the Sciences. New York: Collier Books.

McAllester, Mary (1989), The Philosophy and Poetics of Gaston Bachelard. Madison, Wisconsin: University of Wisconsin Press.

Merleau-Ponty, Jacques (1983), "Laplace, un héros de la science 'normale'." Pp. 173-192 in Thuillier et al., La Recherche en histoire des sciences. Paris: ed. du Seuil.

Meyerson, Émile (1921), De l'Explication dans les sciences. Paris: Payot.

----------. (1930 [1908]), Identity and Reality. London: George Allen & Unwin Ltd.

----------. (1931), Du Cheminement de la pensée. Paris: Félix Alcan.

----------. (1933), Réel et détérminisme dans la physique quantique. Paris: Hermann & Cie. Editeurs.

----------. (1985 [1925]), The Relativistic Deduction: Epistemological Consequences of the Theory of Relativity. Dordrecht: D. Reidel Publishing Co.

Miller, Arthur I. (1986), Frontiers of Physics: 1900-1911. Boston: Birkhauser.

Ory, Pascal, and Jean-François Sirinelli, (1986), Les Intellectuels en France, de l'affaire Dreyfus à nos jours. Paris: Armand Collin.

Pais, Abraham (1982), 'Subtle is the Lord...': The Science and the Life of Albert Einstein. New York: Oxford University Press.

----------. (1991), Niels Bohr's Times, in Physics, Philosophy and Polity. Oxford: Clarendon Press.

Parodi, Dominique (1925), La Philosophie contemporaine en France: essai de classification des doctrines. Paris: Félix Alcan.

----------. (1930), Du Positivisme à l'idéalisme: études critiques. Paris: J. Vrin.

Paul, Harry W. (1979), The Edge of Contingency: French Catholic Reaction to Scientific Change from Darwin to Duhem. Gainesville: University Presses of Florida.

----------. (1972). The Sorcerer's Apprentice: The Scientist's Image of German Science, (1840-1919). Gainesville: University Presses of Florida.

----------. (1985). From Knowledge to Power: The Rise of the Science Empire in France, 1860-1939. Cambridge: Cambridge University Press.

Pestre, Dominique (1984), Physique et physiciens en France: 1918-1940. Genève: ed. des Archives Contemporains.

Pickering, Andrew (1984), Constructing Quarks: A Sociological History of Particle Physics. Chicago: University of Chicago Press.

Planck, Max (1960), A Survey of Physical Theory. New York: Dover.

Poincaré, Henri (1968 [1903]), La Science et l'hypothèse. Paris: Flammarion.

Popper, Karl (1992), "Realism and the Aim of Science." From the Postscript to The Logic of Scientific Discovery. New York: Routledge.

Reichenbach, H. (1954), The Rise of Scientific Philosophy. Berkeley: University of California Press.

----------. (1978), Selected Writings: 1909-1953, vol.2. Dordrecht: D. Reidel Publishing Co.

----------. (1982), Philosophical Foundations of Quantum Mechanics. Berkeley: University of California Press.

Renaud, Paul (1934), Structure de la pensée et définitions expérimentales. Paris: Hermann & Cie. Éditeurs.

Rey, Abel (1907), La Théorie de la physique chez les physiciens contemporains. Paris: Félix Alcain.

Ruegg, Henri (1989), "Le Problème du réalisme en physique quantique." Pp. 129-150 in Pierre Jacob, (dir.), L'Âge de la science: lectures philosophiques. Paris: ed. Odile Jacob.

Sachs, Mendel (1988), Einstein versus Bohr: The Continuing Controversies in Physics. New York: Cambridge University Press.

Schilpp, Paul A., ed. (1963), The Philosophy of Rudolf Carnap. LaSalle, IL: Open Court.

Shapin, Steven, and Simon Schaffer, (1985), Leviathan and the Air-Pump: Hobbes, Boyle and the Experimental Life. Princeton: Princeton University Press.

Schuster, John and Graeme Watchirs, (1990), "Natural philosophy, experiment and discourse: beyond the Kuhn/Bachelard problematic." Pp. 1-48 in Le Grand, ed., Experimental Inquiries. The Netherlands: Kluver Academic Publishers.

Tint, H. (1964), The Decline of French Patriotism, 1870-1940. London: Weidenfeld and Nicolson.

Toth, Imre (1983), "La révolution non-Euclidienne." Pp. in 241-264 in Thuillier et al., La Recherche en histoire des sciences. Paris: ed. du Seuil.

Ushenko, Andrew Paul (1949), "Einstein's influence on contemporary philosophy." Pp. 609-645 in Paul A. Schilpp, Albert Einstein: Philosopher-Scientist. Illinois: The Library of Living Philosophers.

Vincenti, Walter (1990), What Engineers Know and How They Know It. Baltimore: Johns Hopkins University Press.

Weber, Eugene (1986), France: Fin de Siècle. Cambridge, MA: Harvard University Press.

Welton, Donn (1977), "Structure and genesis in Husserl's phenomenology." In F. Elliston and P. McCormick, eds., Husserl. Expositions and Appraisals. Notre Dame: University of Notre Dame Press.

Wright, Gordon (1981), France in Modern Times: From the Enlightenment to the Present. New York: Norton and Co.

Books and articles on Bachelard

Aron, Raymond (1965), Notice sur la vie et les travaux de Gaston Bachelard (1884-1962). Paris: Firmin-Didot et Cie.

Barreau, H. et al. (1974), Bachelard. Centre International de Cérisy-la-Salle. Union Générale des Éditions: 10-18.

Barzin, Marcel (1962), "Éloge de Gaston Bachelard," Lettres 23:341-343.

Bhaskar, Roy (1975), "Feyerabend and Bachelard: two philosophies of science," New Left Review 94:31-55.

Brosse, Jacques (1962), "Gaston Bachelard -- portrait," L'Arc 20:92-96.

Canguilhem, G. (1983), Études d'histoire et de philosophie des sciences. Paris: J. Vrin.

Catesson, J.(1955), "Autour d'un cogito scientifique. Gaston Bachelard, le matérialisme rationnel," Critique 95:352-360.

Champigny, Robert (1972), "Gaston Bachelard", in John K. Simon, ed. (1972), Modern French Criticism: From Proust and Valéry to Structuralism. Chicago: University of Chicago Press.

Christofides, C.G. (1961), "Gaston Bachelard's phenomenology of the imagination," Romantic Review 52:36-47.

Colloque du Centennaire (1984), Gaston Bachelard: l'homme du poéme et du théorème. Dijon: Éditions Universitaires de Dijon.

Dagognet, François (1986), Bachelard. Lisboa: ediçoes 70.

Delorme, Albert (1963), "In Memorian - Gaston Bachelard," Revue de synthèse 32:481-483.

Durand, Gilbert (1964), "Science objective et conscience symbolique dans l'oeuvre de Gaston Bachelard," Cahiers internationaux de symbolisme 4:41-59.

Egyed, Bela (1980), "Marxism, science and ideology," Social Praxis 7:91-127.

Faye, Jean-Pierre (1959), "L'Apport de Gaston Bachelard," Arguments 12-13:41-44.

Foucault, Michel (1972), "Bachelard, le philosophe et son ombre," Le Figaro littéraire 1376.

Gagley, Jacques (1969), Gaston Bachelard ou la conversion à l'imaginaire. Paris: ed. Marcel Rivière et Cie.

Gaukroger, Stephen (1976), "Bachelard and the problem of epistemological analysis," Studies in the History and Philosophy of Science 73:189-244.

Ginestier, Paul (1968), La Pensée de Bachelard. Paris: Bordas.

Granger, Gilles-Gaston (1947), "Visite à Gaston Bachelard, philosophe des sciences et des reves," Paru 26:55-59.

-----------, (1974), "Janus Bifrons," Nuova Corrente 64:203-219.

Grieder, A. (1986), "Gaston Bachelard: 'phénoménologue' of modern science," Journal of the British Society of Phenomenology 17.2:107-123.

Guilermit, Louis (1963), "Bachelard ou l'enseignement du bonheur", Annales de l'université de Paris 33:40-45.

Gutting, Gary (1987), "Gaston Bachelard's philosophy of science," International Studies in the Philosophy of Science 2:55-71.

Hyppolite, Jean (1964), "L'Épistemologie de Gaston Bachelard," Revue d'histoire des sciences XVII:1-11.

Jones, Mary McAllester (1991), Gaston Bachelard: Subversive Humanist (Texts and Readings). Madison, WI: The University of Wisconsin Press.

LaFrance, Guy, ed. (1978), Gaston Bachelard: profils épistemologiques. Canada: Presses Universitaires d'Ottawa.

Laméere, Jean (1938), "Notes et discussions: à propos du livre de M. Gaston Bachelard - La Formation de l'esprit scientifique: contribution à une psychanalyse de la connaissance objective," Revue internationale de philosophie 1:189-191.

----------. "In memorian - Gaston Bachelard (1884-1962)," Revue internationale de philosophie 17:3.

Lecourt, Dominique (1972), Marxism and Epistemology: Bachelard, Canguilhem and Foucault. London: Humanities Press.

----------. (1974), Bachelard ou le jour et la nuit (un essai du matérialisme dialectique). Paris: Bernard Grasset.

----------. (1974), Pour une critique de l'épistémologie (Bachelard, Canguilhem, Foucault). Paris: Maspero.

----------. (1978), L'Épistémologie historique de Gaston Bachelard. Paris: J. Vrin.

Lescure, Jean (1983), Un Été avec Bachelard. Paris: Luneau Ascot Éditeurs.

-----------. (1962), "Le Dernier cours de Gaston Bachelard," Le Figaro littéraire 861.

-----------. (1963), "Paroles de Gaston Bachelard: notes sur les dérniers cours de Gaston Bachelard à la Sorbonne," Mercure de France 348:118-125.

-----------. (1963), "Mon maitre: Gaston Bachelard," Marginales 32:69-73.

MacAllester, Mary (1984), "Bachelard twenty years on: an assessment," Revue de littérature comparée 58.2:165-176.

-----------. (ed.)(1989), The Philosophy and Poetics of Gaston Bachelard. Washington: Center for the Advancement of Research in Phenomenology & University Presses of America.

Margolin, Jean-Claude (1983), Bachelard. Paris: Écrivains de Toujours/Seuil.

Martin, Roger (1963), "Dialectique et esprit scientifique chez Gaston Bachelard," Les Études philosophiques 4:409-419.

Pariente, Jean-Claude (1985), "Rationalisme et ontologie chez Gaston Bachelard," Bulletin de la société française de philosophie 79:1-36.

Romeu, Pierre (1963), "Monsieur Bachelard," Annales de l'université de Paris 33:46-47.

Rummens, Jean (1963), "Gaston Bachelard: une bibliographie," Revue internationale de philosophie 4.56:492-504.

Smith, Roch C. (1982), Gaston Bachelard. Boston: Twayne Publishers.

Salomon, Jacques (1945), "Gaston Bachelard et le 'Nouvel Esprit Scientifique," La Pensée:47-55.

Therrien, Vincent (1970), La Révolution de Gaston Bachelard en critique littéraire: ses fondements, ses techniques, sa portée. Paris: Klincsksiek.

Tiles, Mary (1984), Bachelard: Science and Objectivity. Cambridge, MA: Cambridge University Press.

Vadée, Michel (1975), Gaston Bachelard ou le nouvel idéalisme épistémologique. Paris: Éditions Sociales.

Voisin, Marcel (1968), "Bachelard," Cahiers rationalistes 7. 257:177-216.

Weber-Pignet, J. (1941), "Gaston Bachelard - La Philosophie du Non," Revue philosophique de la France et de l'étranger 66:440-447.

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FOREIGN LANGUAGES: Portuguese, French, English, Spanish, and Italian

EMPLOYMENT HISTORY

Assistant Professor: Appointed Assistant Professor of Philosophy, Grand Valley State University, Michigan, beginning September 1993.

Instructor for PHIL 2606:Reason and Revolution in Science', Philosophy Department, VPI&SU (1991-1992).

Discussion leader for PHIL 1204:Knowledge and Reality, Philosophy Department, VPI&SU (1989-1990; 1992-1993).

Research assistant: Graduate Program in Science and Technology Studies, VPI&SU. Assigned to the Center for the Study of Science in Society (1991); Summer Seminar in Philosophy of Biology for College Teachers, Department of Philosophy, VPI&SU (1989).

Teacher at the Liceu, Lisboa, Portugal:philosophy, psychology, sociology (1980-1988).

PUBLICATIONS

"The epistemology of Gaston Bachelard and the French scientific and intellectual environment in the first half of the twentieth century." Forthcoming in Jean Gayon, ed. Paris: J.Vrin (21 pgs. typescript, in French)

"Humanism in Shakespeare: two tragedies (Romeo and Juliet and Hamlet)." Forthcoming in Revista Portuguesa de Filosofia. Ed. by the College of Philosophy, Braga, Portugal. (31 pgs. typescript, in Portuguese)

Principles of Psychology. Lisboa, Portugal: Ed. Platano. First edition 1988. Second edition, 1989. Third and Fourth editions, 1990. Fifth and Sixth editions, 1991. (295 pgs., in Portuguese)

WORKS IN PROGRESS

"The Scientific Philosophy of Gaston Bachelard." In review.

"Twentieth century philosophies of architecture and the ecological approach to visual perception." In review.

"Health policies and State intervention: a story of changing attitudes toward health care in Portugal (1944-1983)." In review.

INVITED PRESENTATIONS

"L'Épistémologie de Gaston Bachelard et le milieu scientifique et intellectuel français dans la première moitié du 20ème siècle." Colloquium L'Actualité Épistémologique de Gaston Bachelard, Université de Bourgogne, Dijon, France, January 1992.

PRESENTATIONS

"The Hunting of a context for Gaston Bachelard's scientific philosophy." Science Studies Program, Virginia Polytechnic Institute and State University, Blacksburg, February 3, 1993

"The impact of the 'scientific-technological revolution' on Soviet Science Policies." Society for Social Studies of Science, MIT, Cambridge, Massachusetts, November 1991.

"Gaston Bachelard's socio-historical epistemology." History of Science Society Annual Meeting, Seattle, Washington, October 1990.

"The Scientific Philosophy of Gaston Bachelard." 7th Annual Graduate Symposium, Virginia Polytechnic Institute and State University, March 1991.

RESEARCH INTERESTS

French Epistemology (Philosophy, history and sociology of science and technology)

Portuguese Public Health (History and politics of public health institutions)

Theories of Architecture (History, philosophy and psychology of architecture)

PROFESSIONAL SERVICE

Referee for Society for Philosophy and Technology
Referee for Perspectives on Science: Historical, Philosophical, Social
Consultant for Foreign Language Requirement

ADDITIONAL TRAINING

"The Professor as Researcher" (workshop), January 7-8, 1993
"Training the Future Professoreate" (workshop), August 1992

PROFESSIONAL ASSOCIATIONS

American Philosophical Association
Society for Philosophy and Technology
International Society for the History, Philosophy and Social
Sociedade Portuguesa de Filosofia
History of Science Society

Maria Teresa Castelão Pereira
April 1993