WHO KILLED HISTOLOGICAL POSITIVISM? AN APPROACH TO CLAUDE BERNARD'S EPISTEMOLOGY

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ABSTRACT. Along with numerous scientific contributions, Claude Bernard left a lucid and detailed defense of the applicability of the experimental method in physiology. Bernard explained that a rational therapeutic was absolutely necessary in clinical work, that this brought a pathology based on critical analysis of empirical data, and that such pathology would reach genuine knowledge when physiology had been freed from dogmatic attitudes. He set up a program, attacked by currents of thought hostile to the suppositions involved. The confrontation usher two tasks: first, to dismask the weakness of such currents; then, to define the role of conceptual-free invention and, likewise, how the methodological and epistemological guarantees of future physiology should act. This article examines such complex task; it enlarges on the reasons which have led Bernard to be seen as a precursor of Popper's philosophy of science, and it pays special attention to the common factors in the historical context in which both Bernard and Popper developed their thoughts.

KEY WORDS. C. Bernard, experimental physiology, hypotheses, Positivism, Vitalism, cellular theory, Empiricism, histology, Rationalism, K. Popper.

INTRODUCTION

I shall begin with a simple game. I will give the reader four short quotes and he or she must ask him or herself which might come from texts written by Claude Bernard and which from works by Karl Popper. The quotes are these:

1st. "Without a hypothesis, in other words, without an anticipation of the facts, there is no science [1]."

2nd. "Theories are no more than provisional ideas which we have about things in a particular state of our knowledge... Progress consists of endeavouring [to criticise them]... [2]"

3rd. "We should always be ready to criticise a theory, we should always imagine that it is vulnerable; consequently, we should not believe [blindly] in any theoretical principle [3]."

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And 4th. "When we make a general conception of science, the only thing we can be sure of is that all theories are false. They are no more than partial truths which we find necessary as they are like steps which we use to advance in our research... [4]"

The attentive reader will probably have realized that none of these fragments were written by Popper. In fact, all quotes have been taken from two of Bernard's works: the *Introduction to the Study of Experimental Medicine* and the *Principles of Experimental Medicine*. However, anyone who knows well Popper's philosophical world will, I think, agree that any of the quotes given above could easily form a part of an article, a conference or a book by sir Karl. Of course, this is not enough to establish a relationship between the conclusions they reached concerning certain methodological and epistemological aspects of science. To do so requires more than finding statements taken out of context, although they seem to say the same things. We must ensure that these statements, within the respective scientific and philosophical environments where they were formulated, share, in fact, a fundamental base. The objective of this paper is to make this comparison. I will rebuild the historical framework of the Second Empire in which Bernard worked and in which it was affirmed that more than a physiologist he was *Physiology* itself. I should like to point out that if I were to stop at the affirmation that Bernard attacked inductivism to bring forward a non-verificationist—and, to a certain extent, Popperian—interpretation of the experimental method, I would merely be pointing out something which is already sufficiently well known. I should mention here the contributions made by Canguilhem, Grmek, and especially César Lorenzano, in the study of this fact [5]. Even so, I should like to go further still, because, together with this suggestive similarity which must, naturally, be taken into account, there are other, no less outstanding, similarities.

THE INTELLECTUAL AND SCIENTIFIC ENVIRONMENT

Claude Bernard lived between 1813 and 1878. When he finished his university education in 1843, it did not take long for him to realize that he was much more interested in research than in the clinic. Before finishing his studies, he had already begun to work alongside Magendie, the most significant figure at that time in French physiology. He became a doctor of science in 1853 and, the following year, he was awarded the Chair of Physiology, which had been created for him in the Sorbonne. A little later, he took over from Magendie as professor in the College of France and was later made director of the Department of General Physiology in the Natural History Museum in Paris. He published more than two hundred scientific works concerning innumerable physiological problems; however, his contribution to physiology was centered on five clearly defined

areas: the study of the function of the pancreas in digestion; the explanation of the glucogenic function of the liver; research into the processes involved in the regulation of the temperature of the human body; the examination of the function of the *internal medium* in the conservation of the conditions necessary for the life of cells, and finally, the presentation of the first works which would give birth to toxicology. Whilst all of this was tremendously important for the history of biological science—given that the results obtained by Bernard showed the efficiency of the method used to practice and understand physiology—Bernard's most significant contribution to life sciences consisted in establishing the exact methodological conditions used as basis for his work; in defining the critical requirements for theoretical creation, and in establishing the nature of the epistemological framework for applying experimental research in the examination of physiological processes.

Bernard was not the first person to carry out experiments in physiology. The Alexandrian doctors, Galen, Harvey, or Boerhaave and von Haller—who were much closer to him—repeatedly applied experimental techniques and reached significant conclusions as a result. Bernard's contribution to physiology was a detailed and rigorous justification of the absolute need for the inclusion of an experimental focus, and he set up a teaching program, as we shall see, surrounded by currents of thought which were openly hostile to the different suppositions implied. It is not easy to make a summary of the state of physiology in the first third of the ninetienth century, but we can say—if we simplify things—that Bernard had to define his own position by confronting, in essence, three precise approaches which could be found in three fundamental chapters of biology—positions that had a great weight in France: the vitalism of the histologists who formed the group of Montpellier; the positivism of the school of microscopic anatomy at the University of Paris, and the physiological empiricism which was represented non other than by its direct master, François Magendie. Perhaps we should add that the mechanical biology of the XVII century was at that time considered to be a futile and sterile essay, whose principles and ideals were best forgotten.

In fact, the influence of the theories and successes of the mechanical philosophy on biological theories has been very noticeable for more than one hundred years. The corpuscular hypotheses; the reduction of biological phenomena to processes in which everything that could be found was no more than new forms of matter in movement governed by principles of conservation; the possibility of applying mathematical analysis and an experimental focus in the area of biomechanical systems—which had previously been considered to be irreducible—together provoked an ingenuous optimism which, anyhow, did not take long to fade away. The fecundity of *mathematical physiology* was seen to be very limited. It led to a

dead-end and was only successfully applied to a small number of problems, related to the way in which the muscles and the skeleton were involved in the movement of animals.

Biological physics would have less and less influence as the XVIII century progressed and general anthropological conceptions were more widely studied, such as those defined by La Mettrie in *Machine Man* or by the Baron d'Holbach in his *The System of Nature*. Geometrical physiology was a failed attempt which, despite everything, left for future science its certainty that natural legality also reached the region of vital phenomena; that these were always subject to an order and a causal determination similar to those which govern other natural phenomena; its confidence in biophysics which had converted observation, and especially experiments, into the means of gaining knowledge regarding laws, believing that the basic entities which make up living beings were susceptible to experimental explorations. It was a legacy which the majority of physiologists rejected during the second half of the century and, consequently, could not have reached Claude Bernard directly. However, Bernard would become the undisputed heir to many of these Cartesian objectives.

From the time of Paracelsus, the theory of biological functions counted on another tradition of research, which Jan Baptista Van Helmont decisively contribute in conserving during the period which saw the birth of modern science. It formed part of a vision of nature which, taken as a whole, meant a global challenge to the notes that incorporated the image of the universe-machine. Physiological tradition evolved at the heart of this panvitalist doctrine where natural dynamics had nothing to do with local displacements, for which the last components of the cosmos were not substances but dynamic dispositions, and where discursive knowledge that can be based on logics or mathematics had no interest because it had no value. Authentic knowledge was only considered to be the *intuitive* understanding that each individual was able to acquire within the individual powers that, it was imagined, populated the world; to put it another way, knowledge came from a subjective communion with nature, in unique acts which were incommunicable and only explicable through allegory.

This panvitalism was converted to vitalism through the work of Stahl—a professor in Halle and doctor to the Prussian court from 1716. The reader may have heard of Stahl as the creator of the flogist hypothesis, but our interest here is his open opposition to mechanism and the radical animist inspiration introduced in his theory on organic functions. He maintained that the functional integration which can be observed in any organism, that can unquestionably affect chemical and mechanical processes, was due to the presence and efficiency of a supramechanical and suprachemical foundation: the vital principle. Death is no more than the extinguish-

ment of the physiological order, when the mechanical or chemical phenomena act in an autonomous, blind and *dis-animated* [6] way. It is a principle which conserves the chemical components of the living body, sustaining the biological organization in time. In other words, in the same way that the principal ally of mathematical physiology had been the idea of permanence of order in space, vitalist physiology would make the vital force responsible for the preservation of order in time. Stahl was convinced that the active principle for all animal and vegetable life would remain hidden from mechanics. The correct understanding of a living being required a description of the chemical activity which took place therein, but this activity did not itself provide the reason for subsistence; such subsistence was supported and directed by an immaterial agent.

Vitalism, as a metaphysical horizon of physiology, had a large number of followers in Europe, although its most relevant centre for development throughout the eighteenth and nineteenth centuries was in the University of Montpellier, where Bordeau, Barthez and Bichat were studying and teaching. There was already a physiological school present and active within the historic scenario in which Bernard was to be educated. Bichat became the most respected figure in a new system which proposed a differentiation between the vital principle within different vital forces inherent in each tissue. Given that in his *General Anatomy*, of 1802, he maintained that there were twenty-three different tissues, this meant that there would also be the same number of different vital forces in the human body. The tissue would, therefore, be the fundamental morphological and physiological unit for biologists or vitalist doctors. Fibrillar mechanics had given way to histological vitalism. All the tissue structures react to modifications in their surroundings completely spontaneously and independently from physical or chemical laws and conditions. Those unavoidable differences between life and matter, intuition and concept, instinct and intelligence, metaphysics and science—with which Bergson would later build his philosophy—geared towards spontaneity: one of the thesis that Claude Bernard would fight against in all his works.

Despite the fact that it is still present in biological theory, vitalism began to be questioned from positivist and empiricist standpoints in the second third of the XIX century. In France, the anti-vitalist reaction found decisive support in the thinking of Comte, who was critical of any kind of metaphysical commitment or speculative intent in natural science. Comte's philosophy was respectfully accepted in histological research, but it conditioned the advance of microscopic anatomy—paralysing it—as such anatomy had until then been concerned with the evaluation of cellular theory. Charle-Philippe Robin, the leading professor in histology in the Faculty of Medicine in Paris, shared Comte's attitude towards cellular

theory; an attitude which was summed up as follows in the XLI lesson of the *Positivist Philosophy Course*:

> ... these ambitious spirits—Comte was referring to the German cytologists—have endeavored to go beyond the natural term of anatomical analogy, endeavoring to form the same generating tissue from the chimerical and unintelligible assembly of a kind of organic monads which would, therefore, be the true primordial elements of all living bodies [...]. To my understanding, it would be impossible to imagine a more profoundly irrational concept in the anatomical order nor one that would be more likely to directly obstruct the true progress of science [7].

Despite the criticism that Comte had also aimed at the use of the microscope in anatomical research, Robin managed to make him a skillful user of such instrument, although he never stopped being openly opposed to cellular theory; and he was always sure as to which was the correct decision if the choice was between *positive science* and *German metaphysical anatomy*. In this atmosphere, Bernard would show, from the beginning of his career in research, a profound interest in cytology, until he was certain that physiology—"modern" physiology, he will have called it—would have to be built on the confluence between physical and chemical sciences and cellular theory. Or, to use his own words: "it is only in the anatomical structure and the physical and chemical analysis of the properties of organized matter that physiology finds the conditions it is concerned with knowing [8];" however, "the elements of the organism, whatever its actual state, have the cell as their origin [...]. The cell is, thus, the vegetable and animal *anatomical element* [9]." Consequently, Bernard emphasized over and again that everything that manifested itself in a complex way in living beings had its origin in a simpler sub-layer of cellular activity or, to put it another way, that to interpret the functions of an organism it is necessary to know the functions of the cell. He did not therefore identify himself with anticytological positivism which influenced the opinions of many of his colleagues at University in Paris, and he expressly pointed out what he least liked about Comte's philosophy: "positivism which, in the name of science, repudiates philosophical systems, shares with them the disadvantage of being a system [10]."

To conclude this brief outlook at the intellectual and scientific environment in which Bernard lived, we must mention the fact that, for all the years in which he was one of Magendie's disciples and collaborators, he would listen almost every day to his teacher's passionate defense of militant empiricism. Magendie defended and practised an intransigent empiricism, the enemy of any theoretical systemization, whether as the promoter or the result of observation. He accepted generalizations—provided they were not premature—using what he denominated *material*

collected in experience, but he rejected the fact that ideas or hypotheses could mark out the path for a scientist. Facts should speak for themselves, without being associated with any preconceived notion, because the truth would always finally be found in these facts. Bernard remembered having heard the following:

> Each one of us generally compares himself within his sphere with someone more or less grandiose, Archimedes, Michelangelo, Newton, Galileo, Descartes. Louis XIV compared himself to the sun. I am much more humble, I compare myself to a rag and bone man, my hook in my hand and my basket on my back, passing through the roads of science and picking up whatever I find on the way [11].

Magendie, to sum up, not only considered an obstacle to believe that theory—theoretical expectations or hypotheses—came before experience, he even believed it was possible that this was not the case. It was enough to observe, establish the facts, be guided by them and allow them to speak for themselves. Suppositions, regardless of how they acted, could only corrupt observation.

These were the currents of thought, the schools and attitudes or opinions which were most widely accepted during the years in which Bernard was studying. In the face of these tendencies, he endeavored to define what conditions would be necessary for physiology to become part of experimental science; conditions which, in turn, would allow medicine to exist free from doctrines, systems and uncontrollable assumptions. At the beginning of the nineteenth century, medicine had no critical nor anti-dogmatic basis. Bernard liked to use an anecdote, attributed to Laplace, when he wanted to explain this. On a certain occasion, someone asked Laplace: "How can you admit doctors in the Academy of Science? None of them are truly wise. And medicine is not even a science; it is still no more than mere empiricism." To which Laplace replied: "I do not admit doctors into the Academy of Science because they are wise, but rather to allow them to be with those who are [12]." Laplace died in 1827, when Bernard was fourteen, and so, the kind of medicine they both knew must have been very similar. Bernard repeatedly explained that a rational therapeutic method was absolutely necessary in the clinic, this required a scientific pathology, and such pathology could only obtain the level of scientific knowledge when it could use both a scientific and experimental physiology. What then could count as to satisfy this conditions? Bichat's vitalism denied the regular or legal concatenation of biological phenomena, since the free action of an immaterial principle—the vital principle—was completely spontaneous. In his *Physiological Research into Life and Death,* Bichat had been very clear: "...given that the essential characteristic of vital properties is *instability,* and that all the vital functions are susceptible to a

multitude of varieties, nothing on these phenomena can be predicted or calculated [13]." Post-vitalist histology, on the other hand, interpreted cellular theory as an unjustifiable deviation, connected to *Naturphilosophie* and subject to a means of research—the microscope—which was considered to be unreliable by the Academy of Medicine in Paris. Finally, Magendie's empiricism, an enemy of hypotheses and theories, opposed to reasoning becoming a part of observation, could be defined by a sentence which Magendie often repeated: "When I experiment, I have only my eyes and my ears, I have absolutely no brain [14]." In this way, Bernard had to build his physiology within a confused atmosphere of biology without hypotheses, without laws, without microscopes and, as if not enough, without brains. He had a double task before him: first, to uncover the weaknesses of the schools, currents of thought and traditions mentioned above; and then, to give a detailed account of what would be the methodological and epistemological guarantees of physiology in the future. Now we can see that he completed both tasks using an approach, perspective and proposals which were significantly Popperian. We will also see that he did so by returning to the old truths of Descartes and iatromechanics: The certainty that physiological processes were subject to laws, in the same way as all the other phenomena in nature; the conviction that such laws could be established; and the certitude that the way in which this could be achieved was through the application of the experimental method.

THE EPISTEMOLOGICAL BET

Claude Bernard saw in the vitalist physiology of the School of Montpellier a dogmatic system which was immune to critical examination. He thought that it was, in his own words, putting forward a lazy doctrine, given that it left scientist defenseless, transforming metaphors into substantial entities and physiology into metaphysiology [15]. "All *a priori* conceptions of life, regardless of whether life is considered to be a *principle* or a *result*, only give rise to insufficient definitions; and this is as it should be, given that the phenomena of life can only be known *a posteriori*, as with all the other phenomena of nature [16]." As he understood it, we are faced with genuine scientific hypotheses when we work with explanatory proposals which are subjected to experimental checking; otherwise, we get lost in the corridors of systems if we make hypotheses immutable. Bernard defined for himself the objective of establishing a frontier between speculative physiology and scientific physiology, between physiology and pseudophysiology. He considered that only the physical and chemical conditions of biological phenomena appeared in the direct area of our experience. These are what we can get to know and which, far from spontaneity, present a regular course, a legal reiteration. On one side of the boundary or frontier

that he wanted to draw were systems and doctrines, in other words, the regions of dogmatism; on the other side were theories. In a similar context, Popper wrote: "Einstein was looking for crucial experiments whose agreement with his predictions would by no means establish his theory; while a disagreement, as he was the first to stress, would show his theory to be untenable. This [...] was the true scientific attitude. It was utterly different from the dogmatic attitude... [17]" He then added: "Thus I arrived at [...] the conclusion that the scientific attitude was the critical attitude, which did not look for verifications but for crucial tests; tests which could refute the theory tested, though they could never establish it [18]." And together with these ideas on theories, it was not long before the question of the progress of knowledge was brought up. Popper's approach is well-known; I reproduce it here as it is formulated in one of the conferences collected in *Objective Knowledge:* "...the progress of knowledge is from old problems to new problems, by means of conjectures and of critical attempts to refute them [19]." Bernard also wanted to eliminate the intromission of systems in physiological research, and he presented what deserves to be called his "criterion of demarcation." Like Popper, he considered the progress of knowledge to be subject to the use of this criterion; at the same time, he rejected its possible identification with empirical confirmation:

> The main differentiating character of a theory, as opposed to a system or doctrine, is that a theory is always open to the progress obtained from experience. A theory is only considered immutable by hypothesis and with the purpose of claiming contradictory facts so as to see whether it will resist or succumb. A theory will last so long as it resists experience; it can be modified and changed until such a time as it is conquered by facts [...]. Progress, therefore, consists of eliminating theories. A systematic scientist [...] endeavours to save his system; unlike an experimental scientist, who is happy to change his opinion, he is indignant if he has to do so. On the other hand, a theory which is substituted dies on the field of honor; the theory has reclaimed the new facts which have killed it, but it has made science move forwards [20].

To sum up, Bernard, denied the right of the clairvoyant and doctrinarian expert to be received in the public forum of scientific debate, where he had to argue in favor of the aptitude or efficiency of hypotheses. He stated this at the end of his *Introduction to the Study of Experimental Medicine:* "...the experimental method [...] is impersonal, it destroys individuality... [21]," but he could well have used different words, such as those from *The Open Society and its Enemies:* "... 'scientific objectivity' is not a product of the individual scientist's impartiality, but a product of the social or public character of scientific method... [22]" Visionaries no longer had a place in physiology; evenmore, those who considered themselves custodians of knowledge which cannot be modified, that is absolute and definitive and, especially, which cannot be proven should not be admitted. According to

Bichat, physical properties were different from vital manifestations; it could even be said that they are opposed to the phenomena of life. According to Bernard, it was necessary to abandon the idea that there was an antagonism between the general exterior forces and the interior vital forces of organisms [23]:

> I recognize [...] special procedures of manifestation in vital phenomena [...]. In fact, in living organisms there are anatomical apparatus or organic instruments which belong to them and which cannot be reproduced outside of them; but the phenomena manifested by these living organs or tissues do not, however, have anything special in either their nature, nor in the laws which govern them... [24]

He believed that physiology, pathology and medicine would move on from a period of darkness and enter into a period of maturity if the environment of research and action in physics and chemistry were to be accepted. In this environment, ideas and theories are transitory conceptual assumptions whose life is necessary temporary and which will have to be sacrificed as knowledge evolves. On the other hand, the immunizing strategies against rational criticism in science should be stopped:

> ...when we have formulated an idea or a theory in science, our aim should not be to conserve it, looking for facts to support it and ignoring everything that might disprove it. Quite the reverse, we should examine those aspects which appear to disprove our theory with the greatest care, given that true progress always consists of replacing an old theory with a new one which includes more aspects or facts. This is proof of progress because, in science, the significant precept is to modify and change ideas as we advance. [...] In experimental sciences [...] as truths are always relative, it is only possible to advance through revolution and through the absorbing of the old truths into a new scientific form [25].

The Popperian variation for this same subject sounds very similar; if we look, for example at *Conjectures and Refutations*, we find:

> ...the rationality of science lies not in its habit of appealing to empirical evidence in support of its dogmas [...] but solely in the *critical approach:* in an attitude which, of course, involves the critical use [...] of empirical evidence (especially in refutations). For us, therefore, science has nothing to do with the quest for certainty or probability [...]. We are not interested in establishing scientific theories as secure, or certain, or probable. Conscious of our fallibility we are only interested in criticizing them and testing them, hoping to find out where we are mistaken; of learning from our mistakes; and if we are lucky, of proceeding to better theories [26].

Experimental physiology, as conceived by Bernard, carried on from the ambitious plan of mechanical biology of the seventeenth century, transferred to chemical biology. The route that Descartes and Galileo's friends

and disciples had wanted to mark out for biology became blocked when the physics upon it was based failed to support it. Bernard showed that this stranded biology could continue its old course. Bordeau or Bichat's vitalist systems were the dogmatic systems of the iluminated, protected against any demand of empirical control, which made vital phenomena the spontaneous and unpredictable and sovereign manifestation—as we have seen—of a hidden principle; systems which had been supported by the palpable insufficiencies of the premature program put forward by the iatromechanics. Then halfway through the nineteenth century things were very different. Physiological research could establish ties with physics through calorimetry, energetics or thermodynamics, as well as being able to consider specialised physiological chemistry. This is the period of Berzelius, Helmholtz, Mayer, Magnus, Ludwig, Liebig, Pflüger and Frankland. Progress in laboratories was by then a fact. Bernard himself had successfully solved basic problems of physiological activity. It is the beginning of a period of generalized enthusiasm, of unlimited expectations in the explanatory capacity of the science of biological functions. The physiologist seemed to be responsible for aspiring to, and finally achieving, truth. Popper would have had his doubts about such epistemological optimism. We may remember his words in "Back to Presocratics": "...our attempts to see and to find the truth are not final, but open to improvement; [...] our knowledge [...] is conjectural; [...] it consists of guesses, of hypotheses, rather than of final and certain truths; [...] criticism and critical discussion are our only means of getting nearer to the truth [27]." Bernard already had warned his contemporaries in very similar terms:

> With the help of experiments we analyse, we disassociate phenomena in order to reduce them to the simplest relationships and conditions [...]. Despite our efforts, we are still a long way from truth [...], and it is probable [...] that we will be unable to see the naked truth. But this should not put us off because we are getting closer to the truth every day... [28]

The critical revision made by Claude Bernard to the suppositions of vitalism—one of the three currents he had to answer to—was, in conclusion, decidedly Popperian. Bernard realized that it was absolutely necessary to draw a dividing line between science and pseudoscience; he tied theoretical change to the replacement of old theories for new ones, and he proposed, therefore, a notion of the progress of knowledge which depended on innovation and which was distant from accumulated growth. In one of his notebooks he wrote: "Science does not grow in a regular and successive way, it advances in fits and starts. The changes in theories are what mark these jumps forward [29]." Popper would certainly have agreed. We should now evaluate what was Bernard's relationship with histological positivism, the school which required him to explain his posture.

In philosophy—and also in the philosophy of science—the admission of one's own mistakes is an infrequent occurrence; even more infrequent is the recognition of one's own crimes. Popper is, therefore, a notable exception. No one who has read *Unended Quest*—and, more specifically, the chapter entitled "Who killed Logical Positivism?"—can forget the complacency with which he admits his participation in this assassination [30]. It is true that he admits that he did not act alone, but his confession leaves us in no doubt. After asking himself, "who has done it?", he replies: "I fear that I must admit responsibility. Yet I did not do it on purpose [31]." It is surprising that in this too Bernard was ahead of him. It was he who killed *histological positivism* or, to be fairer, he was one of the people who were mainly responsible for its disappearance. But there is more to it: the motives for the crime and the murder weapon were, likewise, totally Popperian. Bernard killed histological positivism with a metaphysical research program, as long as positivist histology prevented the creation of hypotheses around the real morphological-functional constitution of living beings. He never intended to give an opinion on the essential nature of life. The rejection of essentialist explanations meant that he could not accept Bichat's attitude, but he understood that between Bichat's essentialism and Robin's phenomenism there was a third position: it was possible to test theories which, without offering final explanations, would be concerned with the real anatomical ordering and the real physiological activity presented by organisms—whilst recognizing, as we have seen, that these would always be precarious and partial approximations to the truth. This distancing from essentialism has at times be interpreted as an adhesion to positivism, when, in fact, Bernard had to fight against both equally fiercely [32]. In a letter to Mme. Raffalovich—an admirer and friend—he stated firmly: "I find you too enthusiastic about narrow and hollow concepts adorned with the name positivism [33]." Popper also suffered from this misunderstanding, as we know from his own words: "... philosophers in England and America—he remembered—seem to have taken me for a logical positivist—or at best for a dissenting logical positivist [34]." Whatever the case, what is interesting is that they both share a realist position which is shown in a similar way.

In "A note on Berkeley as to Mach and Einstein [35]," Popper classifies essentialism as unsustainable as it is concerned with a quest for final explanations, yet states that this condemnation does not mean an acceptance of positivism. He later explained in his autobiography how far he always felt from the phenomenism of Mach:

> On the philosophical merits of Boltzmann and Mach my judgement is frankly partisan. Boltzmann is little known as a philosopher; until quite recently I too knew next nothing about his philosophy, and I still know much less about it than I should. Yet with what I know I agree; more closely perhaps than with

> any other philosophy. Thus I greatly prefer Boltzmann to Mach [...]. Boltzmann and Mach both had a great following among physicists, and they were involved in an almost deadly struggle. It was a struggle over the research programme of physics, and over the 'corpuscular' hypothesis; that is, over atomism and the molecular or kinetic theory of gases and of heat. Mach was opposed to these 'metaphysical' hipotheses [...]; he hoped to extend the [...] 'purely descriptive' method to the whole of physics [36].

This was likewise Robin's purpose, a contemporary of Mach and—let us not forget—professor of histology in the University of Paris. He did not want to go beyond the mere histological description; he wanted to avoid—as Comte had advised—any hypotheses regarding the real composition of tissues and, more specifically, cellular hypothesis. In other words, Robin's phenomenism could not be reconciled with a hypothesis which referred to the basic units of biological systems, as neither was Mach's phenomenism reconcilable with any hypothesis on the elementary units of physical systems. Still, Bernard not only accepted cellular theory, he also helped more than most towards its consolidation within biology in the second half of the nineteenth century.

Cellular theory belongs to a metaphysical research program which initiated with the analytical anatomy and physiology of the seveteenth century. It was a program centred on the idea that the forms and functions we find in living beings are the product of the forms and functions of their primary units. At the heart of this program was the theory of fiber of the iatromechanics and the different versions of the cellular theory which arose during the XIX century [37]. Bernard did not accept cellular theory as being the path to the discovery of the essence of life; he considered such an approach inapplicable and useless. He thought, despite everything, that cellular microprocesses and cellular architecture would serve as part of the explanations which would legitimately account for the true structure of the tissues and their specific physiological activity. When, in 1855, Mathias Duval took over the Chair of Histology from Robin, histological positivism had its days numbered in Paris. Bernard was not the only person responsible for the death of histological positivism, but he was the most significant agent; nobody was as highly respected as he was, there was nobody as influential as he was in French physiology at that time. He said unambiguously:

> ... it has been generally established, thanks to the work of the histologists, that the organism is made up of groups of cells [...], modified on various levels, associated and assembled in different ways [...]. We have replaced [tissues] with a single element: the cell; it is identical in the two kingdoms, the animal and the vegetable kingdom; a fact which is demonstrated by the unity of the structure of all living beings [...]. It is this anatomical element which is the basis of all [biological] organization. It was a dream to try to define life [...], to try to

> penetrate into its essence [...]. However, either as an *independent being* or as an *anatomical element* of higher beings, the cell is, therefore, the simplest living form [...]. The law of *the construction of organisms and organic perfecting* is confused with the laws of *cellular life* [38].

Bernard showed that the dilemma between vitalist and positivist histology was false. There was more than a third path between essentialism and phenomenism: detailed and critical realism; the same thing that Popper would later recommend. What role should observation be given? What role should the relationship between theory and experience play? Strict empiricism—the third of the currents of thought he had to respond to—seemed to him narrow and distanced from true scientific practice. He insisted on the basic function of hypotheses and he rejected that a set of methodological steps could be found to guarantee the success of experimental research. "The experimental method—he stated—will not give [...] new, fertile ideas to those that do not have them; their only purpose will be to direct the ideas of those which have them... [39]" I will now give a brief summary of how he reacted to the obstinate faith in empiricism of his best teacher, François Magendie.

In a first instance, I must make a trivial observation. Bernard, who knew how much importance his teacher gave to observation, affirmed that experimental physiology, together with other experimental sciences, was not a Baconian product. In his opinion, there were two reasons for this: because he said he could not "accept that Bacon would have created a scientific philosophy for which we should have to feel grateful [40]," and because the experimental science movement was already in existence; to prove this, he had no more than to quote some of the scientists and philosophers who were contemporaries of Bacon, such as Kepler, Galileo, Boyle or Descartes. It is easy to think that he would unreservedly subscribed to the sentences Popper dedicated to Bacon in *The Logic of Scientific Discovery*, when he refers to "his myth of a scientific method that starts from observation... [41]" Bernard believed that Magendie was fooling himself when he supposed that, by means of a simple comparison, facts could explain themselves. He considered that uncontrolled speculation of the kind promoted by Romantic philosophy was responsible for the appearance of that kind of scientist who was closured by empiricism and who could not see in science any more than an accumulation of naked facts [42].

For Bernard, observations had a significant role to play in science during the phase in which we become familiar with a problem or when we are testing a hypothesis; such observations deserved the name 'scientific'. "Empiricism—he liked to say—is a narrow tower from which we can only escape using the wings of hypothesis [...]. Without hypothesis, in other words, without an anticipation of facts, there is no science [43]." This is one of the fragments I quoted at the beginning of this paper. Emphasizing this

still further, he continued: "the experimenter who does not know what he is looking for does not understand what he finds [44]." Hypotheses—which he sometimes denominates ideas *a priori* with the intention of emphasising that they do not come simply and immediately from experience—are made for observation, but do not stem from observation. They are the driving force behind experience, but they are not the effects of experience. "The scientist—he affirmed—throws out his hypotheses like a snail his antennae to feel out and touch space [45]." What is important is that if he runs into an obstacle, if nature replies "no", he should be willing to abandon the hypotheses. This image would have been a good way of illustrating Popper's suggestions in "The bucket and the searchlight" when he states: "We may characterize an expectation as a *disposition to react, or as a preparation for a reaction*, which is adapted to [or which anticipates] a state of the environment... [46]" In the other passages I quoted at the beginning of this paper, Bernard stated that "theories are no more than provisional ideas which we have about things in a particular state of our knowledge [47]." "We should always be ready to criticize a theory, we should always imagine that it is vulnerable; consequently, we should not believe [blindly] in any theoretical principle [48]." "When we make a general conception of science, the only thing we can be sure of is that all theories are false. They are no more than partial truths which we find necessary as they are like steps which we use to advance in our research... [49]"

We can see that these affirmations belong to a vision of science that is deliberately distanced from inductivism and the idea of growth or progress through simple accumulative enlargement. Inductivism had appeared during this period, not only through the influence of Magendie, but by way of many biologists who saw inductivism as the only possible way to fight against doctrinism and dogmatism. Schleiden—one of the most notable botanists of the first half of the century—published between 1842 and 1843 his *Principles of Scientific Botany*, a title which was later replaced by *Botany as Inductive Science* [50], in which he expressed the certainty that a scientific biology could only be inductive. Bernard reacted against this empiricism, considering it ingenuous. Moving away from dogmatism should not lead to physiology or medicine's returning to the time of its initial development—like hypocratic or Sydenham's empiricism—marked by an unquestionable observational approach. The development of scientific knowledge always requires a hypothesis, "in such a way that when we want to move from a particular case to a principle, in other words, when we think we are inducing, in fact what we are doing is deducing [52]." Nevertheless, our theories are instruments which serve simply to allow us to explore the world, and so "it is necessary to change them—he explained—when they have fulfilled their role, in the same way as we change a blunt scalpel when it has been used for a long time [53]."

Thus, anticipating Popper's statement, whereby "the alleged inductive method of science had to be replaced by the method of [...] trial and [...] error elimination, which was the mode of discovery of all organisms from the ameba to Einstein [54]," this eminent physiologist from the France of Napoleon III considered that the only method for experimental science was that which was based on hypothetical anticipation and on the negative responses of the world; he suggested that this kind of orientation in nature guides all living beings, although he was thinking of snails rather than amoeba, and of Virchow rather than Einstein.

Anyhow, both Bernard and Popper were aware that this was, more readily than a chosen method, the type of exploratory method being followed, yet which can never guarantee success. Bernard stated: "Method alone produces nothing and to give too much power to the method is a mistake which is made by certain philosophers [55];" Popper wrote along: "If anyone should think of scientific method as a way which leads to success in science, he will be disappointed. There is no royal road to success [56]." Popper's way of overcoming the problem of induction also endeavors to show the way towards a solution to the old problem of the rationality of our beliefs. This solution invites us to replace the idea of belief by the idea of action, where an action is considered to be rational if it is in accordance with the state of the critical, scientific discussion of the day. Rational action is the action which is chosen after criticism; to choose rationally is to choose critically [57]. If rationalism has to become an epistemological bet, it can only be understood as rationalism based on invention and criticism. Bernard had replied to the demands for empiricism made by Magendi and Schleiden with this same formula, with this selfsame reply: "Science—he says in *Principles of Experimental Medicine*—which is based on rationalism must, however, ensure that it does not fall into the trap of its own excesses. The secret consists in not allowing the hypotheses a free reign [58]." We have already seen that we can use the wings of hypothesis to escape from empiricism, but this flight must be restricted time and again by the responses we obtain from nature. The secret lies in a rationalism based simultaneously on invention and criticism.

CONCLUSION

In conclusion, the historical context in which Bernard had to define the conditions which would make possible an authentic experimental physiology has significant analogies with the intellectual environment in which Popper began to examine dogmatic thought; to design his new criterion of demarcation; to move away from positivism, and to resolve the problem of induction. Bernard had to fight against the dogmatic physiological system represented by vitalism; he was confronted by a powerful school

under the influence of Comte—the positivist school of histology—and he spent the most significant period of his training under the direct teachings of a radical empiricist. The challenges were parallel and the solutions coincident. It is true, of course, that Popper's philosophical activity goes much further than the methodological or epistemological concerns that he might share with Bernard, but it is also true that where their interests coincide, so do their attitudes. This latter has been the object of this paper.

Claude Bernard conceived the elimination of theories as being the basis for progress in knowledge, as long as theories can only be conceived through hypothesis and as their purpose is to find facts which contradict them; he explained that in experimental science, truth is always relative and it is only possible to advance by means of revolutionary ruptures; he suggested that the conquering of truth in absolute terms is impossible, although it is possible for us to get closer and closer to it; he was highly critical to the suppositions made by histological positivism in order to defend a theory on the real morphological-functional make up of living beings, to declare that descriptive histology was insufficient and to work within a metaphysical program of research; he believed that essentialism and phenomenalism could not monopolize all the options of the theory of knowledge; he confessed that it was not possible to define the methodological rules that would ensure success; he distanced himself from inductivism and all that Bacon represented; he affirmed that there could be no science without hypotheses, without the anticipation of facts; that hypotheses were based on free invention and that they were not produced spontaneously through observation; he saw the scientist launching his hypotheses and then waiting for the world to respond, much in the same way as a living being explores its surroundings through trial and error, and he maintained that rationalism, the effective ingredient of scientific knowledge, must always be dominated and guided by **c**riticism. Is it possible to think of anyone with better credentials than Bernard to be called unquestionably a Popperian scientist?

I should like to conclude with an amazing observation. It has always surprised me that in all his work Popper only mentions this eminent promoter of experimental physiology three times, if I am not mistaken. He does so in *The Myth of the Framework* [59], reproducing some words of Bernard which he has taken from an indirect source: Jacques Hadamard's book *The Psychology of Invention in the Field of Mathematics.* Hadamard quotes the following sentence from Bernard, that Popper repeats: "Those who have an excessive faith in their ideas are not well prepared for discoveries [60]." It is the only time that Bernard's words are included in a text written by Popper, who, nevertheless, names Bernard in another two places. When referring to the conviction, which has been shared by many scientists, that there exists a criterion of objective progress of knowledge, Popper states,

in a chapter in *In Search of a Better World:* "It is that simple and rational criterion that has dominated the development of the natural sciences since Copernicus, Galileo, Kepler and Newton, since Pasteur and Claude Bernard [61]." Finally, Bernard is alluded to in "Evolution and the tree of knowledge", in a footnote which Popper added when he revised the conference prior to its publication. The footnote is included when the author is about to argue that theory precedes experience, which is against what almost everyone else had thought, with the exception perhaps of Darwin and Einstein. The footnote reads as follows: "Sir Peter Medawar has pointed out to me that I ought to have mentioned here, besides Darwin and Einstein, Claude Bernard [62]." I agree with professor Medawar. I believe Bernard deserved to be recognized as a firmer, more exact and more complete representative of Popper's concept of scientific knowledge than either Darwin or Einstein.

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NOTES

1 Bernard, 1987, p. 77.
2 *Ibid*., p. 121.
3 *Ibid*., p. 253.
4 Bernard, 1966, pp. 70-71.
5 See the references at the end of this paper.
6 See Laín, 1978, p. 340.
7 Comte, 1893, p. 419.
8 Bernard, 1989, p. 155.
9 Bernard, 1885, p.184.
10 Bernard, 1865, p. 366.
11 Magendie's words are repeated in Bernard, 1857, pp. 12-13.
12 Bernard, 1947, p. 216.
13 Quoted in Bernard, 1885, p. 58.
14 Magendie's statements appear in Bernard, 1989, p. 105.
15 See Bernard, 1885, p. 57.
16 *Ibid*., p. 32.
17 Popper, 1976, p. 38.
18 *Ibid*.
19 Popper, 1972, p. 260.
20 Bernard, 1947, p. 121.
21 Bernard, 1966, p. 365.
22 Popper, 1945, p. 220.
23 Bernard, 1885, p. 29.
24 Bernard, 1989, pp. 157-158.
25 Bernard, 1966, pp. 78-79.
26 Popper, 1963, p. 229.
27 *Ibid*., p. 151.
28 Bernard, 1966, p. 99.
29 Bernrad, 1965, p. 173.
30 See Popper, 1976, pp. 87-90.
31 *Ibid*., p. 88.
32 Kolakowski and Coleman, together with others, think that Bernard developed a positivist physiology (see Kolakowski, 1981, pp. 94-99; and Coleman, 1983, pp. 258-265). Smith, on the other hand, thinks that he was a fervent materialist (Smith, 1976, pp. 291-294). There have even been people who consider him to be an Aristotlean (see Foulquié, 1954, p. 147). Bergson finally had to explain that despite the pretensions of some, Bernard had not been a vitalist scientist (see Bergson, 1970, pp. 1433-1440).
33 Quoted in Foulquié, 1954, p. 117.
34 Popper, 1976, p. 87.
35 Popper, 1963, pp. 173-174.
36 Popper, 1976, p. 157.
37 See González Recio, 1990, pp. 83-109. On the epistemological value of the theory of cells in successive versions, see González Recio, 1992, pp. 67-84.
38 Bernard, 1885, pp. 185-86, 193, 343 and 354. Bernard's italics.
39 Bernard, 1966, p. 68.
40 Bernard, 1947, p. 189.
41 Popper, 1959, p. 279.
42 See Bernard, 1989, pp. 104-122.
43 Bernard, 1947, p. 77.

44 Bernard, 1867, p. 185.
45 Bernard, 1947, p. 78.
46 Popper, 1972, p. 344.
47 Bernard, 1947, p. 121.
48 *Ibid*, p. 253.
49 Bernard, 1966, pp. 7071.
50 *Die Botanik als induktive Wissenschaft. Grundzüge der wissenschaftliche Botanik nebst einer Einleitung als Anleitung zum Studium der Pflanzen*, Leipzig, 1849-1850.
51 See Bernard, 1947, p. 4.
52 Bernard, 1966, p. 90.
53 *Ibid*, p. 79.
54 Popper, 1976, p. 52.
55 Bernard, 1966, p. 68.
56 Popper, 1972, p. 265.
57 Popper, 1976, p. 87.
58 See Bernard, 1947, pp. 53, 114-115.
59 Popper, 1994, p. 7.
60 Hadamard, 1945, p. 48.
61 Popper, 1984, p. 41.
62 Popper, 1972, p. 258.

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